ORGANIZATIONAL BEHAVIOR 3

HISTORICAL ORIGINS, THEORETICAL FOUNDATIONS, AND THE FUTURE

JOHN B. MINER
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JOHN B. MINER

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DEDICATION

To the memory of the intellectual leaders who made this book possible:

Chester I. Barnard
William J. Dickson
Henri Fayol
Mary Parker Follett
Kurt Lewin
Elton Mayo
Fritz J. Roethlisberger
Frederick W. Taylor
Max Weber
Building on Miner’s *Organizational Behavior: Foundations, Theories, and Analyses* (Oxford University Press, 2002) the M.E. Sharpe Organizational Behavior series consists of the following volumes—

1. Essential Theories of Motivation and Leadership (2005)
2. Essential Theories of Process and Structure (2005)
4. From Theory to Practice (2006)
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This treatment of organizational behavior presents a framework for viewing the world of organizations. It is a way in, a basis for orientation, an approach to getting one's bearings. Broadly speaking, the intended audience is anyone in search of knowledge or with special interests regarding the scientific foundations, early contributors, historical origins, status of theory, and future of the field of organizational behavior. Some of the ideas presented are original; many more derive from others. My intent is to provide an introduction for those who lack an extensive familiarity with how organizational behavior came to emerge as a field of specialization in the business schools, as well as a commentary on the present state and future directions of the field. I believe that even those who are conversant with the foundations and origins of the field will find new ideas and interpretations here.

Who then might be interested in reading this book? Certainly anyone who is considering studying organizational behavior, or who is engaged in such study, or who has studied in the field previously (especially where a major in organizational behavior is involved); this is true whether the study is at the undergraduate, masters, or doctoral level. Managers and administrators in any type of organization constitute a potential audience (given what we know about their concerns, this should be particularly true of those at the higher levels). Many professionals and scholars within organizational behavior or in related disciplines will find the book relevant. This includes people in industrial/organizational psychology, organizational sociology, economics, political science, cultural anthropology, human resources and industrial relations, educational administration, and public administration; it also encompasses contingent disciplines in the business schools, such as strategic management, entrepreneurship, and operations management, as well as those involved in areas that interact with organizational behavior there, such as accounting and marketing, and business school deans. Finally, those with a specialty in business or management history will find much of interest here.

To provide a better idea of whether in fact this is a book that would be of interest to you, and from which you might learn, let me outline the content in more detail.

In Part I the initial chapter deals with the relationships to practice and with the philosophy of science, especially theorizing. Chapter 2 focuses on theory in more detail. This is followed by a chapter on the conduct of research and another dealing with the way scientific knowledge accrues in a field such as organizational behavior. Part I concludes with a treatment of the historical course that idea-development has followed, starting with philosophy and ending with the embedding of scientific theory in organizational behavior. The discussion throughout this part is intended to show how the field has moved to become a science and what specific meaning this process carries.

Part II takes up the sets of ideas that predated the emergence of organizational behavior, and the people who espoused them—Elton Mayo and those at Western Electric (including William Dickson) and the Harvard Business School (including Fritz J. Roethlisberger) who worked with him; Chester Barnard at New Jersey Bell and at Harvard; Kurt Lewin, the
psychologist, at universities in Germany and the United States; Mary Parker Follett, who had a career as a social worker in Boston and later as a speaker; Max Weber, the legal scholar in Germany, who became widely recognized as a sociologist; Henri Fayol, the French manager; and Frederick Taylor, the Philadelphia engineer and management consultant. The treatment in Part II is historical, an attempt to position these diverse people in terms of their impact on the field of organizational behavior as it came into being and on the vitality they gave to its theories.

Part III considers the actual emergence of organizational behavior against the background of the environmental forces that impinged upon it. First were several reports on the business schools that gave major consideration and impetus to what ultimately was called organizational behavior. Second was the recruiting of social scientists into the business schools. Third was the strong influence that psychology exerted on the emergence of organizational behavior. Fourth was the prevailing climate of uncertainty that existed both within the business schools of the time and in the outside environment that affected them. Fifth was the creation of an intellectual environment that commingled ideas from numerous disciplines, illustrated in this instance by a discussion of the ideas that influenced my own theoretical work.

In Part IV, I present a set of research studies intended to assess the status of organizational behavior theory over the period since the field’s beginning. This presentation follows the outline typically characteristic of research treatments in the social science literature and draws heavily upon the material considered in Chapter 3. Chapters 17 and 18 take up a study concerned with the importance, validity, and usefulness of the various theories. Chapters 19 through 21 delve into the institutionalization of these same theories. This latter research is prefaced with a treatment of institutional theory as it relates to theories generally, and of conformity theory from psychology. The methodology of this research and the findings are new and have not been published previously.

Part V takes the history presented in the preceding four parts and maps it onto identity theory to create a picture of organizational behavior’s identity. This picture is then used in the final chapter to project my vision for organizational behavior’s future. This vision is predicated on an expectation of continued growth—into practical applications, and derived from disciplinary consolidations. It makes a case for the synergistic expansion of both MBA and PhD programs.

At several points in this book the theories developed in organizational behavior are given attention, but more as to their overall effectiveness and impact than with regard to the specifics of each theory and the research on each. A comprehensive discussion of the individual theories, however, is presented in two other books of this series containing theoretical statements, relevant research, practical applications, and a critical evaluation theory by theory. These books are titled Essential Theories of Motivation and Leadership and Essential Theories of Process and Structure.

Although the structure of each chapter in the following is typically determined by its unique content, certain features are retained throughout. All chapters are introduced with an outline covering the various headings of the chapter. This outline provides a road map facilitating progress through the discussion, and serves as a guide to finding a way out should the reader get lost en route.

Each chapter, and the part introductions, contains a listing of references employed in the presentations. These are used partly to document statements made in the text, but they also provide sources to use for follow-up should the reader wish to learn more about a particular
subject. The total number of these chapter-end references amounts to almost 800. As might be expected the recency of these references varies with the recency of the material covered. Thus parts II and III, dealing with the historical origins, contain many historical references, of which only 25 percent date from 2000 on. However, parts I and IV contain references published in 2000 or later at rates in the lower range of 40 percent, and in Part V, dealing with the here and now and with the consequences of history, 70 percent of its references are from 2000 and later. In spite of the historical emphasis, this volume is thoroughly up to date.

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A major debt in preparing this book is acknowledged in the dedication. Without the efforts of these forebears, the field of organizational behavior, and this book, may well have never existed.

I am also indebted to Oxford University Press for giving me permission to use material from *Organizational Behavior: Foundations, Theories, and Analyses* wherever in the present volume it proved appropriate. Harry Briggs at M.E. Sharpe has shown himself to be both a very helpful person and a highly proficient editor; it has been a pleasure to work with him. Amy Odum, my project editor, has been equally helpful.

Finally, in the absence of any university support, my wife, Barbara, has taken on all of the numerous tasks involved in the preparation of this book, other than writing it. I thank her not only for her dedication and efficiency, but for her support and love.
What is organizational behavior? It is a social science discipline—much like cultural anthropology, economics, political science, psychology, and sociology. This means that it utilizes the scientific method to establish truth and to validate its theories. It is a discipline that historically has had its intellectual home in business schools. It is a new discipline relative to the other social sciences, having its origins in the mid-twentieth century. The key points are that it is a science and that it has a history, which, though short, has been quite turbulent.

Although the exact boundaries of the discipline are somewhat fuzzy (see Blood 1994), organizational behavior’s focus is clearly on the world of organizations. The concern is first with the behavior and nature of people within organizations, and second with the behavior and nature of organizations within their environments. The term organizational behavior initially referred only to the behavior and nature of people in organizations. Given the fuzziness of its boundaries, the discipline always had a tendency to stretch beyond that domain, however. By the time it approached twenty-five years of age it clearly had staked a claim to incorporating the behavior and nature of organizations as well (see Miles 1980). This is historically consistent in that both the study of the behavior and nature of people and the study of the behavior and nature of organizations emerged in the business schools in the same places at the same times.

In line with its professional school origins, organizational behavior is an applied discipline, concerned with matters of practice and application. Despite this orientation, it currently has relatively few members who actually devote their primary professional efforts to the practice of organizational behavior in business and other organizational settings; rather, most are concentrated in academia—teaching, writing, and conducting research. In my opinion this is unfortunate; the field would be better off not by reducing its academic efforts, but by expanding its practitioner efforts. We will return to this theme in various ways throughout this book.

Several other terms have become intertwined with organizational behavior over the years, although none has achieved quite the same level of acceptance. One is organization theory, which has come to refer almost exclusively to the study of the behavior and nature of organizations in their environments. A second is organization(al) science, which appears to cover
essentially the same ground as organizational behavior, and which in many respects I prefer as a designation for our field (see Miner 1984). However, right now organizational behavior has won the day. Finally, there is the term organization studies, which also has a broad connotation extending, at least in the recent period, beyond the science of organizations to incorporate several different philosophic positions (see Clegg, Hardy, and Nord 1996).

Having explained what organizational behavior is, I need to say something about what it is not. It is not strategic management, a field that has emerged and achieved stature more recently than organizational behavior (see Schendel and Hofer 1979) and has differentiated itself at the border that previously existed between organizational behavior and economics, borrowing from and overlapping with each. Furthermore, organizational behavior is not economics, although in recent years there has been some confounding of the two fields and there are those who foresee a possible future takeover of organizational behavior by economics (see, e.g., Pfeffer 1995). There is even some theory as to how this takeover might come about (Ferraro, Pfeffer, and Sutton 2005). However, economics was well established in business schools long before organizational behavior arrived, and organizational behavior was spawned, in large part at the behest of economists, as a separate and distinct discipline. Historically, the two are clearly different entities with very different origins.

Finally, organizational behavior is not philosophy. That, however, is a rather complex story. As a science our field is closely tied to, though separate from, the philosophy of science. In this respect it is like all other sciences, and the relationship can be expected to continue as long as organizational behavior defines itself as a social science. But philosophy has been threaded into organizational behavior in other respects as well from the very beginning, not always to the benefit of either field. Sometimes, in the hands of certain individuals, organizational behavior and philosophy have become almost indistinguishable from one another. Understanding what is involved here requires a background in the nature of science, scientific theory, scientific research, and in the history of science—in short in the scientific foundations of the field. It also requires a background in the ways in which philosophy has become threaded into organizational behavior at various points in time. These matters are considered in the chapters of Part I.

The primary focus of this book, however, is on the heritage of organizational behavior, both within science and within the historical roots of the field. In point of fact, we all participate in various organizations such as schools, companies, and hospitals throughout our lives, and we devote a large percentage of our time to such participation. Most people would like to function more effectively in organizations and to contribute to more effective functioning of the organizations themselves. It seems logical that the more we know about organizations and the way they operate, the better our chances of coping with them adequately and of achieving our own goals within them and for them. Giving us this knowledge is what organizational behavior aims to do.

In writing this book I attempt to establish the Zeitgeist, the intellectual climate, within which organizational behavior emerged and gained acceptance. An idea too outlandish even to be considered at one point in time in one cultural context may win wide acclaim later after times have changed (Boring 1950). Thus, an understanding of the backgrounds out of which ideas came to the fore, and of the values that nurtured and thwarted them, is crucial to a real appreciation of any field. The context that spawned organizational behavior, including certain ideas and theories that were already on the scene under other disciplinary names, is therefore of prime importance in understanding the true meaning of what the field has become.

As a foundation for understanding what is involved here, it is essential to know what scien-
tific theory is and what it is not, as well as how theory relates to research, and where organizational behavior stands relative to the dictates of science. These are the concerns of Part I.

REFERENCES


Theory and Practice
Science Defined
The Role of Theory in Science
Theory Defined
How Theory Works
Assumptions of Science
Rules of Scientific Inquiry
Conclusions

Theory is the cornerstone of any science. It provides the ideas that fuel research and practice. Theories of organizational behavior are potentially as useful when applied to organizations as theories of physics and chemistry are when used in developing new manufacturing technologies and consumer products, or theories of biology are in advancing medical practice. However, the relationship between theory and practice (or application, or usefulness) in organizational behavior is often misunderstood. For many people the term theory evokes images of a speculative, ivory-towered world, far removed from reality.

THEORY AND PRACTICE

Theories do not sound helpful in understanding the practical facts of organizational life. Yet one hears such statements as that of the eminent psychologist Kurt Lewin (1945), who said that “nothing is so practical as a good theory.” And this dictum has continued to receive widespread acceptance over the years since (see, e.g., Van de Ven 1989 and more recently, Lundberg 2004).

Confusion on this score is in fact widespread; the subject requires consideration here at the outset because a particular reader’s preconceptions regarding the theory-practice relationship (or the lack thereof) can color that person’s thinking about the entire field. The idea that theory is somehow “ivory tower” while practice is “real world”— and that the two are distinct and separate— has often permeated discussions of business school education and of the role of the organizational behavior discipline (Raelin 1993; Wren, Buckley, and Michaelson 1994), and it continues to do so (Das 2003; Donaldson 2002). Even attempts to clarify this situation seem to lead only to deepening confusion (Brief and Dukerich 1991).

What then is the state of the situation at the interface between academic theory and research, and the world of application? What do studies tell us? One of the most comprehensive of such studies deals with the research knowledge, much of it theory based, of human resource (HR) managers (Rynes, Brown, and Colbert 2002; Rynes, Colbert, and Brown 2002). This investigation indicated that these managers were not very knowledgeable regarding the research evidence; they were only neutral on the value of research findings for practice, and most read very
little in the research literature. Yet those few who were more conversant with the research worked for more financially successful companies. A difficulty appears to be that many HR managers rely almost entirely on the popular press for knowledge input (Mazza and Alvarez 2000), and often get wrong information from such sources. Not surprisingly, the popular press tends to pick up on temporary fads and fashions that are “hot” at the time, many of which are simply recycled versions of old ideas that had been discarded (Spell 2001).

Another study, focused on a specific theory, failed to find evidence of an understanding of this theory among managers, although MBA students were better informed (Priem and Rosenstein 2000). Thus, practicing managers could not go in the directions prescribed because they lacked the knowledge to do so. Although value and motivational differences are involved here (see Brooks, Grauer, Thornbury, and Highhouse 2003), this in itself would not logically account for the academic-managerial gap found; the problem appears to be in not going to appropriate sources of information (Roehling, Cavanaugh, Mynihan, and Boswell 2000). Research to this effect continues to mount with additional study (Nowicki and Rosse 2002).

The data thus seem to indicate a substantial gap between theory and perceived usefulness in practice. Yet there are reasons to believe that this gap can be reduced under appropriate circumstances (Rynes, Bartunek, and Daft 2001). One objective of this volume is to facilitate this process and accordingly to narrow the gap so that practitioners will come away with a greater appreciation of the value that organizational behavior theory can bring to practice. Examples of recent academic-practitioner collaborations on research studies (Ford, Duncan, Bedeian, Ginter, Rousculp, and Adams 2003; Rynes and McNatt 2001) and of increasing concern about linking theory to practice (Cooper and Locke 2000) give reason for optimism in this regard; so, too, does the recent finding that there is a positive correlation between a managerial panel’s assessment of practical relevance and an objective index of the academic quality of published articles (Baldridge, Floyd, and Markóczy 2004). The publication of a special issue of Human Resource Management dealing with the contributions of theory and research to closing science-practice knowledge gaps represents a particularly fortuitous sign (Burke, Drasgow, and Edwards 2004).

In this context let me return to Lewin’s (1945) dictum. What Lewin meant by a good theory is one that is validated by adequate research. To be truly useful, a theory must be intimately intertwined with research, and to the extent that it is, it has the potential for moving beyond philosophic speculation to become a sound basis for action. Good theory is thus practical because it advances knowledge in a field, guides research to important questions, and enlightens practice in some manner (Van de Ven 1989).

Some theories are obviously more concerned with application than others. Some, at the time of inception, may fail to meet the test of usefulness, only to find their way to a juncture with practice later on. Some theories are never tested, or fail the test of research, and they are not very good theories, at least as far as anyone can tell. In any event a good theory has the potential for valid applications and thus can prove useful if correctly applied. A theory in an applied field, such as organizational behavior, that is so divorced from application (so ivory tower?) that it has no potential for speaking to practice is very unlikely to be a good theory. Yet, one must recognize that some of organizational behavior’s premier theorists eschew practical relevance in favor of knowledge for its own sake (see, e.g., March 2003), and some from organizational behavior who have moved to the world of practice find little of value in the theories of their field (Greiner 2002, on Steve Kerr). Unanimity here is hard to come by.
SCIENCE DEFINED

Keeping in mind this desideratum on relationships to practice, let us turn now directly to the matter of science. Science is an enterprise by which a particular kind of ordered knowledge is obtained about natural phenomena by means of controlled observations and theoretical interpretations. Ideally, this science, of which organizational behavior is a part, lives up to the following:

1. The definitions are precise
2. The data-collecting is objective
3. The findings are replicable
4. The approach is systematic and cumulative
5. The purposes are understanding and prediction, plus, in the applied arena, control (Berelson and Steiner 1964).

The usually accepted goals of scientific effort are to increase understanding and to facilitate prediction (Dubin 1978). At its best, science will achieve both of these goals. However, there are many instances in which prediction has been accomplished with considerable precision, even though true understanding of the underlying phenomena is minimal; this is characteristic of much of the forecasting that companies do as a basis for planning, for example. Similarly, understanding can be far advanced, even though prediction lags behind. For instance, we know a great deal about the various factors that influence the level of people’s work performance, but we do not know enough about the interaction of these factors in specific instances to predict with high accuracy exactly how well a certain individual will do in a particular position.

In an applied field, such as organizational behavior, the objectives of understanding and prediction are joined by a third objective— influencing or managing the future, and thus achieving control. An economic science that explained business cycles fully and predicted fluctuations precisely would represent a long step toward holding unemployment at a desired level. Similarly, knowledge of the dynamics of organizations and the capacity to predict the occurrence of particular structures and processes would seem to offer the possibility of engineering a situation to maximize organizational effectiveness. To the extent that limited unemployment or increased organizational effectiveness are desired, science then becomes a means to these goals. In fact much scientific work is undertaken to influence the world around us. To the extent applied science meets such objectives, it achieves a major goal.

THE ROLE OF THEORY IN SCIENCE

Scientific method evolves in ascending levels of abstractions (Brown and Ghiselli 1955). At the most basic level it portrays and retains experience in symbols. The symbols may be mathematical, but to date in organizational behavior they have been primarily linguistic.

Once converted to symbols, experience may be mentally manipulated, and relationships may be established.

Description utilizes symbols to classify, order, and correlate events. It remains at a low level of abstraction and is closely tied to observation and sensory experience. In essence it is a matter of ordering symbols to make them adequately portray events. The objective is to answer “what” questions.
Explanation moves to a higher level of abstraction in that it attempts to establish meanings behind events. It attempts to identify causal, or at least concomitant, relationships so that observed phenomena make some logical sense.

**Theory Defined**

At its maximal point, explanation creates theory. Scientific theory is a patterning of logical constructs, or interrelated symbolic concepts, into which the known facts regarding a phenomenon, or theoretical domain, may be fitted. A theory is a generalization, applicable within stated boundaries, that specifies the relationships between factors. Thus, it is an attempt to make sense out of observations that in and of themselves do not contain any inherent and obvious logic (Dubin 1976). The objective is to answer “how,” “when,” and “why” questions.

Since theory is so central to science, a certain amount of repetition related to this topic may be forgiven. Campbell (1990) defines theory as a collection of assertions, both verbal and symbolic, that identifies what variables are important for what reasons, specifies how they are interrelated and why, and identifies the conditions under which they should be related or not. Sutton and Staw (1995) place their emphasis somewhat differently, but with much the same result. For them, theory is about the connections among phenomena, a story about why acts, events, structure, and thoughts occur. It emphasizes the nature of causal relationships, identifying what comes first as well as the timing of events. It is laced with a set of logically interconnected arguments. It can have implications that we have not previously seen and that run counter to our common sense.

**How Theory Works**

Figure 1.1 provides a picture of the components of a theory. A theory is thus a system of constructs and variables with the constructs related to one another by propositions and the variables by hypotheses. The whole is bounded by the assumptions, both implicit and explicit, that the theorist holds with regard to the theory (Bacharach 1989).

Constructs are “terms which, though not observational either directly or indirectly, may be applied or even defined on the basis of the observables” (Kaplan 1964, 55). They are abstractions created to facilitate understanding. Variables are observable, they have multiple values, and they derive from constructs. In essence, they are operationalizations of constructs created to permit testing of hypotheses. In contrast to the abstract constructs, variables are concrete. Propositions are statements of relationships among constructs. Hypotheses are similar statements involving variables. Research attempts to refute or confirm hypotheses, not propositions per se.

All theories occupy a domain within which they should prove effective and outside of which they should not. These domain-defining, bounding assumptions (see Figure 1.1) are in part a product of the implicit values held by the theorist relative to the theoretical content. These values typically go unstated, and, if that is the case, they cannot be measured. Spatial boundaries restrict the effective use of the theory to specific units, such as types of organizations or kinds of people. Among these, cultural boundaries are particularly important for theory (Cheng, Sculli, and Chan 2001). Temporal boundaries restrict the effective use of the theory to specific time periods. To the extent they are explicitly stated, spatial and temporal boundaries can be measured and thus made operational. Taken together, they place some limitation on the generalizability of a theory. These boundary-defining factors need not op-
erate only to specify the domain of a theory, however; all may serve in stating propositions and hypotheses as well. For example, time has recently received considerable attention as a variable that may enter into hypotheses (George and Jones 2000; Mitchell and James 2001).

Organizational behavior has often been criticized for utilizing highly ambiguous theoretical constructs—it is not at all clear what they mean (see, e.g., Sandelands and Drazin 1989). This same ambiguity can extend to boundary definitions and domain statements. In a rather cynical vein, Astley and Zammuto (1992) even argue that this ambiguity is functional for a theorist in that it increases the conceptual appeal of a theory. Conflicting positions do not become readily apparent and the domain of application may appear much greater than the empirical reality. Such purposeful ambiguity creation can cause the constructs and ideas of a theory to be extended into the world of practice to an extent that is not empirically warranted. Not surprisingly these views immediately met substantial opposition (see, e.g., Beyer 1992). The important point, however, is that science does not condone this type of theoretical ambiguity. Precise definitions are needed to make science effective (Locke 2003), and a theory that resorts to ambiguity is to that extent a poor theory.

ASSUMPTIONS OF SCIENCE

Science must make certain assumptions about the world around us. These assumptions might not be factually true, and to the extent they are not, science will have less value. However, to the extent science operates on these assumptions and produces a degree of valid understanding, prediction, and influence, it appears more worthwhile to utilize the assumptions.

Science assumes, first, that certain natural groupings of phenomena exist, so that classification can occur and generalization within a category is meaningful. For some years, for instance, the field then called business policy, operating from its origins in the case method, assumed that each company is essentially unique. This assumption effectively blocked the
development of scientific theory and research in the field. Increasingly, however, the assumption of uniqueness has been disappearing, and generalizations applicable to classes of organizations have emerged (see, e.g., Steiner and Miner 1986). As a result, scientific theory and research are burgeoning in the field of strategic management.

Second, science assumes some degree of constancy, or stability, or permanence in the world. Science cannot operate in a context of complete random variation; the goal of valid prediction is totally unattainable under such circumstances. Thus, objects and events must retain some degree of similarity from one time to another. In a sense this is an extension of the first assumption, but now over time rather than across units (see McKelvey 1997 for a discussion of these premises). For instance, if organizational structures, once introduced, did not retain some stability, any scientific prediction of their impact on organizational performance would be impossible. Fortunately, they do have some constancy, but not always as much as might be desired.

Third, science assumes that events are determined and that causes exist. This is the essence of explanation and theorizing. It may not be possible to prove a specific causation with absolute certainty, but evidence can be adduced to support certain causal explanations and reject others. In any event, if one does not assume some kind of causation, there is little point in scientific investigation; the assumption of determinism is what sparks scientific effort. If, for instance, one assumes that organizational role prescriptions do not influence individual performance, then the whole area of organizational design moves outside the realm of scientific inquiry. Organizational behavior must assume some kind of causal impact of the organization on its members. It then becomes the task of science to determine the nature of this impact.

Finally, because science is firmly rooted in observation and experience, it is necessary to assume some degree of trustworthiness for the human processes of perceiving, remembering, and reasoning. This trustworthiness is always relative, but it must exist to some degree. The rules under which science operates are intended to increase the degree of reliability with which scientific observation and recording operate. The purpose is to achieve an objective, rational, replicable result that will be convincing to those who are knowledgeable in the area of study.

RULES OF SCIENTIFIC INQUIRY

First, if the findings of research are to be replicated and the generalizations from research are to be valid, concepts must be clearly defined in terms of the procedures used to measure them. This has been a problem in the field of organizational behavior. On occasion, theoretical concepts are stated in such an ambiguous manner and the conditions for their measurement left so uncertain that the researcher is hard put to devise an adequate test of a theory.

Second, scientific observation must be controlled so that causation may be attributed correctly. The objective is to be certain that an outcome is in fact produced by what is believed to produce it and not by something else. Control of this kind is achieved through the use of various experimental designs, or through measurement and statistical adjustment as discussed later in Part I. In the complex world of organizational functioning, establishing controls sufficient to pin down causation has often proved to be difficult.

Third, because science is concerned with generalization to contexts that extend far beyond a given experiment, it is essential that research utilize samples that are adequate in both size and conditions of their selection. One must have confidence that the results obtained are
generalizable and can be put to use outside the research situation. The field of statistics becomes important for organizational behavior because of its potential for determining how much confidence can be placed in a particular research outcome.

Fourth, and this bears repeating, science requires that its propositions, hypotheses, and theories be stated in terms that can be tested empirically. This is where philosophy and science part company. Unfortunately, in the past, organizational behavior has not always clearly separated scientific from philosophic statements. The result has been considerable confusion, and on occasion effort has been wasted on attempts to test theories that are not really testable as stated. Bacharach (1989) provides a good discussion of this falsifiability requirement.

These rules of scientific inquiry serve to influence research design and the conduct of research. McKelvey (1997) labels the rules as indicative of an “organizational realist” approach to organizational behavior.

CONCLUSIONS

The philosophy of science as set forth here places considerable emphasis on the role of theory. The reason is that although quantum leaps in science are very rare in any event, they are only possible if theory provides the opportunity. Organizational behavior has had its share of theories, and enough of these have proved useful to move the field forward quite rapidly. However, it is important to understand that further progress requires more good theories, and these will be created only if the field fully recognizes what theory is and how it operates. Yet, theory only becomes useful if it is validated by research. Managers should not accept theories and apply them to their work unless there is reason to believe that the theories are empirically valid. At the same time, research results are the agents that determine whether theories are true or false. How good research is conducted is discussed later in Part I, but first we need to consider how good theories are constructed.

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CHAPTER 2

THEORY BUILDING AND KINDS OF THEORIES

Theory Building
Micro, Macro, and Meso Theories
Typologies as Theory
Grounded Theory
Defining a Good (or Strong) Theory
Criteria
Implications of Good and Bad Theory
Conclusions

Theories can be good or bad, or, more frequently, somewhere in between; they can seek truth or some other goal. Many additional ways to classify theories exist as well. An understanding of how theories are built, and what types of theories result, may help to clarify the role of theory in organizational behavior and to further establish standards that the field must meet as a science.

THEORY BUILDING

A distinction is often made between deductive and inductive theory. In building a theory by deduction one first establishes a set of premises. Then certain logical consequences of these premises are deduced and subsidiary concepts are established. The starting point is rational thought, and logical consistency is a major concern in development of the theory. Often such theories are stated in mathematical terms.

Inductive theory, in contrast, builds up from observation, often from research, rather than down from a set of premises. Essentially one puts together a theory that best seems to explain what is known in a given area at the present time. Then new tests of this theory or of hypotheses derived from it are carried out just as they would be if the theory were developed deductively.

Gottfredson (1983) points to three ways in which inductive theory may be developed from research findings. First, one may immerse oneself in the data generated by past research, but with a healthy skepticism regarding any interpretations by others found with these data. Second, one may pick one or more specific patterns of results to explain, thus narrowing the theory-building task to a more limited domain than general theory. Finally, one may try to resolve inconsistencies, anomalies, puzzling results, and incompatible points of view in the literature and in the data reported there.

A major pitfall in the use of the inductive approach in theory building is that the research from which the theory is induced may become confused with an adequate test of the theory. Thus, the same research is used twice for two different purposes, and a self-fulfilling prophecy results. In the case of truly deductive theories, this is not possible. When theories are developed inductively, it is crucial that they be tested on a new sample in a manner that is entirely independent of the pretheory research. If one goes back to the prior sample or to data used in developing the theory, anything unique and ungeneralizable
(attributable to chance fluctuation) in that particular situation is very likely to be confirmed. As a result, a theory that is erroneous insofar as generalization and practical usefulness are concerned may well be accepted.

It is actually more useful to think of theories as falling at points along a deductive-inductive continuum than as falling into distinct categories. Probably no theory is completely devoid of some inductive input. On the other hand, there are instances arising from entirely inductive processes. Such instances are often referred to as dust-bowl empiricism, implying that no theory is involved at all. However, the result may look very much like a theory.

An example of dust-bowl empiricism would be a study in which a great many measures, say several hundred, are obtained on a sample of organizations. These data are then put into a computer, and closely related measures are identified through the use of correlation techniques, factor analysis, or some similar procedure. What emerges is a set of hypothesized relationships among variables—a set of statements very much like an inductively derived theory. This “theory” is then tested on a new sample of organizations, using the appropriate measures to make sure that it does not incorporate relationships that represent mere chance fluctuations associated with the particular sample from which the theory was induced.

Any theory, irrespective of the method of construction and the extent of research confirmation, should always be treated as provisional in nature. Theories are constructed to be modified or replaced as new knowledge is developed; this is the way science advances. Furthermore, modification on the basis of research tends to be inductive rather than deductive. Findings emerge that do not quite fit the existing theory. Accordingly, the theory is changed so that these new data can be explained, and a test is then made of the revised theory. As a result of this kind of theoretical tinkering, even predominantly deductive theories may take on a strong inductive element over time; if they do not, they may well be replaced.

MICRO, MACRO, AND MESO THEORIES

Micro theory in organizational behavior deals with the behaviors and nature of individuals and small groups in organizations. It has been strongly influenced by psychology and many theorists of this kind were originally trained in that field. A good understanding of the micro approach can be gained from a reading of Staw (1991). Motivation, leadership, and team building are essentially micro subjects, although theories in these areas may contain variables that extend beyond that designation.

Macro theory focuses on the behavior and nature of organizations, not of individuals and groups. Parts of the organization may be of concern as well, and so may the environment surrounding the organization. Sociology has played a role in the development of macro theory very similar to that played by psychology in micro theory. In a companion piece to the Staw (1991) article, Pfeffer (1991) offers a good example of how macro theorizing works. Theories of organizational process and structure represent predominantly the macro approach.

This distinction between micro and macro levels has been part of the organizational behavior field since its early years (for a recent example of this distinction, see Wright and Boswell 2002). A more recent arrival, at least in terms of terminology, is meso theory. House, Rousseau, and Thomas-Hunt (1995) define the meso approach as concerning the simultaneous study of at least two levels, where one level deals with individual or group processes or variables and one level deals with organizational processes or variables, and bridging or linking propositions are set forth to relate the two levels. An example of meso theorizing is presented in a book by Tosi (1992).
Tosi’s (1992) book contains a number of theoretical propositions that may be used to illustrate the macro-micro-meso distinctions:

The relevant environment of an organization is defined as external organizations or institutions which have direct effects on decisions and processes in the focal organization (29)—macro.

The degree of volatility of the environmental sectors affects the structure of subsystem relationships in organizations (34)—macro.

When individual personality manifests itself it usually does so with respect to interactions with others or toward the organization, not in terms of work patterns or levels of performance (82)—micro.

A particular leader action is interpreted and attributions are made in the situational context. Different situations may result in different attributions about the same acts. It is the nature of the attribution, not the behavior itself which is related to effectiveness (196)—micro.

The dominant form of conflict in organic organizations is rivalry. The bases for the rivalry will be

1. competition for resources for projects in process and/or
2. status-based competition between specialists from different disciplines.

There will be moderate to low levels of vertical conflict in organic organizations (110)—meso. Power striving predispositions will lead to power striving and political behavior when organizations are loosely coupled (128)—meso.

**TYPOLOGIES AS THEORY**

A number of theories set forth various categories of organizations, environments, people, or groups, usually in the range of two to five. These formulations may deal with ideal types, sets of intellectual, hypothetical constructs created purely to study variety and change, which are not necessarily found in their complete form in the real world at all (Lammers 1988). At the other extreme are formulations that utilize only empirically derived clusters, based on real world data, which are created using the techniques of dust-bowl empiricism (Ketchen and Shook 1996). There are variants between these two as well.

The terms typology and taxonomy may be applied to these formulations, but they have not been used in a consistent manner, and there is less-than-universal agreement either on definitions or appropriate approaches (Rich 1992). There are even those who decry the use of such classification systems entirely, viewing them as inherently unsound (Donaldson 1996). Given this situation, a working approach to theories of this kind is needed. In what follows I believe the discussion is consistent with the dominant position in the field of organizational behavior at the present time. If not, the position is at least a widely accepted one (Doty and Glick 1994; Miller 1996; Sanchez 1993).

The term typology is used to refer to a set of types developed on an a priori conceptual basis to operate as, and serve the purposes of, a theory. These constructs may be of an ideal nature or they may to varying degrees be intended to reflect the actual nature of the real world. These conceptual typologies are viewed as theories, and they may be good or not so good just like any other theory. Taxonomies, on the other hand, are empirically derived clusterings developed through multivariate analysis of existing data. As such they are data, not theories; description, not explanation. However, theoretical formulations may be devel-
oped inductively starting from taxonomies, thus folding a taxonomy into a more comprehensive theoretical system. Thus, a taxonomy alone does not constitute a theory, but each instance needs to be considered separately. For a more extended treatment of these matters applied to the area of entrepreneurship, the reader is referred to Miner (1997).

**GROUNDED THEORY**

Grounded theory focuses on qualitative data for the purpose of developing systematic, limited-domain theories about observed phenomena. It derives its data from participant observation, direct observation, semistructured or even unstructured interviews, analysis of documents, and case studies in much the same manner that an anthropologist might use in studying a culture. Facets of these research data are sorted out of the mass of available qualitative information by means of consciously adopted strategies. These emerging concepts, grounded in the data, become the foundation of a growing theoretical understanding of the phenomena studied (Glaser and Strauss 1967; Turner 1983).

Such a theoretical approach is inductive, and the results are theoretical accounts of relatively small segments of reality. This process attempts to distill out the essence of these segments, and in doing so creates a theory that is rich in terms of the depth of its content, but not broad. These grounded-theory accounts may be used to develop more formal theory, however, by focusing on a domain of broader interest, generalizing from the specific. Within organizational behavior one will find little by way of grounded theorizing in the original sense. On the other hand, more formal theories having their origins in such grounded theorizing are in evidence. In any event, it is important to keep in mind that the proper role of grounded theory is to generate theories, not to test them (Parry 1998). For a more detailed discussion of the use of grounded approaches in theory development, see Locke (2002), and for even more detail, Locke (2001). An article by O'Connor, Rice, Peters, and Veryzer (2003) provides insight into how interdisciplinary teams may be employed to develop grounded theory.

Grounded theorizing and qualitative research are often seen as one and the same thing, and indeed they are overlapping. Both are “most useful in the early stages of research to develop theoretical propositions and derive hypotheses” (Hemingway 2001, 47). Thus, I discuss them here in connection with theory building, rather than in terms of research methodology. Realistically, publishing studies of this kind is much more likely to occur in connection with the development of theoretical propositions than as a distinct research contribution, at least within organizational behavior.

**DEFINING A GOOD (OR STRONG) THEORY**

In order to evaluate theories, science needs some criteria for deciding whether a theory is good or not so good. It is evident from what has been said already that some explanatory statements may not meet the requirements of scientific theory at all, and that what was good theory at one time may be not-so-good theory some years later, as knowledge develops.

**Criteria**

First, theories should contribute to the goals of science. They should aid understanding, permit prediction, and facilitate influence. The more they do these things, the better they are. A theory that is comprehensive in its coverage of the phenomena that it explains is preferable
to one that is limited in scope. However, broad scope alone is not enough. Many so-called
grand theories attempt too much and fail simply because they do not really explain the wide
range of phenomena they attempt to consider.

Second, there should be a clear delineation of the domain of the theory as indicated in
Figure 1.1. The boundaries of application should be specified so that the theory is not uti-
lized in situations for which it was never intended and is therefore useless. Definition of the
coverage of a theory has often been neglected in the social sciences generally (Dubin 1978),
and the field of organizational behavior is no exception.

Third, theory should direct research efforts to important matters. The number of research
studies that could be done in the world is almost infinite. Yet most of these studies, even if
the time and effort to carry them out were available, would not yield significant results in a
statistical sense, and many of those that did would be trivial in terms of their usefulness.
Good theory helps us focus research efforts on salient variables, identify important relation-
ships, and come up with truly significant findings in every sense of the word. Basically, then,
good theory protects the researcher from wasting time.

Fourth, theories, at their best, yield a kind of added value to research efforts. If several
key hypotheses derived from a theory are confirmed by research, then the whole body of the
theory becomes available for use. Thus, theory-based research has the potential for yielding
not just a few isolated facts, but powerful explanation and prediction across the whole do-
main of the theory. This aspect of good theory is one of its most practical consequences.
Unfortunately, many theories do not have this cumulative character.

Fifth, theories should be readily testable. It should be clear exactly what must be done to
either confirm or disconfirm them. On occasion experimenters will carry out studies that
they believe to be adequate tests of a theory, only to have the theorist say, “That is not what
I meant.” When theory is well formulated, this situation should rarely arise. Ideally the theo-
rist will identify the variables of the theory in operational terms.

Sixth, good theory is not only confirmed by research derived from it, but also is logically
consistent within itself and with other known facts. In the case of complex theories, it is
entirely possible to develop propositions that would predict diametrically opposed outcomes
in the same situation. This is particularly likely to happen when the theorist comes at the
same subject matter from different directions, using different concepts and assumptions.
Such internal, logical inconsistencies must be ironed out if the theory is to be of much use.
Furthermore, theories do not exist in a vacuum; they are part of the total body of scientific
knowledge. At any given time it may not be entirely clear how a particular theory fits into the
larger scientific configuration, but a theory that from the outset quite obviously does not fit
at all is to that degree deficient. Theories should build on what is known and fit consistently
into the entire network of existing knowledge (Hartman 1988).

Seventh, the best theory is the one that is simplest in statement. If a given set of phenom-
enon can be explained parsimoniously with a few variables, that theory should be preferred
over one that achieves the same level of explanation with a much more complex set of
variables and relationships. Science does not value complexity in its own right; there is
enough of that all around us in nature. Highly complex and involved theories are often very
difficult to put into practice. Thus, the ultimate objective must be to replace them with sim-
pler explanations. Unfortunately, the process of inductive theory modification often demands
that new constructs and variables be added continually as unanticipated findings emerge and
need to be explained. Under such circumstances a theory may fall of its own weight, for it is
just too cumbersome to be useful.
A number of these points, and a few more, are summarized in a set of guidelines for writing good theory set forth in the introduction to a special section on theoretical models and conceptual analyses contained in the Journal of Applied Psychology:

1. Good theory offers novel insights.
2. Good theory is interesting.
3. Good theory is focused and cohesive.
4. Good theory is grounded in the relevant literature but offers more than a review or integration of this literature.
5. Good theory presents clearly defined constructs and offers clear, thorough, and thoughtful explanations of how and why the constructs in the model are linked.
6. Good theory is testable.
7. Good theory... has practical implications.
8. Good theory is well-written. (Klein and Zedeck 2004, 932–33)

Implications of Good and Bad Theory

Theories that consistently fail to attain criteria such as these (and thus ultimately emerge as bad) can have negative consequences for science (Webster and Starbuck 1988). They can well sustain themselves for a considerable period of time and lead science in wrong directions. They can also produce confusion and conflict that blocks scientific progress. All this argues for immediate testing of new theories so that their status can be established quickly. Without this, the risk of impediment to scientific advance is substantial. Unfortunately, organizational behavior has not always achieved the urgency in testing its theories that might be desired.

Writing in the Harvard Business Review under the title "Why Hard-Nosed Executives Should Care about Management Theory," Christensen and Raynor (2003) note that good theories are valuable in part because they help make predictions; since reliable data are available only from the past, using strong theories of causality is the only way a manager can peer into the future with confidence. In addition, sound theories help to interpret the present and thus to understand what is happening, and why. Good theories make it possible to differentiate the signals that portend significant changes in the future from the noise that means nothing.

At a very high level of abstraction the ultimate goal of science, and its theory as well, is to discover truth. This involves a firm belief that there is a reality out there external to the observer within which this truth exists. Knowing this truth based in reality is plagued with uncertainty. Science seeks truth with the full recognition that it never can be known with absolute certainty—only approximations to certainty are possible. This view has been categorized as “scientific realism” or “organizational realism” (McKelvey 1997). Such a view reflects the predominant position in organizational behavior at the present time.

However, a minority position does exist. This position emphasizes the socially constructed nature of organizational phenomena and espouses a subjectivity that seems to deny the existence of outside reality altogether (see Weiss 2000 for an extended discussion of this view). Accordingly, truth takes a backseat to novelty, provocativeness, and uniqueness. In this view the goal of theory construction and the basis for theory evaluation is not truth, but uniqueness (Mone and McKinley 1993). These are not the values of science, but they do reflect a current philosophic position. We will return to so-called contra views of this kind later in Part I, but for now it is sufficient to indicate that the fact that a theory is socially constructed
does not necessarily make it incompatible with truth and objectivity—thus with the goals and methods of science (Meckler and Baillie 2003).

CONCLUSIONS

On the evidence, a number of organizational behavior’s theories appear to have met the criteria for labeling them as good at the present time. Other books in this series present these theories in detail and the research dealing with them. There are nineteen such theories treated in Organizational Behavior: Essential Theories of Motivation and Leadership (Miner 2005) and twenty-one such theories treated in Organizational Behavior: Essential Theories of Process and Structure (Miner 2006). These theories are not necessarily good (or strong) in every respect, but to be so designated and thus considered “essential,” they must achieve at least one of the criteria. The details on these measurements are given in Part IV of this volume. For now, it is sufficient merely to recognize that organizational behavior has advanced to the point where it possesses a goodly number of theories that are essential, and, thus, make a substantial contribution to the knowledge base of the field.

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CHAPTER 3

MEASUREMENT OF VARIABLES AND DESIGN OF RESEARCH

Measuring Variables
Reliability
Validity
Research Design
Laboratory Experiments
Field Experiments
Quasi-Experimental Designs
Common Method Variance and Bias
Requirements for Conducting Experimental Research
Conclusions

To a substantial degree, the value of a theory is inherent in the research it sparks and in the extent to which the theory is confirmed by this research. Research is possible, however, only to the extent that measures of the variables of the theory are developed, that is, to the extent that the constructs are made operational. These twin topics of measurement and research concern us here. The objective is not to provide a detailed treatment. However, in later chapters we will be asking questions on occasion such as “Does this measure really effectively represent the constructs of a theory?” and “Does this research provide an appropriate test of the theory?” The answers to these questions will draw on some knowledge of both measurement procedures and research design, and the ensuing discussion is intended to provide a basis for understanding in these areas, as well as to indicate the types of considerations with which organizational behavior must deal.

MEASURING VARIABLES

In the past the measures used in organizational research have often fallen short of what might be desired (Price and Mueller 1986). Many of organizational behavior’s theories utilize constructs far removed from those previously measured in the social sciences. Thus, it has been necessary in many cases to develop reliable and valid measures to represent new constructs, which is a time-consuming process. Many organizational measures are still at a primitive stage of development, and this situation can seriously hamper the interpretation of research results. This matter of effectively converting constructs into variables (see Figure 1.1) is what concerns us here.

Reliability

A major concern in research is the reliability of measurement. Measures that are sufficiently stable and unambiguous will not produce sizable differences in score values when applied to
the same phenomenon on separate occasions. The reliability of a measure is usually established by a correlation coefficient. Different approaches are used to determine this reliability coefficient, but all approaches approximate the ideal procedure, which utilizes parallel forms of the same measure. Parallel forms exist when two indexes of the same construct contain the same number of items of each type, concentrate equally on the various aspects of the construct, and produce the same average scores and distributions of scores through the range of possible values. Once such parallel measures have been developed, reliability is determined by administering both measures in the same sample and correlating the scores on the two measures.

The value of a reliability coefficient fluctuates to some extent, depending on whether the parallel form or some other approach is used. However, if one wishes to use a measure in an individual situation—to measure the work motivation of a particular person, for instance, or to compute the average span of control in a certain company—reliability coefficients above .90 are required. If, on the other hand, one is dealing with group data such as mean work-motivation scores in two units of a company or average span of control in relation to profitability in a number of companies, values down to about .70, and sometimes less, are typically acceptable. These standards represent what amount to “rules of thumb” or working conventions. Like many such conventions in science, they are enforced by gatekeepers such as journal editors and thesis or dissertation chairpersons.

The matter of reliability of measurement is important in research because it is impossible to interpret outcomes when unreliable measures are used and results are not statistically significant. The failure to obtain evidence of a relationship between two variables could be due to the fact that there is no relationship. But if one or both measures of the two variables are unreliable, a relationship may well exist that has not been discovered because of inadequate measures. The only satisfactory way to resolve this uncertainty is to develop and use measures of high reliability. Then if relationships are not found, they are very unlikely to exist in the world of reality. For an example of how reliability estimates may be used to differentiate measures see Loo (2002).

Validity

The variables of a theory need to be made operational in the form of specific measures. Accordingly, the measures must truly reflect the underlying constructs; they must provide valid data regarding the phenomena that they are supposed to represent. If, in fact, they measure constructs other than the ones they are intended to measure, the theory may well be assumed to be disconfirmed when it is actually correct. Worse still, a theory may be accepted when in fact its variables have been incorrectly stated (Edwards 2003).

If a measure is what it purports to be, there are certain phenomena to which it should be related and certain other phenomena to which it should not be related. In some cases there are other indexes of a construct available. Often, when a new and highly innovative theory is under test, other measures are not available. Nevertheless, it should be possible to identify certain relationships that would be expected to appear with a high degree of likelihood. In this process, however, it is important not to rely on face validity alone. The measure that looks to be appropriate as an index of a given variable on further investigation may or may not prove to tap that construct.

As we shall see later, establishing the validity of a particular construct measure is not easy. To some degree, the answer is always inferential (Cortina 2002). Yet there are organizational
measures in which one can have considerable faith, while there are others that, even after long years of use, leave considerable doubt as to their construct validity. Certain statistical procedures have been developed to aid in construct validation (Bagozzi, Yi, and Phillips 1991), and these can be quite useful under appropriate circumstances. However, they do not circumvent the need for close reasoning and careful research design. In any event, in spite of occasional instances of confusion, reliability and validity need to be clearly differentiated (Schmidt, Viswesvaran, and Ones 2000). Validity coefficients tend to be much lower, with correlations of less than .20 being considered low, those in the .20 to .30 range being medium, and those above .30 being high; correlations of .50 or above are clearly very high (Hemphill 2003).

A final point, however, should be noted with regard to the matter of construct validity. There has been a tendency in recent years for reports of studies to neglect dealing with the validity of key measures (Scandura and Williams 2000). In fact, there are those who argue that the term construct validity is an invalid concept, in need of elimination from the language of organizational behavior (Locke 2003). This is not the position taken here. Research aimed at validation, and construct validation in particular, can have important implications for the inductive reconstruction of a theory, or for its abandonment. We need more of this type of research, not less (see Glaser 2004).

**RESEARCH DESIGN**

Research conducted to test theories characteristically investigates hypothesized relationships between variables. Such research is first concerned with whether a relationship exists at all and then with the causal nature of that relationship. Research focused on the existence of a relationship is relatively easy to conduct; however, research into the causal problem is clearly much less tractable.

The study of causation typically requires the collection of data over time, on the premise that the cause must be shown to precede the effect. There are now techniques, however, known collectively as causal modeling approaches, that under appropriate circumstances can be used with data collected at one time, as well as longitudinally. These techniques have expanded in number, in complexity, and in explanatory power over the past twenty years. Their use is increasing rapidly, and they appear to offer considerable promise in evaluating causal hypotheses (Williams, Edwards, and Vandenberg 2003; see also Drasgow and Schmitt 2002). In any event, the use of longitudinal designs should be employed if at all possible; longitudinal research involving data collection over time appears to be on the increase in organizational behavior (Hunt and Ropo 2003).

A second factor that makes identification of causal relationships difficult is the necessity for establishing adequate controls. Control may be accomplished statistically through the use of procedures that measure unwanted variables and then remove their effects from the relationship under study. However, these statistical techniques require that the data satisfy certain assumptions, and in many cases it is not at all clear that these assumptions can be met. The alternative is to control variables through the original design of the study. That is not always easy.

**Laboratory Experiments**

Much of the research on causal relationships has been done in the laboratory. An extreme instance of this laboratory research is computer simulation in which no real subjects are involved. More frequently, the experiment is of the small-group or group-dynamics type;
experimental variables are introduced among subjects, often college sophomores, and the results are measured under highly controlled conditions. Because the study is conducted outside the real world of ongoing organizations, it is easier to use longitudinal measures and to control unwanted variables. Yet, even here, major difficulties in maintaining controls exist. Furthermore, the results are very much a function of the variables considered (this is particularly true of computer simulations). If the real world is not effectively modeled in the laboratory, or at least the key elements of that world, the results of laboratory experiments will not transfer.

This said, it appears that in many areas such transfers do occur (Locke 1986). Laboratory studies often appear to be well conducted, or, conceivably, field research is deficient in important areas, with the result that similar results are obtained. In any event, the evidence to date is that laboratory research, with its greater control, is much more valid than previously anticipated. There may be conditions under which this is not true. A degree of field research on laboratory findings still seems warranted. But, assuming initial confirmation, the need for extensive reiteration of these initial results does not seem as great as previously thought.

**Field Experiments**

The ideal situation is to take the techniques of sample selection, repetitive measurement, and variable control associated with laboratory research into the real world and conduct the same kind of research with ongoing organizations. In such a context the myriad variables that may be important do in fact operate, and any results obtained there can be expected to characterize the actual organizations to which any meaningful theory is addressed. The problem is that all the difficulties of designing and conducting good experiments that were so easily handled in the laboratory now become overwhelming. Real organizations have innumerable ways of resisting and undermining objective scientific research—not out of contrariness, but because the goals of the real world and the laboratory are different.

The difficulties of conducting causal research in organizations may be illustrated using a study by Belasco and Trice (1969) on the effects of a particular management development program. The study utilized 119 managers divided into four groups. Managers were assigned to each group on a random basis within sex, type of work supervised, and division groupings. In this manner, as many factors as possible were held constant across the four groups to control for spurious factors that might contaminate the findings and make causal attribution difficult.

One group of managers was pretested, trained, and posttested on knowledge, attitudes, and behavior. The objective was to see if a change occurred on any of these factors.

A second group took the pretest, received no training, and then took the posttest. If this group changed as much as the first, clearly, the training was not the cause of change. If this group did not change as much as the first, the training remained a strong contender as a cause.

A third group underwent no pretest, received training, and took the posttest. By comparing the posttest result for the third group with that for the first group, it was possible to identify any apparent change due to a sensitizing effect of the pretest (the groups were similar in all other respects). The problem addressed here is control for any effects the pretest may have had in alerting the managers to what they were supposed to learn later in training.

The fourth group received no pretest, no training, and only the posttest. This group, in comparison with the others, yields a measure of the effects of the passage of time only, and therefore isolates time from either repeated measurement or training as factors.
Clearly this kind of research requires a large number of subjects, the opportunity to assign them to groups as desired for research purposes, and extensive collaboration from the sponsoring organization throughout the study. And, as elaborate as the research plan is, it could be argued that a fifth group, undergoing some training of a relatively neutral nature, should have been included to create a placebo situation and cancel out any so-called Hawthorne effect produced by receiving special attention. Thus, even this very complex experiment cannot be said to have achieved the ideal in terms of control. Such studies are very difficult to conduct, yet they continue to appear in the literature (see, e.g., Probst 2003). However, it remains true that field experimentation appears to be on the decline in organizational behavior, often giving way to statistical methods of assessing the plausibility of models positing causality (Greenberg and Tomlinson 2004).

**Quasi-experimental Designs**

Realistically elegant research designs with all possible controls are unlikely to be implemented in many organizations and if an organization does decide to go this route, it may well be an atypical organization. Accordingly, certain variants have been proposed (Cook, Campbell, and Peracchio 1990; Evans 1999). These designs represent major advances over the noncausal, correlational analyses, but no such study alone answers all questions. Basically, these studies utilize as many components of the ideal experimental design as possible, while recognizing that it is better to conduct some kind of research related to causes than to do nothing. It is hoped that the larger number of research investigations carried out will compensate for the relative relaxation of control requirements. Accordingly, several interlocking investigations should develop the same level of knowledge as one very elegant study. On the other hand, it is easy to relax scientific standards to the point where replication is not possible, and thus not obtain scientific knowledge that can be substantiated. Some trends in qualitative research on organizations show this tendency. It is important to maintain a clear distinction between scientific research and personal narrative in testing organizational behavior theories.

A number of examples of well-conducted quasi-experiments exist in the recent literature. The typical design calls for some combination of the elements considered in the previous section (see, e.g., Markham, Scott, and McKee 2002). A particularly good discussion of the limitations that may be inherent in the quasi-experimental design is contained in Morgeson and Campion (2002). Descriptions of how quasi-experimental designs may be utilized in studying promotion effects are presented in a series of studies conducted within an international bank based in Hong Kong (see, in particular, Lam and Schaubroeck 2000).

**Common Method Variance and Bias**

Common method problems can arise from having a common rater provide the measures of variables, a common measurement context, a common item context, or from characteristics of the items in a measure. Of these, obtaining measures of both the predictor and criterion within the same study from the same person produces the most pronounced such results; these biases can be quite substantial (Podsakoff, MacKenzie, Lee, and Podsakoff 2003). Thus, when the same person reports on the two types of variables, that person may change the correlations in an attempt to maintain logical consistency. The results are a function of the measurement method rather than of the underlying constructs.
Individuals’ reports of their internal states (such as expectancies) may be obtained at the same time and from the same person as reports of past behavior related to these internal states. As a result of a desire to maintain cognitive consistency, these correlations can be inflated substantially (Lindell and Whitney 2001). This bias is introduced because of the measurement approach taken and the failure to use more appropriate designs.

Solutions to this type of problem, as is typical in organizational behavior research, focus on designing the problem away or controlling it with statistics. Unfortunately, in the past, however, many studies have been conducted that did neither of these, thus simply ignoring the problem. What is needed is to separate the measures of the variables involved by using different sources, and thus different research designs. An alternative is to use measures of variables that are not self-evident (such as projective techniques), so that the individual cannot mobilize attempts to attain cognitive consistency. Attempts to solve common method problems through the use of statistical approaches have been numerous, but as yet, no widely accepted solution has emerged. The common method problem has historically been a particularly frequent source of error in organizational behavior research.

**Requirements for Conducting Experimental Research**

Blackburn has set forth a list of what he labels the ten commandments for conducting experimental research. These can serve as a guide in assessing research used to test theories in the organizational behavior field.

1. Thou shalt assess the extent to which the change actually took effect.
2. Whenever possible, thou shalt use multiple measures.
3. Whenever possible, thou shalt use unobtrusive measures.
4. Thou shalt seek to avoid changes in measurement procedures.
5. Thou shalt endeavor to use a randomized experimental design whenever possible.
6. In the absence of random assignment, thou shalt not select experimental or control groups on the basis of some characteristic that the group may possess to some unusual degree.
7. Thou shalt use appropriate statistical analyses to examine the differences between the experimental and control groups.
8. Whenever possible, thou shalt collect time-series data.
9. To the greatest extent possible, thou shalt protect the employee, the organization, and the experiment in that order.
10. Thou shalt report fully and honestly the procedures and results of the research. (Blackburn 1987, 137–38)

Many of these points are illustrated in a book edited by Frost and Stablein (1992), which provides detailed descriptions of what actually happened in connection with seven research studies. This book is also a good source of information regarding ways in which qualitative research may be employed for purposes of inductive theory development.

**Conclusions**

This chapter has delved into the characteristics of research that can be used to test scientific theories. In all of this, it should be understood that organizational behavior research may serve additional functions beyond merely testing theory. Hypotheses derived from practice
may be evaluated through research to determine whether what has been assumed to be true is really true. A reason that present particular problems may be studied to obtain a clearer picture of the landscape. The point is that scientific research in organizational behavior is not simply a matter of theory testing. Yet theory testing is probably the most important function of organizational behavior research because a well-validated theory can establish a wide range of knowledge. However it is used though, organizational behavior research needs to become more innovative to face new challenges and devise new designs (see Bazerman 2005). Over-reliance on statistical approaches may well prove to be self-defeating.

In Chapter 4, we turn to the matter of establishing scientific knowledge through a consensus of knowledgeable scholars, and the ways in which consensus may be fostered and disrupted by the influence of human values. En route we will consider some of the objections and outright attacks that have been mounted against a science of organizational behavior.

REFERENCES


CHAPTER 4

KNOWLEDGE OF ORGANIZATIONAL BEHAVIOR

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Conclusions

The high visibility of certain formulations that are clearly closer to philosophy than to scientific theory has led in the past to questions about whether organizational behavior truly possesses any theories at all (Tosi 1984). This negative position has received additional support from some individuals, a number of them scientists who place very little stock in theory building in any event, preferring the slow but solid pace of unswerving empiricism. Furthermore, there is a rather sizable body of literature that raises serious objections to the scientific concepts we have been considering—to the scientific method and to the assumptions on which it is based. If one follows these views, a quite different picture of our theoretical knowledge of organizational behavior emerges.

OBJECTIONS TO SCIENTIFIC DICTATES

A common method of dealing with antithetical positions is to simply ignore them, thus avoiding the need to cite them or to consider the views at all (Martin and Frost 1996). Clearly, I could do this here. Yet the concept of science set forth in the preceding pages is what underlies the whole field of organizational behavior, and to simply ignore objections to it does not appear either to be intellectually honest or to truly reflect the reality of the times.

Frontal Attacks

One such “contra” position is that science as a whole, and certainly the organizational behavior part of it, has not proved convincing as a superior form of knowledge, that new narratives and new epistemologies are needed to supersede science, and that, basically, science has had its day and has now run out of steam (Burrell 1996). This is an across-the-board dismissal, and applies to all aspects of science. In my opinion, this line of assault requires an
equally direct response. Given the realities of the world around us, such arguments for the demise of science make no sense, and are best lumped with similar “end of the world” scenarios. Yet, they persist (Alvesson 2003).

In addition to such blanket attacks, a number of more specific objections have been raised that typically focus on some aspect of scientific theory and/or research. One such approach is to challenge the various assumptions of science (Kilduff and Mehra 1997). For instance, the argument may be that natural groupings of organizations, groups, and individuals do not occur, that uniqueness is everywhere, and thus generalization from samples is not warranted. A nother such argument is that things change so fast that the stability and constancy science requires is nonexistent; science gives way to journalism—the recording and explaining of fleeting phenomena. A third challenge asserts either that events are not determined, and thus cause-effect relationships do not exist, or that social science, as distinct from natural science is concerned with meanings and significance, not causes. Finally, the trustworthiness of human processes of perception, memory, or reasoning may be questioned, thus introducing challenges to the observation and experience on which science is based. Advocates of these positions tend to give more credence to qualitative than to quantitative research (Kilduff and Kelemen 2003). Qualitative research is accordingly moved from its role as an adjunct to inductive theory building to a central role in theory testing.

Other objections are concerned with the objectivity and relevance of scientific research (see Ghate and Locke 2003). These views may emphasize the fact that people as the subjects of research react differently when they become aware of the researchers’ hypotheses or experience a feeling of being controlled in the experimental situation; thus, the research process itself poses a threat to generalization. Alternatively, research studies, especially laboratory studies, may be viewed as lacking the realism required for generalization. Objections of these kinds seem to assert that all organizational behavior research is bad research and that researchers cannot overcome these threats to their findings through creative methodologies because objectivity is impossible to obtain. Data such as those summarized in Locke (1986) on the close proximity of laboratory and field research findings are totally ignored.

Postmodernism and Siblings

Some of the strongest attacks on social science, and inherently on organizational behavior, stem from a group of philosophies called critical theory, poststructuralism, and postmodernism (Agger 1991) or perhaps some combination of these terms (Voronov and Coleman 2003). These philosophies all had their origins outside the United States and it is there that they originally had the greatest impact. In certain respects they have been influenced by Marxist ideology (see Barrett 2003). These views differ in a number of their aspects (see Vibert 2004), but the opposition to social science is pervasive, as reflected in the positions noted in the previous section. Science is portrayed as a source of authority and a perpetrator of the status quo. As such it must be replaced. Objective analysis and a reliance on mathematics are rejected. In point of fact, this contra position operates to oppose anything that is institutionalized—that has achieved legitimacy and is taken for granted (Alvesson 2003; Clegg and Kornberger 2003). Thus, science, its theory, and the like are merely part and parcel of a much wider enemy.

The preferred approach to gaining knowledge in these philosophies is one that focuses on obtaining detailed understandings of specific situations at a point in time. This approach has much in common with that of grounded theory, although references to that specific proce-
dure by name appear to be rare since Silverman (1971). Studies in this vein collect a great deal of information, and often present much of the raw data to the reader in undigested form in lieu of statistical analyses. Typically the studies are used to both create theory and confirm it at one and the same time. Literary methods and storytelling may be used to present the results of data collection (Jermier 1985). Indeed, the analysis of language and its usage has become pervasive within postmodernism (Alvesson and Kärreman 2000).

When grounded theory is used to create more formal theories, it parts company with postmodernism and its siblings to join company with science. This distinction is important. The qualitative approaches involved may serve to generate scientific theory, or they may yield the self-fulfilling prophesies of postmodernism.

Threats from Within

Although critical theory, poststructuralism, and postmodernism have come only relatively recently to the United States, there have been manifestations of similar ways of thinking here for some time. This has been most characteristic of those in the organizational behavior field who espouse humanistic values with a substantial amount of passion (see Lawler, Mohrman, Mohrman, Ledford, and Cummings 1985; Tannenbaum, Argulies, and Massarik 1985). Argyris has attacked scientific research methodology on numerous occasions and proposes an anthropological approach, devoid of statistical analysis, to replace it (see, e.g., Argyris 1980). He continues to emphasize a similar position (Argyris 2003).

These attacks from within are described by Donaldson (1992), an Australian, as an out-growth primarily of certain trends in organizational behavior in the United States. He summarizes this complex of ideas as follows. It:

1. stresses the empirical world as subjectively perceived and enacted rather than as brute fact,
2. asserts the superiority of qualitative over quantitative methods,
3. reveres paradox in both the content of theory and the formal expression of theory,
4. holds that scientific creativity is primarily linguistic inventiveness,
5. sees itself as championing creativity,
6. is countercultural in the sense of being ever-ready to cock a snook at the establishment and established ideas, and
7. would also claim that practicing managers would be better aided not by plodding positivism but by taking a mind-trip. (Donaldson 1992, 462)

This description is presented in connection with a rebuttal to an article by Astley and Zammuto (1992), to place that article in context. Many other rebuttals to the various objections to scientific dictates exist in the literature. Among these Weiss (2000) is particularly impressive. Donaldson (2003) provides an analysis that points up the logical inconsistencies of postmodernism. McKelvey (2003) castigates postmodernism for ignoring research and the falsifiability of theories. Not infrequently, the objections create a description of science that, although incorrect, makes it easy to mount an attack. In this process, science may very well be redefined as art, with all the freedom to embody values and eliminate burdensome rules that art permits. Many of those who object to the standards and strictures of good theory and good research seem to be trying to remove what they perceive to be barriers that keep them from using the garb of science to advocate their values. This is a tactic that goes back
to Mayo (see Chapter 6). Good science—whether in the form of theory construction or exemplary research—is very hard work. The rules of the game are onerous and they make good science difficult. But that is as it should be; they are there for a reason.

Furthermore, as Pescosolido and Rubin (2000) note, “The fault of postmodernism is that even in its radical insistence on diversity, it insists that its practitioners ‘line up’” (71). There is no freedom from rules of some kind, even in postmodernism.

VALUES AND KNOWLEDGE

Values are conceptions of good and bad that tend to carry with them a great deal of emotion. They attach to certain ideas and patterns of behavior and they provoke behavior consistent with the values as well. For an in-depth treatment of this values construct as related to organizations see Maierhofer, Kabanoff, and Griffin (2002). For a more comprehensive treatment of values in general and the current status of the construct see Hitlin and Piliavin (2004).

Values in Organizational Behavior

Organizational behavior appears to have been influenced by two primary value dimensions throughout much of its history. One is the dimension extending from humanistic to scientific values. In recent years, the humanistic pole has increasingly been joined by the often similar values of postmodernism and its siblings. The other dimension is essentially disciplinary in origin. At one end is psychology, while the other end is anchored primarily in sociology, joined on occasion by anthropology, political science, and economics. Basically, these are values related to micro and macro levels of analysis.

This second, disciplinary dimension has undergone some transformation over the years. In an earlier period, the dimension ranged from behavioral science (dominated in large part by psychology) to classical management theory. As classical theory has faded from the scene (see Miner 1995), the value differentiation involved has been replaced by one within the behavioral science designation itself. In recent years, it appears to be particularly concerned with variations in the value placed on the study of individuals in organizations (see House, Shane, and Herold 1996 and Nord and Fox 1996).

Values of these kinds can play a useful role in theory construction, in part by focusing attention on specific areas of endeavor, and in part by motivating concerted efforts to construct theories that end by fostering understanding and prediction. However, values other than those that foster objectivity have no place in the conduct of research and thus in testing theory. To the extent they might intervene at this stage, replications of initial studies should serve to identify them. Finally, values can reappear in the evaluation segment of the overall theory process, the part that involves reaching a consensus among knowledgeable scholars regarding the goodness of a theory, and thus the contribution to knowledge involved (Miner 1990). The result is that those with different values may evaluate through different lenses, and, as a consequence, consensus may be hard to obtain. For a recent discussion of how values may play a role in science, see Risman (2001).

Dispositions Versus Situations—A Value-Laden Controversy

A n example of the way in which values may produce different views and impede consensus is provided by the dispute over the study of individuals in organizations noted previously.
This dispute simmered over a period of ten years or more before bursting into flame (Davis-Blake and Pfeffer 1989). The latter paper contained an attack on the dispositional approach that underlies the concept of individual differences and the application of personality theory in organizational behavior. Dispositions are defined as unobservable mental states (constructs) such as needs, values, attitudes, and personalities that are relatively stable over time and that to varying degrees serve as determinants of attitudes and behavior in organizations. The argument is that dispositions are a mirage and that the only significant determinants of individual organizational behavior are situational in nature. Thus, an antithesis is created pitting psychological constructs against sociological.

Later Nord and Fox (1996) authored a paper with the thesis that the individual (and individual personality) has disappeared from organizational behavior, and been replaced by a contextual dimension consisting of attributes of the physical and social systems in which people exist (situations). The intent is to document the view that theories and research dealing with individual personality and dispositions have lost status to the point where organizational behavior is no longer interested in individual differences (and by implication should not be). There is reason to believe that this second attack from the sociological perspective may leave something to be desired in its coverage of the personality-related literature, but as an attack by fait accompli it clearly reveals the values of the authors.

These position statements from the situationalist perspective have not gone unanswered. In defense of the dispositional view, George (1992) offered a detailed consideration of much of the theory and research supporting an important role for personality in organizational behavior. House, Shane, and Herold (1996) also make a strong case for the retention of personality-based perspectives. The following quote appears to present a more balanced view of the issues and leaves the door open to both types of theory and research. It provides an instance of how extreme values may be reconciled and consensus thereby achieved.

[Personality is important for understanding at least certain classes of organizational phenomena. Obviously, this does not imply that situational factors are unimportant. Rather, it suggests that organizations do not stamp out all individual differences; being a member of an organization does not neutralize or negate one's own enduring predispositions to think, feel, and act in certain ways. An extreme situationalist perspective denies organizational participants their individuality and exaggerates organizations' abilities to manipulate and control their members. Likewise, an extreme dispositional position credits too much power to the individual and ignores important situational influences on feelings, thoughts, and behaviors. Hence, personality and situational factors are needed to understand much of organizational life. (George 1992, 205–6)]

Positive Organizational Scholarship

Positive organizational scholarship is a value orientation of very recent vintage—too recent to be able to say where it is going as far as organizational behavior is concerned (Bernstein 2003; Cameron, Dutton, and Quinn 2003). It had its origins in psychology where it was in many respects a reaction to clinical psychology with its emphasis on illness, disorder, and thus the negative aspects of the human condition. Basically, it is a movement, unrelated to any specific theory, that values excellence, thriving, flourishing, abundance, resilience, and virtuousness—anything associated with positive human potential.
Because scholarship here means science and scientific research, this is not another name for postmodernism. Certain ties to an earlier humanism and to the values of organization development are evident, but the coverage is broader than that. As noted, psychology is the discipline of origin, but the movement is interested in influencing any field that finds its values attractive. In short, it is a community of scholars, currently based in the school of business at the University of Michigan, devoted to learning about positive aspects of the human condition, particularly as reflected in organizational functioning. The scope of the movement is as yet unclear; it may ultimately come to represent a reemergence of humanism in organizational behavior, or something else. But the appeal to strong values and the attempt to mobilize them in support of its aims is clear. This is particularly evident in recent promotional writings on the subject (see, in particular, Luthans 2002; Luthans and Youssef 2004).

THE ROLE OF CONSENSUS

Threats to a unified science of organizational behavior take two major forms—those that relate specifically to science, including its theory and method, and those that impair unity by jeopardizing the creation of a stable and widely recognized body of knowledge that might be presented to practitioners as a basis for their actions. The latter is the concern of this discussion.

The Consensus Problem

A lack of consensus appears to exist in the field of organizational behavior, and, as a result, the field's limited amount of hard knowledge is often bemoaned. The evidence is there, but the consensus of knowledgeable scholars that makes it knowledge is often out of reach because conflicting values block the way. Testimony to this effect is not hard to find.

In the introduction to his volume dealing with organizational behavior's conceptual base, Hartman (1988) discusses this fragmentation using terms such as "disarray," "no consensus," "conflict," "disunity," and "disagreements." The authors of a subsequent handbook of the field (Clegg, Hardy, and Nord 1996) use their introduction to paint a picture that presents organizational behavior as infused with controversy and partisan politics; this latter volume appears in its own way to contribute to the fragmentation as well, even to extol it.

A well-argued treatment of the consensus problem is that of Pfeffer (1993), which subsequently has sparked a great deal of debate pro and con. The thesis of this paper is that when sciences have developed shared theoretical structures and methodological approaches about which there is substantial consensus, these sciences and their members have experienced a number of positive consequences including increased allocations of monetary and other resources. Organizational behavior, being fragmented as it is, holds a position low in the pecking order when rewards and resources are distributed among the sciences. In short, we are not viewed as doing a very good job, and this is true because of our lack of consensus. Pfeffer argues that consensus can be attained through the efforts of an elite network of individuals who utilize political positions and processes to impose a uniformity of view on a discipline. He seems to say that this should happen in organizational behavior. This appeal for consensus is reiterated in a later paper (Pfeffer 1995), but it is apparent that he prefers consensus around certain theoretical positions over others.

Not surprisingly a number of organizational behaviorists jumped up to dispute Pfeffer on a variety of grounds. In general, the thrust of these views is that consensus is not really a
desirable goal after all, and that enforced consensus is particularly undesirable. Tolerance for diverse approaches, theories, and methods should not be suppressed, and, in any event, there is no one best way that clearly deserves a dominant position. On occasion, this rebuttal is mixed with a substantial dose of antiscience rhetoric (Van Maanen 1995).

All this having been said, it remains true that science relies on some degree of consensus among knowledgeable scholars, and that science has proved over and over again that its methods can advance understanding, prediction, and control to the benefit of human society. Certainly, some degree of disconsensus can be absorbed, and innovative, creative contributions should not only be tolerated, but supported. The questions are how much consensus is needed and in what areas; these are empirical questions as Pfeffer (1995) notes. It is amazing, once the emotions that values arouse are activated, how difficult it is to see the balanced, middle ground. For a balanced discussion of these issues from a perspective tempered by the passage of time see Fabian (2000).

The Inability to Compare Competing Theories

One outgrowth of the consensus problem is a view that one cannot decide objectively between competing theories that use different languages, hold different assumptions, and utilize different constructs, thus reflecting totally disparate value systems. Under these circumstances, comparisons are impossible in the same sense that “comparing apples and oranges” is impossible. Science is said to be at a loss in such instances, and, with the theory pluralism that we face at present, science becomes essentially useless (Scherer and Dowling 1995). Note that this argument requires a large number of very different theories coming together from different directions to offer contradictory solutions to common problems. This must be so if science is to be effectively neutralized. Thus, “create as many new and unique theories as you possibly can” becomes the rallying cry of proponents of this view; they are out to sink consensus (Clegg and Ross-Smith 2003). Practitioners, in particular, are said to be left helpless to make decisions in the face of this barrage of competing theories and may be expected to eschew organizational behavior altogether.

The response to this line of reasoning is that it creates a pseudo problem, a mirage, that is readily soluble in that science serves to test theories through research that is just as applicable to competing theories as to any others (McKinley 1995). Although valid, this position nevertheless needs some amplification.

First, the theory pluralism that exists at present is not made up exclusively of scientific theories. There are a number of philosophic statements in existence that do not generate testable hypotheses and thus are not falsifiable. Subtracting this philosophic content substantially reduces the degree of theory pluralism. For example, many of the phenomena that exist in this world have multiple religious explanations, and scientific explanations (confirmed by research) as well. To include the religious “theories,” which are untestable, as part of the total count of scientific theories is unwarranted.

Second, a close study of existing organizational behavior theories reveals that the most frequent situation is one where the theories occupy different, nonoverlapping domains. There are instances of overlap and even some cases of competing positions, but this is not the norm by any means. Those who argue that an inability to compare competing theories is a major barrier to attaining consensus are simply wrong, at least insofar as organizational behavior is concerned; there are not that many competing theories, once theoretical domains are clearly drawn (see Miner 2002).
Third, competing theories can be compared using appropriate research designs. Differential experimentation that serves to determine the relative effectiveness of various approaches or hypotheses is commonly conducted. Any good theory contains clear specifications for operationalizing its variables and these may be used in comparative research. For a good example of how research may be conducted to deal with competing theoretical positions, see Latham, Erez, and Locke (1988), and a more extended treatment of this research contained in Frost and Stablein (1992). What is clearly evident here is that with sufficient creative input into the research process, science can handle competing theoretical positions (see also McKinley 1995 on this point). Thus a basis for achieving consensus, where it might otherwise appear to be lacking, does exist within science.

Fourth, a consensus of knowledgeable scholars can develop in the absence of full agreement among protagonists. Rensis Likert’s theory of systems 1–4 and 4T (see Miner 2002) was found to be deficient in certain respects, based on extensive research conducted to test aspects of the theory. Yet, to my knowledge, Likert never repudiated his theory, and continued to hold out against the growing consensus until his death. This is not unusual and it does not matter. A few voices in opposition do not vitiate consensus.

Fifth, it is not correct to say consensus is totally lacking in organizational behavior; agreement among knowledgeable scholars in support of a theory occurs quite often (Miner 2003). But the qualifier among knowledgeable scholars is important here. Organizational behavior has developed a breadth and depth of information that defies comprehension by a single person. There are specialties and subspecialties, and it is these that furnish the knowledgeable scholars whose judgment is at issue. To add in the many who know little or nothing about a particular theory and its research is bound to create an appearance of disconsensus as competing values become involved against an ambiguous (uncertain) background, but that is not the kind of consensus science seeks.

Sixth, practitioners (such as managers) do not require a consensus on the part of organizational behavior to utilize the tools, technology, and theories of that discipline. It would certainly be helpful if such a consensus existed, but managers in a particular area of a business are not necessarily uninformed consumers; they can make judgments as to the validity and usefulness of what comes to them from organizational behavior, and they do so all the time. Many of these inputs from organizational behavior prove useful and help to solve important practical problems. As a former practitioner of organizational behavior in the personnel research unit of a large corporation and a consultant in that area throughout my professional career, I can attest to the practical value of these inputs. In actuality, the freedom from political wars that the practitioner has may compensate for any lack of knowledge. Certainly errors are made, but low levels of consensus do not prevent practitioners from making choices among the potpourri of organizational behavior tools, technologies, and theories. And again, as Donaldson (1992) contends, the degree of consensus available to practitioners is probably greater than the critics have maintained.

The Road to Consensus

It becomes apparent from the above discussion that it would be very useful to have an operational measure of consensus on various matters, within organizational behavior. With hard data on what knowledgeable scholars think, it would be possible to avoid much of the ambiguity that surrounds this treatment.

Actually, during the 1980s, a certain amount of data on the extent of consensus around
first-generation organizational behavior theories that had achieved considerable visibility did become available (Miner 1990; see also Lee and Earley 1992). The correlations among data from different sources ranking these theories as to their validity ranged from .74 to .94. This is indicative of a considerable amount of consensus. More recent research of this nature provides evidence of increasing consensus around a number of theories in organizational behavior (Miner 2003). The field is still very young and it is too early to expect high levels of agreement, but we are moving in that direction.

One might think that consensus could be obtained by noting the most frequently cited publications in the field, and then building a picture of organizational behavior’s knowledge base from the content of these publications. Unfortunately, however, evidence indicates that those publications that do particularly well in citation counts do so not because of the perceived quality of the publication or its usefulness to practitioners, but because of the usefulness to scholars of the field in carrying out their professional tasks (Shadish 1989). This is not the stuff out of which a picture of our knowledge base can be created, at least without adding in some significant correction factors. Furthermore, the use of citation counts in this manner has been a source of considerable controversy on many grounds (see Hébert 2004).

Yet there are multiple signs pointing to improving consensus as organizational behavior matures. One such sign is the increasing degree to which citations to other disciplines have been appearing in the journals (Blackburn 1990). Discourse across disciplines appears to be on the upswing; talking only with those people within the field, who represent a reflection of one’s own image, is decreasing. When communication opens up in this way, at least the potential for greater consensus opens up as well.

A second encouraging sign is the relatively recent emergence not of meso theories per se, since such theories have in fact been in existence for some time, but of an explicit concern with the identification and creation of such theories, which bridge a major value gap in the field. To the extent they prove valid, meso theories can represent a major integrating force within organizational behavior.

In writing meso theory, one is forced to deal with both psychological and sociological variables as well as with the literatures that surround those variables. The result should be an integrated theory that not only ties together the two levels of analysis, but commits the author to some type of synthesis of the two value positions. Accordingly, a strong commitment to a meso approach to theorizing can go a long way toward fostering consensus, and firming up a stable knowledge base for organizational behavior.

In short, although it does seem that consensus is at a rather low level overall within organizational behavior, there are subfields and sectors where this is not the case. Thus, a body of accepted knowledge does exist within the field, and is available to practitioners— a smaller body than many would desire, but still important. Furthermore, there are certain trends in evidence that seem to argue for improving consensus in the future. We return to this matter of consensus in Part IV. However, for the present, it is important to keep in mind that whatever progress is being achieved on this front is made against the efforts of “contra” advocates who seek to undermine consensus wherever they find it.

CONCLUSIONS

In the preceding discussion, certain terms that are to be found in the references, and that are often used in the literature, have been deliberately avoided. This is in part because these terms have taken on a variety of value-laden excess meanings that tend to stereotype the
In some instances, the terms are too ambiguous for most scientific purposes as well. Kuhn (1970), in introducing the term “paradigm,” intentionally used it with a wide range of meanings (Astley and Zammuto 1992), and it continues to possess this same ambiguity today. In addition to paradigm, I have avoided such terms as “normal science,” “positivist theory,” and “incommensurability” for the same reasons. I join McKelvey (1997) in urging that we purge our literature of terms that we truly do not understand.

This is not to say that most of the concepts that appear to be covered by these terms are not treated—to the contrary they are treated in detail, but using other words. Nor am I trying to avoid labeling my own position. The discussion here clearly identifies my commitment to science and spells out at considerable length the concept of science I have in mind—one that in my opinion has served organizational behavior exceedingly well over the years. Terms such as “paradigm,” “normal science,” “positivist theory,” and “incommensurability” come to organizational behavior from philosophy, however. As a result, there is no commitment to make them precise and specific, in the mode of science. At the same time, there is no necessary commitment on the part of organizational behavior to make them part of our vocabulary—and we should not.

To fully understand the role that philosophy has played in organizational behavior, the discussion here in Chapter 4 needs to be placed in the context of the intellectual history of our field and how it has developed. That controversy and conflict, and value-laden emotionality, characterize the present scene is self-evident. But that the same emotionality was pervasive in the past is not so well understood. Nor is the fact that philosophy was a factor in the mix then, just as it is today. We turn to these historical concerns in Chapter 5.

REFERENCES


CHAPTER 5

THE CONTEXT OF THEORY AS ORGANIZATIONAL BEHAVIOR EMERGED

Some History of Science
   The Role of Philosophy
   Psychology as an Example
Schools and Organizational Behavior
   Harold Koontz and His Jungle
   The Views of William Scott
   Charles Perrow’s History of Organizational Theory
Schools and More Schools
The Intellectual Context in Related Arenas
The Disappearing Schools
Conclusions

With some understanding of science and its processes, it is appropriate to ask about the intellectual context that existed as organizational behavior developed. Remember, we are talking about a field that at the time had very fuzzy boundaries, a condition that remains true today, although I believe to a lesser extent. Nevertheless, we need to understand where this discipline with its fuzzy boundaries stood in the late 1950s and 1960s in terms of its intellectual development. For that we must turn to some history of science, so as to get the positioning right.

SOME HISTORY OF SCIENCE

In its early days organizational behavior went through a transitional process, gradually moving from the realm of philosophy to that of science. A n understanding of this transitional phase, and what typically happens in it, is essential to any discussion of the intellectual environment that existed.

The Role of Philosophy

Philosophy may be viewed as an effort to discover the ultimate nature of things through a process of systematic reflection. The term is also used to refer to a set of speculative beliefs or convictions. In either sense it departs from science in that it does not emphasize controlled research; its output of ideas is not subject to confirmation and rejection based on research evidence.

Within the milieu of organizational behavior philosophy initially took the form of attempts to discover the essential elements of the managing and operating of organizations through systematic reflection.
Similar efforts characterized the early work in the physical and biological sciences. Many conceptions regarding the nature of the universe and the world around us existed before physics, chemistry, astronomy, and biology assumed major roles in this regard. In each instance the desire for something more definite, for more valid understanding and prediction, led to the gradual development of a science where only an aspect of philosophy had been before.

In the natural sciences (Roberts, Hulin, and Rousseau 1978) and in the social sciences as well (Wadia 1968), it has been typical for the interval between the development within philosophy and emergence as a science divorced from philosophical origins to be marked by a great deal of conflict among various schools of thought. This is a time when true theory begins to emerge, but research evidence to discriminate among theories is lacking. Because the needed research has not yet been completed, or on occasion cannot be completed because the theories are untestable, diatribe and invective often replace it. This is a phase that tends to dissipate when theories and hypotheses are adequately tested: when there is enough research evidence so that sophisticated rationalization is no longer of any avail.

Psychology as an Example

Among the social sciences, psychology provides a good example of this historical process. Psychology traces its philosophical origins to the writings of such men as Descartes, Leibniz, Locke, Berkeley, Hume, Hartley, the Mills, Bain, Kant, and Herbart (Boring 1950). They dealt with topics that later became the subject of psychological theory and research. It was not until the 1870s that psychology as a science showed clear signs of emergence.

In the ensuing decades, a variety of schools began to appear. By 1933, Heidbreder was able to describe Seven Psychologies; her chapter titles are also descriptive:

- Titchener and Structuralism
- The Psychology of William James
- Functionalism and the University of Chicago
- Behaviorism
- Dynamic Psychology and Columbia University
- Gestalt Psychology
- Freud and the Psychoanalytic Movement

In a book that appeared in its first edition in 1931, Woodworth and Sheehan (1964) describe:

- Functional psychology
- Structural psychology
- Associationism
- Psychoanalysis
- Personalistic and organismic psychologies
- Purposivism or hormic psychology
- Behaviorism
- Gestalt psychology

All of these schools had been initially established by 1912.

Schools tend to be identified with specific individuals and/or universities. With the deaths
of founding fathers and changes in university faculties, the schools of psychological thought gradually faded. It is not that they disappeared; nor have they been replaced by other schools. Each has contributed in important ways to present knowledge. Yet no grand superschool or synthesis of conflicting views has appeared. It is simply that the outpouring of psychological research since World War II has caught up with the schools. As a result, we know considerably more about which of the pronouncements from each school are correct, which are wrong, and which are unimportant.

Currently, there is considerable development of theory in psychology, although many scholars would argue for more. However, a theory does not now typically accelerate into a school with polemics and counter-polemics. Rather, new theories are subjected to the criterion of scientific research by which they stand or fall. This is not to argue that conflict has disappeared from psychology. That is probably too much to ask of any field. But much of the conflict follows the lines of specialties and subspecialties, rather than theories. Theories generate conflict primarily when they remain untested, and that is mostly when they are new. New theories, by definition, do not have the time to generate large followings and solidify these followings into schools, however.

I should emphasize that the concept of schools as a transitional state described here should be distinguished from the view of "research schools" associated with an "active stream of empirical research" set forth by McKinley, Mone, and Moon (1999, 635). My concept of schools involves very little, if any, research in the most active phase.

SCHOOLS AND ORGANIZATIONAL BEHAVIOR

During the 1960s, organizational behavior was at the height of its schools phase. Many lists of these schools were proposed, often to showcase a particular school that the author advocated. This was the intellectual environment of the period, and for that reason we need to consider some of these lists. One such list was incorporated in the Gordon and Howell (1959) report prepared for the Ford Foundation (see Part III):

Management analysis—the quantitative approach extending from Taylor to operations analysis [see Chapter 11].
Organization theory—the social science approach to the study of organizational phenomena.
Principles of management—the views of Fayol and his followers [see Chapter 11].
Human relations—the approach to supervision that emerged from the Hawthorne studies [see Chapter 6].

In some instances these schools dealt with quite different subject matters; then the controversy was likely to be over method, not theoretical content. However, there were problem-focused conflicts as well, where theories appeared to clash. With all the (perhaps intentional) ambiguity regarding domains and concepts, however, it was often difficult to say whether or not real antitheses existed.

Harold Koontz and His Jungle

Probably the most widely recognized listings of schools were those proposed by Koontz. There were two of them, published almost twenty years apart. These listings are given in Table 5.1. Koontz himself identifies with the management process or operational school. In
Over the interval between the two listings, what Koontz calls the “jungle of schools” appears to have become much denser. By 1980, the original six schools have increased to ten and one of them has split into two as well. There has been considerable renaming and some addition of key individuals. Much of what is added stems from new developments, but there is some rethinking of prior views also. In any event Koontz’s formulations make it evident that there was a great deal of conflict and uncertainty involving different groups and that even identifying these groups with certainty presented major problems.

Table 5.1
Koontz’s Schools in the Management-Theory Jungle at Two Points in Time

<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Management process (Fayol, Taylor, Mooney, Brown, Urwick, Koontz)</td>
<td>Operational—emphasizes concepts, principles, theory, and techniques underpinning practice.</td>
</tr>
<tr>
<td>2. Empirical (Dale)</td>
<td>Empirical or case—emphasizes the study of managerial successes and failures in individual cases. (Harvard Business School)</td>
</tr>
</tbody>
</table>
| 3. Human behavior (Tannenbaum, Weschler, Massarik, Dubin) | (a) Interpersonal behavior—emphasizes individuals and their motivations; psychology. (Maslow, Herzberg, Litwin)  
(b) Group behavior—emphasizes behavior of people in groups; sociology and cultural anthropology. (Argyris) |
| 4. Social systems (March, Simon, Barnard) | Cooperative social systems—emphasizes human relationships within cooperative social systems. (Pareto) |
| 5. Decision theory (Luce, Raiffa) | Decision theory—emphasizes the central role of decision making. |
| 6. Mathematical (Miller, Starr, Churchman) | Mathematical or management science—emphasizes mathematical processes, concepts, symbols, and models.  
7. Sociotechnical systems—emphasizes harmonizing social and technical systems. (Trist)  
8. Systems—emphasizes the systems view of organizations as an assemblage of interconnected or interdependent parts forming a complex unity. (Johnson, Kast, Rosenzweig)  
9. Contingency or situational—emphasizes the idea that what managers do depends on a given set of circumstances. (Lorsch)  
10. Managerial roles—emphasizes roles developed from observation. (Mintzberg) |

The Views of William Scott

William Scott proposed two sets of schools at different times. The first (Scott 1961) notes three influential theories rooted in three separate schools:

1. The classical doctrine—deals with the anatomy of formal organization (Taylor, Mooney, Reiley, Brech, Allen).
2. Neoclassical theory of organization—identified with the human relations movement and was inspired by the Hawthorne studies (Gardner, Moore, K. Davis).
3. Modern organization theory—possesses distinctive qualities in its conceptual-analytic base, its reliance on empirical research, and its integrative nature; the only meaningful way to study organization is as a system (March, Simon, Haire).

Here, Scott identifies with the third alternative, but in a later publication this preference is less clear (Scott 1968). Table 5.2 sets forth this new position. Scott talks about creeds and other options in describing these five alternatives. The first three—scientific management, human relations, and industrial humanism—are stated as creeds, but implicit in them are distinct schools of thought. Yet they are not the same schools as in 1961. The remaining two alternatives—constitutionalism and management science—reflect a new wave of literature that is both growing and impressive, but as yet undigested by scholars of the field. By implication these are projected schools of the future. Scott seemed to believe that the management science approach would win out ultimately. For the present, however, we are left with a picture of conflicting and changing ideologies, creeds, theories, and schools.

Charles Perrow’s History of Organizational Theory

Perrow (1973) presents what amounts to a running commentary in which one school supersedes another over time. Yet vestiges of prior schools remain, so that into the early 1970s some version of all the schools still exists. These schools are as follows:

1. Scientific management or classical (Taylor)
2. Human relations (Barnard, Roethlisberger, Dixon—later Bennis, Likert, McGregor, Blake)
3. Bureaucratic (Weber, the numbers men)
4. Political science—power, conflict, and decisions (Simon, March)
5. Technology (Woodward, Thompson, Lawrence, Lorsch, Perrow)
6. Institutional—goals, environments, and systems (Selznick, Katz, Kahn)

This approach repackages some of the ideas and people considered previously. It places more emphasis on sociology-based schools—not surprising in that Perrow is a sociologist. The account is entertaining, but at times somewhat confusing. There is reference to a Buck Rogers school best represented by Bennis, but Bennis is also a member of the human relations school. What does come through strongly is that, amid the controversy, schools rose to power and sank almost to oblivion very rapidly, and with little or no reference to scientific research.
### William Scott's (1968) Implicit Schools

<table>
<thead>
<tr>
<th>Schools</th>
<th>Starting points</th>
<th>Assumptions about the nature of man</th>
<th>Attitude toward power</th>
<th>Justification for the use of power</th>
<th>Means of handling conflict</th>
</tr>
</thead>
<tbody>
<tr>
<td>Scientific management</td>
<td>Management</td>
<td>Weak, corrupt, and invariable</td>
<td>Keep power submerged as far as possible beneath the surface of interpersonal relations</td>
<td>Necessary for maintaining order and getting people to act contrary to their nature</td>
<td>Mutuality of interests through productivity</td>
</tr>
<tr>
<td>Human relations</td>
<td>Management</td>
<td>Good, mildly plastic, capable of wide differences among individuals</td>
<td>Necessary to preserve order and induce organizational solidarity</td>
<td>Integration of interests through manipulation of needs</td>
<td></td>
</tr>
<tr>
<td>Industrial humanism</td>
<td>Man</td>
<td>Good, plastic, and capable of infinite perfectability</td>
<td>Necessary to hasten the progress of individual perfection and organizational harmony</td>
<td>Leveling, confrontation, personal awareness, interpersonal competence, and intergroup problem solving leading to consensus and democratization</td>
<td></td>
</tr>
<tr>
<td>Constitutionalism</td>
<td>Processes that produce formal and informal pacts</td>
<td>Predisposed to act in a framework of negotiated laws and rules—political</td>
<td>Power determines who gets what; a fact of organizational life; should not be submerged</td>
<td>Does not need justification</td>
<td>Rule by mutual expectation, which reduces ambiguity and conflict</td>
</tr>
<tr>
<td>Management science</td>
<td>Technology</td>
<td>Materialistic, pleasure-seeking, comfort-loving, motivated to produce and consume</td>
<td>Power should be manifest and vested in a technical elite because they have the expertise to use it</td>
<td>Rationality</td>
<td>Equitable and generous distribution of rewards based on technical ingenuity and efficiency</td>
</tr>
</tbody>
</table>

Schools and More Schools

There are a number of other lists from the 1960s that exerted a degree of influence at the time, usually in favor of one school or another. Among these are Bennis (1966), Etzioni (1964), Gordon (1963), Krupp (1961), Pugh (1966), and Woolf (1965). These classification systems often use new names and introduce new insights, but basically they are concerned with some combination drawn from the lists of schools already considered. As a group, however, they demonstrate exactly how great the preoccupation with conflicting schools was at the time. This preoccupation extended to myself (Miner 1971). My version ran as follows—classical (Fayol, Urwick), human relations (Mayo, Roethlisberger), structuralist (Weber), behavioral humanist (Likert, McGregor), and decision making (Simon, Barnard).

A particularly interesting portrayal is set forth in Figure 5.1. Luthans (1973) positions his seven schools in a sequence over time with all coalescing into the contingency school shortly after 1980. Note, however, that this sequence was proposed at a point in time just beyond the midpoint of the diagram. Things have not turned out exactly as projected, and, in fact, a problem exists because the contingency school can be traced back to the 1960s. Nevertheless, the idea of a sequence of schools, with some being superseded by others, matches Perrow’s approach—and both were proposed in 1973. Contrary to Koontz, these views depict a jungle of reducing density. It is hard to find agreement in the schools literature.

THE INTELLECTUAL CONTEXT IN RELATED ARENAS

A question arises as to the intellectual environment elsewhere in the world, outside the United States and particularly in Europe where organizational behavior had a good foothold. Infor-
Information in this regard is provided by Hofstede and Kassem (1976). Things appear to have been very different there. One factor was that few business schools existed outside the United States in the early period. As business curricula did emerge they took on forms that differed substantially from what existed in the United States (Thompson 2004).

What became organizational behavior in Europe focused on macro structural issues and was dominated by sociologists housed in sociology departments or in independent research units. The predominant methodology was comparative case analysis. The ideology was focused on conflict, consistent with a Marxist orientation. In general, the ties to practice were limited and theorizing was accordingly more abstract than practical. Although Europe fed into the mélange of schools existing in the United States at various points, and had held a dominant position at one time prior to World War II, with Fayol, Weber, Lewin, Pareto, and the like, less schools-type controversy existed there during the 1950s and 1960s. The major problems and differences across countries appear to reflect variations in acceptance of American business school models both as to extent and as to specific aspects (Üsdiken 2004). See also Kieser (2004) for Germany; Kipping, Üsdiken, and Puig (2004) for France, Italy, Spain, and Turkey; Engwall (2004) for Sweden, Finland, Denmark, and Norway; and Tiratsoo (2004) for Britain. The range of variation has indeed been great.

A second arena relates to practice, especially in the United States. Did the schools phenomenon carry over into management and produce the same set of disparate approaches there? The results of a comprehensive survey conducted by the National Industrial Conference Board (Rush 1969) is revealing on that score. First, the survey indicates that the ideas of six social scientists had particularly captured the attention of managers—McGregor, Maslow, Herzberg, Likert, Argyris, and Blake. All fit into what I have called the behavioral humanist school. Organizational behavior outside this school was hardly recognized.

Second, and consistent with the above, business applications of organizational behavior were best described as follows:

It is an applied science that is normative and value centered.
Although oriented toward economic objectives, it is humanistic and optimistic.
It is concerned with the total climate or milieu and with the organization as a total system.
The major concern is with the use of groups, particularly with the aim of promoting participation.
The development of interpersonal competence is a primary goal.
It is an ongoing process to manage change. (Rush 1969)

The data indicate that although conflict was the order of the day in academe, this was not the case in the business world; the battle had been won there almost before it started. Yet this was true only for those who had an opinion. If asked what views they held relative to organizational behavior, a number of business people of that time would certainly have answered “none” or “don’t know.”

Finally, a question arises regarding the spillover effect from organizational behavior to personnel management. Consistent with the times, controversy appears to have prevailed in personnel as well (Dunnette and Bass 1963; Patten 1964). Much of this controversy was schools related, harking back to roots in a much earlier period extending to the 1920s (Kaufman 2001).
THE DISAPPEARING SCHOOLS

Over the years, and certainly since the early 1980s, the schools controversy has diminished steadily. Many of the schools seem to have disappeared. Some have left the field, some have faded into nonexistence, some have engaged in a process of co-optation to the point where they are hardly recognizable. A few with the support of research have become established as major theories or theoretical positions. These schools with all of their divisiveness are now of primarily historical significance (see Duncan 2004); they are no longer a dominant feature on the organizational behavior scene. This is what would be expected, given the history of other sciences. But there surely was an extended period of high controversy and uncertainty in the theoretical world of organizational behavior before the schools began to disappear.

Some, no doubt, would contend that this schools phase disappeared only to be replaced by a new schools phase that is just as controversial and interlaced with ideas from philosophy. I do not believe this is true, although a case might be made in this regard. It is apparent, for instance, that the macro component of organizational behavior is infused with what look to be schools—population ecology, neo-institutional theory, resource dependence theory, structural contingency theory, and perhaps others. However, on closer scrutiny it becomes evident that these “schools” are surrounded by a body of research—research that takes them out of the realm of philosophy and into science. They are testable and they have been tested; perhaps not to the degree they should be and not to an extent that would produce consensus, but sufficiently to remove them from philosophy. Thus, these are not schools in the same sense that classical management and human relations were.

The same cannot be said for postmodernism and its siblings. They are very much in the realm of philosophy and they have entered organizational behavior primarily through the medium of theory, and to some extent research as well, as they seek to redefine research to a particular type of qualitative standard. They have had considerable appeal in some quarters based on a largely countercultural emphasis. They seem to eulogize divisiveness and conflict to a point that appears anarchist in nature. They are philosophical positions with nontestable propositions, they have not generated a body of scientific research, and they arouse considerable emotionality. This looks very much like a school or a set of quite similar schools. Why we are now experiencing this throwback to an earlier period is not entirely clear. It may be that the activism of the 1960s and early 1970s produced a group of people in organizational behavior who are now of an age to exert considerable influence and who are predisposed to accept “contra” positions that are antiscience because science represents control and authority.

A final point arises out of the fact that both the first generation (with its schools) and the second generation (with postmodernism) within organizational behavior have created and experienced substantial conflict. Which had more? I do not think we know, and presumably we never will. However, the potential negative consequences of the first generation conflicts were greater, simply because the field was more fragile and could point only to a small body of research to bolster its scientific status.

CONCLUSIONS

The intellectual environment of the early period was characterized by organizational behavior’s status somewhere between philosophy and science. The schools phase was in control—or perhaps out of control is a better description. The solution to this source of confusion has
clearly been scientific research, lots of research. Over time, slowly and very hesitantly, with slips backward in individual cases, organizational behavior has been able to establish itself, and move to a scientific footing (see Goodrick 2002). That status is currently under attack from some quarters, but it remains strong. Science, with its emphasis on testability and falsifiability via research, has taken hold and provided a method for defusing controversy.

As Lawrence (1987) noted the period from the late 1950s on was a very productive one for theory construction. This appears to be because the unfreezing process associated with social scientists entering the business schools forced a number of people and lives out of established patterns, with the result that ideas that had never been juxtaposed before suddenly were. New combinations were everywhere, available for the picking. And as people did the picking, new theories were created. Certainly, the harvest at that time was strongly influenced by prevailing values. Some combinations of ideas that were ripe for picking were allowed to wither because the values of the times did not give them visibility. Yet a great many new combinations were found. Thus, all the controversy appears to have had its share of positive consequences in the form of a burgeoning output of new and valid scientific theories. Again, science has been both the recipient and the source of these benefits.

REFERENCES


PART II

MULTIDISCIPLINARY ORIGINS OF ORGANIZATIONAL BEHAVIOR

We have considered the scientific foundations of organizational behavior and how the field has progressed as a science. We have looked into the uneasy relationship with philosophy that continues to yield both positive consequences in fostering theory and negative consequences in diluting the commitment to science. As the story unfolded, especially in Chapter 5, we began to look at the historical origins of the field, but with particular emphasis on the process of scientific development (see Goodrick 2002). Now we focus on the subject of historical origins, a topic that is of concern in both parts II and III. Understanding these historical origins is no less important than understanding the scientific foundations, if one is to truly comprehend organizational behavior in the present (Bedeian 1998).

In Part II, the discussion deals with those origins that have influenced the course organizational behavior has taken, but that were not at the time part of the field (because it did not exist). These origins span a roughly fifty-year period during the first half of the twentieth century. The key names are Elton Mayo, Chester Barnard, Kurt Lewin, Mary Parker Follett, and Max Weber, joined by Henri Fayol and Frederick Taylor whose influence on organizational behavior was of a very different type than the preceding five. As will become evident, the disciplinary bases of these individuals are extremely diverse.

A word is in order regarding the way in which these key influentials were identified. The starting point was to establish my own list, working from a background of experience in writing an earlier book (Miner 1971). The result was the seven individuals noted above. This list was then checked against inclusion in various books that consider the contributions of key individuals to organizational thinking, and which go back to an earlier period before the emergence of organizational behavior while also including intellectual contributors as well as practitioners. These validation sources were Developments in Management Thought (Pollard 1974), Great Writers on Organizations: The Omnibus Edition (Pugh and Hickson 1993), and Management Innovators: The People and Ideas That Have Shaped Modern Business (Wren and Greenwood 1998).

Four of the seven individuals are spotlighted in all three books, thus clearly supporting their inclusion here; these are Mayo, Barnard, Follett, and Taylor; Fayol is considered twice, but not in Wren and Greenwood (1998). The latter volume gives particular attention to U.S. contributors, although others are considered as well, and includes many practitioners who are not known for their writings; this one omission of Fayol does not seem to invalidate his being profiled here. Weber is presented only in Pugh and Hickson (1993), a curious situation that seems to reflect the fact that the other books are less concerned with contributions to sociological thinking and macro-level theorizing. Since I want to give full coverage to the
origins of organizational behavior in this particular regard, not including Weber would seem to be an act of neglect.

Finally, there is Kurt Lewin who is on my list but is not even mentioned in the indexes of any of the three sources used to validate my selections. In spite of this situation, I believe a good case can be made for the inclusion of Lewin among the early contributors to the development of organizational behavior, and I attempt to make that case in Chapter 8. However, the reader should be aware that in this single instance the field really has not supported my judgment.

Further validation evidence can be taken from more recent sources that arrived subsequent to my actual selection of the influentials. Most noteworthy is a report on the most influential management books of the twentieth century, in which books by five of the seven are profiled—only Follett and Lewin are omitted (Bedeian and Wren 2001). A book by Hoopes (2003) dealing with the gurus who created modern management fails to consider Lewin, Weber, and Fayol, and another source (Witzel 2003) focusing on fifty key figures in management omits a majority of the seven (Mayo, Barnard, Lewin, and Weber). An article in the Harvard Business Review titled “Who Are the Gurus’ Gurus?” (Prusak and Davenport 2003) notes only Kurt Lewin among the forty-eight listings. This is the only instance in which Lewin is noted. Clearly, there is wide variation as to who are considered influentials. However, many of those included on these lists had no direct relationship to the emergence of organizational behavior per se as a discipline. Thus, for instance, Benjamin Franklin who has been identified as a founding father of management as a field (McCormick 2001) has no such relationship to organizational behavior.

Given the fact that in Part II we are concerned only with the predecessors and roots of organizational behavior, not with the theorists and researchers who came on the scene as the field moved into high gear, it is important to identify a point at which organizational behavior became established as a distinct discipline. There is a certain arbitrariness in doing this because the emergence was actually quite gradual and the consensus that ultimately established the fact of existence developed gradually as well. The problem of pinpointing a date is further confounded by the fact that during the 1940s and 1950s a number of often competing designations existed—human relations, human organization, administrative science, behavioral science, and organization theory as well as, toward the end, organizational behavior. Nevertheless, I believe the founding can be narrowed down to a specific time period.

Some level of concern with organizational issues existed within psychology from the point that industrial psychology came on the scene in the first decade of the 1900s (Gilmer 1981). However, the term “organizational” was not added to the name of the division of the American Psychological Association involved until 1970, long after organizational behavior had clearly come into existence. There was something variously referred to as organizational psychology or managerial psychology (Leavitt 1958) present well before 1970, but it existed as part of psychology, not as a separate discipline.

Somewhat the same situation occurred within sociology and anthropology. Whyte (1983) dates the beginnings of “our field” back to the initiation of the Hawthorne research in the 1920s. However, that field carried the title human relations and it was defined narrowly. Lawrence (1987), pointing largely to developments at the Harvard Business School, argues for a beginning in the early 1940s. Greiner (1979) puts the origins of the field in the latter part of that decade. All three of these individuals had ties to Harvard and to sociology/anthropology. In the 1940s, human relations was the prevailing term (Gardner and Moore 1945). Over time, the subject matter broadened somewhat, but there remained a close asso-
Within anthropology, the Society for Applied Anthropology used the title Human Organization for its journal from the late 1940s on (Adams and Preiss 1960). However, again, this was a development within anthropology, not separate from it. The human relations movement came closest to breaking away from the basic disciplines, but its ties to science were shaky and its influence peaked during the 1940s and early 1950s (Porter and Bigley 1995); its focus and content covered only part of what became organizational behavior.

The term management was used in the business schools for many years and is reflected in the founding of the Academy of Management in 1936. Yet, at its beginning, this was not a scientific discipline, and it has come to possess a breadth that extends well beyond organizational behavior—to strategic management and production management, for instance.

Given all this, the time frame for the creation of organizational behavior seems to come down to sometime in the 1950s. Roberts, Weissenberg, Whetten, Pearce, Glick, Bedeian, Miller, and Klimoski (1990) say it was the early 1950s, based on their interviews with early contributors to the field. Blood (1994) takes the position that organizational behavior really began with the pronounced movement of social scientists into the business schools starting around 1955. Goodman and Whetten (1998) attribute the beginning of the field to this same phenomenon. Elsewhere I have ascribed the birth to the latter 1950s (Miner 1997). The Administrative Science Quarterly and the Journal of the Academy of Management, both major publications that have come to stress the scientific foundations of organizational behavior, were founded in the business schools in 1956 and 1958, respectively.

A particularly helpful treatment of the historical development of the study of organizations is provided by Scott (2004). This piece is primarily concerned with developments within sociology, but extends to organizational behavior as well, which is said to have exhibited “a common heritage and bear similar birthmarks” (4). In both areas “organizations emerged as a recognized field of social scientific study during the 1950s” (3). Within sociology, the organizational terminology first appeared in the discipline’s division structure in 1968, thus at roughly the same time as in psychology.

On balance, 1955 appears to be at least adequate as a designated start-up date, with the recognition that five years either way from this median would not represent a real distortion. Insofar as theory is concerned, the first generation of contributions that arose out of the initiation of organizational behavior appears to have extended over a period of roughly twenty years from 1955 to 1974 (see Miner 2002).

Table II.1

<table>
<thead>
<tr>
<th>Name</th>
<th>Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>Max Weber</td>
<td>1904</td>
</tr>
<tr>
<td>Frederick Taylor</td>
<td>1911</td>
</tr>
<tr>
<td>Henri Fayol</td>
<td>1916</td>
</tr>
<tr>
<td>Mary Parker Follett</td>
<td>1924</td>
</tr>
<tr>
<td>Elton Mayo</td>
<td>1933</td>
</tr>
<tr>
<td>Chester Barnard</td>
<td>1938</td>
</tr>
<tr>
<td>Kurt Lewin</td>
<td>1939</td>
</tr>
<tr>
<td>Estimated date of organizational behavior's founding</td>
<td>1955</td>
</tr>
</tbody>
</table>
I use the date 1955 in Table II.1 to set a baseline against which the various contributors to Part II are compared in terms of the dates of their major contributions toward the development of organizational behavior.

REFERENCES


CHAPTER 6

ELTON MAYO AND HAWTHORNE

The Cast of Characters
The Research at Hawthorne
    The Relay Assembly Test Room
    Research Innovations in 1928
    The Bank Wiring Observation Room
    The Counseling Program
Interpretations
    Mayo
    Roethlisberger and Dickson
Complications
Conclusions

The Hawthorne research was carried out at a large manufacturing plant operated by Western Electric, a subsidiary of AT&T, on the outskirts of Chicago. This Hawthorne Works was the site of various studies extending from November 1924 to February 1933; a related endeavor focused on employee counseling was initiated in February 1936 and terminated for all practical purposes in early 1956.

This research through much of its history was a joint effort involving Western Electric personnel, Harvard Business School professors, and Massachusetts Institute of Technology (MIT) professors. However, the task of describing and interpreting the studies and results fell to the Harvard contingent primarily. Elton Mayo was the dominant force within that contingent, and his ideas are the ones that have lived on to influence organizational behavior the most.

The major sources for these ideas are two books (Mayo 1933, 1945). Two additional books provide not only reflections of Mayo’s ideas, but more detail on the research at Hawthorne as well (Roethlisberger and Dickson 1939; Dickson and Roethlisberger 1966). A volume containing papers produced at a symposium convened to honor the fiftieth anniversary of the initiation of the Hawthorne studies provides both a testimonial to the significance of the research and a picture of its influence (Cass and Zimmer 1975). In the following treatment, I rely not only on these sources but also on the excellent historical discussion provided by Gillespie (1991).

THE CAST OF CHARACTERS

Elton Mayo was born in Brisbane, Australia, in 1880. In the family tradition, he began a medical career there, but failed his second-year exams. Additional courses in medicine in Edinburgh and London moved him no closer to an MD degree. Subsequently, he returned to Australia and completed a degree in philosophy and psychology. He then taught philosophy for a number of years at the just-established University of Queensland. By 1922, he had decided to leave Australia for the United States.
In this country, he ultimately established contact with a number of influential financial and psychological leaders and through them obtained a research position at the University of Pennsylvania (Trahair 1984). This position and almost all of Mayo’s subsequent work was funded by the Rockefeller family and later the Rockefeller Foundation. Yet the research conducted with these funds in various Philadelphia textile mills had limited success, largely due to the overriding constraints imposed by practical business considerations. As a result Mayo, with the help of his foundation friends, moved to the Harvard Business School in 1926 and became involved in the Hawthorne research in 1928. He remained at Harvard, and involved primarily with Hawthorne matters, until his retirement in 1947. He then assumed a position as adviser on industrial relations to the British government, but died two years later in 1949.

As this brief biographical sketch indicates, Mayo achieved little success in life prior to his Harvard appointment at the age of forty-six. An effective speaker and proficient in cultivating influential friends and mentors, he nevertheless had little by way of academic credentials and practically no training in the conduct of scientific research. In actual fact, he was much more a social philosopher than the psychologist or psychiatrist he would have preferred to be.

Fritz Jules Roethlisberger was a protégé of Mayo, eighteen years his junior, who was introduced into the ongoing Hawthorne research in 1930. He had an undergraduate degree in engineering from MIT and dabbled with studying for a doctorate in philosophy at Harvard, but never completed the degree requirements. (He did receive an M.A.) Mayo obtained funding for Roethlisberger from the Rockefeller Foundation, and the two worked together for many years at Hawthorne and Harvard. Ultimately, Roethlisberger rose to full professor at the Harvard Business School and had a distinguished career as a writer and teacher in the areas of communications, training, and counseling. Like Mayo, he was not schooled in the conduct of scientific research. He died in 1974 at the age of seventy-six.

William Dickson came to the Hawthorne research in 1929 in a summer job intended to support his work on a PhD in economics at the University of Chicago, which he had begun the previous year. He never returned to school, rising to various positions in personnel research at Hawthorne and retiring in 1969 as assistant superintendent for personnel research at New York headquarters. In late 1932 Dickson went to Harvard to help write up the research conducted to that point. He remained at Harvard until February 1936 writing Management and the Worker, and then he returned to Hawthorne.

Dickson was one of many Western Electric personnel managers involved in the research, but he was the only one who assumed a major and continuing role in presenting it to the public. Note that Dickson, like Mayo and Roethlisberger, was not well trained in the conduct of scientific research. This is important, as we shall see shortly. Yet the Rockefeller philanthropies spent over a million and a half dollars supporting Mayo and his colleagues in the twenty-year span from 1923 to 1943.

THE RESEARCH AT HAWTHORNE

Table 6.1 presents a chronology of the various studies. Note that Mayo was not involved at all in the illumination experiments and only entered into the relay assembly test room a year after it had been designed by Hawthorne personnel. At no point did he actually remain in Chicago to conduct any research project for an extended period; his visits were in and out, and frequently there were long periods in between. Among the Harvard people, Roethlisberger was present the most, but he did not enter on the Hawthorne scene until the relay assembly
test room work was more than half completed. Clair Turner, professor of biology and public health at MIT, worked entirely independently of the Harvard contingent and focused on the relay assembly test room. He did not contribute to the interpretation of the research, and one gets the impression that his input overall was marginal at best.

The illumination experiments were part of a broad research program under the direction of the National Research Council. The impetus behind this program came from the electrical industry, which wished to foster the installation of artificial lighting throughout manufacturing industry. The underlying hypothesis was that doing so would raise production levels substantially. The lead researchers for the Hawthorne segment of the research program were from MIT. The experiments appear to have been well conducted using both experimental and control groups, and looked into payment schemes and variations in supervision as well as lighting. The studies were unable to establish any consistent difference between experimental and control conditions, and thus the hypothesis of an increase in production with better lighting was not confirmed. No final report was ever prepared on this research; the National Research Council withdrew its support in early 1927 (see Wren and Greenwood 1998).

### The Relay Assembly Test Room

Believing that the key to productivity increases resided more in the workers than in the environment, the Western Electric people set up a small group of six female assemblers in a separate room to continue the research. The design called for the group to serve as its own control; there were no control groups per se, although some comparison data were obtained from regular operations. Changes in experimental conditions were introduced in each of the twenty-four periods based primarily on the hypotheses of the observer who shared the room with the assemblers. No overall program for experimental changes existed at the beginning.

#### Table 6.1

<table>
<thead>
<tr>
<th>Event</th>
<th>Date</th>
</tr>
</thead>
<tbody>
<tr>
<td>Illumination experiments</td>
<td></td>
</tr>
<tr>
<td>First series:</td>
<td>November 1924 to April 1925</td>
</tr>
<tr>
<td>Second series:</td>
<td>February to April 1926</td>
</tr>
<tr>
<td>Third series:</td>
<td>September 1926 to April 1927</td>
</tr>
<tr>
<td>Relay assembly test room</td>
<td>May 1927 to February 1933—twenty-four experimental periods</td>
</tr>
<tr>
<td>Mayo (Harvard) and Turner (MIT) come aboard</td>
<td>April/May 1928</td>
</tr>
<tr>
<td>Second relay assembly group</td>
<td>August 1928 to March 1929</td>
</tr>
<tr>
<td>Interviewing program (plantwide)</td>
<td>September 1928 to January 1931</td>
</tr>
<tr>
<td>Mica splitting test room</td>
<td>October 1928 to March 1931</td>
</tr>
<tr>
<td>Bank wiring observation room</td>
<td>November 1931 to May 1932</td>
</tr>
<tr>
<td>Counseling program (plantwide)</td>
<td>February 1936 to early 1956</td>
</tr>
</tbody>
</table>
Experimental variables tested at one time or another, and often in conjunction with one another, were pay system variations, rest pauses, morning tea, early stopping times, changed positions, days off, and variations in weekly hours.

Productivity was measured in terms of assembled units produced per hour. The average output per employee per hour over the full course of the study was as follows:

<table>
<thead>
<tr>
<th>Periods</th>
<th>Duration</th>
<th>Output</th>
</tr>
</thead>
<tbody>
<tr>
<td>1–4</td>
<td>20 weeks</td>
<td>50.6</td>
</tr>
<tr>
<td>5–8</td>
<td>26 weeks</td>
<td>57.1</td>
</tr>
<tr>
<td>9–12</td>
<td>37 weeks</td>
<td>61.9</td>
</tr>
<tr>
<td>13–16</td>
<td>75 weeks</td>
<td>68.1</td>
</tr>
<tr>
<td>17–20</td>
<td>80 weeks</td>
<td>69.1</td>
</tr>
<tr>
<td>21–24</td>
<td>32 weeks</td>
<td>70.5</td>
</tr>
</tbody>
</table>

From period 17 on the hours worked per week were reduced, largely as a result of decreased product demand occasioned by the onset of the depression. Note that at this point production leveled off considerably, rather than rising sharply as it had previously.

As a set of experiments, the relay assembly test room research left much to be desired. In particular, using the subjects as their own controls, rather than introducing control groups, made attributing causation almost impossible, especially given the changing economic circumstances in the United States over the period of the research and the increase in labor union activity. The experiments violated many of the requirements for conducting experimental research (see Chapter 3). For example, many appropriate statistical analyses were not carried out at the time of the research, and two low-producing subjects were removed from the experiment entirely during period 7 and replaced with much more productive assemblers.

**Research Innovations in 1928**

Close to the one-year point of the relay assembly room experiments it became evident to the Hawthorne people that the research was not yielding definitive information. Accordingly, a number of innovations were introduced. First, the Harvard and the MIT consultants were brought in. This produced some new hypotheses to investigate, but no clear interpretations of the existing data. Second, several new studies were introduced to get at specific hypotheses. These utilized typists, mica splitters, armature straighteners, and a second group of relay assemblers as subjects.

Some of these studies appear to have disappeared into oblivion over time with no record of the results. Full reporting of all the studies at Hawthorne did not occur. One suspects that the researchers simply lost interest in research that did not appear to be going anywhere.

The second relay assembly group was instituted to study the effects of a group payment system, but with other conditions held constant; thus, the subjects remained under regular supervision and in normal working conditions. The results provided considerable support for the influence of payment systems on output, and went at least part way toward explaining the first relay assembly findings. The study was short-lived, however, because of conflict between the experimental group and the regular department.

The mica splitting test room was established to study the effects of various experimental conditions when workers were paid an individual piece rate. Again there was a rise in productivity, although some drop-off occurred later, apparently under the pressure of de-
pression-related factors. All in all, this research, too, seemed to indicate that earnings levels were important in any rise in output.

The final innovation introduced in 1928 was what ultimately became a plantwide interviewing program. Interviewing had been employed with workers who were under study as early as 1925, but now it was applied everywhere. The result was something like a large-scale attitude survey. The results were used to improve working conditions and to design supervisory training programs. Extensive analyses, including statistical analyses, were made of the interview data. However, no effort was made to compare the subsequent productivity of groups that did and did not undergo the interviews. Thus, the overall contribution to unraveling the relay assembly results was minimal. To Mayo’s credit, however, he was largely responsible for introducing interviewing techniques of a nondirective nature that paralleled the development of nondirective counseling in psychology (Mahoney and Baker 2002).

The Bank Wiring Observation Room

The bank wiring observation setting was a separate room, but otherwise standard working conditions were maintained. No experimental interventions were introduced. There was an observer in the room, but supervision was handled in the regular manner. Output proved to be steady, but somewhat below management expectations. It was restricted by the workers apparently out of a fear that management would take advantage of them if they performed to capacity. This, however, was not a new finding; evidence of similar phenomena when piece-rate systems of payment are used had existed for many years. Two cliques clearly developed among the fourteen male workers, and each of these exhibited very different group norms.

This was intended as an observational study of group dynamics. It was essentially anthropological in nature and was strongly influenced by the noted anthropologist W. Lloyd Warner, then on the faculty at Harvard. Warner, however, did not publish these findings. The observations clearly indicated that management’s control over the workers was of a limited nature. There was little here to help explain the earlier results.

The Counseling Program

Counseling was introduced at Hawthorne some three years after the prior studies had ceased; thus, nothing happened during the depth of the depression. From 1936 to 1941, the program grew steadily at Hawthorne, but during much of the period, it was small in scope. From 1942 to 1948, counseling became an established personnel activity and spread to other Western Electric facilities. By 1948, some fifty-five counselors were employed at Hawthorne covering two-thirds of the workforce. The period from 1949 to 1955 was one in which the program became increasingly an object of critical evaluation and the number of counselors declined steadily. By early 1956, no counselors were on the rolls, although the program structure existed on paper for another five years.

In what sense was this counseling activity part of the Hawthorne research, rather than simply a transient personnel procedure? Initially, the goals of science and research were an inherent part of the program, and counseling was viewed as a human relations effort directly linked to the prior inquiries, especially the interviewing. However, other than some studies of the manifest content of employee concerns, little research was conducted. Research that might provide useful input to management decision making was minimal at best. No evaluative studies comparing counseling with no counseling, or one type of counseling approach
with another, were undertaken. The counselors were not researchers, and ultimately the research function disappeared from the program completely.

Why did the counseling program cease? Dickson and Roethlisberger (1966) decline to deal with this issue, but Gillespie (1991, as cited in Hoopes 2003), offers five reasons:

1. The costs of the program were high and clearly evident, while the benefits were difficult to demonstrate, especially given the lack of evaluative research.
2. The program became bureaucratized and lacked a clear goal; once the research function disappeared the major objective was survival.
3. The decentralized structure introduced into Western Electric made for many more small plants, with the distance between management and the worker sharply reduced, thus obviating the need for counseling.
4. As unions came to represent more Western Electric employees and became more powerful, the need for the often-parallel counseling structure decreased; some no doubt went so far as to argue that the growth of union representation reflected the failure of the counseling program.
5. As supervisors were increasingly trained in human relations approaches they came to replace the counselors to a substantial degree; there never was a meaningful integration of the human relations training and counseling functions. It has been claimed that the human relations training was intended to make therapists out of the supervisors, in Mayo’s image (Hoopes 2003).

Put more succinctly, the reason for the demise could well have been that “it simply may not have worked” (Highhouse 1999, 328).

**INTERPRETATIONS**

As we move from the conduct of the research and personnel programs at Hawthorne to the dissemination of interpretations of research results and program evaluations to the public, an important division of labor should be noted. At Hawthorne, management controlled the various activities, but the interpretation process was carried out primarily at Harvard by Mayo, Roethlisberger, and Dickson, supported by Rockefeller Foundation money. As a consequence, these writers had a great deal more discretion than would have been the case had personnel researchers at Hawthorne done the writing. This is not to say that the two groups were necessarily at odds, only that Mayo and those who worked with him on preparing the interpretations were far removed from the research scene and in a position to introduce their own views. None of these people had full knowledge of what had happened at Hawthorne.

**Mayo**

In Mayo’s view, the conduct of the Hawthorne research had revealed many inadequacies in existing scientific approaches and had accordingly forced major changes in scientific method on the investigators. Thus, at the outset, there was an attempt to avoid the constraints of good science and gain acceptance for approaches that otherwise might be questioned. Furthermore, Mayo took the position that the research was undertaken, not to find ways of increasing productivity, but to promote scientific understanding—the development of more precise and biological knowledge of the human situation in industry and of the conditions that affect
human capacity for work. The significance of the Hawthorne studies extended far beyond a single company’s desire to increase output.

Mayo discounted the charge that industrial work was by its nature monotonous and fatiguing. What the researchers did in the relay assembly test room was strengthen the inner equilibrium of the workers so that they were able to achieve a mental steady state, which, in turn, made them resistant to a wide range of external conditions. Worker complaints should be viewed as more indicative of the person’s situation and internal state than of workplace conditions; complaints about supervision were particularly prejudiced and irrational.

Drawing upon the psychiatric literature, Mayo argued that workers could be jolted into obsessive thinking and mild neuroses by events that disturbed their equilibrium. Output would decline accordingly. Restriction of output was thus an exasperated response to a poor work situation, not a rational means of collective action. This poor work situation was in turn a reflection of the general social disorganization of an industrial society, where traditional social norms and values had broken down under the stress of rapid social change. Thus, the relay assembly and bank wiring rooms were placed at opposite poles of social organization, with the latter more characteristic in industrial society.

Mayo used the relay assembly room to illustrate the need for teamwork and cooperation. Feelings of security and certainty derive from membership in a group. Managers need to foster the organization of work teams and the participation of these teams in the purposes of the organization. Consultation with the workers, and preferably self-determination by the workers themselves, should be encouraged. Workers should be provided an opportunity to talk about their problems, and supervisors should be trained to cope with these human situations. The objective in all of this is to create a kind of spontaneous cooperation throughout the company of the kind that characterizes well-organized social structures. Thus, the three persistent problems of management are:

1. The application of science and technical skill to some material or product to meet the economic needs of society,
2. The systematic ordering of operations, and
3. The organization of teamwork, and thus of sustained cooperation, to meet the social needs of society.

The problem of the administrative elite was that it was technically and economically oriented, and lacked understanding of human and social needs. The future of society depended on a managerial class that could restore opportunities for human collaboration and fully recognize the need for social solidarity. Revolution occurred when the ruling class failed to administer society effectively, and that required a class, trained by the universities to use methods of promoting human collaboration effectively (human relations training).

Roethlisberger and Dickson

Roethlisberger and Dickson (1939) generally follow Mayo philosophically, but they write from a stance that is much closer to Hawthorne and that deals much less with societal problems. They note the tendency of complaining workers to project their troubles on others and thus to overthink the situation. Meanings are colored too much by personal experiences outside the work situation. Complaints could not be taken seriously as criticisms of company policies or conditions. This obsessive thinking is widespread and has
much in common with the preoccupations found in psychoneurosis. To hide industrial problems under general terms such as fatigue, monotony, and supervision may well fail to differentiate among the various types of interference involved, and thus among the different kinds of disequilibrium that may be operating.

Industry is faced with two major sets of problems—problems of external balance (economic) and problems of internal equilibrium. The latter are concerned with maintaining a social organization where individuals and groups can work together to satisfy their personal and collective desires. In this context, informal organization, although capable of restricting output, is not bad. It is, in fact, a necessary prerequisite for effective collaboration. The parts of a plant social system are interdependent; changes in one part create changes in other parts. If a small change is introduced, equilibrium will be disturbed, and a reaction to restore equilibrium will be activated. The technical organization can change more rapidly than the social; the formal organization more rapidly than the informal. In these disparities, there exists the potential for imbalance, which can breed distrust and resistance to change.

Returning to the research itself, the authors deny that fatigue was a causal factor in the relay assembly room. Rest pauses produced increased output, not because they reduced fatigue, but because of their social function; it was their meaning that was important. Similarly, physical conditions of the work were not related to output rate; nor was the repetitive nature or monotony of the work. Monotony can be reduced by social interaction, but if this is restricted or proscribed, morbid preoccupations and daydreams may surface, to the detriment of output. Such obsessive thinking can breed complaints and grievances as well.

Wage incentives and wage incentive systems were a continuing concern of the Hawthorne research. Yet the interpreters consistently felt that none of the results gave the slightest substantiation to the view that the workers were primarily motivated by economic interest. In fact, they contended that the efficacy of a wage incentive is so dependent on its tie to other factors that separating it out as an independent cause is impossible.

Mayo, and Roethlisberger, too, emphasized the importance of social needs and sentiments, in addition to economic needs, in work and productivity. They stressed the role of informal groups and organizations and the need for equilibrium between these and the technical and formal organizations. Roethlisberger, in particular, was concerned with problems of communication, especially interpersonal communication (as between supervisor and worker). Communication and listening skills were stressed as crucial to effective managing (Roethlisberger 1953).

Complications

Mayo, Roethlisberger, and Dickson said that the social factor had been badly overlooked, and that a full understanding of factory work required going beyond the considerations emphasized by scientific management to matters such as employee attitudes, social motives, and group processes (especially informal processes). Thus, the human relations movement was born, and a major part of the foundation for the organizational behavior discipline was put in place. A number of the larger companies soon instituted human relations training programs for supervisors, as well as attitude surveys (the Hawthorne interviewing program was one such approach) and counseling programs designed to improve employee attitudes. All these represented major influences on practice, and, to a degree, theory as well.

The idea that industrialization had disrupted traditional social relationships was important in this thinking. Workers were frustrated in a social sense; informal groups, restriction of
output, and the like represented their attempts to deal with social frustration. A new social code had to be reestablished to facilitate collaboration in industrial organizations. Without it, workers would continue to experience social deprivation and a kind of social pathology; productivity would suffer as a result. Here, the interpretations became more tenuous; it is not clear that the Hawthorne research said this.

Skepticism on this score has accelerated over the years to the point where it is now widely accepted that Mayo, and Roethlisberger and Dickson, went well beyond the Hawthorne data in setting forth their interpretations. This view is reflected in writings from a variety of political and disciplinary perspectives (Bramel and Friend 1981; Yorks and Whitsett 1985; Wren 1987). Managerial bias on occasion has been attributed (Brief 2000). One could argue that the Hawthorne data provided an inductive source from which the authors’ theory was derived. There are two problems with this:

1. That is not what the authors said; they claimed that their interpretations reflected the actual results of the Hawthorne studies and that those studies validated their theories.
2. The authors’ interpretations are a mixture of social philosophy and theory that are so generally stated that direct tests would be difficult. Much of Mayo’s writing, in particular, is clearly social philosophy masquerading as science. In short the interpretations are not really theory at all.

Many people have interpreted the Hawthorne research in ways that differ from the Harvard group. In fact, interpreting the studies has become something of an intellectual game among scholars of the field, a game with few rules and many outcomes. I believe this has happened because the research was so poorly designed and conducted that its only product was ambiguity. As a result, textbook accounts often have been insufficient and misleading (Olson, Verley, Santos, and Salas 2004). Remember that the social factors the Harvard authors invoke were never measured in the experimental research at all. We simply do not know what happened at Hawthorne, why production rose in the relay assembly test room, and so on. We never will. The research was so out of control that it cannot yield answers.

One hope is that after-the-fact statistical analyses might salvage something. Mayo and Roethlisberger were not trained in statistics and offer little other than examples of raw data. Turner did carry out some early statistical analyses on the relay assembly test room data, but no evidence of important conclusions exists. Thomas North Whitehead (1938), an economist in the Harvard group, subjected the data to extensive analyses, repeating Turner’s work and extending it in many directions. Yet there were few significant findings. No significant correlations could be established between the various independent variables and the dependent variable of output. Because there were no measures of the social factors considered in the interpretations, their impact could not be assessed in any meaningful manner, although Whitehead tried to do so using the output data.

Much later, Franke and Kaul (1978) carried out extensive statistical analyses on the original data, using techniques not available at the time of the studies themselves. These analyses played up the significance of factors such as the replacement of subjects, the pay system, and the depression, but again the lack of measures of social factors confound the interpretations. The major conclusion from all of the statistical efforts has to be that one cannot compensate for basic deficiencies in research design and measurement in this manner.

An article by George Strauss (1998) says, “Forty years of research have fine-tuned some of the original findings but have done little to shake their essential validity. It is nice to see
them in wide acceptance today” (11). This article featured a discussion of “the famous Hawthorne research.” Yet, given the lack of statistical or any other type of established, meaningful, findings, one has to question Strauss’s conclusion, at least with regard to Hawthorne. There were no clearly established findings of whose validity one could be sure.

CONCLUSIONS

The people whose names have come to be associated with the Hawthorne studies have backgrounds—although not necessarily very extensive ones—in medicine, philosophy, psychology, and economics. None held a doctorate and none had been trained in research. Yet these people concentrated on the study of behavior in organizations at a point in time when few had such interests. Furthermore, they trained others in human relations and in fact established a school of thought around that viewpoint. Many today are inclined to associate these views with sociology, and the followers of the Hawthorne researchers tend to identify with that field. Yet the reality is that Hawthorne attracted a multidisciplinary hodgepodge.

Some idea of the influence on organizational behavior exerted from Hawthorne may be obtained by looking into the fiftieth-anniversary volume (Cass and Zimmer 1975). The research itself was severely flawed, but the ideas that grew up around it have had a significant impact. Of the sixteen professional papers in the anniversary book, nine are authored by prominent organizational behavior theorists of that time. All nine either recognize the importance of Hawthorne for their own work or for related developments in the field. The names of these theorists are Paul Lawrence, Robert Kahn, Edgar Schein, Richard Hackman, Edward Lawler, Harry Levinson, Fred Fiedler, Victor Vroom, and Jay Lorsch. Of these, the direct lineage from Hawthorne is most in evidence for Lawrence and Lorsch who received their doctorates from the Harvard Business School and remained there. Yet the truly significant conclusion is that this diverse array of theorists, from a wide range of backgrounds and orientations, identify Hawthorne as a major intellectual building block of organizational behavior.

An interesting point, however, involves the citations the anniversary volume’s authors make to Hawthorne’s cast of characters. Roethlisberger and Dickson are cited frequently; yet Mayo appears but once—in Kahn’s (1975) paper on the “Hawthorne effect,” which interprets that effect as primarily a matter of participation in decision making. The inescapable conclusion is that Mayo’s direct impact on the field seems to have paled over the years. There can be no doubt that during the 1930s and 1940s, Mayo was at the height of his influence. I do not know why this influence has declined, but I believe it has. It may be that his message was especially attuned to the problems of his time and lost impact as new ideas thrust their way to the fore (see O’Connor 1999).

Next, we consider the views of Chester Barnard, a product of a very different background, but associated in his scholarly period with the same group at Harvard. As is the case in this chapter, the Barnard scenario extends across both business practice and the academic world. He has exerted influence on a number of aspects of organizational behavior, but particularly on work related to managerial decision making (see Miner 2006).

REFERENCES


CHAPTER 7

CHESTER BARNARD’S VIEWS ON MANAGEMENT

Background
The Pareto Circle
The Scholar-Manager Combination

Theory and Philosophy
Open Systems Concepts and Cooperation
Formal and Informal Organization
Incentives and Authority
Decision Making
The Executive Functions
Status Systems and Leadership
Responsibility and Morality

Conclusions

Chester Barnard’s career overlapped with those of the Hawthorne researchers both temporally and geographically. His speeches and writings span four decades—from the 1920s to the 1950s. However, his most important contribution was the widely acclaimed book The Functions of the Executive (Barnard 1938). This book and another containing his addresses and papers (Barnard 1948) provide the basis for this discussion. In addition, I have drawn heavily on the treatments of Barnard’s life and writings by Wolf (1974) and Scott (1992).

To place Barnard’s ideas in context it is important to know that he spent his entire career either becoming, or serving as, a top-level executive. Thus, his writings represented essentially an avocation. He wrote from his experience, his reading of the work of others, and his personal contacts, primarily with scholars on the Harvard faculty. He was not a researcher; he viewed himself as a theorist, and in many respects he was, but his writings contain a goodly amount of untestable philosophy as well. Thus, I refer to his “views” on management in the title of this section.

BACKGROUND

Barnard grew up in a poor but highly intellectual family. At an early age, he showed a preference for elite organizational affiliations, the first example of which was attending Mount Herman, one of the better-known New England prep schools. Subsequently, he went on to Harvard, where he majored in economics. Based on this fact and other considerations as well, some economists have claimed Barnard as one of their own (see, e.g., Williamson 1990). However, he himself had serious misgivings about the value of economics for understanding organizations, and it is clear that his thinking was influenced more by people outside that field than within it.

In 1909 Barnard terminated his studies at Harvard just short of his degree. Clearly, his limited means were a major factor contributing to this decision. He obtained a job in
the Statistical Department at AT&T and rapidly emerged as an expert on telephone rates. He remained in a corporate staff capacity for over ten years, moving then to a vice president position with Pennsylvania Bell. He became president of the newly formed New Jersey Bell Telephone Company at the age of forty-one, a position he held for over twenty years.

In 1948 Barnard left New Jersey Bell to become president of the Rockefeller Foundation. He remained in this position until he reached the mandatory retirement age of sixty-five. Subsequently, he served for a number of years on the board of the federal government’s National Science Foundation. Although a part-time position, this work demanded a great deal of his time, particularly during his four years as chairman. He retired from this position in 1956 and died five years later (1961) at the age of seventy-four.

The Pareto Circle

The Pareto Circle was an informal group of scholars centered on the Harvard Business School. The membership changed somewhat during the 1930s and early 1940s when the circle existed, but all were men of considerable academic stature. Among them were Elton Mayo and Fritz Roethlisberger, sociologists George Homans and Talcott Parsons, anthropologist Clyde Kluckhohn, psychologist Henry Murray, and economist Joseph Schumpeter. The leader and driving force behind the group was Lawrence Henderson, a noted biochemist with an MD degree who was housed in the business school and taught sociology there. Barnard was not a full-fledged member of the circle, having more of an adjunct status, since his major employer was New Jersey Bell. However, he was frequently at Harvard serving on committees, giving lectures, and talking with faculty members. He was influenced considerably by the Pareto Circle, and he in turn influenced the thinking of several of its members including his friend Henderson. Actually Barnard’s initial entrée to his Harvard relationships came through his efforts to raise money to endow professorships there. This led to his being appointed to an advisory committee and ultimately to his Pareto Circle ties (Wren and Greenwood 1998).

The members as a group were politically conservative and a major force drawing them together was a desire to refute the rising influence of Karl Marx at a point during the depression when his views had widespread acceptance among the Harvard faculty. The focal point for their discussions and writings was a book by the Italian sociologist Vilfredo Pareto (1935). Pareto emphasized concepts such as social systems, social equilibrium, cooperation rather than the class conflict of Marx, and elite leadership. We have already seen many of these ideas in Mayo’s interpretations of the Hawthorne research. Pareto’s book seemed to provide an ideological defense against the rising tide of Marxism (Keller 1984). Barnard’s own conservatism, as well as his desires for the stimulus of academic association and an elite reference group, drew him to this circle. His views, like those of Mayo, were strongly influenced by Pareto’s writings and the circle’s interpretation of them.

The Scholar-Manager Combination

Barnard was almost unique among scholars of his time in that his primary career was that of manager, not academician. This brought him knowledge and a perspective that others could not emulate; the unusual combination involved made for a great many creative ideas, ideas
that have had an immeasurable impact on organizational behavior. But was Barnard as effective as a manager as he turned out to be as a scholar?

The answer is that, although he had many strengths, he probably was not as effective. A number of those who worked for him at New Jersey Bell appear to have been lukewarm about his skill as a manager, viewing him as a better staff man (which he was for many years) than a line executive. Also, in spite of being on a fast track destined for promotion to the highest levels of corporate management, with the president of the company his mentor, he ended his AT&T career with one of the operating companies, not corporate headquarters. New Jersey Bell itself failed to produce a profit picture that equaled several other operating companies during his tenure. Return on invested capital was in the 7 percent range in the early years, but declined to 1 percent as he left for the Rockefeller Foundation.

One possible reason for Barnard’s lackluster performance as a company manager is that he did not devote his full energies to managing. Throughout his business career he was frequently involved in outside activities—serving on and for a variety of associations, boards, committees, councils, commissions, and administrations. He held several governmental appointments and was president of the United Service Organizations during World War II. All this required many absences, often of considerable duration, and much delegating to those who covered for him at New Jersey Bell. Furthermore, Barnard did a great deal of reading related to his scholarly pursuits and wrote extensively, although more frequently in the form of speeches and for private distribution rather than for publication (Wolf 1974 lists 108 articles, lectures, and manuscripts). With the time spent at Harvard, these outside activities would seem to have stretched the man rather thin insofar as his New Jersey Bell work was concerned. It looks very much as if he enjoyed being a scholar and working in outside roles that challenged his intellect more than managing his company. He does not appear to fit the picture of the man of action that characterized the most successful managers of the period (Locke 2000).

THEORY AND PHILOSOPHY

Barnard’s views are expressed not as a single formulation with a rich network of relationships between variables, but for the most part as a set of independent topic discussions, consistent with the fact that the initial mode of their presentation was a lecture series. If the reader is contemplating reading this material in the original, however, I should warn that this may turn out to be a difficult task. Barnard himself recognized this fact, but felt it was justified by the nature of the material. Hoopes says, “Barnard could write sentences of lovely rhythms, but he freighted them with abstract diction that requires heroic effort to puzzle out. . . . The Functions of the Executive . . . its turbid prose seems to meet some felt need within the management movement for a cabalistic book of bottomless meaning . . . despite the atrocious prose Barnard’s main ideas are fathomable” (2003, 174–75). With this recognition, I take up the task of interpretation.

Open Systems Concepts and Cooperation

If there is an overarching concept, this is it. Formal organization is a type of cooperation that at its best is conscious, deliberate, and purposeful. To achieve this cooperation, changes must be made in the motives of organization members, changes that without such efforts
would not occur. When favorable to the cooperative system, these motive changes become resources to the system. Cooperative systems must somehow create a surplus of member satisfactions. If the member gets back from the cooperative system only what is put in, no incentive or net satisfaction exists and cooperation is unlikely.

A n organizational system is composed of the activities of human beings. These activities become a system by virtue of the fact that they are coordinated. The activities are not confined to the behavior of members or employees of the organization. They include anything that represents a contribution or receipt of energy by the system—a customer making a purchase, a supplier delivering supplies, an investor providing capital. Organizational systems of this kind have the characteristics of any system. Thus, they are entities that must be treated as a whole because each part is related to every other; the components are accordingly interdependent variables.

**Formal and Informal Organization**

A formal organization is defined as a system of consciously coordinated activities or forces of two or more people. The survival of this organization requires maintaining equilibrium, both internally and in relation to the external situation. It is in fact the services, behavior, and influences of people that constitute the organization, and consequently there must be a willingness on the part of people to contribute effort to the cooperative system. This willingness cannot develop without an objective for cooperation—what is believed by the organization's contributors to be its purpose. Communication is the means by which the common purpose is established.

The communication system is a system of objective authority and operates on certain principles:

1. Channels of communication should be clearly established and known.
2. A definite formal channel of communication to every organizational member should exist.
3. The line of communication should be as direct or short as possible.
4. The complete line of communication should be used so that no points on the line are bypassed.
5. People such as officers, supervisors, etc. who serve as communication centers should be competent.
6. The line of communication should be kept constantly open when the organization is functioning.
7. Every communication should be authenticated so that the communicator is actually known to occupy a position of authority. (Barnard 1938 175–80)

Complex organizations are built out of basic units overlaid with units of executive structure. The size of units is determined by the limitations of effective leadership. These limitations in turn depend upon the complexity of both purpose and technology, the difficulty of communication, the extent to which communication is needed, and the complexity of existing interpersonal relations. Thus, spans of control are established.

The defining characteristic of informal organization is that it occurs and continues without a conscious joint purpose. However, common or joint results may well occur. This type of organization establishes various attitudes, understandings, customs, and the like; it also creates conditions that may give rise to formal organization. Within the context of formal
organizations, informal groupings have the functions of fostering communication, maintaining cohesiveness, and providing for feelings of personal integrity and independent choice. Informal organization may occur at the executive level, as well as among workers, where it serves to ensure compatibility of personnel and a degree of fit within the executive ranks. It represents the political aspects of personal relationships within formal organizations, and serves a useful purpose in communicating opinions and suspicions that cannot pass through formal channels. A fully developed informal executive organization appears indispensable to the effective functioning of the formal organization.

**Incentives and Authority**

One way an organization may secure contributions is through providing inducements. Inducements may be in the form of material things, such as money; personal nonmaterial opportunities, such as prestige or power; desirable physical conditions of work; ideal benefactions, such as pride of workmanship and a sense of loyalty; associational attractiveness in the sense of social compatibility at work; adaptation of conditions to established habits and attitudes of members; the opportunity for enlarged participation in significant events; or the condition of communion, whereby people feel a sense of social integration and mutual support. In general, material rewards, particularly money, are ineffective beyond a minimal subsistence level (as per Mayo; see Chapter 6).

If an organization is not in a position to provide incentives of these kinds sufficient to obtain the required contributions, the alternative is to resort to persuasion. Under the category of persuasion, Barnard notes coercion by force; the rationalization of opportunity through various types of propaganda and appeals to existing motives; and inculcation of motives, via education and cultural indoctrination.

Organizations are effective primarily because they can achieve cooperative efforts from a number of individuals who, as a total, integrated system, represent more than the sum of the individual efforts. Individuals are induced to contribute to such a cooperative system through the use of various incentives, including pay. One of the things they give is a willingness to follow authority. Thus, Barnard’s view of authority is bottom up, and by the consent of the governed. Individuals decide what range of authority they will let the organization exercise over them and thus accept; within this range they will do whatever they are told—the zone of indifference. The size of this zone is influenced by the inducements offered: the more the organization gives, the wider the range of personal discretion the individual is willing to give up.

How this acceptance theory of authority and the zone of indifference work may be illustrated from the consulting field. A consultant working for a large firm expects to be away from home overnight during the regular work week, and it is a matter of indifference whether this involves being sent to Seattle, St. Louis, or San Antonio. However, spending weekends away from home is another matter; such a requirement would meet with considerable resistance. But a change in inducements—perhaps the offer of a partnership in the firm—could shift the balance in this regard so that contributing weekends away from home, when requested by the managing partner, might well be accepted with no questions at all.

Communications have the presumption of authority when they originate in a communications center, and are within the scope of that center as well as relevant to the situations recipients face. This is the authority of position. However, there are some individuals who,
because of superior ability, are able to exercise authority regardless of position. This type of
dependence on personal respect to obtain contributions is the authority of leadership. When
these two are combined people generally will grant considerable authority, accepting orders
that extend far beyond the zone of indifference.

Decision Making

Free will is limited in part because the power of choice disappears if the number of equal
opportunities is large. Thus, in many areas the processes of decision are essentially means of
narrowing choices to the point where humans can deal with them. The capacity of most
people to make decisions is quite narrow.

Acts of decision involving logical processes are characteristic of organizational behavior
more than individual behavior. Within organizations, the decision process deals first with
the choice of whether or not to contribute effort. Second, there are decisions of a nonpersonal
nature that relate to organizational effects and purposes. The latter may be delegated, while
the former may not. In this connection, Barnard endorses the position that executives should
not decide questions that are not immediately pertinent, should not make decisions prema-
turely, should not make decisions that cannot be implemented, and should not decide mat-
ters that others should decide.

The analysis required for an organizational decision is in essence a search for strategic
factors. These are the factors whose control will establish new systems or sets of conditions
that meet the needs of the purpose at hand. The discrimination of strategic factors is largely
a technological matter depending on the procurement of methods of magnifying details.
Among these methods are chemical analysis, mechanical analysis, telescopes, statistical pro-
cesses, and above all in the business world, balance sheets.

Although he recognizes and extols logical reasoning for a number of purposes, Barnard
also has many good things to say of nonlogical processes and intuition. In numerous cases,
we make decisions in a nonrational manner and then impute rational processes to the out-
comes. This type of rationalization leads to a general deprecation of nonlogical process and
an exaggeration of the degree of rationality that exists in the world. Here, Barnard seems to
be closely allied with Mayo, and both clearly take their lead from Pareto.

The Executive Functions

Executive functions serve to maintain the system of cooperative effort. As Barnard presents
them, they differ substantially from the management functions proposed by many other writ-
ers. The first involves providing a system of communication. Inherent in this function are
two tasks—to create positions as communication centers, and to select people to fill these
positions who will communicate effectively. Either may be changed on occasion to perfect
the total communication process by adjusting one to the other.

The second function is to promote the securing of essential efforts (personal services)
from individuals. This involves bringing people into a cooperative relationship with the
organization through recruiting; then eliciting the necessary services, after people have been
brought into the relationship, by arranging for appropriate inducements and incentives. Fi-
nally, the third function is to formulate and define purpose. This requires the assignment of
responsibility and the delegation of authority. The organization established to define pur-
pose is an organization that specifies the work to be done.
This three-pronged executive process is devoted to the integration of the whole; thus, to finding the effective balance or equilibrium between local and broad factors, between general and specific requirements.

Status Systems and Leadership

By 1938, Barnard had not tackled the question of status systems, but he did turn his attention to the subject later. There are two types of these systems. The first type is functional. It is lateral or horizontal in nature and here status depends on the particular function performed, or better, on the potential for performing a function. Those with functional status are viewed as experts, able to give advice in a particular field; they are seen as specialists or professionals who have expert knowledge by virtue of their education and particular experience. The second type of status system is scalar. Here, the system is vertical in nature, and status is determined by superiority or subordination in a chain of command (formal authority), as well as by the nature and extent of assigned jurisdiction.

Status is established and maintained by various ceremonies, insignia, titles, perquisites, and restrictions of office. Status considerations are a constant matter of concern for executives, particularly with regard to the system of organizational communication, the system of incentives, and as a means of fixing responsibility. Status systems have their origins in differences in the acquired characteristics of people, in the varying difficulties of particular activities, and in the social evaluation of these activities.

As distinct from authority, Barnard had relatively little to say about leadership by 1938, but his comments later are interesting. He notes that the democratic process adds greatly to the complexities of leadership. The leader must deal not only with the technical situation external to the organization and the internal operative organizational situation, but now with the democratically produced majority opinion and often with various minority positions as well. These political factors can produce so many complications that it is common practice (especially in business) to exclude much of what is done from the democratic process. There are many instances where the introduction of democracy is clearly superior, but in many other situations (such as those involving crisis or danger) democracy is an inferior approach and cannot be practiced effectively. This should be recognized.

Responsibility and Morality

Responsibility is the power of a private code of morals to control the behavior of an individual in the face of strong contrary desires. Responsibility is thus that aspect of an individual through which whatever morality is present becomes effective in behavior. The private code of morals that derives from a formal organization is an aspect of the organization personality and also of the zone of indifference. An important feature of executive work is that it imposes the need to create moral codes for others, above and beyond one's own moral code. In this connection it is widely recognized that executives are expected to create (secure, inspire) morale in an organization.

Toward the end of his life, Barnard (1958) returned to these ethical issues. Being social systems, organizations give expression to (reflect) mores, culture patterns, assumptions, convictions, unconscious beliefs, and the like that make these organizations largely autonomous moral institutions on which instrumental political, economic, religious, or other functions are then superimposed. In the case of various specialized functions, such as those of the
executive, it is very difficult to convey to outsiders the nature of the moral problems that exist. Public misunderstanding of organizations is due primarily to a lack of appreciation of the moral elements involved in various specialized activities, and to the great difficulty of explaining what these moral elements are. One gets the impression that Barnard feels that only fellow executives can understand and deal with the moral conflicts inherent in executive work; outsiders simply do not understand.

CONCLUSIONS

Barnard’s views and writings have had a tremendous impact. He brought a new view to the scholarship of organizations, a special combination of sociology, psychology, economics, and practical managerial experience. Many of the first-generation theories of organizational behavior have been enhanced by the thinking of this man.

On the other hand, it is well to recognize that there are those who find his unreconstructed managerialism excessive. Scott’s comments in this regard are to the point:

> I am ambivalent about Barnard. On the one hand, I am suspicious of the concentrated organizational power that he advocated, and I detest an elitism based on it. I find the subordination of an individual’s moral character to the imperatives of contrived organizational collectives, which he also recommended, a repugnant idea. On the other hand, his intellect, his vast accomplishments in the affairs of business, government, philanthropy, and public service, his prodigious writings, and his profound personal integrity impress me beyond telling. (Scott 1992, xii)

I do not find in Barnard’s published writings the elitism that Scott notes, but elitism of this kind was clearly in evidence within the Pareto Circle at Harvard. To the degree that Barnard’s influence has been less than it might have been, his identification with and championing of a managerial class appear to be responsible.

I should note that Barnard had a strong influence on Herbert Simon and his formulations regarding decision-making processes. Simon repeatedly acknowledges this influence in his Administrative Behavior (1947). In fact, Barnard critically reviewed the original manuscript for that book and wrote a foreword for the first edition. Simon’s views on the decision to participate in an organization and on organizational authority derive from Barnard. In many respects, Simon took the abstract philosophizing of Barnard and converted it into testable propositions subject to scientific research.

Barnard’s acceptance theory of authority has had wide appeal among those who developed theories supporting participative management (see Miner 2006), which has more recently come to be called empowerment in organizations. This includes the theorists, led by Rensis Likert, who worked together at the University of Michigan, and a number of advocates of organization development. These people were clearly strongly influenced by Kurt Lewin to whom we will turn next, and he is their major intellectual parent, but Barnard is widely cited by them as well. He also had considerable impact on systems theorists of all persuasions (see Katz and Kahn 1978; Thompson 1967).

Chester Barnard’s unique contribution was that he was able to combine a firsthand, personal knowledge of managing in a large corporation with not only scholarly knowledge but also a particular type of scholarship attuned to the values and viewpoints of the times. His ties to Harvard, the Pareto Circle, and the Hawthorne researchers provided a perspective that
made his writings much more broadly acceptable than would likely have been the case otherwise for a practicing manager. When two streams of ideas that have not previously been placed in close proximity are combined like this, the result is usually a creative flaring. Certainly it was in Barnard’s case.

Kurt Lewin, the subject of the next chapter, exhibits some of the effects of a similar combining of disparate perspectives. However, the disparate factors involved in his contributions were quite different in nature, as we will see.

REFERENCES

Background
  Germany
  United States
Contributions Relevant for Organizational Behavior
  Leadership Climates
  Change Processes
  Expansion of Agenda
  Level of Aspiration
Conclusions

Kurt Lewin was trained as a psychologist; he was a researcher as well as a theorist. His contributions to psychology were eclectic in terms of area, and it was only toward the end of his life that he concentrated on the social psychological studies and ideas that make him particularly relevant for organizational behavior. Barnard apparently read Lewin and was influenced by his views on field theory, which closely approximated those of Barnard and the Pareto Circle regarding open systems theory (Wolf 1973).

Early in his career, Lewin’s major contributions were in the area of personality theory. He wrote several books on that subject during this period and established himself as a major theorist (Hall and Lindzey 1957). However, his interest in personality theory gradually gave way to social psychology and in particular to group dynamics and action research. He was a man of broad humanist sympathies and democratic values, differing in his political perspective rather sharply from Mayo and Barnard. In the late 1930s and 1940s, when his writings of most relevance for organizational behavior appeared, Lewin wrote primarily in the form of journal articles, and papers in edited volumes.

BACKGROUND

Kurt Lewin’s life was split into two distinct phases. The early period, spread over forty-three years (1890 to 1933), was spent based in Germany. He lived and worked in the United States from 1933 to 1947, when he died from a heart attack (Lewin, M. 1992). Lewin was Jewish, and he moved to this country because he foresaw the persecution that was to come under Hitler and the Nazis.

Germany

Lewin was born in Prussia and attended the Universities of Freiberg and Munich before receiving his doctorate from the University of Berlin in 1914. He served for four years in the German army during World War I and then returned to Berlin where he rose to professor in
1926. He was strongly influenced during the German years by the Gestalt psychologists with whom he worked, including Max Wertheimer and Wolfgang Köhler. Over time he developed a group around himself at the Psychological Institute of the University of Berlin, consisting of a number of like-minded scholars.

During this German period, Lewin published some articles that presaged his later social psychology (Papanek 1973). One paper dealt with job design in farm work, while another focused on humanizing the activities of factory workers employed under Taylor’s scientific management. There was a theme of job enrichment in these papers, and also in several additional studies of the field of forces operating on individual textile workers. These early papers illustrated Lewin’s use of field theoretic concepts, linked laboratory experiments to applied problems (action research), and focused on individuals not groups.

Early in the 1930s, Lewin was invited to Stanford University by Lewis Terman, originator of the Stanford-Binet tests. He returned to Germany, but apparently this visit set in motion a process that shortly resulted in a permanent move to the United States.

**United States**

Presumably as a consequence of his association with Terman and of some research he had conducted in the area, Lewin came to the United States perceived as a specialist in child development. He first joined the School of Home Economics at Cornell University as a child psychologist and after two years moved to the Child Welfare Research Station at the University of Iowa. Both of these positions were generously funded by the Rockefeller Foundation. At Iowa, Lewin began to concentrate more on groups rather than individuals, and again, as in Berlin, he surrounded himself with a group of scholars, many of whom joined him in his research (Ash 1992).

At Iowa, Lewin moved increasingly toward a kind of industrial social psychology focused on groups and action research. To further this objective he ultimately moved to the Massachusetts Institute of Technology in Cambridge where he became director of the Research Center for Group Dynamics, whose funding was arranged primarily by Lewin himself. Again, he created a group of young scholars interested in the topics that he found interesting. After Lewin’s death, the center moved briefly to Syracuse University and then on to the University of Michigan where it became part of Rensis Likert’s Institute for Social Research (Cannell and Kahn 1984). The center served over the years to foster Lewin’s early work on sensitivity training and T-groups through the development of the National Training Laboratories, and also established a long informal exchange with the Tavistock Institute of Human Relations in England. An outgrowth of this relationship was joint publication of the journal *Human Relations*. Thus, Lewin’s influence was extended in many directions.

**Contributions Relevant for Organizational Behavior**

Lewin typically formulated many of his field theoretic ideas in terms of mathematical symbols and spatial diagrams. The most widely cited of his mathematical formulations is \[ B = f(P, E) \] — that is, behavior is a functional interaction of person and environment. However, many such formulas are sprinkled throughout his works, as are life-space diagrams and force-field pictures (see, e.g., Lewin 1943).

How important these accoutrements really are to his theorizing is a serious question. They
appear to represent a personal language that serves to describe what is already known much more than to predict experimental results. Although they may foster understanding, and many who have struggled with them may have major doubts even on that score, these after-the-fact formulas and diagrams in and of themselves clearly are not predictive theoretical statements or hypotheses. Lewin’s field theory is not a mathematical theory in spite of the symbolic nomenclature used to present it (Hall and Lindzey 1957). Thus, in the following I will utilize only the verbal language.

As indicated, Lewin directed his theorizing, and his research, to multiple fields. This was true even within organizational behavior. Thus, it is not surprising that although his major concern was within the field of motivation, he started out by testing certain ideas regarding leadership, and the effects of leadership climates. This breadth of application was typical of the early theorizing leading up to the actual creation of organizational behavior (Miner 2002). However, the leadership theory fed into the later motivation theory.

**Leadership Climates**

Conclusions on the effects of varying leadership climates were derived from experimental research (Lewin, Lippitt, and White 1939; Lippitt and White 1958) conducted at the University of Iowa. This research served to establish Lewin’s reputation in the United States. In it, the effects of authoritarian (German) and democratic (American) leadership climates were contrasted. The research reflects in a microcosm the pattern of Lewin’s own life and is a direct outgrowth of the juxtaposition of the two cultures. In addition, the study, apparently as a result of a misunderstanding among the researchers (Wren 1994), came to include a laissez-faire condition or climate as well.

The ways in which these three climates were operationalized are set forth in Table 8.1. In the major study, the four groups experienced all three climates in succession; thus, comparisons could be made across groups and also within groups (using a group as its own control). The subjects were eleven-year-old boys grouped into five-member clubs that met after school to participate in various activities—mask-making, mural painting, soap carving, model airplane construction, and so on—over an experimental period of five months.

Clubs were matched on patterns of interpersonal relationships; intellectual, physical, and socioeconomic status; and personality characteristics to control for these factors. The four adult leaders were systematically assigned across climate conditions and clubs. The behaviors and conversations of both leaders and club members were recorded in detail by four observers. These were then categorized for purposes of quantitative analysis. The interrater agreement of the observers was 0.84. Observations of leader behaviors, and reports obtained from the boys, indicated that the experimental conditions did in fact “take.”

The findings with regard to aggression in the authoritarian groups were mixed—in some clubs, manifestations of aggression were very frequent, and, in other clubs, very infrequent (apathy). Democracy seemed to produce a mid-range frequency of aggressive expression and laissez-faire a rather high level (presumably because there was no control on emotional expression from either the leader or the group). The democratic climate was much liked, the authoritarian much disliked, and, on balance, the laissez-faire context was more liked than disliked.

Achievement levels were much higher under the authoritarian and democratic climates than under laissez-faire. This failure of laissez-faire leadership has remained in evidence across many studies since (Eagly, Johannesen-Schmidt, and van Engen 2003). The lack of
accomplishment in the latter instance was clearly evident to the boys and a source of dissatisfaction. They talked about doing better, but could not coordinate their efforts to do so. The major difference between authoritarian and democratic groups was that in the first instance productivity dropped off sharply when the leader left the room; in the democratic clubs it did not. The results as a whole seemed to offer strong confirmation of the hypothesis that democratic leadership climates are to be preferred, although the high performance in the authoritarian groups was not hypothesized.

### Change Processes

Working from these findings on the value of group decision and democratic process, Lewin (1947, 1958) developed a theoretical structure to deal with change processes, and carried out a program of research on change. The result was a good theory that has indeed turned out to be very practical (Kaufman 2004; Lundberg 2004). It has served for many years as a guide for organizational development practitioners (Goldstein 1993; Marshak 1993). Recent work suggests that Lewin’s force-field analysis applies within a limited domain and that there are situations beyond the boundaries of that domain in which Lewin’s views are less applicable. Nevertheless, this is a theory that has moved application a long way (Burke 2002). In the past few decades, Lewin’s views on change have come under attack as simplistic and outmoded, but they continue to receive strong support as well (Burnes 2004).

The research on change was initially conducted to foster changes in eating habits necessi-
Housewives were identified as the gatekeepers whose decisions would have to be altered if broader change were to occur. The research design involved exposing groups of housewives to attractive lectures linking nutrition to the war effort and attempting to persuade them to change the eating habits of their families. A nother set of groups was given the same information, but the leader rapidly moved to a group discussion of how “housewives like ourselves” might react to the prospect of change. A group decision was reached in the sense that there was a show of hands as to who would commit to initiating the change.

This experimental approach was used to increase the use of beef hearts, sweetbreads, and kidneys; to expand home consumption of fresh and evaporated milk; and to foster giving cod liver oil and orange juice to their babies by new mothers. Other studies used similar designs to study changes in the productivity levels of factory sewing-machine operators and the decision to eat whole-wheat rather than white bread by members of a students’ eating cooperative. In all of these instances, the evidence provided strong support for the use of group decision techniques in motivating change.

The underlying theory states that before change, the force field is in equilibrium between forces favorable to change and those resisting it; or as Lewin preferred to say, a quasi-stationary social equilibrium exists. For change to occur, this equilibrium must be disturbed, either by adding forces favoring the desired change or by diminishing opposing forces. Ultimately, a new equilibrium is established, but in the former case at a high tension level and in the latter case at a low tension level. Since high tension has the potential for aggressiveness, emotionality, and a lack of constructiveness, diminishing resistance to change is usually the preferred approach to change. Group decision appears to be such an approach. Figure 8.1 provides a picture of Lewin’s force-field analysis.

Social habits represent inner resistances to change. The inner resistance must be unfrozen, usually by the action of some party external to the group, an environmental force. Groups hold to standards that can represent forces against change. If an individual varies widely from such a standard, that person will be ridiculed, punished, and ultimately rejected from
group membership. Assuming a desire for group membership in good standing, members can be expected to hold closely to the standards of the group. However, if the group standards can be modified in some way, then the individual would be expected to change as well so as to stay close to them. This is what group decision is about—modifying group-based habits that serve to resist change.

This whole process may be conceptualized as one of first unfreezing the existing level, second moving to a new level, and then freezing group life on the new level so that a permanent and stable equilibrium that resists further change is established. Unfreezing is often difficult and may require different approaches under varying conditions. Accordingly, merely applying the group decision approach, to increase factory production standards for instance, without a thorough study of the circumstances, is unlikely to prove effective. Models of this unfreezing-moving-freezing kind have continued to be applied to motivate change. A recent example involves the process by which institutional change occurs (Greenwood, Suddaby, and Hinings 2002).

Lewin and Grabbe (1945) note that one way of changing an aspect of an individual is for that person to accept membership in a new group, a new culture, with different standards and values. New perceptions result from new identifications and memberships. However, for these changes to occur, the new group and culture cannot be forced upon the person; that only creates hostility. Belongingness to the new group must be experienced as an “in-group” feeling. To achieve this, the group must be voluntarily chosen. It has been noted that people who at one point in their lives identify with the far-left end of the political spectrum (socialism, communism) may at another point come to identify with archconservative causes at the opposite pole. Lewin attributes these types of changes to shifting group identifications and memberships. New values are accepted as the individual comes to experience belongingness in a new in-group.

Expansion of Agenda

As his ideas regarding group decision making, democratic leadership, and change developed, Lewin expanded his activities into the national arena (Marrow 1969). He was instrumental in organizing the Society for the Psychological Study of Social Issues, which united various liberal groupings within the American Psychological Association and promoted action research beyond the universities. During World War II, he did morale research for the military, then community research for the American Jewish Committee. All this eventuated in his move to the Massachusetts Institute of Technology and the Center for Group Dynamics. In this period, he established a relationship with Alfred Marrow, a psychologist who also headed the family firm known as Harwood Manufacturing Company. Harwood, a textile firm, became the site for a number of well-known studies carried out by researchers from the Center for Group Dynamics, but it was Lewin who first developed the contact in order to extend his research into the industrial arena.

Another manifestation of action research was the development of sensitivity training and T-groups. This innovation is usually traced to a workshop conducted at New Britain, Connecticut, State Teachers College in 1946 by Lewin and others from the Center for Group Dynamics (Bradford, Gibb, and Benne 1964; Berg 2003). However, it is apparent that Lewin was doing something very similar several years before at Iowa (Papanek 1973). T-groups are often very useful as a method of unfreezing resistances in a group. The lack of structure and ambiguity create a fertile ground for new learning and new values. Clearly, in the latter
years of his life, Lewin moved from theory and university-based research to action research intended to produce changes in the world around him. He often advocated engaging the subjects of research in the design and conduct of the studies in which they participated, in order to foster a sense of “ownership” of the results and thus to facilitate change.

**Level of Aspiration**

Lewin’s statements regarding level of aspiration are important as they relate to subsequent theory and research in the area of goal setting. They are thus somewhat separate from the work on leadership climates and change processes, although field theory continues to be employed. Lewin himself was primarily a theorist in this area, basing his formulations on the research of others (Lewin 1936; Lewin, Dembo, Festinger, and Sears 1944).

Levels within a person’s goal structure may include a dream goal, a somewhat more realistic wish goal, the goal sought when an effort to be objective is involved, and a minimal goal, should bad luck be operating. Somewhere on this scale is what the person really tries for at the time in a given situation—the action goal. In addition, some ideal goals will be established above this. Knowledge of one’s standing relative to a group will have an influence here, as will individual differences with respect to seeking success and avoiding failure.

Three variables appear to play a primary role in setting goals—the seeking of success, the avoiding of failure, and a cognitive factor representing a probability judgment. These factors operate in a context involving a choice of future objective. The strength (valence) of the success and failure variables and the value of the subjective probability estimate depend on numerous factors in the life space, particularly the way past experience with the task is viewed, the standards introduced by group and cultural comparisons, and the individual’s personality.

An example of how these factors may operate in practice is given by Marrow (1972), who describes a situation at Harwood on which Lewin offered his advice. The company had moved its operations from New England to Virginia and as a consequence had to train a large number of new machine operators. The standard for acceptable production under the piece rate system was sixty units, brought with the company from New England. However, a number of apprentices were having difficulty reaching this standard during training, and turnover was unusually high. Lewin suggested that the company look into the circumstances under which people who quit experienced success and failure.

There was ample evidence that desire for success was consistently high, but fear of failure seemed to accelerate over the training period. Over time, doubts seemed to arise as to whether the sixty-unit goal was possible; progress slowed noticeably as that goal came in sight. The problem was not money, since the standard wage was guaranteed irrespective of production level. The closer they got to the goal the less the apprentices expected to reach it. They saw how hard it was and their cognitive probabilities of goal attainment shifted accordingly. A check on turnover rates indicated that for those who reached thirty units the figure was 20 percent; at forty-five units it was up to 60 percent; at fifty-five units it was an unusually high 96 percent; over the goal of sixty units, turnover dropped to 11 percent. As tension and frustrations spiraled upward, more and more apprentices left the field and became turnover casualties.

The solution was to eliminate the sixty-unit goal and replace it with a series of short-range goals that rose slowly to the sixty-unit figure, thus permitting a continuing feeling of success. When this change was made, turnover decreased by more than half, and the escalating
tension so apparent previously was no longer in evidence. Fear of failure had declined and probability judgments regarding goal attainment were higher. There was no longer a reason to leave the field.

CONCLUSIONS

As Wolf has noted Lewin:

... has had a significant impact upon modern management. His concepts are widely used and his students are among the dominant contributors to human relations, personnel, and industrial psychology. Yet many students of management are completely unaware of Lewin and his contributions. His name is seldom mentioned in texts dealing with management or personnel relations. The failure to recognize Lewin's role in the evolving field of management is a serious neglect, for his philosophy of science, research methodology and approach provide a potential for advancing the discipline of management. Kurt Lewin was an innovative researcher whose ingenuity in experimental design provides a scientific basis for many of our current concepts. (Wolf 1973, 322)

Within organizational behavior Lewin has had a major impact on organization development, on the various theories that serve to foster participative management, on work in the field of group dynamics, and on theorizing related to goal setting. Among those who contributed to the origins of organizational behavior, he is unique in that he was trained in the conduct of science and able to carry out significant research himself. Lewin brought science to the study of organizations, but he also brought humanistic and democratic values (Lewin, G.W. 1948); in many respects his research supported his values. He was not guilty of using science to provide cachet for his personal philosophy, as Mayo and even some of Lewin's followers appear to have been.

Lewin's contributions were diverse, but they were all made in the name of psychology; in fact, he died before organizational behavior was born. Certainly, Lewin was interested in applying his ideas to organizational practice and he developed methods by which this could be accomplished, particularly in the areas of motivating change and fostering participative decision making. His theories were not totally valid on the evidence that exists at the present time, but they represented a solid beginning on which others could build.

Given what we know now, it is worthwhile returning to a question raised in the introduction to Part II: Is Kurt Lewin a legitimate influential worthy of a key role in the annals of organizational behavior with the likes of Mayo, Barnard, and the others? I submit that he is, and that the evidence contained in this chapter provides sufficient justification. But then, why is it that his contribution has not been widely recognized? Why do those who chronicle the historical origins of the field fail to include his name?

One possibility is that Lewin's values served to limit his place in organizational behavior's history. He is, for instance, given chapter-level treatment in several books chronicling the history of ideas in various psychological subject areas—personality (Hall and Lindzey 1957), learning (Hilgard 1948), and experimental psychology (Murphy 1949). Perhaps his liberal values are more acceptable to psychology than to organizational behavior. Of the seven contributors considered in Part II, Lewin indeed would appear to be the farthest to the left politically. Yet organizational behavior historically has been anything but a bulwark of the right wing, and ideas such as Lewin's have been readily accepted in the field. The same can
be said for his unique status among the seven as a psychologist and a research scientist. Psychology and research have permeated organizational behavior from its beginning; there seems little here to explain the rejection. The thought occurs that his being a Jew may have been a factor. However, the professions, of which organizational behavior is one, have provided a source of opportunity for Jews, not a place where discrimination operates to limit recognition (Korman 1988).

Other hypotheses might be run up against the data in the same manner, and in all likelihood would fail to meet the test, just as those already considered have. My guess is that there really is no one factor that provides an explanation. Rather, I suspect the key is that Kurt Lewin was so different from the others on so many grounds (other than being male). To date at least, this is not the kind of person who fits whatever stereotypes we may have of the key influentials in organizational behavior’s history. It is hoped that this situation will change with time.

Mayo, Barnard, and Lewin all made their major contributions in a period extending from the latter 1920s to the beginning of the 1950s. Thus, they provide what amount to proximate contributions to the emergence of organizational behavior as a separate field. Their work was followed very closely by that emergence, and a number of their students and contemporaries were in the vanguard as organizational behavior came on the scene. In the next three chapters, we consider a group of contributors who are farther back in the temporal pipeline. Yet they are equally significant for the origins of organizational behavior. We start with Mary Parker Follett, the only woman among the seven.

REFERENCES


Background

Views on Business Management
  Conflict
  Power
  Giving Orders
  Authority
  Leadership
  Coordination and Control
  Management as a Profession

Conclusions

Mary Follett was born in 1868 and died in 1933; her influence on organizational behavior was exerted only a few years earlier than that of the people considered in previous chapters. She was a political and social philosopher whose occupation was social worker. Her early writing was in the field of political science, and it was only in her later years that she began to focus on business organizations specifically.

After writing several books concerned primarily with political science and democratic process, which earned her substantial acclaim in that field (Sethi 1962), Follett’s first book to deal directly with organizational behavior issues was Creative Experience (Follett 1924), which is also regarded as her masterwork (Witzel 2003). Her subsequent writings were prepared initially as lectures, which she gave frequently as her ideas became better known. Sources for these papers are Metcalf and Urwick (1940), Fox and Urwick (1973), and Urwick (1949). I have relied on these books, as well as on Graham’s (1995) very comprehensive treatment of Mary Parker Follett’s life and work; Tonn’s (2003) more recent biography contains further details.

BACKGROUND

Follett grew up outside Boston in a well-to-do family from which she ultimately received an inheritance that made her financially independent. She attended Thayer Academy in the Boston area and entered what was shortly to become Radcliffe College. After a year at Cambridge University in England and considerable time spent preparing her first book on the workings of the legislative process, Follett received her degree from Radcliffe at the age of thirty. Her major areas of study were government, philosophy, and economics. Subsequently she did postgraduate work in Paris, but did not receive a degree. She returned to Boston in 1900. She remained there until the latter years of her
life. Her circle of friends and acquaintances included a distinguished group of writers, philosophers, lawyers, politicians, business people, and aristocrats; she drew upon them extensively for her lectures and writing.

As a social worker, Follett developed programs to provide social, recreational, and vocational guidance opportunities to young people. During her twenty-five years in this occupation she managed various activities, initiated ideas, and implemented them. She served on a series of boards and committees dealing with such matters as placement bureaus, community centers, and minimum wage administration. She was particularly influential in getting placement centers established in the Boston public schools.

During the 1920s Follett became increasingly interested in business organizations and their functioning. She was never a management consultant in the usual sense of that term, but she did advise business managers frequently, and she lectured widely on business topics. These lectures were given to various conferences, university groups, and professional associations in both the United States and England. From 1929 until her death, Follett lived in England and studied industrial conditions in that country. Her final lectures in early 1933 were given at the London School of Economics.

The extent to which discrimination in some form contributed to Follett’s having received or not received the recognition that is her just due is a matter of some debate. The pros and cons of this issue are considered in the preface (Rosabeth Kanter) and the introduction (Peter Drucker) to Pauline Graham’s (1995) book. Certainly, gender per se did produce certain problems at the time that would not have stood in the way of an equally situated male. Yet Follett had the advantages of inherited money and entrée to important people, too (Hoopes 2003). Without doubt, she is unique among her peers in the success she achieved and in the early influence she exerted on organizational behavior. Among those who exerted similar influence, she is the only woman; that in itself is testimonial to the importance of her views. For those who may wish to pursue the more recent experiences of women in organizational behavior, I would recommend the book edited by Cyr and Reich (1996).

VIEWS ON BUSINESS MANAGEMENT

Although not trained in psychology, Follett read widely in that field, and this influence shows at many points in her writings. In particular she was attracted by the concepts of gestalt psychology, which serve to emphasize the total situation surrounding any event.

Furthermore, at this early point she recognized what various organizational behavior theorists have stressed since—that people often act first, and only after the fact develop reasons for what they do. Follett thus refers to the inevitable tendency to make an ideal of what is already fact. In this and many other respects, she represents a prophet for views and concepts that were to reemerge many years afterward.

Some sense of her approach is inherent in the following:

1. Behavior is both internally and externally conditioned.
2. Behavior is a function of the interweaving between activity of the organism and activity in the environment.
3. By this interlocking activity of individual and situation each is creating itself anew.
4. Thus, relating each anew.
5. Thus, producing the evolving situation.
Conflict

Conflict is neither good nor bad, it is simply a matter of difference. Three ways of dealing with conflict exist—domination, compromise, and integration. Domination is immediately effective, but not always successful in the long run. Compromise requires that each side give up something so that the activity interrupted by the conflict may continue. Yet the need to back down to some extent makes compromise unattractive to all.

Integration is thus the preferred way of dealing with conflict. Here, a solution is found that permits both sides to achieve their goals; neither must sacrifice anything. This approach by its nature leads to inventing new approaches to a situation; to finding new ways that are mutually attractive. Unfortunately, however, not all conflicts are amenable to solution via integration.

The first rule is that for integration to occur all parties must bring the whole situation out in the open, uncover the issues. If issues are evaded or suppressed, it usually means that someone is really seeking domination, not integration. When the field of desires is clearly evident, the situation may be evaluated, and then revaluation can occur so that ways in which interests may fit with one another may be identified. The most dramatic features of a conflict situation need not be the most significant for resolution; identifying the real issues may require some search.

The second rule is that the demands of both sides must be broken down into their constituent elements. This breaking up of wholes is crucial. In the process, there must be careful scrutiny of the language used in order to establish what is really meant; what do the symbols say?

In all of this, it is important to anticipate demands, differences, and sources of conflict. One must prepare for response, must carry out the inventive process in one’s mind, so that its products may be considered in advance to see if they might be worthy. This is difficult because one is always dealing with an evolving situation, a moving target created by the interactions involved in real time.

First, one obstacle to integration is that it requires substantial talent—sometimes a capacity for brilliant inventiveness. Not all people have this talent. A second obstacle is that many people enjoy domination, often to the point that they cannot give it up. Third, the conflict may become the subject of theorizing, of intellectualization, rather than being taken as a matter for specific action. As a consequence, getting down to some definite activity may be inordinately delayed. Fourth, the language used may represent an obstacle because in some respect it raises a red flag. Conflict resolution requires careful attention to language. Fifth, there is the matter of the undue influence of leaders (the manipulation of the unscrupulous and the suggestibility of the crowd). This can be a major barrier to integration. Finally, because we are typically trained more in the process of domination than in the art of cooperation, many people have not learned how to integrate.

Perhaps some understanding of the prophetic nature of these views may be gained from the fact that forty years later Walton and McKersie (1965) gave integration a central position in their typology of labor negotiation approaches. They reference Follett frequently on the subject.

Power

Follett provides what she says is an interim definition of power. It is the ability to make things happen, to serve as a causal agent, to initiate change. In contrast, control is power exercised as the means to a specific end, and authority is control vested in a position.

Power usually implies power-over, the object being some person or group. Within busi-
ness organizations, this type of independent power is not desirable. Rather, joint power is preferable, a concept of power-with that is co-active rather than coercive. Although Follett discusses power-over at length, she provides much less detail on power-with, except to say that the distinction has status in law—one has rights over a slave, but rights with a servant. The impression one gets is that power-with implies the pursuit of a common purpose.

One method of reducing power-over is through the use of integration. Integration of a party’s desires makes it unnecessary to gain power over others to satisfy those desires. In the process of developing an integrative solution, a back-and-forth interactive influence is put into play that makes for a buildup of power-with. Consideration of known facts tends to reduce power-over, while withholding of facts serves as a means to increasing power-over.

Power-over can be reduced through (1) integrating; (2) recognizing the need to submit to the law of the situation, where facts are known and the logic of these facts is compelling for all; and (3) making the business a functional unity, where each component has its function, corresponding as far as possible to available capacity, and it has the authority and responsibility that go with the function.

Follett did not believe that power could be delegated because power does not become genuine until it is matched by an equal capacity to exercise it. Thus, delegating power to workers can well be fruitless. A manager cannot share power with anyone who is in a subordinate position, but opportunities for others to develop their own power can be provided. In this view, approaches that are predicated on a balance of power, equal power, and even collective bargaining to the extent it relies on a forced equilibrium, are dedicated to power-over and thus rejected. Equal power sets the stage for a fight; power-with is unifying and thus makes fighting unnecessary. Follett was an advocate of employee associations that can bring about a greater functional unity within an organization, but not of international unions that seek only to increase their own power. The theory of power advocated by Follett has reemerged more recently in discussions of the contemporary empowerment movement (Boje and Rosile 2001).

**Giving Orders**

Orders should not come down from on high, but rather should arise out of the work to be done; thus, many subordinates may contribute. People should not “obey orders,” but instead should follow standard practice. Such standard practice is based on research that may include input from anyone in the organization. Managers serve as a source of information regarding standard practice, once it is developed; there is no call for arbitrariness in this picture.

Arbitrary approaches have several disadvantages, one of which is that useful input from the person doing the job is lost when cooperation from that source is not invited. Second, arbitrariness promotes friction and antagonism. Third, pride in the work tends to be sacrificed, and thus interest in the final result is lost. Finally, a sense of responsibility in the subordinate is decreased; if blind obedience is required, the subordinate cannot be held responsible for the outcome.

How can these negative consequences be avoided? One way is by depersonalizing orders. Orders should arise out of the demands of a situation, not of a person. Yet fear of exercising authority, with a consequent laissez-faire leadership, should not govern either. The demands of the situation do need communication and explanation. Furthermore, arbitrary order giving can be reduced by virtue of the fact that the manager serves as trainer. Valuable training
in new methods, to explain more fully what has previously been misunderstood, as well as to get an employee started on a job, are essential to work performance. A third rule is to explain the reasons for an order to the extent that is feasible and needed. Finally, everyone in an organization should have a full understanding of the organization’s purpose, and thus of the underlying reasons behind a directive.

Managers need to be trained in giving orders. This training should consider ways of preparing the way for orders by creating attitudes that will foster carrying them out; it should deal with the provision of stimuli and incentives to follow the methods suggested; it should emphasize the creation of opportunities to convert these methods into habits. Orders do not obtain their validity from the consent of those who receive them. They achieve validity from a long process to which both order giver and receiver have contributed for some time.

Authority

Research and scientific study must determine function, and people should have authority exactly equal to their function or job. The job itself, rather than position in a hierarchy, is the main source of authority. Thus, authority comes with knowledge, with experience, and with the skill that permits applying that knowledge and experience.

A management decision is only a moment in a lengthy process. The growth of a decision, and thus the gradual accumulation of authority, not the last step, is what needs to be studied. Focusing on the last step only, on the highest-level manager involved, creates the illusion of authority (Follett 1926). Before decisions are made, almost everything in that decision has already been considered and added. Given the conclusions and judgments made at lower levels and then passed up to the chief executive, there are few instances where the decision can actually be changed dramatically at the top. Interacting influences escalate to a point at which we tend to believe the decision occurs, but this is only the illusion of final authority, and it blinds us to the true nature of the decision process. This is not to say that final authority is totally nonexistent, only that it is often overemphasized.

Genuine authority does not stem from separating people into classes specifying those who command and those who obey. It stems from one person’s work fitting into the work of another, and then, through this intermingling of forces, a power being created that in turn serves to control the forces that generated it.

Leadership

Leadership is not primarily a matter of personality, and particularly not a matter of a dominant personality. The requisites of leadership are first, a full knowledge of one’s job; second, the ability to grasp a total situation in all its aspects, and to find the unifying thread; third, the ability to organize the experience of a group so as to obtain the full power of that group, and direct it to the common purpose; fourth, the ability to anticipate the evolving situation, to see possible new paths and to try them, as well as to measure results with a view to determining how functional the various paths are.

The role of the follower is also important in leadership. That role is less to follow, than to keep the leader in control of the situation. Both leader and follower are following a common purpose. Followers should help leaders do this, just as leaders should help followers.

Follett comes down strongly in favor of leadership that derives from function, as opposed to leadership based on either position or personality. She does not deny the existence of the
latter two, however. In fact she provides a long list of leadership qualities—sincerity, steadfastness, control of temper, ability to develop others, willingness to serve as an example, and many others. But she believes, consistent with her views regarding management as a profession, that leadership based on function and capacity to perform the function are crucial. Accordingly, leadership is not something one is born with; it can, at least in part, be learned.

Coordination and Control

Follett has much to say on the topics of coordination and control. For amplification of her treatment the reader may wish to consider a paper by Parker (1984) and a chapter by Daiute (1964) in his series of essays. However, her main points are considered in the following.

The essence of business organizations is that they somehow combine their multiple responsibilities, their scattered authorities, and their varied types of leadership into a single unity. Three principles of unity may be identified. The first is that a system of cross-functioning between units be established so that frequent communication occurs both horizontally and vertically. This may be informal, but it should be expected. There may be a system of committees created to serve the cross-functioning purpose. Second, although differences may be settled by domination, compromise, or integration, the latter should be paramount. Integration is particularly useful in resolving differences between managers and experts. Arbitration, because it involves domination, is not as effective. Third is the development of a sense of collective responsibility. Joint responsibility for the enterprise as a whole is a major contributory factor to enterprise success. Out of these three principles comes unity, but unity must be maintained on a continuing basis by a host of ongoing processes. It is always threatening to disappear.

With regard to control Follett notes two points. Control is much more a matter of controlling facts than of controlling people. With cost accounting and unit budgeting, control has become more impersonal, and this is as it should be. Arbitrary orders thus give way to the study of data and analyses. Second, central control is not a matter of radiation from a central source of authority, and thus superimposed, but a gathering of controls that exist at various points throughout the enterprise. Commands from the top are replaced by the correlation of many controls.

Follett equates organization with control, in the sense that the object of organization is control. But she also tends at times to view control and coordination as involving the same processes. Thus, her four fundamental principles of organization are stated as principles of control and coordination as well:

1. Coordination should involve the reciprocal relating of all the factors in a situation. This reciprocal relating requires the interpenetrating of every part of the organization by every other part and then once again by every other part as it has been permeated by all. This is the goal of all coordination efforts. It does not involve any loss of individuality for the sake of the whole, however.
2. Coordination should be by direct contact of the responsible individuals.
3. Coordination should occur in the early stages. If people are confronted with policies that have already been formulated, and are merely asked to endorse them, not only is their input lost, but antagonism seems inevitable. This principle applies as well to managers and workers. Managers and workers should share in joint control, but only as the parties are competent to have a valid opinion. And joint control means being brought in at the early stages.
4. Coordination should be a continuing process that goes on all the time. In this way the circle from planning to activity and then from activity back to further planning remains unbroken.

**Management as a Profession**

Follett sincerely believed that management should seek to achieve the stature of a profession, and that it was indeed in the process of becoming a profession. In discussing this aspect of her work, I draw on two papers (Follett 1927, 1955).

Follett treats at some length the various criteria for becoming a profession. She notes first a foundation of science and a motive of service. Business is one of the necessary functions of society, and as such, like the professions, it is much more than a method for private gain. In addition, the professions tend to elicit love of the work, and a sense of satisfaction when that work is done well. Professions establish group codes of conduct so that the errors of the personal equation may be corrected. Professional associations are formed that serve to establish, maintain, and improve standards; to keep members up to standards; to educate the public so that these standards can be appreciated; to protect the public from those professionals whose behavior has failed to reach the standards or who deliberately flaunt them; and to protect individual professionals from each other. In essence, the objectives of a professional association represent a corporate responsibility.

Business can well learn from the professions when it comes to maintaining standards. It also needs to do much more by way of educating the public to its standards, and in fact to develop these standards fully. Management has in place some of the basic essentials of a profession, but there is a need to do more on this score.

Scientific method is increasingly being applied to management as reflected in the scientific management of Taylor and his followers, in the increasing specialization or functionalization of management, and in the decline in arbitrary authority (replaced by scientific knowledge). What is needed now is to apply science to the whole of business—to human relations problems as well as the technical ones. Another requirement is to analyze managerial work in a manner comparable to what the Taylor system has done with workers’ jobs.

Management also needs to do much more to organize the body of knowledge on which it is to rest. With more systematic comparison of experiences and experimentation, standards can be developed more fully. Furthermore, a body of knowledge to be used in the "professional" training of managers can be established. This training is an essential condition for management to become a profession. Remember that when Follett wrote there were just a few business schools in existence, and these few had only a short history. Her sole mention of these schools is to praise the use of the case method at the Harvard Business School as a means to organizing the experiential knowledge of business.

**CONCLUSIONS**

Follett wrote in a charming style. Her approach is to serve as a reporter on cutting-edge developments in the business world, to describe what the most progressive companies are doing, and to show why these developments are the road to the future. All the while she is in fact propounding a normative philosophy of what the business world should be. Her reports are not merely reports, but examples for her advocacy. They may well be somewhat selective, but then she is not engaged in journalism, she is setting forth her philosophy. In any
event, part of the appeal of Follett’s writing is that it provides a picture of her culture—partly Boston Brahmin and partly British. The examples are taken from personal experience, the issues of the times, and the business world as she knows it, but they all serve to paint the picture. It is very hard to disagree with what she has to say. On top of everything else, she is against fighting, does not like domineering, bossy people, and believes strongly in the value of knowledge.

Much has been made of Follett’s probably unintended capacity for prophecy. Her ideas have frequently been picked up by others, typically many years later, and either expanded or, in some cases, utilized entirely in the manner in which she stated them. I have noted some instances of this, but my presentation does not do her justice in this regard. Detailed discussions of her prophetic contributions are scattered through the commentaries in Pauline Graham’s (1995) book, in particular, those written by Rosabeth Kanter, John Child, Nitin Nohria, Warren Bennis, Henry Mintzberg, and Paul Lawrence.

These commentaries also raise a question regarding the extent to which Follett has been adequately credited when her ideas are utilized and incorporated in the writings of others. The upshot of this discussion appears to be that, not infrequently, well-known theorists of the first generation do draw upon Follett without citing her. Sometimes the omissions seem to be unintentional; the ideas were in the air at the time, readily available for the picking. Yet in other cases, Follett’s concepts appear to have been quite consciously misappropriated. It serves no purpose to name names on this score, but the reader should be aware that Follett’s influence extended well beyond what a mere citation count would seem to indicate.

Many organizational behavior theorists do credit Follett for making important contributions to their thinking. Thus Likert, in developing the theory of System 4 and 4T, notes the importance of integrative solutions, of the law of the situation, and of the authority of facts (Likert and Likert 1976). Lawrence and Lorsch (1967), in elaborating their contingency theory of organization, draw upon Follett to describe methods of achieving integration and to explain alternatives to formal, authoritarian organizational structures. Various organization development advocates make use of such ideas as integrative solutions to conflicts (Bennis, Benne, and Chin 1969) and the law of the situation (McGregor 1967). Weick (1995), in discussing the process of enactment in his book on sensemaking in organizations, draws heavily on Follett’s view that behavior is both internally and externally conditioned. Much of the impetus to participative management is credited to Follett (Kaufman 2001).

The very frequency with which Follett’s ideas have influenced later thinking suggests that many people often consider them valid. However, there are instances in which some degree of skepticism seems warranted. It is easy to lose one’s critical sense while reading her prose. Take, for instance, her advocacy of management as a profession. Many of the steps to this end that she advances have been taken, many additional advocates have come on the scene (see, e.g., Squires 2001). Yet management is still not a profession; you can get into it from many directions, not just via an MBA from a school accredited by the Association to Advance Collegiate Schools of Business. This prophecy has been slow to come to fruition.

The problem as I see it is that Follett’s experience dealt entirely with medium-sized and large corporations. She had little knowledge of entrepreneurship and she did not consider it in her writings. Yet entrepreneurs with very little formal education of any kind, and few other accoutrements of a profession either, can make tremendous contributions to society in
the form of employment, products, services, capital creation, and so on. And they do this labeled as managers. It would not be in the interest of society to exclude them from a managerial profession as unqualified. Therein lies the problem for management as a profession.

Although the issues she deals with are only partially overlapping, Follett writes from a position that is similar to Barnard. She has a certain practical proclivity, and she clearly understands and sympathizes with management. Yet there are aspects of her writing and of the stand she takes on some issues that tend to endear her to those of a more humanist bent. In fact, she has been adopted by many within organizational behavior who identify with her behavioral humanist approach, and it is often these people who ascribe to her the greatest skills as a prophet (see, for instance, the commentaries by Bennis and Lawrence in Graham 1995).

In contrast, Max Weber, the subject of Chapter 10, has come to be perceived much differently; his views have often been allied with the classical management school. One cannot help wondering whether those who have provided the building blocks of organizational behavior would be entirely comfortable with the ways in which history has treated them, and categorized them.

REFERENCES

Background

Weber was originally educated in Germany as a lawyer. Much of his early writing dealt with legal history. In addition to law and history, Weber produced major contributions in the fields of economics, religion, and political science. However, he became best known in sociology,
the field in which his theory of bureaucracy had the greatest influence. Though associated with several German and Austrian universities, Weber stayed longest at the University of Heidelberg. Due to a substantial inheritance and intermittent periods of debilitating depression, he did not teach regularly and in fact spent a number of years as a private scholar (Marianne Weber 1975). When he did teach, his subject was economics (Swedberg 2003).

Although not of major concern here, Weber was more widely known initially for his thesis that one consequence of the rise of Protestantism in previously Catholic Europe was the development of capitalism (Weber 1930). In this view, the Protestant ethic said that God intended profitability, and blessed it as well; that waste, and thus not devoting time to profitable labor, was contrary to God’s will; that division of labor was to be desired, since it contributed to the quality and quantity of production; and that hard work was in the nature of a duty to God, which contributed to the accumulation of wealth that in turn should be put back into capitalistic endeavors. These views reflect the scope of Weber’s intellect. In many ways, he was rewriting history. There is some evidence of this tendency in his writings related to bureaucracy as well.

**WEBER ON BUREAUCRACY**

Weber’s approach to theory construction is scholarly, and his statements are often documented from a historical perspective. Though some people, particularly those of a human relations bent, typically view classical management theory (see Chapter 11) and bureaucratic theory as comparable, even to the point of not differentiating between them, a reading of the two indicates major differences. Weber is primarily interested in the role of bureaucracy in the historical development of society and its organizational forms. Classical theory focuses on problems of managerial practice. Both fail to operationalize variables and conduct relevant research, but Weber is much more concerned with clarity of definition.

There has been considerable controversy regarding certain aspects of the translation of Weber (Weiss 1983). In order to make as clear as possible the interpretation I place on his writings, the following discussion utilizes direct quotes frequently. These quotes are consistently from the more comprehensive Roth and Wittich (1968) translation.

**The Nature of Organization**

Weber sees an organization as a particular type of social relationship that is either closed to outsiders or limits their admission and has its regulations enforced by a chief, usually with the assistance of an administrative staff. The key factor is some hierarchy of authority that serves to ensure that members will carry out the order governing the organization. This order may be self-enacted or imposed by an outside agency. Organizational structure refers to the specific manner in which the authority is distributed.

The concept of rules plays an important role in Weber’s theory, especially rationally established rules. A formal organization is one with a continuously and rationally operating staff. Such a staff possesses power—a probability that its commands will be obeyed. It utilizes discipline—a probability that as a result of habit, commands will result in immediate and automatic obedience.

This staff, which is comparable to the managerial component of today’s organizations, is a special group that can be trusted to execute existing policy and carry out commands. It may be tied to the chief in a number of ways, including custom, emotion, and material interest. A
key factor in the continued domination of the organization by those at the top is the law of the small number:

The ruling minority can quickly reach understanding among its members; it is thus able at any time quickly to initiate that rationally organized action which is necessary to preserve its position of power. Consequently it can easily squelch any action of the masses threatening its power. . . . Another benefit of the small number is the ease of secrecy as to the intentions and resolutions of the rulers and the state of their information. (Weber 1968, 952)

**Pure Types of Authority: Rational-Legal**

Weber's theory gives considerable attention to concepts such as authority, domination, command, power, and discipline; this focus appears to have alienated theorists of a human relations orientation. Yet these are important concepts for organizational theory.

A authority is said to be legitimized or validated by appeal to one or more of three possible grounds—rational-legal rules, personal authority invested with the force of tradition, and charisma. These are pure types that rarely occur alone in nature. In practice, systems of authority typically are mixtures or modifications of the three.

Rational-legal authority provides a basis for the organizational structure termed bureaucracy. It involves:

[A ] system of consciously made rational rules (which may be agreed upon or imposed from above), which meet with obedience as generally binding norms whenever such obedience is claimed by him whom the rule designates. In that case every single bearer of powers of command is legitimated by the system of rational norms, and his power is legitimate insofar as it corresponds with the norm. Obedience is thus given to the norms rather than to the person. (Weber 1968, 954)

Norms are established because of expediency and/or value-rationality; they apply to the members of the organization, but may extend beyond that to the sphere of power of the organization. There is a consistent system of rules—stated in the abstract, but applied to particular cases. Even those in authority are thus subject to an impersonal order. A n individual obeys only as an organization member and in response to the law or an impersonal order, not to an individual. Thus, obedience is required only within a legitimate, rationally established jurisdiction.

The categories of rational-legal authority are described as follows:

1. A continuous rule-bound conduct of official business.
2. A specified sphere of competence (jurisdiction). This involves:
   a. A sphere of obligations to perform functions which have been marked off as part of a systematic division of labor.
   b. The provision of the incumbent with the necessary powers.
   c. That the necessary means of compulsion are clearly defined and their use is subject to definite conditions. . . .
3. The organization of offices follows the principle of hierarchy; that is, each lower office is under the control and supervision of a higher one. . . .
4. The rules which regulate the conduct of an office may be technical rules or norms.
5. [I]t is a matter of principle that the members of the administrative staff should be completely separated from ownership of the means of production. . . . There exists, furthermore, in principle complete separation of the organization’s property (respectively capital), and the personal property (household) of the official.

6. [T]here is also a complete absence of appropriation of his official position by the incumbent. . . .

7. Administrative acts, decisions, and rules are formulated and recorded in writing.

8. Legal authority can be exercised in a wide variety of different forms. (Weber 1968, 218–19)

**Traditional and Charismatic Authority**

Traditional authority derives from the personal loyalty associated with a common upbringing. It is based on the sanctity of long-standing rules and powers, on tradition, and custom. To some extent these traditions specify the exact content of command, but they may also provide a wide range for individual discretion. Thus, traditional authority attaches to the person, not to an impersonal position. It tends to be present where positions of power are filled on the basis of family membership, as in kingdoms and family-owned firms. Here, as in the traditional family, obedience is to the person.

Like traditional authority, charismatic authority is also personal. The leader’s personality interacts with followers so that they attribute supernatural, superhuman, or at least exceptional powers to the leader. Charismatic authority rests on recognition by others and results in complete devotion to the leader. The hierarchical powers on which charisma is based must be frequently demonstrated and serve to benefit the followers, or authority will disappear. Typically, a charismatic community emerges over which the leader often exercises arbitrary control. Irrationality and emotional ties are characteristic. Economic considerations are downplayed. Free of ties to rules, whether rationally or traditionally derived, this kind of authority can be a major force for change and revolution.

**Combined Authority**

Authority and willingness to obey are based on beliefs. These beliefs, which bestow prestige, are typically complex, and accordingly few organizations operate from a single authority base. Rational-legal authority tends to become infused with tradition over time. Bureaucratic organizations tend to be headed at the very top by charismatic leaders, not bureaucratic officials, and they function more effectively if this is so.

Historically, many organizations develop from charismatic to rational-legal to traditional, and then, as traditional authority fails, organizations return to the revolutionary charismatic form. Charismatic authority alone is highly unstable. The charismatic community that maintains itself over time must become rationalized or traditionalized to some degree. This routinization of charisma is particularly important to succession.

Weber views the emergence of an administrative staff as essential to stable organization. Continued obedience requires an effort to enforce the existing order, and this in turn is a consequence of a solidarity of interests, a consistent value system extending beyond the chief and the staff. At some points, Weber appears to equate the very existence of an organization with some degree of rationalized bureaucracy, but he is not consistent in this regard.
Though a generally authoritarian orientation is often attributed to bureaucratic theory, Weber is clearly positively disposed toward certain democratic and collegial forms. In establishing patterns of succession and routinizing charismatic systems, elections and other democratic procedures may emerge. Such procedures can be a major anti-authoritarian force and a force for rationality. As organizations become large, full collegiality is no longer possible, but other forms of democracy involving representation are still viable. In fact Weber tends to associate the democratization of society with the growth of bureaucratic organizations. Under such conditions, bureaucracy contributes to the leveling of social and economic differences. At the same time, democracy may come into conflict with bureaucratic tendencies under certain conditions; for instance, a bureaucratic emphasis on career service may conflict with democratic endorsements of election for short terms and the possibility of recall from office.

Aspects of Bureaucracy

Many aspects of bureaucracy have been considered as a natural outgrowth of the discussion of different types of legitimate authority—especially the rational-legal type—but other factors are involved. Furthermore, Weber viewed bureaucracy as a modern organizational form, superior to other forms in a number of respects. He tended to associate it not only with societal, if not organizational, democracy, but also with the growth of a capitalistic, economic system and with a certain disesteem for “irrational” religion.

Bureaucracy involves sets of jurisdictional areas ordered by rules. Needed activities are assigned to jurisdictions as duties, authority to elicit behavior to carry out these duties is strictly defined and delimited, and the filling of positions is based on preestablished qualifications. There is a clearly established hierarchy of subordination and appeal. Management positions presuppose thorough training in a specialized area. In addition, a comprehensive knowledge of the organization’s rules is required. In a fully developed bureaucracy, the needs for the various kinds of knowledge and expertise and for their application are sufficient to produce a full-time position.

Entry into bureaucratic management is based on a set course of training and usually on performance on prescribed examinations. Appointment tends to be by superior authority (election represents a departure from true bureaucracy). After appointment, the individual enters on a career in the organization and can expect to progress up the hierarchy. Compensation is a salary plus a pension in old age. Certain rights go with appointment to the office and protect an incumbent against arbitrary, personal action.

Bureaucratic systems dominate through knowledge, and this fact gives them their rationality. The result is a climate of formal impersonality, without hatred or passion and hence without affection or enthusiasm. Movement toward such an organizational form is fostered by sheer growth of an organization, and consequently of the administrative task. Bureaucracy is also fostered by the qualitative expansion of administrative tasks—the knowledge explosion and the taking on of added activities by the organization.

Normative Statements

Weber described bureaucracy at considerable length and placed it in historical perspective, but he also hypothesized that the kind of organization he described would be more effective than alternative forms on a number of counts:
The purely bureaucratic type of administrative organization—that is, the monocratic variety of bureaucracy—is, from a purely technical point of view, capable of attaining the highest degree of efficiency and is in this sense formally the most rational known means of exercising authority over human beings. It is superior to any other form in precision, in stability, in the stringency of its discipline, and in its reliability. It thus makes possible a particularly high degree of calculability of results for the heads of the organization and for those acting in relation to it. It is finally superior both in intensive efficiency and in the scope of its operations, and is formally capable of application to all kinds of administrative tasks. (Weber 1968, 223)

And again:

The fully developed bureaucratic apparatus compares with other organizations exactly as does the machine with the non-mechanical modes of production. Precision, speed, unambiguity, knowledge of the files, continuity, discretion, unity, strict subordination, reduction of friction and of material and personal costs—these are raised to the optimum point in the strictly bureaucratic administration. . . . As far as complicated tasks are concerned, paid bureaucratic work is not only more precise but, in the last analysis, it is often cheaper than even formally unremunerated honorific service. (Weber 1968, 973–74)

Nowhere does Weber contend that the members of such organizations will be happier or more satisfied, but he does contend that bureaucracy works, and to some degree it works for individual members also by freeing them from the inequities of arbitrary authority. Ultimately, bureaucracy works so well that it is practically indestructible. Once such a system is set in motion it is almost impossible to stop, except from the very top; the sum of the parts is an effective organization, but no one part, no single official, is powerful enough to disrupt the whole.

**Contrasts with Other Forms**

Weber contrasts bureaucracy with other types of organizations that rely more heavily on traditional and charismatic authority, what he calls prebureaucratic forms. Traditional authority is particularly manifest in patrimonial organizations, which lack the bureaucratic separation of private and official spheres. Under traditional conditions, decisions tend to be ad hoc rather than predetermined by rules, and loyalty is not to the duties of an impersonal office, but to a ruler as a person. Ineffectiveness becomes a matter of arousing the ruler’s disfavor rather than failing to perform the duties of the position. Feudalism involves much that is patrimonial, but relationships are more fixed than in many other such forms. Elements of routinized charisma are in evidence as well.

In a sense, charismatic authority is antithetical to the idea of organization. Yet a personal staff of disciples characteristically arises to form a charismatic aristocracy. The basic system tends to be communal, with the leader issuing dispensations according to his personal desires. Under such conditions, the leader can introduce changes rapidly—there are no rules or traditions to block them. Bureaucracy can produce change as well, but it does so by first changing the material and social order along rational lines, and then changing the individuals.

Discipline in a modern factory is much the same as in the military or on a plantation, but it is more rational:
With the help of suitable methods of measurement, the optimum profitability of the individual worker is calculated like that of any material means of production. On this basis like the American system of scientific management it triumphantly proceeds with its rational conditioning and training of work performances, thus drawing the ultimate conclusions from the mechanization and discipline of the plant. . . . This whole process of rationalization, in the factory as elsewhere, and especially in the bureaucratic state machine, parallels the centralization of the material implements of organization in the hands of the master. Thus, discipline inexorably takes over ever larger areas as the satisfaction of political and economic needs is increasingly rationalized. This universal phenomenon more and more restricts the importance of charisma and of individually differentiated conduct. (Weber 1968, 1156)

Weber does not say he likes all this; his writing is in fact quite objective and neutral. But he certainly respects it, and he appears to some degree to be afraid of it as well.

**SUBSEQUENT REACTIONS AND RESEARCH**

One reason for the extensive discussion of Weber’s ideas that has occurred over the years is the uncertainty as to what he really said. These differences are based only in part on difficulties of translation. Weber often returned to the same subjects, approaching them from different angles and providing fuel for numerous interpretive debates (Turner 1983). Thus, Weber’s statements are on occasion conflicting or ambiguous on the role of collegiality vis-à-vis bureaucracy; the extent to which bureaucracy may be defined as a self-contained and self-perpetuating entity as opposed to a tool of the user; the relationship of centralization to bureaucracy; the power of bureaucracy; the degree of voluntary versus imperative control inherent in rational-legal authority; and the distinction between the concept of organization as a whole and its bureaucratic subtype. Weber strove for clarity of definition but conveyed a message of considerable fuzziness on certain issues (Abrahamsson 1977). Scientifically, the theory is logically inconsistent to a degree.

**Criticisms**

Weber was not totally unaware that there are dysfunctions and unanticipated consequences of bureaucracy. Such matters were considered, though they were not his major concern. Thus, the existence and activities of the informal organization represent departures from the ideal type. That Weber did not deal with such matters at length reflects the nature of his domain choice. He wanted to establish how and why bureaucracy works under ideal conditions. This in itself is a massive undertaking, and one cannot legitimately fault Weber for not extending his theory with the same detail into new domains. Yet, others have done so since. Much the same argument applies to the contention that Weber failed to consider organization-environment interactions and produced an overly limited, closed-system theory. As Aldrich and Pfeffer (1976) point out, Weber’s historical and comparative analyses dealt at length with the impact of social structure on bureaucracy. And McNeil (1978) has drawn upon Weber’s views as a frame of reference for describing how organizations gain power over their environments. Certainly, Weber’s formulations regarding external forces are incomplete, relative to his theory of internal factors. Nevertheless, significant theory construction almost inevitably requires focusing on some one domain at the expense of others.
There has been considerable discussion of the decline of bureaucracy, as well as of approaches to replace it. Perhaps Weber was wrong in emphasizing bureaucracy’s strength and resiliency. This has been the position of many organization development practitioners. As DiMaggio (2001) notes, in recent years bureaucratic structures have become flatter, their headquarters’ staffs smaller, they rely more on teamwork and less on narrow job descriptions, and a variety of types of collaboration have emerged. All in all the structures involved have become looser; however, they remain of a bureaucratic nature. These revised, flatter systems have been found to produce better performance, but beyond that no particular bureaucratic form of organization appears to make any differences (Nohria, Joyce, and Roberson 2003). From a legal perspective (Kraakman 2001), and in the view of a long-term advocate of the use of groups (Leavitt 2003), bureaucracy is here to stay; it has not been and will not be supplanted. For an example of how far the structure may be stretched and still remain bureaucracy, see Ashcraft (2001).

Questions have also arisen regarding the role of what has come to be called universalistic personnel practices, which involve an emphasis on goal-oriented meritocracy, in bureaucracy. Research evidence indicates that where universalism prevails bureaucracy is more complete and effective, as compared with a resort to particularism, with its focus on individual characteristics and favoritism (Pearce, Branyiczki, and Bigley 2000). Thus, as Weber indicated, judgments based on merit do appear to be essential to a well-functioning bureaucracy.

Knowledge and Innovation

With regard to the role of rationality and knowledge in bureaucracy, Weber appears to have been both right and wrong. Knowledge related to strategic decision making and policy implementation for the specific organization does concentrate at the top of the hierarchy and greater rationality tends to accrue with it. However, knowledge that is less organization-specific does not relate to the bureaucratic hierarchy in the manner Weber proposed. This latter, professional knowledge is a source of power, and, in a general sense, Weber was correct in equating authority and knowledge. But he did not differentiate rational-legal authority from the value-rational type that underlies professional systems (Satow 1975). Not only does Weber’s theory fail to comprehend professional organizations, which appear to fall outside its domain, but it also fails to deal with professional systems, knowledge, and authority lying predominantly within bureaucratic organizations. To this latter extent, it is deficient within its own domain.

Such professional components within bureaucratic organizations do not appear to be mere deviant cases, but rather separate, distinct systems with their own characteristics and sources of authority. Accordingly, Weber’s theory is often criticized as failing to provide for organizational innovation beyond what is achieved through professional activities. In fact, bureaucracy is often said to stifle innovation and creativity.

The best evidence on these matters derives from a meta-analysis of a rather extensive literature dealing with the determinants of innovation rates in organizations. The results are given in Table 10.1. Included there are not only the average correlations, but the author’s expected relationships derived from theory and in some instances prior research (Damanpour 1991). In large part, these hypotheses posit a positive relationship between professional factors and innovation but a negative relationship between bureaucratic factors and innovation.

Where positive relationships are expected, they are in fact obtained with only one exception, and that involves a characteristic (managerial tenure) that does not concern profession-
als. Note, however, that significant positive results occur for managerial attitude toward change and administrative intensity, which also are not professional in nature. In any event, the truly professional factors all yield correlations that fit with the expected higher levels of innovation. The presence and effective utilization of professionals does make for a higher innovation rate.

As to the negative impact of bureaucracy, the data present a different picture. As noted, some factors associated with managers, and thus the bureaucratic component, yield positive correlations with innovation. Even more important, support for the bureaucratic hypotheses (at the bottom of Table 10.1) is minimal—only one fit out of four possible is obtained, and that, involving centralization, is far from strong. All in all, there is little here to confirm the idea of an inhibiting impact of bureaucracy on innovation.

One possible explanation of these types of findings is that professional systems generate technical innovations and bureaucratic systems generate administrative innovations (Daft 1982). Thus, each segment has its own special type of proclivity in this regard. Since administrative innovations have been studied less, they would weigh in less strongly in any composite analysis. This type of moderator effect is not supported, however, in the Damanpour (1991) meta-analysis; the two types of systems both appear to be capable of generating innovations of either kind. Truly innovative organizations apparently introduce a prevailing climate in all their components, which fosters innovation of any kind (see also, Damanpour and Gopalakrishnan 2001).

The literature on innovation has continued to produce mixed results and theoretical dead-ends over a considerable period of time (see Hage 1999). In this context, the Damanpour

<table>
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<th>Characteristics hypothesized to have a positive relationship with innovation rate</th>
<th>Meta-analytic finding</th>
<th>Fit</th>
</tr>
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<tbody>
<tr>
<td>Specialization</td>
<td>.39</td>
<td>Yes</td>
</tr>
<tr>
<td>Functional differentiation</td>
<td>.34</td>
<td>Yes</td>
</tr>
<tr>
<td>Professionalism</td>
<td>.17</td>
<td>Yes</td>
</tr>
<tr>
<td>Managerial attitude toward change</td>
<td>.27</td>
<td>Yes</td>
</tr>
<tr>
<td>Managerial tenure</td>
<td>Not significant</td>
<td>No</td>
</tr>
<tr>
<td>Technical knowledge resources</td>
<td>.47</td>
<td>Yes</td>
</tr>
<tr>
<td>Administrative intensity</td>
<td>.22</td>
<td>Yes</td>
</tr>
<tr>
<td>Slack resources</td>
<td>.14</td>
<td>Yes</td>
</tr>
<tr>
<td>External communication</td>
<td>.36</td>
<td>Yes</td>
</tr>
<tr>
<td>Internal communication</td>
<td>.17</td>
<td>Yes</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Characteristics hypothesized to have a negative relationship with innovation rate</th>
<th>Meta-analytic finding</th>
<th>Fit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Formalization</td>
<td>Not significant</td>
<td>No</td>
</tr>
<tr>
<td>Centralization</td>
<td>-.16</td>
<td>Yes</td>
</tr>
<tr>
<td>Vertical differentiation</td>
<td>Not significant</td>
<td>No</td>
</tr>
<tr>
<td>Size</td>
<td>.32</td>
<td>No</td>
</tr>
</tbody>
</table>

(1991) results, based as they are on quantitative findings and relatively free of theoretical preconceptions, stand out as providing the best indication of the state of current knowledge. This position is further supported by an updated meta-analysis focused on the organizational size relationship that reaches the same conclusion as Damanpour’s prior work; size and innovation are positively related (Camisón-Zornoza, Lapiedra-Alcamí, Segarra-Ciprés, and Boronat-Navarro 2004).

Research

Research on the theory, to the extent it has been conducted with appropriate samples and measures, has in general supported the descriptive theory, though more as a composite of variables than as a single type. Formalization, standardization, and specialization tend to be highly correlated, and thus to vary together, as the theory would predict.

A considerable body of research relates large size in both composite units and total organizations to the bureaucratic nexus. There are studies that find only a weak relationship here, and there is reason to question whether size is necessarily a cause of the structure, but size typically has something to do with bureaucracy. Though small bureaucracies are possible, small organizations are more likely to take some other form. Furthermore, bureaucratization seems unlikely to continue unabated beyond some level of growth. Nevertheless, bureaucracy appears to be the preferred method of structuring large organizations of any type. Thus, the overall data support Weber on this point.

The centralization-decentralization variable and results related to it have been the subject of major controversy. Some view Weber’s bureaucracy as incorporating centralization, while others think just the opposite. There is considerable confusion regarding how centralization should be operationalized and what the limits of the construct are. This is only part, but a very important part, of the construct validity problem that sometimes plagues the theory as a whole.

We know little about Weber’s normative theory of bureaucratic superiority. Whether a bureaucratic system at its best is superior to a professional system at its best, for instance, is a completely unanswered question. Furthermore, the significance and role of charismatic leaders at the top of bureaucratic organizations remains highly uncertain. On the other hand, there is evidence that increases in various bureaucratic characteristics, as organizations approach the ideal type, are associated with more positive organizational outcomes. For a discussion of these issues, see in particular Part IV of Miner (2006).

CONCLUSIONS

As Vibert notes, “the bureaucratic form remains the dominant description of large-scale organization for much of the work force” (2004, 88). In addition, a whole set of theories arose out of Weber’s formulations regarding bureaucracy, many of which attempt to move well beyond what has been considered here. Without question, Weber’s insights have exerted substantial influence on subsequent research and theory, although there has been considerable fluctuation in the extent of that impact at different times over the past half-century. Overall, however, our understanding of organizations took a quantum leap when Weber’s theory finally became widely known. Those who desire more comprehensive knowledge of the consequences of that leap should consult Conger (1993) on charismatic leadership and Clegg (1995) on the influence of Weber’s macro theory.
The influence of Weber’s formulations regarding charisma on modern-day leadership theory is evident in the work of House and Bass (see Miner 2005, chapters 18 and 19). Well-known macro organizational behavior contributions that owe a primary debt to Weber include Selznick’s (1949) analysis of the Tennessee Valley Authority and Gouldner’s (1954) study of a gypsum plant. Other sociologists who have drawn heavily on Weber to develop their theories include Blau and James Thompson (both are considered in Miner 2006). Although sociology has perpetuated the Weberian tradition most, political science has been a contributor as well (see, for instance, Victor Thompson, 1961).

Bureaucracy theory did not invent the bureaucratic form; it described it and attributed normative powers to it. Yet, because it was first written in German, and did not come to the English-speaking world for many years, the likelihood that Weber’s writings had a substantial influence on bureaucracy’s widespread adoption seems small. It seems probable that bureaucracy would have existed in large organizations on much the same scale without Weber. Nevertheless, as the theory became more widely known, it no doubt contributed to the refinement of bureaucracies already in existence everywhere.

For many, the classical management thinkers, Fayol and Taylor, are one and the same with Weber. This is a total misconception. There are major differences in the content of the formulations involved as well as in the historical role these formulations have played for organizational behavior. The presentation in Chapter 11 makes this clear.

REFERENCES

CHAPTER 11

CLASSICAL MANAGEMENT THEORISTS: THE ANTIHEROES—HENRI FAYOL AND FREDERICK TAYLOR

Backgrounds
Fayol’s Statements
   Definition of Management
   Principles of Management
   Elements of Management
   Later Statements
Taylor’s Statements
   Principles of Management
   Conditions for Change
   The Exception Principle
Subsequent Treatment
   Research
   Applications
Conclusions

The classical management theorists considered in this chapter played a very different role in the genesis of organizational behavior than did the individuals we have been considering. Their work and ideas are important primarily because they served as objects of attack by various organizational behavior theorists, much less because their thinking served to positively influence later organizational behavior theory. It is important to understand these classical theories so as to appreciate the nature of and reasons for the attacks. The writers who attempted to turn Henri Fayol and Frederick Taylor and their followers into antiheroes were primarily associated with the human relations component of organizational behavior, but there were attacks from other directions as well. For these and other reasons, the classical theories no longer provide an active theoretical thrust within organizational behavior, and they have not for a long time (Miner 1995). There are remaining influences on practice, however.

Given certain perceived similarities between Weber and the classical theorists, it comes as no surprise that certain of the attacks extended to Weber as well. However, Weber has had numerous advocates, primarily among the macro theorists who have clarified and extended his views and put them to research test. A major reason why Weber remains something of a hero within organizational behavior, while the classical theorists have been ignored or labeled with an antihero designation, is that Weber has received considerable support from research, and the classical theorists have had relatively little such support.
Henri Fayol (1841–1925) graduated in 1860 in mining engineering from the School of Mines at St. Etienne in France. He immediately joined the company with which he spent his entire career; this company became Commentry-Fourchambault et Décazeville, and he subsequently became its chief executive officer. Fayol’s rise within this firm was rapid. He became an expert on underground fires and spontaneous combustion, on geological structures in France, and on mine safety. He conducted research in these areas and published it (Breeze 1985). His writings on management came late in his career. Although expounded earlier, his first published statement came in an essay in a French mining journal in 1915 (Witzel 2003). He continued to write on the subject of management until his death in 1925 (Breeze and Bedeian 1988), the same year that his Administration Industrielle et Générale first appeared in book form.

Although this book had some impact in France and was first translated into English in a limited edition in 1929, it was not until twenty years later under the title General and Industrial Management (Fayol 1949) that it received wide recognition. Thus, both Fayol and Weber reached the United States at roughly the same time; they do not appear to have influenced one another.

Fayol’s work spawned a number of elaborations and extensions, especially in the years immediately following its major translation into English; among the best known are those of Urwick (1952), Ralph C. Davis (1951), and Koontz and O’Donnell (1955). The latter is treated in Chapter 5, and has provoked considerable discussion since (see Carson, Lanier, Carson, and Guidry 2000). Subsequent statements in the classical tradition have appeared largely in management textbooks, again, with considerable variation around the central themes (Miner 1971). Early formulations in a similar vein are presented in books by Mooney and Reiley (1931) and Gulick and Urwick (1937), for example. Neither Fayol, nor any of these individuals who espoused comparable views in the classical management tradition, conducted any research to actually test the theory in any form.

Frederick W. Taylor (1856–1915), without the need for translation that Weber’s and Fayol’s works faced, became widely known in this country considerably earlier. Indeed, his theories were known throughout the world, and to Follett, Weber, and Fayol. His major writings were Shop Management (1903) and The Principles of Scientific Management (1911), both of which were widely read shortly after publication (Witzel 2003).

Taylor came from a family of considerable wealth. He grew up and spent most of his life in the Philadelphia area. He attended Exeter Academy in New Hampshire and was destined for Harvard. However, instead of attending college he undertook a four-year apprenticeship as a patternmaker and machinist at Enterprise Hydraulic Works in Philadelphia. From there he moved to Midvale Steel and rose rapidly, to chief engineer, in six years. While at Midvale, he enrolled in a home study program in mechanical engineering given by Stevens Institute of Technology, from which he graduated with an undergraduate degree in 1883, apparently after very little classroom attendance. During this period he invented a wide range of mechanical devices, conducted numerous experiments on metal cutting, began his work on the timing and desired performance of production tasks, and introduced piece-rate incentive systems to the company.

After twelve years at Midvale, Taylor moved on to various positions, most of which were in the role of consultant, at a number of manufacturing firms. There was a considerable stint at Bethlehem Steel. He lectured frequently, including teaching at the Harvard Business School.
every winter from 1909 to 1914. Largely at the suggestion of Louis Brandeis, a lawyer, the term scientific management was applied to Taylor’s system. Over time, the unions became increasingly vocal in their opposition (Hoxie 1915). Taylor died at age fifty-nine.

Largely in connection with his consulting activities, Taylor came to surround himself with a number of people who served both to promote scientific management and in some cases to modify it. Several of these, including Carl Barth, Henry Gantt, Frank and Lillian Gilbreth, Harrington Emerson, and Morris Cooke (Wren 1994), became quite famous in their own right. Selections written by other individuals in the scientific management circle and published in the Bulletin of the Taylor Society are contained in a book by Del Mar and Collons (1976). For a number of reasons, including Taylor’s controversial nature, his important ideas, and the availability of biographical material, the man and his work have been the subject of a spate of biographical books. Kakar (1970) provides a psychoanalytic interpretation. Nelson (1980) places Taylor in the context of the industrialization of America. Wrege and Greenwood (1991) focus on Taylor as an individual and the influence of those around him. Kanigel’s (1997) treatment is characterized by more balance than the others. Certainly, Taylor’s writings are subject to multiple interpretations (see Monin, Barry, and Monin 2003).

**FAYOL’S STATEMENTS**

Fayol’s (1949) major work in the field of management consisted of two parts—“Necessity and Possibility of Teaching Management” and “Principles and Elements of Management.” Two additional parts, “Personal Observations and Experience” and “Lessons of the War,” were projected but never published. These latter parts were to deal with applications, some of which are discussed in Fayol’s later writings and speeches (see Brodie 1967).

At the time Fayol wrote, theory was distinctly lacking in the management field. Fayol felt that there was a great need for management education to supplement technical education such as the training he himself had received in engineering, and he believed that the existence of a body of theory was a necessary precondition for such education. This was the underlying rationale for his theorizing.

**Definition of Management**

Fayol specified the essential activities or functions of industrial organizations to be the following:

1. Technical—production, manufacture, adaptation.
2. Commercial—buying, selling, exchange.
3. Financial—search for and optimum use of capital.
5. Accounting—stocktaking, balance sheets, costs, statistics.
6. Managerial—planning, organization, command, coordination, control.

Management is spread as are all other activities, between the head and members of the body corporate, but it is concentrated particularly within top management. The distinct increase in hypothesized need for managerial ability—as compared with the decrease or stability of other abilities—that comes with an increase in occupational level is indicated in Table 11.1. At the lower levels it is technical ability that matters most; at higher levels it is
managerial ability. The definition of technical ability varies, of course, within commercial, financial, and other functions. The head of a very small firm will need considerable technical ability, but this need, like that for commercial ability, will decrease with increasing size; in contrast, managerial ability requirements will increase with size.

**Principles of Management**

The principles are defined as flexible and capable of adaptation, rather than rigid and absolute. Using them is an art. Fayol set forth fourteen such principles of management. Behavior in accord with them will contribute to a more effective organization. The list is not exhaustive, but the principles apply widely. Regarding them, Fayol said:

> This code is indispensable. Be it a case of commerce, industry, politics, religion, war, or philanthropy, in every concern there is a management function to be performed and for its performance there must be principles, that is to say acknowledged truths regarded as proven on which to rely. (Fayol 1949, 41–42)

1. **Division of work.** Division of labor or specialization at the worker and managerial levels reduces the number of objects to which attention must be given and therefore yields increased quality and quantity of output for the same amount of overall effort. There are limits beyond which division of work should not be carried out, however. How these limits may be determined is not clearly specified.

2. **Authority and responsibility.** Managers should exercise authority both as it derives from the office held and as it derives from the intelligence, experience, and other personal qualities of the manager. At the same time, responsibility must be commensurate with authority in that rewards and penalties accrue, depending on how effectively authority is used. Determining and measuring the authority of a given manager as it relates to a particular outcome and establishing appropriate sanctions are viewed as major difficulties. Fayol recognizes the underlying measurement problems here; he also recognizes a natural tendency to seek authority and avoid responsibility. In neither case does he present a solution, other than to call for integrity and moral character.

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Table 11.1

<table>
<thead>
<tr>
<th>Occupational level</th>
<th>Technical</th>
<th>Commercial</th>
<th>Financial</th>
<th>Security</th>
<th>Accounting</th>
<th>Managerial</th>
</tr>
</thead>
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<td>—</td>
<td>—</td>
<td>5</td>
<td>5</td>
<td>5</td>
</tr>
<tr>
<td>Foreman</td>
<td>60</td>
<td>5</td>
<td>—</td>
<td>10</td>
<td>10</td>
<td>15</td>
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<tr>
<td>Superintendent</td>
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<td>5</td>
<td>—</td>
<td>10</td>
<td>15</td>
<td>25</td>
</tr>
<tr>
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<td>5</td>
<td>5</td>
<td>10</td>
<td>20</td>
<td>30</td>
</tr>
<tr>
<td>Department head</td>
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<td>10</td>
<td>5</td>
<td>10</td>
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<td>35</td>
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<td>15</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>40</td>
</tr>
<tr>
<td>General manager</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>10</td>
<td>50</td>
</tr>
</tbody>
</table>

3. Discipline. Discipline is a condition for effective operation of a business. It consists of obedience, application, energy, behavior, and outward marks of respect, all given on the basis of some formal or informal employment contract between the individual and the firm. To function as it should, discipline requires good managers, clear and equitable agreements, and the judicious application of sanctions such as warnings, fines, suspensions, and other, similar disciplinary actions.

4. Unity of Command. An individual should receive orders with regard to a particular action from one source only. Dual command is to be avoided. Examples of situations where dual command may arise are superiors bypassing subordinate managers to direct that manager’s subordinates, two friends or family members both heading up a firm, unclear boundaries between two departments at the same level, and conditions of role ambiguity in general.

5. Unity of Direction. Unity of direction applies to coordination of effort and is a principle of organizations. A group of activities having the same objective should be placed under a single head and a single plan. Fayol does not discuss the bases for differentiating objectives that might be applied here.

6. Subordination of Individual Interests to General Interest. For effective functioning the interests of the organization as a whole must take precedence over those of individuals or groups. Subordination of interests is one basis for reconciling conflicting interests. In some instances interests of a different order appear to have equal claims. Such conflicts must be reconciled rather than being permitted to continue. Possible means to this end are the firmness and good example of managers, fair agreements, and constant supervision.

7. Remuneration of Personnel. Insofar as possible, payments should be fair and equitable, should reward well-directed effort, and should not exceed reasonable limits. Various methods of achieving these goals are discussed, but without any clear resolution. One index of effective remuneration is that the pay agreement afford satisfaction to employer and employee alike, but it is recognized that this may not be possible. There is a need for precise definitions and operationalizations of concepts such as fairness, reasonable limits, and the like.

8. Centralization. The amount of centralization, as opposed to decentralization, should be optimal for the particular concern. Contingency variables are firm size, personal character of the manager, manager’s moral worth, reliability of subordinates, and condition of the business. The degree of centralization may vary considerably, depending on the relative potential effectiveness of the manager or subordinate. Fayol’s formulations in this area are not specific, but he does recognize a number of relevant contingency factors.

9. Scalar Chain. In the simplest case, communication should occur up and down the scalar chain of authority—in Figure 11.1 from E₁ up to A, and if necessary, back down to E₂. But vertical communication through this many steps may consume too much time. Where speed is essential, firms should resort to what has been called Fayol’s gangplank—horizontal communication authorized by managers at the next higher level. This, too, is indicated in Figure 11.1. In general, Fayol seems to think that horizontal communication should be used more widely than it is. Surprisingly, he did not include the scalar chain principle in all versions of his theory (Brodie 1967).
10. Order. To avoid loss of material, there should be a place for everything and everything in its place. In addition, the prescribed place should be one that facilitates the carrying out of necessary activities. However, the principle of order applies not only to material things but also to people. Thus, there should be an appointed place for each employee, and each employee should be in that place. A gain, the appointed place should be appropriate to the task to be performed. This principle means good organization and selection, and it implies the existence of an organization chart.

11. Equity. Employees should be treated with kindness and justice, which together equal equity. The object is to elicit devotion and loyalty in return. Ideally a sense of equity will permeate the whole scalar chain.

12. Stability of Tenure of Personnel. Employees and managers alike need time to settle into their jobs before they can achieve maximum performance. They should be given this opportunity, and thus considerable stability of personnel should prevail. A lack of stability is both a cause and an effect of poor management. At the same time, there can be too much stability. In common with all the other principles, stability of tenure of personnel is also a question of proportion. No specific guidelines for establishing when the correct proportion exists are given, however.

13. Initiative. Initiative is thinking out a plan and executing it, as well as having the freedom to do these things. Initiative of this kind should be encouraged; it is particularly valuable to an organization in difficult times. The manager who facilitates the initiative of subordinates is far superior to the manager who does not, because initiative can serve as a source of both satisfaction and motivation.

14. Esprit de corps. Essentially this is a principle of unity. Harmony should be fostered and conflict minimized. Unity of command is one means to this end. Fayol comes out strongly against the application of such ideas as “divide and conquer” in the organization. Creating dissension among one’s subordinates thwarts coordination and teamwork. Verbal communication should be used whenever possible because,
being two-way, it permits rapid resolution of conflicts. Written communication often fosters conflict.

**Elements of Management**

Fayol specified five elements.

1. **Planning.** Planning involves foresight—assessing the future and making provisions for it. It requires the development of a plan of action based on contributions from throughout the business. Fayol comes out strongly in favor of planning that is characterized by unity, continuity, flexibility, and precision. One would expect more effective organizations to be characterized by a greater use of planning and forecasting.

2. **Organizing.** Though a distinction is made between the material organization and the human, only the latter is considered. The human organization is established to carry out managerial functions and implement the principles of management. The basic structure is pyramidal in that each group of ten, twenty, or thirty workers brings in a foreman; two, three or four foremen make necessary a superintendent, two or three superintendents give rise to a department manager, and the number of links of the scalar chain continues to increase in this way up to the ultimate superior; each new superior has usually no more than four or five immediate subordinates.

Fayol considers staffing the structure part of the organizing function. In addition to the line positions extending from operatives to the board of directors, it is necessary to fill certain staff positions attached to the general manager’s office that serve to complement the general manager’s capabilities. This is the general staff concept, and there are no levels of authority within this component. Such staff members may be consultants, and they may devote only part of their time to the staff position. To the extent it violates unity of command, Fayol rejects direct supervision of one person by several functional specialists.

Evaluation, especially of managerial personnel, is also part of the organizing function. Among the factors to be considered are health and physical fitness, intelligence, moral qualities, general education, management knowledge, knowledge of the other functions (technical, commercial, etc.), and specialized ability characteristic of the concern. Only the latter requirement varies from one business to another; thus, managerial capabilities are highly transferable.

3. **Commanding.** Command activates the organization structure. It involves knowing the personnel thoroughly, eliminating incompetents, being knowledgeable about employer-employee agreements, setting a good example, conducting periodic organization audits, setting up conferences among one’s chief assistants to establish unity of direction, avoiding an excess of detail, and generally fostering unity, energy, initiative, and loyalty. Being knowledgeable about subordinates, and command in general, is facilitated by a limited span of control. Effective performance of the command function is partly a matter of personal skill and partly a result of having a good grasp of the management principles.

4. **Coordinating.** The various activities of an organization must be harmonized into a single whole, and that is the function of coordination. Basically, it is a matter of establishing rightful proportions for the parts, ensuring that these proportions are maintained, and adapting means to ends. Under such conditions the various depart-
ments work in harmony with each other, communicating as needed, rather than operating in isolation as ends in themselves. The component units know their role in the total effort and what interdependencies exist with other units. Departmental scheduling is constantly fine-tuned to external circumstances, rather than carried out without reference to organizational goals, loyalties, and needs for initiative.

The prime method of achieving coordination is a periodic conference of department heads. Where this is physically not possible, an alternative is to use the liaison officers attached to the staff to coordinate departments. In either case, the need to facilitate horizontal communication is clearly evident. Fayol appears to recognize the difficulty of coordination here, and the common tendency to neglect activities of this kind (see Heath and Staudenmayer 2000).

5. Controlling. Control is the process of checking the realities of operations against plans and taking steps to correct deviations. It assumes the existence of up-to-date plans and the use of sanctions to achieve compatibility with them in a timely manner. Fayol notes that control systems may create duality of management if not devised correctly and monitored effectively. To the extent inspection is inherent in the control system, it should be impartial and objective.

Later Statements

Fayol continued to write on the subject of management during the years between the first publication of General and Industrial Management and his death. Much of this material is not generally available. There have been a few translations into English (Fayol 1937, 1970) and several summarizations of other works have been published (Breeze and Bedeian 1988; Brodie 1967).

Little modification occurred after publication of the primary theory. Many of the later papers deal with applications, and there are numerous restatements of parts of the theory. Fayol did come to view his own formulations and the scientific management ideas of Frederick Taylor as more complementary than conflicting (de Freminville 1927).

TAYLOR’S STATEMENTS

Taylor was concerned primarily with factory management and within that context his emphasis was mostly on the management of production workers. In this respect his frame of reference differs sharply from that of Fayol who developed his ideas from a top management perspective. It is not surprising that the two men, the engineer and the corporation president, created principles of management that differed sharply from one another. Fayol wrote from a macro perspective; Taylor from a micro viewpoint.

Principles of Management

Although often credited with only four principles of management, Taylor’s writings abound with prescriptive statements, often stated in moralistic terms, indicating what a manager should do to improve effectiveness (Filipetti 1953). The basic four, however, are:

1. Develop a science for the work of each person. This involves determining how the work can best be performed by experimenting with it, conducting motion and time studies, and often applying mathematical formulas.
2. Scientifically select the best individual for the job, train that person to be able to perform the job better and better, and, finally, pay higher wages than ever before to reward the increased productivity.

3. Cooperate with the workers to ensure that the work is in fact done in the prescribed manner; make the knowledge of the job (principle 1) and the worker selected (principle 2) come together. This should include, but not be limited to, providing for increased earnings for those who follow the prescribed methods most closely.

4. Divide the work so that activities such as planning, organizing, and controlling are the responsibility of management; the worker, on the other hand, has the responsibility for doing. This division is predicated on the assumption that most workers do not have the capability to create the science of their work.

Conditions for Change

Scientific management based on these principles cannot exist without a complete mental revolution, which takes considerable time to achieve and affects both managers and workers. The workers must learn and accept a new set of duties to management. Restriction of output (Taylor called it “soldiering”) must be abandoned in favor of greater output and making more money. At the same time, management must undergo a mental revolution and assume duties toward employees, including not changing standards when employee earnings increase sharply under piece-rate systems.

In accordance with the principles, Taylor advocated that the planning process be separated from the work and carried out by a centralized planning group; he also advocated that specialized or functional foremanship be introduced. According to this concept, a production worker would report to different foremen depending on which aspect of the work was involved. Thus, an individual might have many bosses, but each would operate within a narrow, strictly delimited zone of influence and expertise. This concept is illustrated in Figure 11.2.

The Exception Principle

Taylor placed strong emphasis on the exception principle. Output standards and routine procedures were to be established through the use of precise measurement; management should then give its attention only to those cases where standards were not met or exceeded to a marked degree, and where established procedures were not or could not be followed. As the exception principle implies, Taylor generally advocated dealing with one individual at a time, but he was not unaware of the ways in which group influences might operate to produce a restriction of output. The discovery of these restrictive practices is widely attributed to the human relations researchers who followed him by some fifteen or twenty years. Yet Taylor’s writings are sprinkled with references to group pressures and their impact on production (Boddewyn 1961).

While the exception principle was intended to focus primarily on ineffective performers and to deal with them, it also singled out outstanding performers, who may have developed an approach to their work that was even better than the one scientific study had identified. Thus, his “one best way” established by job study was not etched in concrete; it could be improved upon, and Taylor had a procedure for flagging possible innovations to improve it.

It should be emphasized that the use of the adjective scientific to describe Taylor’s views is by no means a misnomer. He was an advocate of research and experimentation in management.
and he conducted studies to determine how various jobs might best be performed. He developed a number of techniques for controlled observation and the measurement of behavior. His motion study involved breaking down movements into their component parts and adopting prescribed methods for their execution. This reductionist approach has been traced back to early developments in the physical sciences and the arts (Reeves, Duncan, and Ginter 2001).

**SUBSEQUENT TREATMENT**

Many organizational behavior theorists of other orientations have used the classical theories as foils against which to pit their own formulations (see, e.g., Argyris 1957; Bennis 1966; Likert 1967). These attacks contend that classical theory is inadequate because it fails to predict and explain a large number of phenomena. Under a variety of conditions of organizational membership, technology, environmental context, and the like, the theories do not work. They have also been attacked as failing to meet the requirements of good scientific theory as reflected in a pervasive ambiguity of statement and a lack of logical consistency (Simon 1947, 1976). These criticisms appear justified. Furthermore, both Fayol and Taylor have been attacked as espousing views that are inherently inhumane. Whether this is true appears to depend on what passages are quoted and on one's definition of inhumane. In any event, this issue is beyond the purview of a book such as this, which is primarily concerned with science, not philosophy.

**Research**

There is a rather large body of research that has been marshaled on occasion in support of the formulations of classical theory. However, much of this research was conducted for reasons having no direct relationship to the testing of classical theory. Its relevance is not at all clear.
Fayol and Taylor and their followers did not operationalize their variables; in fact, they described them in very ambiguous terms. They did not do research on their theories and since they were in the best position to know what they really were talking about, the opportunity to test the theories has now been lost.

This is not to say that both Fayol and Taylor did not conduct research; both did. However, their research was in the field of engineering, and, in Taylor’s case, human factors. Neither knew much about the kind of research that came to underlie the field of organizational behavior. That approach was in its early infancy at the time. But the followers extended into a period when much more was known. That they did not, for whatever reason, resort to research on the theories they espoused accounts in large part for the present day failure of those theories.

There have been attempts to tie the classical theories to phenomena of more recent vintage. Hodgetts (1995), for example, did this with Taylor’s principles and Wren (1990) with Fayol’s elements of management. The problem with all such efforts is in the interpretation of Taylor and Fayol. Were they really saying the same things that are now being said? Given the ambiguity of the early statements, it seems possible to make them fit almost any context. But do they really fit?

Applications

Given the fact that both Fayol and Taylor were practicing managers, the appeal of their theories for practitioners is not surprising. Managers use the terminology of the theories widely, and consultants still call on their precepts to substantiate their recommendations, and perhaps to develop them, too. Textbooks have characteristically been modeled around the elements of management for a number of years. Yet studies designed to measure the effects of applications directly are practically nonexistent. There are cross-sectional studies and endless anecdotal testimonials. What is lacking are instances in which classical precepts are implemented, potential consequences are measured before and after, and the results are compared with similar results obtained under controlled conditions.

Fayol was involved in three applications, or potential applications, of his ideas. One was a study within the company he headed, Commentry-Fourchambault et Décazeville (Fayol 1970); the second was a consulting engagement with the French government in the Department of Posts, Telegraphs, and Telephones (Fayol 1937; Brodie 1967); and the third was a similar engagement with the tobacco monopoly in France (Brodie 1967).

Of the three, only the company application can make any claim to success. The firm was declining and on the road to bankruptcy, when a change occurred in 1888, in the way in which the administrative function was carried out; and, without modification of anything else, without improvement in any of the adverse factors, the business began to prosper and has not stopped growing since. With the same mines and factories, with the same financial resources, the same markets, the same board of directors and the same personnel, solely because of a new method of administration the company experienced a rise comparable to its earlier fall. (Fayol 1970, 148)

Success is attributed specifically to a strategic action program based on annual and ten-year forecasts, the existence of an organization chart, observation of the necessary principles with regard to command, meetings of department and division heads, and universal control based on accounting data.
In the case of the Department of Posts, Telegraphs, and Telegraphs, Fayol (1937) points out many departures from his theoretical prescriptions and recommends appropriate corrections; span of control is a major concern. He recommended plans of action, regular meetings, organization charts, horizontal communication, and time study, among other changes. Yet the results were disappointing. The recommendations and results in the case of the tobacco monopoly were much the same. It could be argued that his theory was specific to his own company, where it was developed.

Taylor describes many applications of his ideas and provides glowing testimonials to their success. These stories often stretched the truth to the breaking point (Hoopes 2003; Wrege and Hodgetts 2000). Some view this tendency to prevaricate negatively, while others see the ends as justifying the means (Hough and White 2001). Nevertheless, there seems little doubt that studying jobs and finding out how best to do them can make a difference, although there is nothing to indicate the value added by Taylor’s management principles and the mental revolution. Job study and industrial engineering certainly have survived and prospered, but the rest of scientific management has not fared as well. Functional foremanship failed largely because it created role conflict (and thus violated unity of command). The division of labor involved appears to have been excessive. On the other hand, the exception principle clearly has contributed to practice.

A quote from Wren and Greenwood provides an idea of how Taylor has come to influence practice at the present time:

Many of his notions provided foundations for current practice. Examples include setting standards for evaluating performance and for product quality, and an “exception principle,” under which managers look for both good and bad exceptions to standards so corrective action, such as quality control, can be taken. What is today called Japanese-style management is an outgrowth of Taylor’s ideas, which were introduced into Japan in the early 1910s. The Japanese like Taylor’s emphasis on mutual interests, cooperation, and harmony. (1998, 138)

A final point involves Fayol’s five elements of management. In some form, these elements have been widely used in training managers. They continue to be applied in the structuring of management textbooks. However, the variations in the lists of functions employed by different authors is huge. There is no firm reliance on Fayol’s elements, and indeed, several of his functions may well be omitted from any given list. The tie to Fayol has become tenuous over time. Furthermore, this structuring is a characteristic of management, not organizational behavior, textbooks. One simply would not construct a book on organizational behavior in this manner, thus adding to the distancing that has occurred between the field and classical management theory.

Nevertheless, if the objective is to understand the nature of managerial work, a very good case can be made that Fayol’s elements, in some version or mix, do provide a useful framework (Carroll and Gillen 1987). Furthermore, experience and skill in these kinds of activities make for greater managerial success. Thus, classical theory is not totally devoid of research support in all areas.

**CONCLUSIONS**

The disciplinary origins of organizational behavior may be traced to roots in philosophy, management, sociology, psychology, political science, social work, law, history, and engi-
neering. Truly, the field came together from a very wide range of disciplinary origins. As we will see in Part III, this has represented sources of both strength and stimulation, and a barrier to integration and unobstructed communication.

Yet there is another sense in which organizational behavior’s origins exhibit an amazing degree of similarity—the frequent geographical ties to the Boston area. Elton Mayo came from Australia and touched down at several outlying stations, but he did his major work at Harvard. Chester Barnard grew up nearby and attended Harvard, and in his scholarly years the ties to Harvard were strong and prolonged. Kurt Lewin came from Germany via Iowa; however, he ended at the Massachusetts Institute of Technology and that is where his contributions to the beginnings of organizational behavior became most apparent.

Mary Follett attended the women’s college of Harvard and spent almost her whole life in the Boston area. Max Weber lived and worked in Germany, yet his theories were introduced to this country primarily through the efforts of Talcott Parsons of Harvard. The imputed antiheroes, Fayol and Taylor, not surprisingly, never lived in the Boston area. Certainly Fayol was far away in France, but Taylor did in fact lecture at Harvard a few months a year for five years.

Whether anything in particular should be made of this geographic concentration would require a control group of eminent early contributors to other disciplines before an answer to the hypothesis could be established. I am not in a position to conduct such a comparison study. Yet, on the face of it, the affinity for the Boston area, and for Harvard, do seem unusual.

In closing Part II, the issue of the relative standing now, among this part’s multidisciplinary contributors to the origins of organizational behavior, is worthy of note. Here, I draw upon certain procedures, treated at much greater length in Part IV, used to evaluate theories of organizational behavior.

Using the importance ratings made by seventy-one organizational behavior influentials, Weber currently emerges as the highest rated, with Lewin not far behind; the views of both appear to have achieved institutional status. Among the formulations discussed in Part II, no other shows evidence of institutionalization. In the next grouping, the ideas of the Hawthorne scholars, of Barnard, and of Taylor appear to be rated similarly. Generally, the thinking of Follett and Fayol is viewed as less important.

My own evaluations of the validity of these views single out Lewin and Weber as the ones most likely to have their views confirmed by research, not surprising in the case of Lewin in that he was the only organizational behavior researcher in the group. Weber’s views, however, have been supported by a substantial body of research conducted since by others. None of the other five individuals have achieved anything like this degree of research support for their formulations. Their ideas would have to be classified as somewhat lacking in estimated validity.

My ratings of usefulness for practice put only Lewin’s applications in the top category. Just below this come the ideas of the Hawthorne people, of Weber, of Fayol, and of Taylor. Only Barnard and Follett appear not to have generated applications that would really qualify as useful. Overall there is somewhat greater concern with practice here than is characteristic in organizational behavior as a whole.

Irrespective of these evaluations, however, all of these individuals exerted considerable influence on organizational behavior in certain of its aspects as the field came into being in the 1950s.
REFERENCES


PART III

ESTABLISHING THE GROUND AGAINST WHICH ORGANIZATIONAL BEHAVIOR BECAME FIGURE

In the area of perception, within the larger context of experimental psychology, Gestalt psychologists have long given special attention to figure-ground relationships (Ellis 1950). Figures tend to vary widely in the way they are perceived, dependent on the nature of the ground against which they are superimposed. In the same manner, information is interpreted relative to the context that surrounds it (Erez 1996).

In this part, I explore the ground that existed for organizational behavior and its theories over the period of approximately twenty-five years extending from the late 1940s to the early 1970s. Much of this ground stems from the nature and characteristics of the business schools of that time. By exploring this ground we will gain a much better understanding of the meaning that attached to various activities within organizational behavior itself (as figure). This is the period during which organizational behavior actually emerged and strengthened its position. Knowing how that happened, and against what context, is important to understanding what the field has become today.

The flow from Part II to Part III is continuous. Part II established the ideas that were available and described the people who created them. The scene was set intellectually for the new discipline to appear. However, some catalyst was needed to make the potential a reality, to move across the threshold to identity, to establish the benchmarks indicating that a new discipline has been formed. Beyond this, however, in the early years, certain sources of support were needed to give the fledging discipline strength and an opportunity to grow. With this infusion from outside, capabilities could develop within, eventuating in a set of theories, a body of knowledge, that grew from the discipline itself, thus creating intellectual and scientific legitimacy for organizational behavior.

All this has been a hard-won victory. Many have contributed to it, and I will note a large number of these people in what follows. Perhaps in the process the treatment will provide a better understanding of why those who helped to create and develop the field react as strongly as they often do in opposition to threats to the field’s integrity. In any event, this emergence and growth of organizational behavior against the context or ground that existed inside and around the business schools is the subject matter of Part III. This is the direct history that has shaped the present (see Barley and Kunda 1992 and Bedeian 1998 for various views on this point).
REFERENCES

The Ford Foundation (1954–1964)
  Conferences and Conference Volumes
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Conclusions

Two foundations played a very active role in what happened in the business schools during the 1950s and 1960s. These are the Ford and Carnegie Foundations, each of which independently surveyed the current state of the schools and issued now widely cited reports. A great deal can be learned about the origins of organizational behavior from a perusal of the processes inherent in this foundation activism.

THE FORD FOUNDATION (1954-1964)

Of the two foundations, Ford was by far the most active. Its program applied to the business schools was initiated in 1954 and extended across a period of more than ten years. In the process, more than $35 million was spent (Porter and McKibbin 1988).

Conferences and Conference Volumes

One of the most visible components of the Ford Foundation’s program was the sponsorship of a variety of conferences, usually operated by a particular business school to which money was appropriated for this purpose. These conferences, and the books resulting, provided high visibility for a brand of scholarship that focused on the application of mathematical and social science skills to organizational problems.
Some examples are:

A conference convened at Cambria Pines, California, in 1961 by the University of California at Los Angeles (UCLA) with a wide range of participants including people from both within and outside business schools. These scholars included psychologists, philosophers, statisticians, psychiatrists, sociologists, political scientists, and economists. The publication was entitled Mathematical Explorations in Behavioral Science (Massarik and Ratoosh 1965).

A conference held at UCLA in 1962 involving management theorists, business school administrators, and management practitioners. The management theorists included a range of social scientists. The conference volume Toward a Unified Theory of Management was edited by Harold Koontz (1964).

A conference sponsored by the University of Pittsburgh in that city in 1962. The participants were psychologists, sociologists, management scholars, political scientists, applied anthropologists, and economists. Small grants were made to members of the conference to set up cross-disciplinary seminars, to reward outstanding graduate student research, and to fund exploratory research on organizational problems. The conference volume was called The Social Science of Organizations (Leavitt 1963). This conference was repeated in 1963 and a second conference volume resulted—Approaches to Organizational Design (Thompson 1966).

In addition several variants on this theme were utilized. The University of Wisconsin was funded to bring visiting scholars to Madison for periods of time to lecture and advise on research. These scholars included a sociologist, an industrial psychologist, a social anthropologist, an anthropologist, a mathematical psychologist, and a social psychologist. The papers they prepared were published under the title Social Science Approaches to Business Behavior (Strother 1962). At the University of Oregon, professors from various areas across the campus were brought together to focus on the conduct of an experimental graduate-level course in administrative organization. Papers were prepared by several political scientists, an education professor, and an accountant; they were published in The Study of Administration (Wengert, Harwood, Marquis, and Goldhammer, 1961). The course involved was taught subsequently on a multidisciplinary basis in the business school for a number of years.

The link between these efforts (and there were a number of others like them) and the objective of promoting a multidisciplinary approach to the study of organizations within the business schools is clearly evident.

Retraining and Research Support

Substantial money was applied to the process of retraining existing business administration faculty members. This took a number of different forms. Faculty members were sent to summer workshops conducted most notably at Carnegie Tech and Chicago. The goal was to teach them how to upgrade their courses and research through the use of advanced analytical techniques and approaches. Fellowships were provided for faculty members to study psychology and sociology, mathematics, and statistics. A year-long program was conducted at Harvard to create quantitative upgrading. Efforts such as the one at Wisconsin to bring in visiting professors were often aimed at retraining existing faculty.

Often, large grants were made to specific universities for purposes of retraining, supporting research, and attracting personnel. This was the case at Carnegie Tech, Chicago, Harvard, Massachusetts Institute of Technology (MIT), and Stanford, among others (Sheehan 1964).
From 1953 to 1961, the Ford Foundation gave $2 million to the Harvard Business School, including $100,000 for F.J. Roethlisberger to prepare human relations practitioners to work in industry (Gillespie 1991). Doctoral research was supported and a program to award prizes for outstanding dissertations was introduced. This approach is described by Thomas Carroll, Ford Foundation vice president, in the foreword to Victor Vroom’s published dissertation:

> The competition is intended to generalize standards of excellence in research on business by graduate students. It should give widespread professional recognition to persons recently awarded doctorates in business whose dissertation research is especially distinguished by its analytical content and strong roots in underlying disciplines. It is also intended to give recognition to a select number of persons outside business schools who in their doctoral dissertations pursued with distinction interests relevant to business. (Vroom 1960, v)

The major role played by the Graduate School of Industrial Administration at Carnegie Tech, which had behavioral scientists and mathematicians on its faculty from its inception in 1949, in retraining and research is described in Simon’s (1991) autobiography.

**The State of the Business Schools**

Why did the Ford Foundation invest its money in these ways? The answer is contained in the report prepared by Gordon and Howell (1959) entitled *Higher Education for Business*. This report appeared well after the program of aid to business schools was initiated, as that program was escalating. Robert Gordon, the senior author, was chairman of the Department of Economics at the University of California at Berkeley. He helped prepare the report while on leave with the Ford Foundation beginning in 1956. James Howell was a faculty member in economics at the Stanford Graduate School of Business.

The business schools are described as being in a state of flux and uncertainty. They are searching for academic respectability. Much of business education is at a low level and narrowly vocational in nature. Dissatisfaction with the quality of what is taught is widespread. Personal career counseling of students is nonexistent, and screening of applicants on personal qualities needed for competent performance in business is lacking. Courses are overly descriptive in nature and far too narrow in content. Standards for mathematical competence are low, and coursework in organization and management is in a state of total confusion. The students attracted are certainly numerous, but on balance quite poor.

Most business school faculties appear to be seriously wanting; there is a creeping intellectual obsolescence. The quality of both teaching and research is not high. Faculty members are ill-prepared for both, and in fact little research in the true meaning of the term is being produced. Gordon and Howell (1959) noted that this is the case even though doctoral degrees in economics outnumber those in business administration among business school faculty by almost 20 percent. Doctorates in other fields are infrequent (psychology is in the 1–3 percent range). Yet only 40 percent of all faculty members hold a doctorate. Doctoral training itself in the business schools tends to be of inadequate quality, and the numbers produced are low, relative to need. Porter and McKibbin (1988) interpret the competence levels of business school faculties overall at that time as deserving a grade of D at best, and perhaps less charitably as a clear F, based on the data reported. This situation is reflected in the very low level of esteem accorded the business schools in the university pecking order.
Recommendations Relevant for Organizational Behavior

What do Gordon and Howell (1959) recommend given this situation? Most relevant for organizational behavior is the push for substantially increased input from the disciplines of psychology, sociology, and cultural anthropology, with an assist from geography, political science, and history. Yet there is a curious ambivalence regarding the role of industrial psychology. Some of the work on the measurement and use of personality factors in the selection of potential managers is discussed in a very critical manner, with only the most lukewarm endorsement overall. In addition, the relevance of industrial psychology research for the resuscitation of the academic field of personnel management is completely ignored.

This ambivalence is also evident with regard to bringing the targeted social scientists onto business school faculties permanently, and in substantial numbers. The authors note that retreading existing business school faculty into half-baked social scientists is unlikely to work. They also say that drawing upon the talents of faculty permanently housed in the social sciences does not seem promising either. Yet they do not commit to having needed courses offered entirely within the business schools. The social scientists need not settle down to teaching business courses but only contribute research findings and enrich business teaching. The current influx of some such faculty members is noted as an experiment to be watched. There is little likelihood that the addition to the total supply of business faculty members from this source will be substantial.

Yet the question is posed: What should the business schools teach in order to develop an understanding of organizational behavior and some modicum of organizational skills? Yes, the term “organizational behavior” is used here. First, the inchoate nature of the field is noted. Second, the four different aspects of the field noted in Chapter 5 should be distinguished:

1. Management analysis—a rational approach to resource allocation within the firm, which draws upon production management beginning with Taylor and extends to operations analysis; it is often highly quantitative.
2. Organization theory—an approach concerned with the scientific study of human behavior in organizations or the internal organizational environment; it draws heavily on the social sciences.
3. Principles of management—an approach derived from management practice, which seeks to describe and distill a set of generalizations called principles; it has its origins in Fayol and his followers.
4. Human relations—an approach that emphasizes the development of rules that can be applied to situations managers are likely to encounter; it is focused on practical considerations and situations.

These four aspects, which later came to be labeled “schools,” frequently overlap in content, but tend to have different sets of proponents. All four should be represented somewhere in the business curriculum. Organization theory and principles of management, in some unspecified mix, should be incorporated in the core curriculum. Perhaps some human relations should be included. The authors appear to have difficulty formulating specific recommendations, given the confusion characterizing the field.
THE CARNEGIE FOUNDATION (1959)

The Carnegie report was prepared independently of the Ford Foundation’s effort, although there was some exchange of information. The principal findings and recommendations were reached separately. The Carnegie approach rested almost exclusively on the report itself, following a precedent established by the similar report prepared on medical education many years before (Flexner 1910). That report proved to be extremely influential in reforming medical education in this country.

The report itself was authored by Frank Pierson, professor of economics at Swarthmore College, a liberal arts college that does not have a business school. Its title is *The Education of American Businessmen: A Study of University-College Programs in Business Administration* (1959). Of the book’s twenty-four chapters, only eleven were prepared by Pierson, but these chapters constitute the body of the report itself. The remaining chapters were prepared by thirteen separate contributors and deal with specific aspects of curriculum or other specialized topics. Of those thirteen contributors, eight had long-term ties to some business school; the remaining five had no such ties.

**The State of the Business Schools**

Pierson (1959) had the following to say on the state of business education. Work in such fields as accounting, business law, marketing, and production is the subject of widespread attack, suggesting no difference from trade schools. Developments in management and human relations are the butt of criticism and even ridicule. Students are considered to be of inferior quality. These criticisms all appear justified. Students are often of low capability, and the better students among them find little challenge in their business courses. Most business schools appear to have cut themselves off from other branches of learning, thus removing any serious intellectual content. Academic work of a scientific nature appears to have been avoided, and admission standards are set at a low level. The subject matter taught is ill-defined and lacks a sharp analytical focus. Interest in developing programs of first-rate academic quality appears to be lacking.

Business school faculties overall are in need of major improvements. Given that doctoral programs in business are often ill-defined, the tendency has been to hire economics doctorates. However, the more promising economics graduates have tended to shy away from business school positions. Opportunities for doctorates outside business and economics in business schools have been very limited. A review of faculty requirements, teaching conditions, and teaching methods gives cause for serious concern. Doctoral dissertations in business often invoke descriptions of business practice and require little use of sophisticated analytical methods. The same is true of business school research generally, but the real problem is the dearth of research of any kind.

**Recommendations Relevant for Organizational Behavior**

The primary recommendation is for substantial upgrading in every respect. In particular there is a need for upgrading in the areas of mathematics, psychology, and sociology. Although at the graduate level this might be accomplished through courses offered within the business schools, undergraduate coursework offered for business students in these areas should be left in the hands of the liberal arts departments involved. This, however, places a burden
on these departments to meet the needs of business students. Social psychology and industrial sociology are mentioned as requiring particular attention. Although coursework in management should be incorporated in the core curriculum, the field is so inchoate at present that offering a major in it does not seem appropriate.

With regard to bringing psychologists and sociologists into the business schools, there is again uncertainty. Finding faculty members who are competent in both an applied business field and the underlying social science discipline will be difficult. One or two such individuals might be added to work in management, business policy, marketing, and personnel management. Anyone added in this manner needs to be prepared to make the study of business a permanent intellectual interest and at the same time to maintain close ties to the basic discipline. Whether this can be accomplished on a wide scale seems doubtful. Retraining of existing business faculty in social psychology and industrial sociology, however, is recommended.

**The View from Carnegie Tech**

Within the Carnegie Foundation report, George Bach (1959), dean of the Graduate School of Industrial Administration at Carnegie Tech, has a chapter on managerial decision making as an organizing concept for the business school curriculum. He recommends a foundation stem (concentration) in administrative process and organizational behavior to go along with stems in economic analysis and quantitative methods. This organizational behavior stem should consider the firm as a going organization and also the behavior of individuals and groups within organizations. The content covered should be descriptive of business firms, but more important, it should deal with analytical concepts and theory. The knowledge needed for this purpose is emerging rapidly.

Such an effort will require a considerable number of teachers with new expertise. Borrowing faculty temporarily from outside the business school will not work, nor will having an outsider teach an occasional course. Retooling existing business school faculty to teach organizational behavior is only a partial solution at best. Such people are unlikely to add new knowledge to the field through research and development, simply because their primary training is not in the social science disciplines. They are almost certain to be rapidly outdistanced by new doctoral students coming from the business schools. The only solution is to import faculty trained in the social science disciplines, people who will become thoroughly engaged in business teaching and research. The business schools badly need this kind of importation.

Harold Leavitt (1960), a psychologist and an import at Carnegie Tech in Bach’s school, strongly endorses this position. He feels that social scientists can learn about business more easily than business faculty can retrain as social scientists. He also feels that business school psychology and sociology are quite different from the versions of these subjects to be found in liberal arts departments. At least from Carnegie Tech, which had at least as much experience with these matters as any of the business schools, the call to bring social scientists onto business faculties permanently was loudly and unambiguously expressed. Leavitt (and others at Carnegie) proved himself to be a good prognosticator; he is still at it, predicting what will happen with regard to business structuring (Leavitt 2003) and in the development of technology (Leavitt 2002).

**Effects of the Foundation Activities**

The points made and the recommendations of the two reports are not identical, but they are similar. The picture painted of the ground for organizational behavior is disturbing, yet in
some respects encouraging, too. Both reports recommend bringing social science subject matter and research skills into the business schools. However, contrary to what is often assumed, neither report came out unequivocally in favor of adding a substantial cohort of social scientists directly into existing faculties. A little reading between the lines suggests that the reports’ authors may well have been concerned that any large-scale importation of this kind might create major controversy and conflict in a situation that was already characterized by considerable flux. Thus, other changes might be jeopardized. It remained for Carnegie Tech, a greenfield site that had already incorporated social scientists from the beginning, to speak out strongly on this issue.

A second point, not always widely recognized, is that economists were already well established in the business schools in the mid-1950s. In fact, this was a major problem for the authors of the reports. Even with the extensive participation of economists, the schools had performed poorly and were held in low regard by the rest of the university community. Many business school deans and administrators were economists by training. A major incentive for them to carry out the reports’ recommendations was that by doing so the stature of the whole business area might be raised. It is safe to assume that opinion leaders from within the economics discipline served in a very important way to bring the field of organizational behavior into existence in this early period.

Organizational behavior at the time of the reports was in a state of chaos. It hardly had a name that members of the field would recognize, let alone a domain and a mandate. We will return to this matter of uncertainty later (Chapter 15).

A recent chronicling of this period, and of the effects of the foundations, provides a check on my interpretation of these events in that it brings some different citations to the analysis and develops actual data from a reading of Academy of Management Journal articles in the period 1958 to 1978 (Goodrick 2002). With few exceptions, this presentation comes to much the same conclusions that I have. However, the analysis of published articles, which was concerned with the increase in the number of science-based articles over the period studied and thus the institutionalization of science in the field, does present some difficulties.

The hypotheses of this study were as follows:

1a. High prestige departments are more likely than others to be early adopters of the new paradigm (the move to science).
1b. Prestige of department does not affect adoption of the new paradigm in the later years.
2a. Ph.D.’s trained in disciplines other than management are more likely to be early adopters of the new paradigm than those trained in management.
2b. Disciplinary training does not affect the adoption of the new paradigm in later years.

(Hypotheses 1a and 1b were confirmed—"it was high prestige schools that blazed the trail to science-based conceptions of management education. . . . The results also indicated that other schools then followed suit, leading to a total transformation of the field" (Goodrick 2002, 664). In fact, by the end of the study period, the Academy of Management Journal had changed its policies so that it accepted only science-based empirical research (Miner 1997).

With regard to hypotheses 2a and 2b Goodrick had the following to say:
Contrary to other accounts of this transition (e.g., Miles 1996), the results do not seem to indicate that bringing scholars from other disciplines into management played a major role in the diffusion of the model linking management education to a scientific base. (Goodrick 2002, 664)

This is directly at variance with the position I and others have taken. However, Goodrick does have some afterthoughts on this score; she feels that her findings may reflect the nature of her data source and of her coding system—“Scholars trained in other disciplines but working in business schools may have been more likely to publish in outlets more closely related to their graduate training (I believe this is the case). Also, I may have inadvertently captured other scholars such as sociologists who self-identify as organizational scholars in the management discipline measure” (Goodrick 2002, 664). There is also some question as to how economists were handled. All in all it appears that as Goodrick says there was some “compromising (of) the test of the disciplinary training hypothesis.” This hypothesis needs to be studied using a more appropriate design.

THE AMERICAN ASSEMBLY OF COLLEGIATE SCHOOLS OF BUSINESS (1988)

The American Assembly of Collegiate Schools of Business (AACSB) is a professional body created to represent and accredit business schools. As it has moved recently to accredit business schools internationally, the AACSB has changed its name to The Association to Advance Collegiate Schools of Business (Thompson 2004). Its active participants are the deans of these schools. In the early 1980s, in the context of its planning activities, this body began to grope with various issues related to the future of business education (American Assembly of Collegiate Schools of Business and European Foundation for Management Development 1982). As this effort evolved, a decision emerged to redo the kind of survey initially carried out for the Ford and Carnegie Foundations. The idea was to undertake a twenty-five-year follow-up. This study extended over a three-year period, and a report was ultimately published (Porter and McKibbin 1988).

The study makes possible a good picture of the state of the business schools in the mid-1980s; much survey data were collected through interviews and questionnaires intended to pinpoint the views of a wide range of stakeholders. Comparing these results with those obtained in the 1950s provides a view of the aftermath of the earlier foundation activities. Several aspects of this comparison need to be kept in mind, however. First, this report is internally generated from the business schools, not externally generated about them. Second, financial support for the study was solicited and obtained from some thirty large corporations. As a result of this sponsorship arrangement, the likelihood of the report’s being highly critical of the business schools is diminished, and the ties between these schools and corporate America (not small firms) are accentuated.

Lyman Porter is a psychologist, with a doctorate in that subject from Yale, who came into the business schools on the wave of the foundation activities in the mid-1960s. He had prepared a background report that was utilized in connection with the Gordon and Howell (1959) volume. His entire professional career has been spent at the University of California—first in the psychology department at Berkeley, and then in administration (including a stint as dean) in the School of Management at Irvine. Lawrence McKibbin has degrees in business from Stanford and has had substantial business experience in the financial area. He
helped develop business schools in various countries, served as dean at Wichita State University and the University of Oklahoma, and was president of the AACSB as the follow-up study got under way.

The State of the Business Schools Twenty-five Years Later

The business schools grew— the number of masters degrees awarded went from 4,500 to 60,000, for example. The typical curriculum has become far more academically solid, and thus more respectable. The aptitudes of business students, especially those at the MBA and doctoral levels, have risen dramatically. This improvement is recognized in the university community and in the corporate world. The quality of faculty is at least the equal of what one finds elsewhere on campus, and in some quarters it is said to be higher. The previous derision has disappeared, and the intellectual climate within the schools is much better. Respectability has been attained not only on campus but also in the business community. As a result, relations with business and industry are much improved. Pay levels for business school faculty are well above those in many other segments of the university, something that was not the case in the 1950s.

Porter and McKibbin's (1988, 187) comments on current perceptions of intercampus relations stand in sharp contrast, in a number of respects, to what existed in the 1950s:

- Business schools feel rather good about themselves; deans perceive their schools to have reasonably high status on campus.
- For many academic institutions, there is a constant battle for resources; the typical business school regards itself as overburdened— too many students— and underfunded.
- While there are exceptions, the typical school sees itself as pretty much a “stand alone” operation; there is little interaction— or perceived need for interaction— with other academic units on campus.
- Provosts think highly of their business schools; they believe business faculties are academically competent; but they decry the isolationism of the business school; they do not see campus-wide academic leadership coming from the business school.

Although Porter and McKibbin do not discuss this, values seem to play an important role in the relative isolation of business school faculties. These faculties tend to be more conservative, especially with regard to the role of business in society, than the dominant liberal arts faculties. Surrounded by a predominantly liberal sea, most business faculties have chosen isolationism as the best way of coping with the potential for conflict involved, especially in the aftermath of the activism of the late 1960s and 1970s (Levine 1980; Miner 1974).

The State of Organizational Behavior

As Porter and McKibbin (1988) note, the business schools have hired doctorates with training in disciplines other than the functional areas of business in relatively large numbers. Included are individuals from the social sciences, mathematics, and statistics. The result has been increased status in these areas on campus.

The original push from the foundations was to increase the emphasis on social science and quantitative knowledge. Thus, looking at these two together is appropriate. Table 12.1
provides useful data. Let us focus on the first two groups (business deans and faculty) initially. Here the most pronounced trend is for the faculty to advocate a more quantitative emphasis. In both groups organizational behavior is considered worthy of more emphasis, but only by a small margin.

These results are reinforced by data on the specific subject areas that are thought to need more emphasis. There are four:

- business communications,
- entrepreneurship,
- international business and management, and
- management information systems.

Organizational behavior is not on this list—either for deans or faculty. Furthermore, the faculty listed organizational behavior as one of the areas that should be deemphasized, albeit with only a small number expressing this opinion.

If we now look at the rest of Table 12.1, and contrast the results with the relatively lukewarm support for organizational behavior within the business schools, the disparities are striking. Stakeholders of all descriptions out in the business world want more organizational behavior in the curriculum, and in most cases want less quantitative material. At least from this perspective, the consensus is that the gains that were achieved as organizational behavior became a discipline and established itself in the business schools are not enough; much more is needed.

Table 12.1
Views Concerning the Appropriateness of Current Emphasis in the Business Schools Curriculum on Quantitative and Organizational Behavior Subject Matter

<table>
<thead>
<tr>
<th></th>
<th>Too much (%)</th>
<th>About right (%)</th>
<th>Too little (%)</th>
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<td>Business school deans</td>
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<tr>
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<td>Corporate chief executive officers</td>
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<tr>
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Undernourished Emphases

Porter and McKibbin (1988, 316–25) discuss six areas that they conclude need more emphasis, and in the process, reflect some of the concerns expressed in Table 12.1 from the corporate direction. They say, “The incorporation of concepts from mathematics and the social/behavioral sciences along with an emphasis on the analytical and rigorous, as opposed to the descriptive and superficial, has demonstrably improved the quality of business school curricula significantly over the span of the last two and a half decades” (316). However, in the “more is needed” category they note the following concerns:

1. Breadth—business students are more narrowly educated than they should be.
2. The external organizational environment—there needs to be an increase in attention to the external environment—governmental relations, societal trends, legal climate, international developments.
3. The international dimension—business schools have a long way to go before it can be said that the international dimension has truly become an integral part of their programs.
4. The information/service society—how can an information orientation be effectively incorporated into business courses and research?
5. Cross-functional integration—there is a need to provide sufficient attention to an integrated approach to problems that cut across specific functional areas, problems like those found in entrepreneurship and strategic management.
6. “Soft” (people) skills—current trends in business have the effect of increasing the need for effective skills in dealing with other people, and with oneself.

Obviously, the last of these points—the need for more people skills—has the most relevance for organizational behavior, but all six problem areas offer opportunities for the field. Interestingly, just as in the 1950s, if these opportunities are to be fully realized and major changes invoked, this appears most likely to happen as a result of pressures brought to bear from the outside—in this case from the business community, not as formerly, from the major foundations.

The Influence of the AACSB Report

One reaction to the Porter and McKibbin study is that it is well-done and thorough, but that its impact on business school activities is likely to be minimal for internal political reasons (Cummings 1990a, 1990b). This view is consistent with the findings of a generally status quo position on campus insofar as any further growth of organizational behavior’s contribution and influence is concerned (see Table 12.1). This argument says that the behavioral revolution has occurred, it is over, and little further change is likely. Such evidence as there is tends to support this position (Shipper 1999). For instance, there has been little if any improvement in the managerial (people) skills possessed by MBA graduates since the AACSB report was published. This is despite the demonstration that skills of this kind are valid predictors of managerial performance levels and that they are teachable.

Another view is more negative regarding the Porter and McKibbin effort itself, portraying it as essentially a “power grab” by the AACSB (Calás and Smircich 1990). This interpretation reflects the anti-authority attitudes that have been identified with postmodernism, and
establIsHIng the ground

Endorses much more radical changes than those espoused by the authors of the report. It could be written off as yet another attack on business school deans in their positions of authority, or it could reflect the realities of the exercise of power (see Bedeian 2002). Yet this view does strike a nerve. Should we not have an evaluation of the consequences of the 1950s activism either by the same foundations involved originally or by representatives of our consumers in the business world? This is not what Calás and Smircich (1990) want, but their arguments certainly suggest it. In any event, I strongly suspect that if there is to be further expansion of the role of organizational behavior in the business schools, building on the base of substantial success achieved to date, it will come as a result of further augmented voice from our stakeholders in the business world.

One such voice has emerged, and is in fact becoming accentuated. Although to date this voice has not had specific relevance for organizational behavior, it does have the potential for such application, and it certainly has relevance for the business schools as a whole. This is the evaluation and rating process now joined by Business Week, U.S. News & World Report, the Financial Times, and Forbes. These published evaluations have raised their share of defensiveness (see Elsbach and Kramer 1996). They have also produced some “inflationary” statistics and resulted in several deans losing their jobs. But in the long run such reports could focus on the deficiencies and strengths of various schools, including those in the organizational behavior area. This would serve a very useful purpose.

CONCLUSIONS

The relatively rosy picture of the period since organizational behavior was born can almost certainly be attributed in large part to the impact of the Ford and Carnegie reports and the foundation activism that surrounded them. This is not to say, however, that the business schools as currently constituted and organizational behavior are not without their critics. For a review of some criticisms and a proposal for major change the reader may wish to look at Ackoff (1994). Such continuing skepticism is in fact healthy if it does not expand out of proportion and seeks solutions, not anarchy. The business schools had a very long way to go back in 1950 and they have come a very long way since, largely as a result of the advent of the field of organizational behavior. The AACSB report documents this, but it also points up the need, and potential, for further change.

The behavioral revolution was in many respects an organizational behavior phenomenon. It was fomented to a substantial degree by the Ford and Carnegie Foundations, but behind that was the influence of a number of economists who not only wrote the foundation reports but also implemented their recommendations, and may well have been responsible for getting the foundations involved in the first place. The business schools were in serious trouble on a number of counts, and they were in danger of being overwhelmed by a burgeoning student population, but at the same time, their academic status on campus had sunk to a new low and many economists were caught up in this state of affairs. Economists in business schools and in liberal arts departments sparked the rescue attempt. Neither mainstream psychology nor sociology had any inclination to do this, and the business school disciplines alone lacked the clout to generate such a change. Yet, as the revolution escalated, many individuals and groups joined the bandwagon. It became a real success.

In many respects this success resulted from the efforts of certain opinion leaders, many of them theorists, who came to populate the business schools in the early period. Next, we take a look more closely at these people and how they came to the business schools.
REFERENCES


Shipper, Frank (1999). A Comparison of Managerial Skills of Middle Managers with MBAs, with Masters’ and Undergraduate Degrees Ten Years after the Porter and McKibbin Report. Journal of Managerial Psychology, 14, 150–63.
CHAPTER 13

THE MANAGEMENT LAUREATES

Career Transitions
  Business School Entry
  Disciplinary Sources
Perceptions of Business Schools
  Positive Themes
  Negative Themes
Promotion and Tenure
Sociometric Network Analysis of the Laureates
Consequential Experiences in the Lives of Laureates
  The Initially Published Version
  The Originally Unpublished Paper
Conclusions

Starting in 1992 Arthur Bedeian began to edit a series of volumes of autobiography entitled Management Laureates (Bedeian 1992, 1993a, 1993b, 1996, 1998, 2002). At this writing, there are six such volumes containing fifty-seven autobiographies of outstanding contributors to the management field as a whole. My understanding is that, at least for now, no further publications in the series are anticipated. Bedeian does not say how these laureates were selected, but it can be assumed that although the decision was the editor’s, he was heavily influenced by his knowledge of management history—a field in which he has specialized for many years (see Bedeian 2004a). In any event, there can be no doubt that these people are influentials. There certainly are others who are similarly influential, and indeed a few such people were nominated for inclusion in the volumes who did not contribute autobiographies. Three of the latter were included in our analyses.

The particular value of these autobiographies for current purposes is that they provide insight into the careers of people who were influenced by the activism of the foundations in the 1950s and 1960s. What happened to these individuals, who became influentials or opinion leaders, during the period of change? And in particular, what happened to those who were, or have become, identified with the field of organizational behavior (OB)?

CAREER TRANSITIONS

Table 13.1 contains the results of an analysis of these autobiographical essays, and, in the case of the three who declined the nomination, of other biographical source materials. Note, first, that almost three-quarters of the laureates have come to identify themselves, and be identified, with organizational behavior. This is a very impressive showing for organizational behavior in a period when at its beginning the field did not even exist. The next most frequent disciplinary identification is strategic management, with 13 percent. Furthermore, of the organizational behavior scholars, a majority have made major theoretical contribu-
tions to the field. Forty-three percent of the management laureates are organizational behavior theorists of note and 59 percent of the laureates in organizational behavior are major theorists. At the very least, it can be said that developing organizational behavior theory is not a career route detrimental to success. In fact, theory building seems to be a primary means to recognition. Major theoretic contributions are defined by virtue of selection for inclusion in Miner (2002).

**Business School Entry**

The organizational behavior laureates entered a business school, if they did so, in the period extending back from 1977. Up through the 1950s the rate of entry was relatively slow, and the only source of multiple entries was Carnegie Tech. The real surge came during the 1960s, when the foundations appear to have had their greatest impact. The hesitancy and pessimism the foundations exhibited regarding direct entry into the business schools did not carry the day; the more frontal approach advocated from Carnegie Tech prevailed. The apparent fall-off in the mid to late 1970s appears to be entirely a consequence of the shorter time span

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### Table 13.1


<table>
<thead>
<tr>
<th>Category</th>
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<td>9</td>
</tr>
<tr>
<td>Other (journalism, engineering, math)</td>
<td>3</td>
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</table>

involved and the need for a number of years to achieve laureate status. The last autobiographer first entered a business school in 1977. The three who never moved to a business school include a successful entrepreneur who went from a psychology department to the business world and two sociologists who, despite close brushes with business schools, never actually made the move.

Although the modal pattern is one of entry from employment in a liberal arts department, there are several instances of movement from the business world as well. Among those who made the move during graduate studies and prior to obtaining the doctorate (and thus who held business terminal degrees) there is a heavy concentration in Volumes IV, V, and VI, the more recent of the laureates volumes. The pattern of direct employment in a business school subsequent to a social science doctorate is relatively infrequent. Apparently, in the generation involved, moving to a business school occurred, most typically, after building up some employment experience elsewhere or the individual entered a business school early to obtain a degree. The latter approach has accelerated in the more recent period. It appears to be the method of choice today.

**Disciplinary Sources**

As Table 13.1 shows, a large proportion of the transitions occurred among psychologists; this is the discipline that fed into the business schools the most. The largest number of these people have a background in industrial psychology, achieved through either initial training or career employment. Given the tendency of the foundation reports to ignore this particular group, or even to criticize its work, this result is in certain respects surprising. Not only do a substantial number of people come from this source, but they achieve laureate status as well. Yet one might also expect this, in view of the positive disposition toward business, something industrial psychologists share with many economists.

The only other discipline with any numbers is management. Included here are many who received traditional management degrees, which included very little input from either the social sciences or statistics. These people have become successful in the field of organizational behavior in large part because they were able to reteach themselves subsequent to receiving their terminal degree. They noted where the field was going and then took the matter of getting there into their own hands.

The data from the laureates volumes suggest a great deal of diversity in this group of people who helped lead organizational behavior to its present position. Obviously, the group is not representative of the larger number who decided to try their hand at becoming business professors. Yet the findings from this analysis are informative simply because such a large number of opinion leaders are involved (Katz and Lazarsfeld 1964).

**PERCEPTIONS OF BUSINESS SCHOOLS**

The essays contain a certain amount of commentary regarding the internal environments of business schools. This commentary is uneven in that some of the autobiographies contain much more of it than others. Accordingly, any quantitative analysis seems unwarranted, but the qualitative information does provide a useful addition to our picture of the ground that existed and against which organizational behavior as it emerged became figure.

Remember, however, that we are talking about a very successful group of people. What happened to them may be far from typical. There is also a problem in that the data offer no
method of discriminating what is unique to the emerging organizational behavior field from “academe in general.” We have no control group. In a few instances the laureates provide information linking their experiences to the rise of organizational behavior, but this is the exception, not the rule. More frequently, readers will have to rely on their own sources to fill in the blanks.

**Positive Themes**

One of the most pervasive themes is the emphasis on the value of being able to follow problems wherever they might lead. The business schools are viewed as providing greater freedom in this regard. In contrast, liberal arts departments often create artificial, discipline-based boundaries on intellectual endeavor. I remember, while teaching in a psychology department on leave from my regular business school position, being strongly pressed not to use a book by Amitai Etzioni, a sociologist, in one of my classes, even though it was clearly the best suited to my purpose and had served well in my business school teaching. The following quotes may help to give a better feel for how this problem orientation operated:

> It is here where I believe that professional schools provide a more user-friendly environment. As I look at professional schools, they are developing their own identity. They are increasingly unconflicted about the importance of scholarship that is problem-centered. They are increasingly proud of teaching about a world of practice. They are increasingly providing the conditions where the best and the brightest who seek to be problem-centered can flourish. (Argyris 1992, p. 54).

> My big career shift occurred in 1962 when I moved from the Department of Psychology at Berkeley where I was visiting into the Graduate School of Management at the University of Pittsburgh. I felt I shared more interests in common with problem-oriented management faculties than with the discipline-oriented psychology faculties. (Bass 1992, p. 76)

> I've kept looking for important problems where research might contribute to a solution as well as contribute to theory. . . . I’ve been lucky to have been based at the Harvard Business School (HBS) all these years. There’s a strong problem-solving orientation at HBS and I’ve been given support and encouragement all along the way. (Lawrence and Lawrence 1993, p. 113)

This problem focus carried with it in many instances an opportunity to deal with practical issues and the applications of organizational behavior. At the same time, it seemed to be part of a broader freedom to teach and research whatever seemed to be appropriate. This freedom, although not universal, was widespread; many laureates experienced it in one form or another:

> I asked McGregor (Bavelas was away) for guidance. Doug said quite bluntly that it did not make any difference, that what was needed was my approach, and that I should figure out for myself what and how to teach. This message correlated with what Dean Edward Pennell Brooks had told me about the Sloan School. He didn’t care what we did so long as it was a new approach, and so long as it was clearly different from the “school up the river.” Innovation was the watchword of the day. . . . I fell back on basic psychology and was quite academic in my approach (Schein 1993, 42). . . . The Sloan School’s message on research was very similar to the message on teaching— “You figure it out.” (ibid., p. 43)
Had I been offered a position in a psychology department my research would not look at all like it does today. I like to take a much broader brush to a problem than is characteristic of most research done in psychology departments. For that I am grateful to my business school setting. (Roberts 1998, p. 219)

Reflecting on my administrative experiences, I have looked at several dean’s positions. However, these positions required that we move from Dallas. . . . The demands of deanship also conflict with my desire for independence and opportunity to participate in executive development programs throughout the world. These considerations led to a decision to remain at SMU in a faculty, as opposed to administrative position. (Slocum 1996, p. 292)

Closely related to this greater freedom is the exposure to new approaches that a number of laureates reported. For example:

The Ford Foundation, through its prestige and resources, successfully encouraged a rather radical change in the educational practices of American business schools during the 1960s. The Ford Foundation encouraged many schools to recruit to their faculties social scientists, mathematicians and statisticians . . . Thus the attainment of my Ph.D. in 1960 was a photo finish with my obsolescence. . . . In the mid-1960s, the editorial review boards of the journals reflected the new wave of rigor that was becoming pervasive in U.S. business schools. . . . I either had to retrain or resign myself to being a second-class academic from the outset. . . . I chose to retrain. (House 1993, pp. 51–52)

The organizational behavior group at Yale in many respects was very cohesive. We met regularly to talk about research and debate issues. For all of us it was a stimulating, growth producing experience. It opened my eyes to a set of organizational issues that I had not been exposed to at Berkeley. (Lawler 1993, p. 86)

Chris Argyris became the prime agent of my unstuckness, beginning with his suggestion that I sit in for him at Yale for a year while he was on leave. A dazzling set of developments—practical and conceptual—followed a sudden distancing from the turmoil at Illinois. In close cadence, I was exposed to the “laboratory approach,” my first T-Group, to the people who were doing most with “action research,” to the NTL clients who provided numerous opportunities to put new concepts into place, and especially to rich opportunities to gain new skills and orientations. (Golembiewski 1992, p. 389)

Inherent in the intellectual freedom and openness to new ideas was an overall climate of intellectual stimulation. This seemed to be part of the culture in many schools, permeating faculties and doctoral students alike. The increased concern with practice and application on occasion was a force in creating this intellectual stimulation also. There are a number of comments around this theme:

This is as good a place as any to talk about the remarkable string of doctoral students that I have been privileged to be associated with as a major advisor during my years in GSA (GSM) at Irvine. . . . [M]y energy and efforts at UC-Irvine has been invested— with abundance of pleasurable returns— in working with doctoral students. (Porter 1993, pp. 18–20)

I learned what was going on in leading business schools and the interests of their faculties. It was a stimulating year! Influences upon my research efforts will be noted later. . . . At Harvard I became familiar through my fellow students with the curricula and research programs of leading business schools and gained respect for them. (M ahoney 1998, pp. 144–45)
I gave my best to Auburn University and received incomparably more from the rich friendships and intellectual stimulation than I could give in return. . . . [W]hatever career success that I may have enjoyed was largely determined by being able to spend the first years of my career in such fine company. The environment within the Management Department was one of nourishment, excitement, and collegiality. Above all, no one was afraid of excellence and achievement in others. (Bedeian 1998, p. 17)

[T]he SIU business school was a good place to work and provided good travel and other support, and the teaching load was relatively light. I look back on my years at SIU with pleasure. Much of the time I was there we had what is now called a “hot group” . . . totally consumed with a steady outpouring of conference papers and proceedings, journal articles and special issues, book chapters, and books . . . a high level of professional activity . . . ties with international scholars. (Hunt 2002, pp. 174, 179)

The autobiographies contained many references to the supportive environments found in the business schools. Some of these references were specific and personal, some more general:

My chairman was John F. Mee, and he was a truly remarkable, supportive person. He undoubtedly helped me more than any other person in my career. He gave me a very good start with supervision of doctoral dissertations, guided me through my first major consulting job, and supported me with the faculty. He gave me my start in the Academy of Management and helped me arrange to coauthor my first book. (Davis 1992, pp. 275–76)

My acquaintances who took jobs at MIT and Stanford never fulfilled the promise they had shown as students, whereas almost all my colleagues at Purdue went on to exceptional success. I attribute this outcome to our supportive environment. Purdue’s deans rewarded professors who did research with kind words and summer support; and they let us create a separate department of behavioral scientists. (Starbuck 1993, p. 77)

The Aston Group came as near to the ideal of a community of professional practice as I have ever experienced in my whole career. Derek [Pugh] and David [Hickson] have had a hugely positive influence on the formation of everyone who has worked with them. . . . [P]eople who worked on the programme or studied under its members have a deep sense of identity that stems from both the professionalism and personal support that was its hallmark. (Child 2002, p. 24)

Perhaps other positive themes could be teased from the autobiographies, but the preceding are clearly the most important. There is, however, one other theme that is not so widely mentioned, but may be of more significance than the written statements imply. The laureates are successful people in the field. You would think that being rewarded for their performance—in terms of money and status—would be important to them. Little is said on this score, but what is said is worth noting:

I rose quickly through the ranks in the Academy of Management, serving in various capacities, including chairman of the Organizational Behavior Division, editor of the Academy of Management Journal, and eventually President. Wisconsin provided an ideal environment for that type of professional development. I was affirmed and rewarded for those kinds of contributions. (Cummings 1992, p. 245)

I was happy with my colleagues at Berkeley. . . . By the late 1970s we had quite a bit of
power and prestige in the school and, compared to many other organizations groups, had good security and status. (Pfeffer 1996, pp. 214–15).

One of the reasons I have remained at Maryland was because I was treated very well by business school Dean Rudy Lamone. Rudy was strongly merit-oriented and always rewarded people for their performance, keeping political issues to a minimum. (Locke 1993, p. 228)

Negative Themes

These generally positive features are counterbalanced by a certain amount of discipline-based conflict. Those who had occupied the territory first were not always willing to give up the ground they already held. In some respects, organizational behavior served to expand the size of the total domain, but this was not always true. Conflicts over ownership of courses, budgets, positions, respectability of academic endeavors, and the like are reported. Most frequently these controversies involved professors with a principles of management persuasion or economists, simply because these were the largest groups likely to have overlapping turfs with organizational behavior. To a certain extent, the fears of the foundation report authors appear to have been realized, even at Carnegie Tech, which started out so well.

Where conflict did occur, and this was far from universal, it often simmered below the threshold, influencing events but not becoming overt. When the battle really heated up, however, the usual outcome was that organizational behavior lost, at least temporarily. Sometimes this meant moving to another department within the business school, or outside. Sometimes the move was temporary, sometimes permanent. Not infrequently, the leader of the organizational behavior component left the university involved. Yet the meaning of this is hard to evaluate. These people were much in demand, opportunities were all around them, and making a move usually resulted in a substantial compensation increase. There certainly was some push from within in these cases, but once that happened the pull from without often came to dominate.

As we turn to the perceptions of various laureates regarding this conflict, it is important to recognize that a business school that is conducive to the development of organizational behavior at one point in time may be anything but at another point. Swings of major amplitude can occur almost overnight; a prominent factor is some change in the administrative component of the school. With this in mind, let us look at what the autobiographers say:

The promotion process . . . did not proceed smoothly. . . . [There] was opposition to my promotion by a group of economists who felt my work was too based on the study of attitudinal data. In their research paradigm attitude data were soft data, and did not warrant scientific research. . . . [M[y promotion was put off. (Lawler 1993, p. 88)

[Note: Lawler left Yale shortly afterward.]

I joined a business school in which organizational behavior came to be treated as heresy. I only really woke up to the importance of tenure at the University of Rochester after I found myself in a management school in 1972 in which the dominant powers reached the judgment that organizational behavior was without validity or relevance for management. Life was a matter of trade-offs. The economic dynamics of the free market outside the firm could be used in the same way to fully explain the competitive dynamics inside the firm. (Bass 1991, p. 85)

[Note: Bass left the University of Rochester shortly afterward.]
Conflict . . . developed between a colleague and myself at Oregon. . . . [T]he colleague was made head of the newly formed Personnel and Industrial Management Department. . . . He seemed to view the department as the bottom link of a hierarchic chain of command; he was in charge. My view was more like a community of scholars with a senior scholar representing the group. . . . Things got so bad at one point that my courses, and myself with them, were all transferred to the Accounting and Business Statistics Department. . . . That move was countermanded by a dean returning from sabbatical. (Miner 1993, p. 300)

[Note: Miner left the University of Oregon shortly afterward.]

Too often we take for granted the institutional settings in which scholarship takes place. Nurturing of collegiality is a function which is either overlooked or relegated to deans or department chairs. My experience at both Yale and Carnegie Mellon taught me the fragility of these settings—of how quickly a tradition of collaboration could erode or be destroyed altogether. (Vroom 1993, p. 277)

Such conflict was not limited to interdisciplinary confrontations. There were schisms occasioned by other factors as well. The following quotes provide examples:

By the mid-1970s OB was split. On the one hand were the scientists, on the other the humanists . . . who carried the normative optimism of the early 1960s to the extreme. For them, there was a very simple one best way: it involved sensitivity training, encounter groups, OD and the like. . . . The differences were dramatically illustrated in a meeting which David Bradford, Hal Leavitt, Ray Miles, and I (George Strauss) organized in Berkeley around 1973 of a group that later became the Organizational Behavior Teaching Society. At one point we divided into subgroups to discuss what OB should teach. One group, with Bob Tannenbaum as its guru, sat on the floor. The other, which included Larry Cummings as spokesperson, sat around a table. The reports of the groups were predictable. (Strauss 1993, p. 176)

Georgia Southern College was a nightmare. The culture within the School of Business Administration was rife with backbiting and politics. Much of this emanated from the management style of the School’s dean. . . . [T]he dean’s antics had resulted in the formation of two principal coalitions whose members literally would not speak to one another. . . . To make matters worse, my success in publishing was a threat to all sides. By “speeding up the race” I had inadvertently discredited the self-serving belief that by being at a small school with limited resources and a heavy teaching load, one couldn’t be expected to do much more than meet classes. (Bedeian 1998, p. 14)

The Milwaukee chair provided me a good salary, a light teaching load, and discretion about what I taught. At age 40, I thought I had the kind of position professors dream of. What I had not seen were the resentments of less lucky professors and an ideological split in the business faculty. . . . [T]wo professors told me I had disappointed them by not leading a revolt against the dean. They said they had voted to hire me because they had expected me to lead a revolt. (Starbuck 1993, pp. 93–94)

Problems with deans were in fact rather frequent:

The new Dean’s style of management was quite different from [the] previous Dean’s . . . his first year at Texas A & M was quite difficult. In fact the next two years of my term as Depart-
The Management Laureates

ment Head under his Deanship were exceptionally difficult and challenging. . . . I experienced many frustrations. The major problem I encountered was the new Dean's desire to have a much more centralized decision and control system. (Hitt 2002, p. 135)

After a tiring journey home, I found myself on a Sunday evening wanting to be with my family, needing to grade a stack of papers, but being forced to talk with the Dean's wife about an issue I didn't consider a high priority (having a working paper series in the college). That was the last straw and I wrote my letter of resignation (as associate dean) the next morning. (Mowday 2002, p. 256)

The matter of discrimination was also among the negative themes. By discrimination I do not necessarily mean the legal type; that needs to be defined by the courts, and none of the instances reported involved legal action. However, feelings and perceptions of discrimination were present. Most instances involved gender issues, and the examples concentrate on these; age discrimination also appears to have been present on occasion.

At Michigan State . . . during my doctoral program the overall situation was somewhat difficult because, initially, I was somewhat ignored by the doctoral students. . . . I am sure that I sometimes got upset when I shouldn't have and let things go by that I should have confronted. . . . Many of the problems seemed to stem from people's prior expectations. . . . During most of my doctoral program, I was unaware of how rare females in management doctoral programs really were. Thus, I did not initially fully recognize how far on the cutting edge of change the Michigan State University Management Department had moved in accepting me. (Bartol 1996, pp. 14–16)

I am the lowest paid full professor in the OB area at Berkeley, and by quite a substantial amount. . . . I make eighty-one cents for every dollar the man who was promoted to the same rank at the same time I was makes. And this is before consulting, summer monies, endowed chairs, etc., which have all advantaged the men in our department, and others across the country. (Roberts 1998, pp. 229–30)

There are other instances of low pay reported in the autobiographies, although that has not been the norm in the group. Typically this type of situation is corrected by a move to a higher-paying position at another university, an option not readily available to Roberts. A more frequent problem appears to have been the heavy teaching loads present in some situations, as well as other teaching-related difficulties:

The teaching norm was survival. Teaching two business policy and one principles of management course six days a week was a hardship. Classes were large and student/faculty interaction was minimal. (Slocum 1996, p. 285)

On arrival, I was immediately thrown in at the deep end into a teaching schedule which required me to teach up to twenty hours a week, including two evenings and Saturday mornings. (Pugh 1996, p. 244)

Increasingly, however, toward my latter years at Northwestern . . . I was spending an increasing amount of time on what I considered to be a performance circus, entertaining students in the classroom in pursuit of teaching evaluations. . . . Many of those MBAs were very intolerant of in-depth thinking, or theoretically developing ideas. . . . I had begun to be really frustrated with this. (Cummings 1992, pp. 247–48)

I found efforts to teach faculty in the [Harvard] Business School psychoanalytic theory
not particularly successful. . . . I was disappointed in the absence of critical intellectual discussion in the Business School. . . . Tenured professors saw no need to learn more, and their juniors depended on them for their promotions. (Levinson 1993, p. 195)

Several laureates note that the Harvard Business School’s unswerving commitment to case collecting and case teaching, although strongly practice oriented, is somewhat intellectually constraining.

The only other negative theme of any significance involved intellectual isolation and a lack of colleagues with whom to work. This problem was most typical in small business schools and small programs but it occurred in larger schools as well. Some examples follow:

My early days at Carnegie were filled with mixed emotions. I found myself surrounded by colleagues in economics, operations research, marketing, and accounting—each of whom had a different language and a set of scholarly pursuits that I found difficult to encompass into my psychological compartments. For the first few months, I kept my office door closed and restricted my conversations with my colleagues to the exploits of the Steelers, the Pirates, or the latest office gossip. (Vroom 1993, pp. 266–67)

My first exposure to the Cornell culture was Urie Bronfenbrenner’s somewhat blunt version of what life there was like. He said in essence, you wouldn’t be here unless you were really good; now all you have to do is more of what got you here and let the rest of us do what got us here. . . . that sounded a lot more anomic than what I had been used to. . . . The place didn’t prove to be quite that harsh. But the amount of interaction with colleagues did go down somewhat, partly because there simply were fewer of them. (Weick 1993, p. 303)

I was growing increasingly restive in Buffalo. I felt out of touch with the main currents in the field, especially the macro side of the field. My department was increasingly micro in focus and the Dean told me flatly that he saw no possibility of hiring anyone else in the macro side of the field. (Beyer 1996, p. 71)

At UCLA I was professor of Organizational Behavior and Social Ecology. . . . I had been asked to go there by the Behavioral Science people, but I found that I wasn’t in their group. I was put in a group called Management Theory, with people I’d never heard of. I was in a new country, in a new department, and I didn’t know the politics. (Trist 1993, p. 208)

On balance, the business schools produced a very mixed reaction from the laureates. In spite of their success in this context, they also experienced some highly traumatic situations. Their salvation seems to have been their mobility; ultimately, most found what they were looking for, simply because they were in demand and lots of opportunities were available. Yet there is a factor inherent in the following quote that should be considered, and that is probably understated in the autobiographies:

On moving to a business school in 1960, I received a handwritten note from a political science savant who minced no words: “You, sir, are a TRAITOR,” a reaction motivated by the difficult times then being experienced by Political Science. (Golembiewski 1992, p. 378)

Promotion and Tenure

One possibility, given all the conflict, is that promotions and tenure would be hard to achieve. We have seen at least one instance where this was in fact the case. Certainly, during the early
1960s, among young assistant and associate professors, there was much discussion of the inherent problems involved. Yet, with all the mobility and with people coming into the business schools from the outside all the time, neither promotion nor tenure presented much of a problem; typically these matters were negotiated before a move. At least that was often the case for those in the laureates’ shoes.

I had earned my doctoral degree in a little more than two years, earned tenure at Berkeley while in my twenties, and came to Stanford as a full professor in my early thirties. (Pfeffer 1996, p. 223)

I was recommended for promotion to full professor. Although when I arrived at Illinois I was interested in being on the fast track to promotion, I felt this promotion, like the first one, was premature. I had only been at Illinois for six years, and I just didn’t think that I had accomplished enough. . . . My department head disagreed, and persuaded me to allow the promotion process to move forward. . . . I became a full professor in 1980. (Oldham 2002, p. 292)

Despite this type of evidence that barriers to promotion were not operative, instances where barriers did exist are cited as well. We have already noted the problems Lawler had at Yale.

AIan Meyer . . . came up for promotion to tenure. The tenured professors’ discussion of this case dramatized the ideological split. An economist who had not published since he received tenure observed that there were already enough high-quality professors in organizational science. Karl Weick had written a letter saying Meyer’s article was the best ASQ had published. . . . A statistician conjectured that Weick was only trying to make it appear ASQ published good articles; because of its lack of statistical rigor, the research did not meet minimum scientific standards. (Starbuck 1993, p. 101)

At UCLA they gave me a very good deal financially and, had the dean been quicker and got me a named chair, I would probably have stayed. He talked about it, but was too slow. (Trist 1993, p. 209)

Atston University had made a serious error in prevaricating over a Chair for Derek Pugh. So in 1968 he moved to the London Business School in his “home town,” where he soon occupied the prestigious Chair in Organizational Behavior. I went with him as a Senior Research Officer to continue working on the National Project. (Child 2002, p. 21)

SOCIOMETRIC NETWORK ANALYSIS OF THE LAUREATES

Additional evidence as to the life and times of the laureates derives from a study conducted by researchers at the University of Alabama at Birmingham. The initial study required contacting the laureates, to the extent this was possible, for the purpose of obtaining a list of the people who had influenced their intellectual development (Duncan, Ford, Rousculp, and Ginter 2002). The objective was to determine whether an invisible college or community existed within the group; such a college was said to be “comprised of a group of scholars and scientists who have a shared paradigm and subscribe to a common journal that connects the members” (Duncan, Ford, Rousculp, and Ginter 397).

The sample actually studied consisted of twenty-eight laureates from the first five of Bedeian’s volumes. The interaction patterns among these individuals are noted in Figure 13.1. No invisible college appears to exist among these laureates; rather, a number of smaller units or intellectual neighborhoods seems to be characteristic. These neighborhoods are iden-
tified as small, uncoordinated, and fragmented. This failure to achieve a true community of scholars is reinforced by the findings on the extent to which the respondents published in the *Academy of Management Journal* in the period of 1958–75:

<table>
<thead>
<tr>
<th>Percent of publications</th>
<th>0</th>
<th>18</th>
</tr>
</thead>
<tbody>
<tr>
<td>1–11</td>
<td>4</td>
<td></td>
</tr>
<tr>
<td>12–21</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>22–31</td>
<td>3</td>
<td></td>
</tr>
</tbody>
</table>

Clearly this was not the preferred medium for connecting the members, in spite of the fact that at the time it was the only publication that had the potential to do so.

This lack of an extended invisible college is not surprising given the incipient nature of the field at the time and the multidisciplinary contributions to its origins. The authors hypothesize that a broader investigation of management today would identify a more developed college, or perhaps several major components of such a college. My guess is that it would be the latter.

**CONSEQUENTIAL EXPERIENCES IN THE LIVES OF LAUREATES**

A second thrust of this same research program relied more directly on a content analysis of the laureates’ autobiographies. This effort examined the consequential events reported by these individuals as being influential in their personal and professional development and the various factors associated with these experiences. There were two versions of this study—one published initially and one published only much later and in another journal.
The Initially Published Version

The initially published version reports on four notable instances of researcher-practitioner collaboration drawn from consequential experiences noted in Bedeian's autobiographies—Roberts (1998), Perrow (1993), Child (2002), and Ansoff (1992). These collaborations not only helped solve practical problems but also contributed major additions to scientific knowledge (Ford, Duncan, Bedeian, Ginter, Rousculp, and Adams 2003). They were:

- Roberts’ work with the U.S. Navy on the management of high-reliability organizations (see Roberts and Bea 2001).
- Perrow’s analysis of the Three Mile Island nuclear power plant disaster involving the recognition that risky technologies exist that cannot be managed (see Perrow 1984).
- Child’s employment at Rolls-Royce contributing to the development of the strategic choice perspective (see Child 1972).
- Ansoff’s activities at RAND Corporation and Lockheed Aircraft that resulted in his formulation regarding strategic decision making (see Ansoff 1965).

The article contains what amount to four case histories of these events. In interpreting these data the authors urge that researchers consider opportunities to collaborate “as research rather than consulting opportunities. . . . Approaching such assignments within the context of consulting creates conflicts of interest, casts suspicion on results, and limits opportunities for access. Moreover, consulting is the core competency of consulting firms, not university-based researchers” (p. 57). I would have to disagree with these conclusions and it does not seem to me that the authors’ data, the experiences of other laureates, or my own consulting activities support their contention. In this regard I offer the following consulting engagements as evidence:

- Schein’s (1993) development of the process consulting approach to organization development out of his consulting experience with Digital Equipment Corporation and other firms (see Schein 2003).
- Blake’s (1992) creation of Grid organization development from consulting engagements with Exxon (see Blake and Mouton 1964).
- Oldham’s (2002) consulting work with “some of the largest organizations in the country” restructuring jobs and developing ideas about the practical problems and pitfalls of job enrichment (see Hackman and Oldham 1980).
- Miner’s (1993) experience in developing an honesty test marketed by Argenbright, Inc. of Atlanta, Georgia, which required considerable research comparing honesty test and polygraph results (Miner and Capps 1996).

My point here is that there are many examples of consulting engagements by laureates that contributed substantially to organizational behavior’s knowledge, and had significant practical consequences as well. I can point to many such engagements of my own that, like the honesty testing work, involved considerable research. The influentials whose thinking is discussed in Part II were almost invariably involved in consulting at some point in their careers, although combining these activities with research was less common. In fact, several of the laureates received their training as consultants directly from a consulting firm. Thus, I submit that consulting work is, and long has been, a core competency of many university-based researchers in organizational behavior.
TheOriginally Unpublished Paper

The original manuscript submitted for publication to the editors and peer reviewers of the Academy of Management Executive was a research study replete with a sample (the laureates of the first five volumes), a measure, data analysis, and results (Ford, Rousculp, Adams, Duncan, Ginter, and Bedeian 2001). How this study became transformed into the four case histories that were the essence of the ultimate publication is not entirely clear, but it does seem that in large part the editors and reviewers became ghost writers, and any clear authorial voice was lost (Bedeian 2004b). In short, we have no idea who said what.

Nevertheless, the original manuscript did contain some important material on the laureates, material that was deleted as the paper moved to publication, but that reemerged in a version published much later in another source (Ford, Duncan, Bedeian, and Ginter, 2005). Most significant for our purposes are the data of Table 13.2. There, the highest representation involves research; treatments of formal education and career activities are also relatively frequent. The research events were dominated by student interactions, the formal education events by mentor interactions (strongly), and the career events by peers (less strongly).

Note that teaching and service activities play a much less significant role for the laureates. Few mention work in industry or consulting. This latter finding seems at variance with what was said previously, but the restriction to student, peer, and mentor interactions (likely to be rare in consulting) may keep this figure down. We know from the data of Part IV that roughly half of the respondents there currently engage in consulting.

What the analyses of Table 13.2 emphasize is that the laureates in writing their autobiographies tend to emphasize research activities, in contrast to what was found in Part II for the preorganizational behavior contributors. This type of research emphasis is further indicated by the fact that the laureates of the early years had predominantly engineering and economics degrees, while from the 1950s on, psychology rose to prominence. There is also evidence of the “late bloomer” effect consistent with the idea that many laureates moved to business schools only after their careers had been initiated elsewhere. Overall, these data give considerable support to the themes that characterize the previous interpretations of the historical development of organizational behavior.

Table 13.2

Percentage of Autobiographically Consequential Events* Devoted to Various Professional Activities

<table>
<thead>
<tr>
<th>Professional activities of laureates</th>
<th>%</th>
</tr>
</thead>
<tbody>
<tr>
<td>Research interests</td>
<td>32</td>
</tr>
<tr>
<td>Formal education</td>
<td>27</td>
</tr>
<tr>
<td>Academic career</td>
<td>25</td>
</tr>
<tr>
<td>Teaching activities</td>
<td>7</td>
</tr>
<tr>
<td>Service activities</td>
<td>4</td>
</tr>
<tr>
<td>Work in industry</td>
<td>3</td>
</tr>
<tr>
<td>Consulting work</td>
<td>2</td>
</tr>
</tbody>
</table>

*Only events involving students, peers, and/or mentors are reported.

CONCLUSIONS

The evidence provided by analyses of the management laureates data indicates that the business schools did change, as the foundation reports anticipated, but once social movements of any kind are set in motion the exact nature of the change is hard to predict, and to control. Certainly, this was true of the changes in the business schools. The influx of full-time faculty was apparently much greater than expected, and the disciplinary mix was different as well. Yet these results stem only from the laureates which, while a key group, represent only a fraction of the total. We need information that obtains from a more comprehensive sample, regarding not only the transition process but also the perceptions of the business schools—their climates and cultures. Chapters 14 and 15 take on this task.

REFERENCES


Chapter 14

Psychologists in Business Schools

Psychologists Associated with Schools of Business and Industrial Administration

The 1962 Survey

Findings
Courses Taught
Supplemental Data from Directory Search

The 1964 Survey
Survey Interpretations
Addendum on Sociology
Conclusions

In the early 1960s a group of psychologists newly relocated into business schools organized themselves informally for purposes of networking and to collect information regarding the psychology-business school interface. The data amassed by this group serve to supplement those from the laureates, and they do so utilizing somewhat more representative samples.

Psychologists Associated with Schools of Business and Industrial Administration

The group that came to call itself Psychologists Associated with Schools of Business and Industrial Administration (PAWSBIA) started operation in 1962, and met at the American Psychological Association (APA) annual meetings. There was some talk of attempting to establish a formal identity within APA, but support for this was lukewarm and it never did happen. The major tangible thing the organization accomplished was to conduct surveys and report on the results. It did, however, provide a certain amount of social support to its members at a time when stress levels were likely to be rather high.

There was a steering committee and at times a chairman of that committee, which is almost all the structure the group ever possessed. In the early years, Bernard Bass (then at the University of Pittsburgh), Raymond Katzell (at New York University), Donald Marquis (at the Massachusetts Institute of Technology [MIT]), and I (then at the University of Oregon) were on the steering committee. Later it came to include Renato Tagiuri (at Harvard University), Lyman Porter (at the Berkeley and then Irvine campuses of the University of California), Frederick Massarik (at University of California at Los Angeles [UCLA]) and Victor Vroom (then at Carnegie Tech).

The PAWSBIA group remained reasonably active until 1966. After that it became largely dormant, but technically survived until mid-1969 when it was officially terminated. Only a year later, the organizational behavior division of the Academy of Management was founded, with several of the key players being prior members of the PAWSBIA steering committee. In many respects the organizational behavior division was an outgrowth of the previously purely psychological activity. At least for many, the two served essentially the same functions.
The two surveys conducted by PAWSBIA provided input to three publications (Miner 1963a, 1963b, 1966). In addition, presentations of the results were made to the American Psychological Association, Academy of Management, and American Marketing Association.

THE 1962 SURVEY

The first survey utilized a mailing list obtained by collecting addresses from those who attended the annual meeting. With some supplementing, and based on an 80 percent return, information of some kind was obtained from twenty-five schools. Reasonably complete questionnaires were provided by twenty of these. Individual replies numbered forty-five, and fifty-five faculty members who taught business administration students were identified. However, the number of psychologists answering individual questions often varied downward from the base forty-five. The universities represented are the larger ones.

Findings

The great majority of the psychologists held professional school appointments only. Only ten had joint appointments in psychology, and only two of the universities utilized the joint appointment exclusively. It is safe to say that at that time psychologists who taught business courses could not assume that they would receive an appointment in liberal arts as well; the psychology departments appeared to be quite restrictive on that score.

Table 14.1 contains data for seven of the twenty questions included in the survey. The listing is selective because a number of questions turned out to be poor. In these cases, the results were ambiguous and it proved impossible to establish what they meant. Adequate pretesting of the questionnaire would have prevented this, but the fact is we did an inadequate job of instrument development. Indeed, that as many questions proved usable as did, is a rather lucky circumstance.

First, the behavioral revolution is by no means a psychological phenomenon. We know that now, but it was true even then. Well over half the schools had other social scientists in the fold—primarily sociologists, but anthropologists and political scientists to a degree as well. Among the psychologists, the industrial area (it only later became industrial/organizational, largely due to the influence of its business school component) is by far the most heavily represented. Again one cannot help wondering why the foundation reports did not recognize this pattern. Social psychology is the only other major contributor. Many of these social psychologists consider themselves sociologists as well, and indeed a number may have been educated in joint programs. The legacy from Kurt Lewin is clearly evident in the proportionate emphasis on social psychology. Adding up the multispecialists who count a business school area, in addition to psychology, as a discipline in which they are proficient yields a total of 36 percent. This then, in addition to sociology, is a major supplementary skill. If industrial relations is added, it equals sociology.

Second, there are certain distinguishing characteristics of these psychologists. One is the fact that not only are they engaged in research, but that fact is formally recognized. Only a third do not have some release time for research; another third have 50 percent or more of their workload allocated to research. Consistent with this emphasis, two-thirds are involved in doctoral education. Note that PhD and DBA schools seem to be clearly separated, and the latter were abundant at the time. The schools surveyed were larger, and were heavily involved in doctoral education. Not unexpectedly, the psychologists who came aboard gravi-
Table 14.1

Responses to Selected Questions—1962 Survey

Do behavioral scientists other than psychologists or economists offer courses in your department or school?
- Yes: 63% of schools
- No: 37% of schools

If yes, what major field:
- Sociologist: 48%
- Anthropologist: 19%
- Political scientist: 14%
- Social scientist: 9%

What fields within psychology do you identify with most strongly?
- Industrial: 76%
- Social: 58%
- General—experimental: 9%
- Clinical/counseling: 9%
- Measurement/statistics: 6%
- Educational: 3%

In what disciplines other than psychology do you consider yourself proficient?
- Sociology: 41%
- Anthropology: 12%
- Business management: 12%
- Personnel management: 8%
- Business organization: 8%
- Marketing: 8%
- Industrial relations: 4%
- Political science: 4%

Is any portion of your workload officially allocated to research? If so, what percentage?
- None: 33%
- < 20%: 13%
- 20–39%: 13%
- 40–49%: 7%
- 50%: 30%
- 100%: 4%

Are you a major professor for the doctorate? If so, what degree?
- Not major professor: 32%
- PhD: 38%
- DBA: 30%

Which would best characterize the situation at your school between the regular psychology department and the school of business?
- Integrated efforts in teaching and/or research: 14% of schools
- Cooperative, mutually helpful but independent efforts: 43% of schools
- Occasional cooperation in teaching and/or research: 33% of schools
- Not on speaking terms; no cooperative effort: 10% of schools

On your campus, suppose a psychologist—(A) was in the regular department of psychology or (B) was in a school of business. Assuming they were of equal rank, competence, stature, and so on, indicate whether A or B or (N) neither would be more favored on each of the following:

Research facilities
- (A) Psychology: 32%
- (B) Business: 11%
- (N) Neither: 57%

(continued)
Table 14.1 (continued)

<table>
<thead>
<tr>
<th></th>
<th>(A) Psychology</th>
<th>(B) Business</th>
<th>(N) Neither</th>
</tr>
</thead>
<tbody>
<tr>
<td>Access to graduate students in psychology</td>
<td>79%</td>
<td>0%</td>
<td>21%</td>
</tr>
<tr>
<td>Prestige</td>
<td>43%</td>
<td>18%</td>
<td>39%</td>
</tr>
<tr>
<td>Teaching load</td>
<td>23%</td>
<td>33%</td>
<td>44%</td>
</tr>
<tr>
<td>Assistance</td>
<td>17%</td>
<td>29%</td>
<td>54%</td>
</tr>
<tr>
<td>Travel funds</td>
<td>18%</td>
<td>32%</td>
<td>60%</td>
</tr>
<tr>
<td>Intellectual stimulation</td>
<td>34%</td>
<td>17%</td>
<td>49%</td>
</tr>
<tr>
<td>Salary, promotion</td>
<td>12%</td>
<td>38%</td>
<td>50%</td>
</tr>
<tr>
<td>Freedom</td>
<td>23%</td>
<td>23%</td>
<td>54%</td>
</tr>
</tbody>
</table>

Assisted rapidly to the doctoral component of their schools and appear to be exerting substantial influence in this area. The foundations should have been pleased with this outcome.

Third, questions were asked dealing with the relationship between the psychology department and the business school. On balance, this relationship tends to be somewhat at a distance, but cooperative; rarely is it strained. The question on which unit provides the more favorable working circumstances indicates that in most respects the two do not differ. This is consistently the modal response, except in two instances, with psychology providing better access to students in that field and somewhat more prestige. Comparing the two units, where differences do exist, psychology comes out on top in four areas and business in another four, with one evenly split. Yet the business schools appear to weigh in most heavily with regard to extrinsic satisfactions—travel funds, pay, promotion, and so on—while the intrinsic rewards of prestige and intellectual stimulation are associated more with the psychology department. Remember that this survey was conducted only three years after the foundation reports were published.

Courses Taught

Table 14.2 lists fifteen courses selected to reflect the existing diversity and taught by psychologists—three at the doctoral level, seven at the masters level, four undergraduate, and one in a nondegree program for upper-level executives. Before considering these in any detail, this discussion should be prefaced with some treatment of what psychologists teach.
Table 14.2

**Courses Taught in the Organizational Behavior Area—1962 Survey**

<table>
<thead>
<tr>
<th>Title</th>
<th>Text</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Comparative Organization Theories</strong></td>
<td>Criteria for a theory. Analysis of theories of small groups, and formal and informal organization. Integration of theories. Each student outlines and abstracts a major theory. It is discussed in class to determine the extent to which it meets the criterion of a theory. Research designs are suggested by class members for testing the major hypotheses contained in the theory. Ohio State University Doctoral</td>
</tr>
<tr>
<td><strong>Organizational Conflict and Change</strong></td>
<td>Factors contributing to internal conflict and to changed patterns of behavior within organizations, from the viewpoint of business management; managerial decision making and conflict; implications of cultural values for the administration of scientific research groups; labor management conflict; coercion and manipulation; planned change. University of Oregon Masters</td>
</tr>
<tr>
<td><strong>Consultative Methods</strong></td>
<td>Methods of developing a consulting relationship with individuals, groups, organizations, and communities. Diagnostic and procedural frameworks for consulting practice, with special emphasis on the mediation role. The use of role playing, organization laboratories, action research, and other approaches in improving various aspects of social systems, for example, decision making and problem solving, adaptive capacity, and integration of system objectives. Case Institute of Technology Masters</td>
</tr>
<tr>
<td><strong>Group and Individual Behavior</strong></td>
<td>By examining relationships developing within their own unstructured group, members explore such interpersonal and group phenomena as membership and participation, power and control, interpersonal trust. Emphasis is placed on the development of each member's ability to understand and respond effectively to ongoing events in a group. Yale University Masters</td>
</tr>
<tr>
<td><strong>Seminar in Research Methods</strong></td>
<td>Logic, measurement, and inference in social science. Operationism, explanation, measurement and observation, scaling, sampling, analysis of data, styles of research, research relationships. Classic social-science works are analyzed as to method, and each student presents a research design at the end of the first term. In the second term, selected substantive areas are further explored and students carry out a research project involving the collection of data, individually or in small groups. Cornell University Doctoral</td>
</tr>
</tbody>
</table>
Leadership Principles and Practice
Knowledge and skills leading to effectiveness in interpersonal relations. Understanding of oneself as a leader, and others as individuals and as members of working groups. Understanding of group process, including group leadership. Lectures and sensitivity training laboratory. University of California at Los Angeles Undergraduate

Principles of Business Organization
The present pattern of business management, development of a theory of management, functions of a manager, authority and responsibility, leadership, the span of management, basic departmentation, line and staff relationships, service departments, centralization of authority, committees, the board of directors, staffing, direction, planning and control. University of Wisconsin Undergraduate

Administration: Theory and Action
Organization and executive behavior is viewed primarily from the standpoint of the effects of internal structure and processes. Concepts in the analysis of organization and administrative behavior, analytic techniques in the understanding of interacting systems of action, models of administrative action, models of organization, values, value determination, value reconciliation, and human consequences of administrative action and organizational models at both the executive and employee levels. Michigan State University Masters

Psychological Foundations of Administration
A study of the psychological principles underlying the relationships of a business firm with (1) customers, (2) investors, (3) the general public, (4) other business groups of suppliers, distributors, dealers, and local merchants. The course aims to provide the student with a knowledge of the research tools of psychology as well as with the practical administrative consequences of this science in the areas of public relations, communication, motivation in advertising and sales promotion, corporate image, plant-community relations, and management appraisal. Boston College Undergraduate

Administrative Practices
The course is concerned with individual, interpersonal, group, and social processes relevant to administrative practice. Methods used vary depending on the topic and phase of the course. Harvard University High-level Executives
Table 14.2 (continued)

General Management Problems
Principles of management applied to selected case histories, relating to problems in the areas of production, marketing, personnel, finance, sales, public relations, and so on. Stress placed on administrative problem solving and decision making at the levels of top and middle management through case studies and business games. Wayne State University Masters

Social Organization of Enterprise
The course deals with human relations in business. Topics such as motivation, communication, and executive development are included. Primarily lecture and discussion, with special projects. University of Notre Dame Undergraduate

Organizations
An integration of scientific studies from many disciplines bearing on problems of organization structure and process, leadership, communication, content, and decision making. University of Denver Masters

Advanced Topics in Organizational Behavior
Intensive examination of selected current issues and developments in theory, research, and practice related to human behavior in industrial and other formal organizations. A seminar course, addressed to advanced problems of both a substantive nature (e.g., new research areas) and a professional nature (e.g., ethical issues). New York University Doctoral

Organization and Staffing
Translation of organization goals and objectives to specific manpower goals and objectives, and staffing to meet the organization’s manpower needs. Techniques for recruitment, selection, and assignment of manpower resources for optimal utilization. University of Minnesota Masters
We found that of the 117 separate courses noted, somewhat over 50 percent were in the field of management broadly defined. Another 25 percent are in personnel management. After that, the courses are widely dispersed—marketing (consumer behavior) and research methods primarily; some pure psychology, labor relations, statistics, and production.

Overall, and this is true of the management area as well, roughly three-quarters of the courses are clearly influenced to a substantial degree by social science content. However, the remaining courses sound quite typical of what existed before the social scientists arrived, and psychologists are teaching them. Such is the situation with principles of management and primarily case courses in industrial administration. It may be that when taught by psychologists these courses take a different form than the catalog copy would suggest, but this is by no means certain.

Of the courses taught by psychologists, 28 percent are exclusively for undergraduates and another 15 percent are upper division courses to which graduate students may be admitted. Thus, in this sample, graduate teaching predominates, even though only three of the schools are exclusively graduate. Cases appear to be just as popular among psychologists as among other business school professors, but they may be different types of cases. Business games, role playing, and sensitivity training are less frequent in terms of the number of universities involved. However, if a school introduces sensitivity training, it tends to do so in several courses.

In Table 14.2 the three doctoral courses—Comparative Organization Theories, Seminar in Research Methods, and Advanced Topics in Organizational Behavior—are all quite sophisticated and new to the business school scene. These are certainly the kinds of courses to which business school teachers-to-be should be exposed. The masters-level courses are more of a mixed bag. Some are a clear reflection of new trends and social science innovations—Organizational Conflict and Change, Consultative Methods, Organizations, Organization and Staffing. Group and Individual Behavior is a sensitivity training course, and questions were being raised about the intellectual content of such efforts, but the input from the social sciences is certainly there. The remaining two courses—Administration: Theory and Action and, in particular, General Management Problems—have more of a traditional management flavor. Yet it is difficult to establish how a psychologist might teach them.

Below the graduate level, the diversity becomes more acute. Leadership Principles and Practice uses a sensitivity training laboratory, Principles of Business Organization is a classical management theory course, Psychological Foundations of Administration definitely draws upon psychological sources while adding a focus that is as much consumer behavior as organizational, and Social Organization of Enterprise is a course in the human relations tradition going back to Mayo and Roethlisberger (see Chapter 6). One suspects that the executive development course, Administrative Practices, has a similar orientation.

All in all, Table 14.2 reflects a tremendous variation in approach. There is no one best way to teach this kind of subject matter. In fact, there are more likely fifteen best ways. To this should be added that with the exception of management principles books, there were few if any basic textbooks available, and little agreement as to content in any event. Without question, the advent of organizational behavior texts later on represented a force for more uniformity. Nevertheless, there remains considerable evidence of continuing diversity (see Rynes, Trank, Lawson, and Ilies 2003).

**Supplemental Data from Directory Search**

In addition to the survey findings themselves, some data on the fifty-five psychologists identified through the survey could be established by resort to the 1963 American Psychological
Association Directory. These directories at that time contained very detailed biographical data. With regard to division membership, Industrial Psychology was highest with twenty-two members, Personality and Social was second with sixteen, the Society for the Psychological Study of Social Issues (which Lewin helped to found—see Chapter 8) was third with eleven, Consulting was fourth with eight, and Evaluation and Measurement had six. No other division had as many as five members. Experimental Psychology had two, Clinical Psychology had three, and Engineering Psychology only one. Of course, some of these psychologists held multiple division memberships. These data from a new source add a certain degree of construct validity to the survey findings, and to the evidence from the laureates as well.

Given the concentration of specialization, some concentration of doctoral-degree-granting institution might be expected as well. Not so. Only Ohio State University (with seven) and New York University (with six) stand out, and both grant a large number of doctorates generally. Harvard University and the University of Michigan each have four; MIT, Columbia University, University of Chicago, and UCLA have three. All in all, twenty-four universities are represented. Of the fifty-five psychologists, 78 percent received the doctorate from a university other than the one where currently employed. Inbreeding is not very pronounced.

We looked at the matter of prior business experience because it seemed feasible to expect the business world might be a major source of business school psychologists. A gain our hypothesis was not confirmed. Of the fifty-five, thirty-nine had had no full-time business experience. A number, however, had done extensive consulting on the side. There were indeed five individuals who had worked for a consulting firm on a full-time basis, usually in a high-level capacity and often for a number of years. Among these people, personal knowledge of business was extensive. Another six people had worked, also after obtaining the PhD, in corporations of various kinds. However, their positions were not at a high level within management, and many did not remain long in business firms. The remaining five worked in business positions prior to obtaining the PhD, either as graduate students or in positions of a nonpsychological nature.

The typical pattern is one of psychological education, followed immediately by academic employment, leading eventually to employment in a business school. We do not know whether this same pattern is characteristic of nonpsychologist business school professors, but it probably is, with the exception that entry into the business school is earlier (either before or immediately after the doctorate). In any event, organizational behavior almost forty-five years ago did not involve any more hands-on business experience than it does today.

THE 1964 SURVEY

During 1963, the PAWSBIA steering committee attempted to obtain funding for a much more extensive survey of psychologists in business schools. We had a pilot study (the 1962 effort) and had gradually accumulated a much larger listing of target individuals and universities. We thought we were ripe for foundation funding to conduct a study that would establish what impact the foundation activism had had some five years after the reports appeared. We could not have been more wrong. The Ford Foundation was rapidly scaling back its investment in business education and the Carnegie Foundation never had made a major investment beyond the report. These were policy decisions at the board level; funding for a survey was out of the question.

As a consequence, PAWSBIA settled for a relatively simple survey of a kind that would permit the development of a member directory containing information on areas of teaching and research specialization. The idea was to distribute such a listing with the purpose of
stimulating informal communication and networking. That was done, with a directory of 174 names and addresses, to most of which were appended statements of areas of specialization. The response rate was 88 percent and 145 completed questionnaires were obtained. The results are given in Table 14.3.

By this time, the term organizational behavior was well established and 78 percent identify with it. Management was endorsed by 47 percent, but in most instances, these are people who also check organizational behavior. On only 6 percent of the questionnaires is this not the case. Adding this 6 percent to the 78 percent gives 84 percent of the respondents who appear to be specializing in the micro and/or macro study of organizations. This is sufficient critical mass—more than 120 people—to say that by the mid-1960s the push to get psychologists with an interest in studying organizations into some relationship with business schools had had considerable success.

The only other field with a substantial representation among psychologists is personnel management. Only nineteen individuals had a marketing/advertising specialization; all who indicated advertising checked marketing as well. It also appears that teaching specialization serves to define specialization in general much more than research/writing specialization does.

In order to get some indication of the degree of concentration of the psychologists within universities, a tally was made using the total address list of 174. There were some universities with five or more people—Yale, the City University of New York, New York University, UCLA, Harvard, Stanford, MIT, Michigan, Cornell, Carnegie Tech, and Southern California. But only one had over ten. A total of forty universities had just one person; another fourteen had two or three. Understand, however, that by no means all of these psychologists are full-time in a business school. Many are in psychology departments or elsewhere on campus, but with some relationship to the business school. Probably, they are best defined in most cases as the kind of people who would have joined the organizational behavior division of the Academy of Management, had such a division existed at that time.

<table>
<thead>
<tr>
<th>Areas of specialization</th>
<th>Percent indicating specialization</th>
<th>Percent indicating teaching specialization</th>
<th>Percent indicating research/writing specialization</th>
</tr>
</thead>
<tbody>
<tr>
<td>Organizational behavior</td>
<td>78</td>
<td>70</td>
<td>58</td>
</tr>
<tr>
<td>Management</td>
<td>47</td>
<td>44</td>
<td>30</td>
</tr>
<tr>
<td>Personnel management</td>
<td>46</td>
<td>45</td>
<td>23</td>
</tr>
<tr>
<td>Experimental design</td>
<td>21</td>
<td>19</td>
<td>6</td>
</tr>
<tr>
<td>Labor relations</td>
<td>14</td>
<td>10</td>
<td>7</td>
</tr>
<tr>
<td>Marketing</td>
<td>13</td>
<td>11</td>
<td>10</td>
</tr>
<tr>
<td>Statistics</td>
<td>12</td>
<td>11</td>
<td>3</td>
</tr>
<tr>
<td>Advertising</td>
<td>5</td>
<td>4</td>
<td>3</td>
</tr>
<tr>
<td>Human engineering</td>
<td>4</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>Operations research</td>
<td>3</td>
<td>2</td>
<td>2</td>
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</tbody>
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<table>
<thead>
<tr>
<th>Students taught</th>
<th></th>
<th></th>
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</tr>
</thead>
<tbody>
<tr>
<td>Undergraduate</td>
<td>55</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Graduate</td>
<td>81</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Managers and administrators</td>
<td>39</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
The conclusion that emerges from this analysis is that, with a few exceptions, the association of psychologists with business schools was a very unstable phenomenon in those days. Remove one or two people and there is nothing there at all. Even some of the schools with relatively large representations have experienced major downward fluctuations over the years.

SURVEY INTERPRETATIONS

It may be of some historical interest to consider the interpretations placed on these findings at the time they were written up. One approach was to consider what psychologists might be doing in business schools, but rarely were (Miner 1963a). There was little evidence of activity in the production area—this in spite of major increases in employment in the human engineering field at the time. One might have thought that in view of the apparent shortage of professors qualified to teach production, experimental psychologists with applied interests would have been recruited to fill the void. There is no evidence that this happened.

Many psychologists with backgrounds in psychometrics, research design, and experimental psychology should be qualified to teach statistics. There were lots of statistics sections to teach. Yet activity on this front appears minimal. The surveys identify very few people doing this.

The marketing and advertising area is another where psychologists could bring skills in market research and consumer behavior to bear, but few are doing so. The difficulty here appears to be a shortage of qualified psychologists. Skills of this kind are typically honed in the business world, and once gained the people who have them are likely to stay where the rewards are greatest.

Finally, there was an almost complete lack of courses that deal specifically with personality theory and what might be called the clinical psychology of industry. Some such material is embedded in organizational behavior courses. However, little evidence was found that clinical psychologists were bringing the subject matter of that field to the business schools as a separate entity. Mayo clearly would have bemoaned the lack of activity on this front.

Approximately half of the business schools surveyed indicated an intent to add behavioral scientists in the near future, but the demand was in organizational behavior to include a number of opportunities for sociologists, and in research design. Production, statistics, marketing, and clinical applications were not where the need was considered greatest.

A point also emphasized is reflected in the following quote:

Psychologists as a group appear to need a much greater commitment to our existing economic system and an increased understanding of the practical realities of the business world. There is a widespread mistrust of business management as it is currently structured, which on occasion seems to border on outright distaste. In the writer’s opinion these feelings must inevitably obstruct and distort the psychologist’s influence on collegiate management education, and his contribution to knowledge of human functioning within the context of modern business organizations. (Miner 1963b, 66)

This is the value clash problem. Without question, it reflects my having recently moved from a business position to academe. Unfortunately, the surveys provide little evidence on this issue. In all likelihood this is a lesser concern for industrial psychologists, and that may help explain why so many from that group were attracted to the business schools. That experimental and clinical psychologists were not attracted may simply reflect the disparity of their values. It
is unfortunate that empirical data on value orientations were not obtained in connection with
the surveys. However, we are not much better informed in this regard today. What we think we
know regarding this key factor is much more inferential than empirical.

The only data of which I am aware in this area derives from a study of mine (Miner 1990),
where judged humanistic values were measured. Humanistic values are not necessarily anti-
business, but they are antigovernment and in opposition to bureaucratic functioning; they may
be traced historically to theories and applications that advocate role empowerment (see Miner
2006), and thus to an emphasis on a type of participative management (Wall, Wood, and
Leach 2004).

The particular study dealt with thirty-five scholars, the great majority of whom identified
organizational behavior as their core discipline. Of these thirty-five, 31 percent were found
to have strong humanistic values, and a total of 66 percent had either strong or moderate
values of this type. This was in 1977, and presumably applied to a period of years extending
back from that point. Thus, approximately two-thirds of organizational behavior scholars in
the early period would appear to have held some values that were in opposition to the struc-
tural forms and existing processes characterizing the business world of that time. These
same structures and processes remain at the forefront today (Leavitt 2003).

ADDENDUM ON SOCIOLOGY

I know of no survey data on the contributions of sociologists in the early days of organiza-
tional behavior similar to those for psychologists. However, some information is provided
by Scott, who “grew up” with both organizational sociology and organizational behavior
starting in the 1950s. Both fields appear to have had their origins in the same time frame; both “exhibit a common heritage and bear similar birthmarks” (Scott 2004, 4). In the early
period many individuals trained in sociology remained in sociology departments, unlike
what happened among industrial and social psychologists. As the organizational behavior
phenomenon continued to unfold this was no longer true, however.

Scott notes that the number of scholars concerned with macro organizational issues in the
business schools far exceeds those in sociology departments. He says: “the number of orga-
nization faculty in our academic departments have dwindled during past decades, raising the
question: Who will train succeeding generations of organizational sociologists? . . . perhaps
we can persuade business schools to subsidize the training of organizational sociologists as
a long-term investment in their intellectual capital” (2004, 16). In any event, the movement
of sociologists into the business schools now appears to have followed along the lines estab-
lished by the behavioral revolution.

CONCLUSIONS

In this and the previous chapters of Part III, I have taken up various types of evidence derived
from systematic attempts to assess the state of the business schools in the early period, and to
establish how the newly entering social scientists impacted these schools. What we find is
considerable confirmation of the same conclusions, derived from data collected in a number
of different ways. How these historical events may have impacted the current scene will be
the subject of Part V. For the present, however, we still need to look into the high level of
uncertainty and instability that characterized the business school scene in this period—to
understand both the stress that it created and the creative spark as well.
REFERENCES

In this chapter, the discussion focuses first on the situation within the business schools including the interface with the broader university, and second on the external environment, especially those aspects of that environment creating uncertainty and instability (see Priem, Love, and Shaffer 2002 for a discussion of sources of uncertainty). The initial impact of the foundation activism and the activities of a number of influential economists was to stir up the business schools, creating high uncertainty levels and considerable disruption of the status quo. The situation was not unlike the unfreezing phase of social change described by Lewin (see Chapter 8). The change process was initiated from the outside primarily, and it created sufficient disequilibrium so that movement to a new level was possible. However, stabilization at the new level did not occur for a long time, and in the interim there was continuing uncertainty as to what the outcome might be and insofar as individuals were concerned whether that outcome would include their participation.

**UNCERTAINTY WITHIN THE UNIVERSITY**

We start with several descriptions of the internal business school environments that existed at the time in particular universities. Chapter 13, in considering the management laureate autobiographies, contains much material of this kind, but a more in-depth treatment is desirable.

**Washington University**

This description is provided by Walter Nord (1996) who entered the business school at Washington University as the first social scientist in 1967. He had just completed a doctorate in social psychology, which included considerable work in sociology, at the same university. Washington University is a private school and its business component at the time was quite small. Nord describes himself as a Marxist in political belief, but indicates that he has kept these values out of the classroom except in doctoral seminars. He has written on Marxism (see Bailey 2003).

Never having had any business coursework himself, and without a role model of any kind, Nord faced a situation in his teaching that provided considerable latitude and few guidelines.
He could experiment as he saw fit and introduce subjects that interested him. Initially he drew heavily on his earlier training, including many ideas from B.F. Skinner’s brand of behaviorism. He used the latter’s Walden Two (1948) in his first-year MBA class. In general other faculty members and administrators were supportive. The small size of the business school, and departments outside it as well, encouraged a great deal of interdisciplinary interaction.

At first the strong practical orientation of the students created some problems. Nord was not used to this from his background and it put him on the defensive. Ultimately, however, it proved to be a stimulus for learning. Both Vroom (1993) and Schein (1993) describe similar experiences. The lack of appropriate teaching materials, including textbooks covering the new material, created additional difficulties. There was little available that dealt with the specific subject matter Nord wanted to teach. However, an opportunity to self-design was fostered by the fact that he was teaching courses that had never been taught at the university before. With no set agenda, he was free to teach things that he wanted to learn about, and he did. His final resolution to the teaching materials problem was to prepare a collection of readings (Nord 1972). An initial anxiety he felt with regard to using cases in teaching was overcome by spending time at the Harvard Business School observing the approach used there.

Nord contrasts his experiences at that time with the current scene. The use of the standardized course evaluations that exist today would definitely have discouraged his innovative tendencies. Then quality was valued without any narrow definition of the boundaries of the subject matter, whereas today that is not true. That he was able to thrive in this situation Nord attributes to the combination of his somewhat anarchistic, iconoclastic personality and a social environment that surrounded him with supportive people who valued quality and continuous learning, while creating a low level of established routine. He continued to thrive, or at least survive, at the Washington University business school for over twenty years. Unfortunately, he does not provide us with information on any ways in which his Marxist values may have created problems in this context.

The following quotes provide a certain degree of authenticity for some of the points made above, while documenting the ambiguity and uncertainty that prevailed:

Because there were few people like me in the business school, and no one, including me, knew exactly what an organizational psychologist should do, I had a great deal of latitude. As a result, I was in a situation where I had room to experiment and introduce things that I was interested in. (Nord 1996, 85)

I did not fit into the established routines and was surrounded by developmentally oriented people. I was always allowed and encouraged to introduce and/or keep open to a variety of perspectives. . . . I had the opportunity to be a self-designing system. . . . The opportunity I had to self-design was promoted by the fact that I was frequently teaching courses that had never been taught locally before. Thus there was no particular set agenda and I was free to bring in things that I wanted. (Nord 1996, 86)

Elsewhere Nord provides a description of aspects of his personality, and gives an indication of why such a person may be one of those who thrive in the business school context of the times:

Basically, I tend to be (based on self-report and comments of others) a very supportive, low judgmental person. . . . I value creativity and latitude and hate details. I do not like to be controlled or to control others. I also like diverse ideas and I value good thinking. . . . I have been
fortunate to be in locations that did not have strong structures and pressures pushing me in any particular direction. This really fit me, although it may not fit others. (Bailey 2003, 382–84)

Carnegie Institute of Technology

The next scenario derives from a very different context and deals primarily with topics other than teaching. It is provided in the autobiography of Herbert Simon (1991) who died in 2001 (see Anderson 2001). Simon came to Carnegie from the political science department of another technological university at a time when the Graduate School of Industrial Administration was being founded (1949). He remained there throughout his career, although his base of operations gradually shifted to the psychology department. It is testimony to his multidisciplinary capabilities that he also received the Nobel Prize in economics.

The starting assumption of the new school appears to have been that the business education of the era was a wasteland of vocationalism strongly in need of transformation to science-based professionalism. The model for the new school emphasized a diverse array of behavioral scientists to include a heavy economics emphasis. There were no classical management theorists already in residence and so this did not represent a problem. However, Simon reports an early conflict between those with a more applied bent and the neoclassical economists who were intent upon retaining their professional purity. March, who was also at Carnegie at the time, reports this same conflict with the neoclassical economists (see Augier and Kreiner 2000). There were also some clear conflicts between Simon’s own theorizing about managerial decision making and the theories espoused by the same economists. These problems seem to have been exacerbated by a university culture that supported the use of certain controls on both teaching and research (which was heavily funded). Which group was to exert the control?

By the early 1960s, it became evident that the neoclassical economists were. At Carnegie, organizational behavior’s dominance ended in the period when it was just beginning to pick up steam elsewhere. The emphasis shifted to sophisticated mathematical techniques in operations research and neoclassical economic theory. A number of people with other interests left for attractive opportunities elsewhere. Simon, who had been developing a research program in cognitive processes and computer modeling, began to withdraw to the psychology department and finally moved his office there around 1970 (Augier 2001).

What is evident here is an escalating conflict between organizational behavior (OB) and economics that existed in spite of the fact that at Carnegie, as elsewhere, economists played an important role in fostering the development of organizational behavior in the first place. Simon also indicates a degree of conflict between the industrial administration segment of the university and the liberal arts segment, especially the humanities. He speaks of the latter’s demeaning attitude toward what they considered vocationalism, of their espousal of uselessness, of their portrayal of skill as a dirty word. It is apparent that here, as elsewhere, the business school was subjected to a degree of one-upmanship and that the dialogue involved often became heated. It was not just business versus the humanities, but science versus the humanities as well.

The situation as it ultimately evolved at Carnegie has been described as follows by a participant:

Carnegie was not the source of interdisciplinary work that it had been in the early sixties. While there were still no departments within the school, the creation of a strong group of economists who were committed to theoretical issues within economics foreshadowed similar
coalitions within operations research and in organizational behavior. Furthermore, Leavitt and March had moved on to other universities; Cyert was about to become president of the university; and Simon had left the field of organizational behavior more than a decade earlier. (Vroom 1993, 270)

**Variability Across Universities**

Although the subject matter of the descriptions differs, the wide disparity between the Washington University and Carnegie experiences is still striking. Lawrence (1987), speaking from his long tenure at the Harvard Business School, has something to say on this score. He notes first that the decade starting in 1958 was an unusually productive time; as organizational behavior grew in this period it produced many innovative theories, and Harvard was a participant in that process. Second, and this too is based on the Harvard situation, the business schools and organizational behavior were subjected to considerable discipline-based stereotyping and even denigration. The field’s members were criticized among other things for being merely servants of power. The echoing voices within the halls of academe became strident here, often loud enough to be heard outside.

Lawrence gives substantial attention to the multidisciplinary nature of organizational behavior. As direct recruitment of psychologists and sociologists increased in the 1960s, the immigrants threatened to overwhelm the natives, with all the tension the foundation reports anticipated. He also notes that organizational behavior exhibited great flexibility in responding to the value issues of the period. Although some may criticize this as faddism, it has the appeal of permitting involvement in social issues at the time they are happening. Lawrence finds this flexibility to follow current problems appealing; certainly, the Harvard Business School has been at the forefront in this regard.

Another source of information is a chapter by Blood (1994) written from the author’s perspective as a long-time administrator with the American Assembly of Collegiate Schools of Business, and before that as a faculty member in the school of business at Georgia Tech (1974–83). Blood is a psychologist, but he feels that the multidisciplinary roots of organizational behavior are hurting the field now and create a fuzzy identity with unclear boundaries.

Although not present at the birth of organizational behavior, Blood is close enough to these origins and knowledgeable enough regarding them to have useful insights. He says:

The field of management was based on anecdotal and experientially derived knowledge; the new OB faculty came from empirically-based social science disciplines. Industrial psychologists brought in their penchant for individual differences research, their correlational approaches to knowledge creation; and their disdain for knowledge that is not empirically authenticated. The clash of epistemologies must have created as many agreements-to-disagree as it created opportunities to pool perspectives. At any rate, in some schools OB supplemented an existing management department. In others, OB supplanted management. In still others, OB grew up as a separate, parallel field that competed for the same topic areas under different names and approaches. (Blood 1994, 211)

A personal example may help to make the tensions and different epistemologies that Blood describes more meaningful for the reader. The following memo was written by my department head at the University of Oregon, a scholar in the classical management principles tradition, during 1964:
In has become increasingly apparent that you are not adhering to proper organization channels in your attempts to foster your self-interest. After a discussion with Dean Lindholm yesterday I came to the conclusion that you should be notified in formal fashion that you are transgressing the fundamental precepts of sound organization. Specifically, I refer to your chronic bypassing of your department head, myself in this instance and Dr. Mark Greene during the previous three years. I suggest that in the future all business matters pertinent to your position in the School of Business Administration be routed through my office, at least as a matter of courtesy, before you approach Dean Lindholm. Bypassing the department head as you have been doing for the past four years is a serious breach of both etiquette and organization structure.

It is evident that the differences were not merely confined to the intellectual treatment of distant business conditions; they applied to the here-and-now university setting as well.

Such conflicts did not occur everywhere, as the Washington University scenario indicates. Furthermore, overlapping jurisdictions with other social science departments outside the business school were much less likely to be a source of tension than overlaps within. The following letter written to me in 1962 by Robert Dubin, research professor of sociology at the University of Oregon, illustrates how potential problems could be and often were handled:

This letter will serve to confirm the conclusions reached in our conference last Friday. The University course committee had recommended that there be consultation between representatives of the School of Business Administration and the Department of Sociology to establish mutual understandings regarding course offerings in the two areas. We acted as the respective representatives and came to the following agreements.

1. That the possibilities of overlap occurred principally in the area dealing with formal organization, variously characterized in sociology as the study of bureaucracy and industrial sociology, and in the School of Business Administration as the study of management and supervision.
2. That the subject matter of such courses in sociology is analytically oriented while the subject matter of such courses in the School of Business Administration is professionally oriented. This difference in orientation is significant as the major criterion for separating courses into the two academic realms.
3. That the respective sociology courses usually are taught at the advanced Undergraduate level, with possibilities of graduate credit, while the complementary courses in business administration are usually taught at the graduate level as part of intensive professional training.
4. That we find no conflicts among the present course offerings or those now proposed by either division of the University. Furthermore, we expect that if the major criterion as set forth in #2 above is followed in the future, such conflicts need never arise.
5. That the Sociology Department would be pleased to send its students undertaking graduate preparation for professional management positions (e.g. supervision of research organizations) into the relevant School of Business Administration courses for training. We view such courses as pertinent to the development of professional management skills, thereby serving the entire University.
6. That the School of Business Administration will continue to make use of the perti-
nent courses in the Sociology Department that contribute to the training of students in the general management area, and such professionally specialized areas as personnel administration and marketing.

I believe these six points cover the substance of our discussions and agreements. Please be sure that the Department of Sociology welcomed this opportunity to examine an area of instruction that is of common concern to our respective departments. We stand ready to continue the cooperation that has characterized our relations in the past.

Where conflict did arise it inevitably created an environment of uncertainty and stress. There was always the possibility that some group would win and some other lose. Simon’s exodus to the psychology department at Carnegie is a case in point. At Oregon by about two years after the time the memo quoted above was written, the existing organizational behavior contingent had disappeared from the university.

UNCERTAINTY IN THE OUTSIDE WORLD

The considerations I will mention in this section differ from those noted previously in that they refer to organizations and events that had their origins outside any particular university. They spread across all, or almost all business schools, and in some cases, had much wider implications than that. Yet they were unsettling and accordingly added to the stress and uncertainty; they probably accentuated the unfreezing as well.

Professional Associations

In the early years there was no professional association for those in the field of organizational behavior. The largest segment were psychologists, and members of Division 14 of the American Psychological Association represented the largest contingent. The problem was that not all of the imports by any means were psychologists and not all the psychologists were of the industrial variety. We saw this in the preceding chapter. Furthermore, Division 14 was strongly oriented to psychology department faculty and their students who had embarked upon a career in the business world. There was curiosity and interest regarding those members who had wandered off into the business schools, but initially at least some skepticism. There was enough ambivalence all around then so that a true joining of the parties was unlikely. It was not until 1970 that Division 14 assumed the title of Industrial/Organizational Psychology, reflecting formal recognition of the tie to organizational behavior.

The Academy of Management was another matter. Initially, its concerns were not those of organizational behavior. However, around 1960, with a stagnant membership, attention began to focus on methods of achieving growth (Wrege 1986), and ultimately this led to a much more open stance toward previously unrepresented groups generally. The problem in the early period, however, was that the interests of the Academy and organizational behavior were unaligned. Information on the content of the Journal of the Academy of Management in the late 1950s indicates that two areas dominated—management education and development (pedagogy) and organization and management theory (principles) (Adams and Davis 1986; Mowday 1997). There was little of an empirical nature. The meetings were characterized by these same emphases.

As Gordon (1997) reports, by the mid-1960s, the situation at the Journal began to change.
There was a shift away from more exclusive identification with general management principles and processes, personnel administration, and the production management of the day to a wider range of concerns. By the 1968–69 period, organizational behavior had become the dominant topic area and that pattern continued for another twenty years with the peak in 1978–79. By 1973–75, this pattern of change produced the first editor of the journal trained as a psychologist (Miner 1997). Clearly, the Academy of Management had by that time become the professional association for organizational behavior. However, before that happened, there was considerable shifting back and forth; this instability did little to ameliorate the uncertainty and lack of a clear professional identity that characterized organizational behavior at the time.

Yet there are still vestiges of these early problems. As the Academy grew, it quite naturally became segmented into divisions. As Cummings (1978) wrote, five of these divisions—Organizational Behavior, Organization and Management Theory, Personnel and Human Resources, Organizational Development, and Organizational Communication—are concerned with organizational analysis or science. He hoped that these divisions could somehow be tied together, and he thought he saw signs of this happening. Those signs are still present, but a single professional entity for organizational behavior or organizational science remains a dream for the future. Without it, a degree of instability is perpetuated.

**Computers**

An article published by Leavitt and Whisler (1958) sets the scene for this discussion. It describes the state of affairs existing at that time occasioned by the rapid ascent of a new technology. This new technology was compounded of (1) high-speed computers that could process information rapidly, (2) statistical and mathematical methods for decision making, and (3) computer programs to simulate higher-order thought processes (which were only on the horizon). These developments, all of which involved computers in one way or another, had introduced a great deal of uncertainty. It was hard to tell what their eventual impact would be.

The question of how computers would influence management, organization structures, employment levels, and the like yielded a rapid response in the form of research on organizations that had introduced computers on a major scale. By the late 1960s, the answers began to come in (see Myers 1967; Reif 1968; Whisler 1970). In this respect, there was uncertainty in the business schools, and elsewhere, for sometime, but research ultimately served to reduce the problem. Other computer-related sources of uncertainty were less tractable.

One had to do with the use of computers to simulate cognitive processes. Where would this lead? How successful would it be? What would be the impact on social science generally, and on organizational behavior? Few had any idea, although Simon and those who worked with him were convinced that important advances were in store (Simon 1991). There was the feeling that major breakthroughs could well reorient the whole study of decision making, but would that occur?

In addition, computers threatened to replace the calculators that had been the mainstay of statistical analyses for years. Research could be simplified enormously, but the technology was slow to develop and programs often did not work. Computer games were the next advance in teaching technology, but few knew how to use them and even fewer how to develop them. Computer technology appeared to be redesigning the jobs of people in organizational
behavior, and many other fields. Yet this was not happening smoothly and what its limits were was hard to establish.

**Student Activism**

One other source of uncertainty came to permeate the external, and ultimately the internal, environments of business schools during the 1960s. These matters relate to student activism and protest and the New Left ideology that characterized the student movement at that time. The events of this period were widely studied and considerable information is available regarding them and the students who participated (see Kerpelman 1972; Lipset and Schaflander 1971; Miner 1974).

Starting in 1962 with peace demonstrations in Washington and the formation of Students for a Democratic Society, the pace of student activism and militancy began to accelerate. By the fall of 1964, the Berkeley campus of the University of California was faced with large-scale demonstrations, which spread to most of the larger campuses in the country. The turmoil that ensued continued to the end of the 1960s and beyond.

The business schools and organizational behavior faced an interesting situation during this period. A major target was the business community and the economic system, consistent with New Left ideology. As contributors to the creation of corporate bureaucracies and representatives of the free enterprise system, business schools were objects of attack. Yet, in general, business students and faculty exhibited neither activist attitudes nor behavior to any significant degree (Peterson 1968). Student activism was at its peak among the social science disciplines, and faculty support was greatest there too—particularly in sociology, but also in psychology and political science, and to a somewhat lesser extent in economics. These statistics posed a problem for organizational behavior because among the relatively conservative business faculties, those who had close ties to the social sciences were sometimes viewed as suspect.

The turmoil was great enough everywhere and it did extend to the business schools to a degree, but whether rightly or not, and we know little on this score, organizational behavior was caught up in the controversy more than most. Its position is best viewed as precarious—from one side it represented the enemy, from the other side it was considered a sympathizer with the enemy. This is the essence of a lose-lose situation, and it made for a few sleepless nights.

**CONCLUSIONS**

As we have seen, the ground that existed during the early history of organizational behavior bordered on the chaotic. This was a period of unfreezing, and gradual movement to a new ground. However, the unfreezing did not stop when the movement began, and continued. This continuing ferment occurred within the business schools, but it was fed as well from the outside, on occasion by factors that had little if any relation to the movement of the social sciences into the business school context. I have chosen to discuss the three such factors that seem most salient to me. This is not to argue that other sources of uncertainty did not operate in that period as well.

The treatment in this chapter has concentrated on portraying the ambiguity and lack of structure characterizing the times, as organizational behavior came into being. We have considered several sources of uncertainty, and we have noted that stress sometimes resulted for participants. Some people did not function well under these circumstances; they needed more structure to be at their best. There were defections from the ranks of organizational
behavior in the early years, people who returned to the basic departments outside the business schools or moved to business employment of various kinds.

Yet others, such as Nord, responded well to the uncertainty; in fact, they appear to have thrived under these circumstances. These were often the people who created the theories that emerged so frequently during the 1950s, 1960s, and early 1970s, the theoretical binge that accompanied the early growth of organizational behavior. In Chapter 16, we turn to these positive consequences of uncertainty and to the process of theory building in the period.

REFERENCES


A PERSONAL AND THEORETICAL ODYSSEY

Biographical Information

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Conclusions

The preceding discussion has provided an extended panorama of the ground against which organizational behavior became figure. However, it remains to show just how the theories and research developed or adopted by organizational behavior fit into and were influenced by their setting. Obviously, different theories achieved their positions on the ground in different ways, and in most instances we can gain only a partial understanding of this process because not all of the facts are available. One solution to this dilemma is to solicit stories from individuals describing their experiences as their theories and research developed. The approach has the disadvantage that it is dependent on the vagaries of post hoc constructions; memory does not always serve us well in these personally relevant situations. On the other hand, there is the advantage that the person has actually experienced the development involved.

This said, I propose to present my own role-motivation theories, and how they came to be, in the hope that this story will prove useful in interpreting others' contributions. In discussing this theoretical approach, I will concentrate on the ways in which a business school environment made a significant contribution. For those who may wish for a more comprehensive statement and evaluation of the theory, see Miner (2005, 2006). In any event, an understanding of my background is essential to effectively interpreting what follows. Thus, to provide a roadmap through the theoretical territory, I start with some biographical information.

BIOGRAPHICAL INFORMATION

My education was all in psychology, both clinical psychology and personality theory. My graduate work was done at Clark University and at Princeton University, which granted the doctorate in 1955. None of the universities I attended had a business school at the time; in
fact, none even offered coursework in industrial psychology. My dissertation involved studying manpower utilization from the perspective of intellectual talent supplies—a combination of administering an intelligence test to a national sample and analyzing the data to determine how well society was using this resource.

Somehow this educational background got converted into rather brief employment stints teaching industrial psychology in the psychology departments at Georgia Tech and Brooklyn College. Next, in 1956, came an offer from Eli Ginzberg, an economist and manpower specialist of considerable international repute, to join the research staff of the Conservation of Human Resources project in the Graduate School of Business at Columbia University. This was my first brush with a business school, and although it did not last very long, it taught me a great deal that was entirely new. At this same time, a personal psychoanalysis that had been in process since my graduate student days was completed. By late 1957, my odyssey had taken me to a position as a psychologist in the corporate personnel research unit of the Atlantic Refining Company in Philadelphia.

While employed at Atlantic I began work on an MBA in industrial relations at the Wharton School of the University of Pennsylvania, thus entering a business school for the second time. Within a year, however, my status shifted from student to adjunct faculty member at Wharton. During this period, George Taylor, economist and distinguished labor arbitrator, became my mentor. He convinced me to move to a business school permanently. With his help, and that of the chief economist at Atlantic who was leaving to join the business faculty at the University of Chicago, a number of job opportunities came my way.

The upshot was that in 1960 we moved west to the University of Oregon, hired by Richard Lindholm, another economist and dean of the business school there. This third brush with a business school proved enduring. There have been moves—from Oregon to Maryland to Georgia State, and ultimately to the State University of New York at Buffalo—but with the exception of a year each on leave with the psychology department at Berkeley and the New York office of McKinsey and Company, my career has been spent in business schools ever since joining the University of Oregon faculty. However, consulting to various business firms has been a major activity throughout, contributing both to my knowledge of business practice and to my research database.

This background discussion has been a somewhat hurried trip, touching down only on those points that are particularly relevant for the ensuing material. A more leisurely voyage is available in Miner (1993a).

MOVING TOWARD A THEORY

One of my activities with the research unit at Columbia was to help prepare a book describing the factors that caused World War II soldiers to be discharged prematurely. We had U.S. Army personnel records, Veterans Administration records, military unit histories, and follow-up questionnaires on these men. From these we developed a schema of the strategic factors that caused failure, and also case histories to illustrate our points. The schema that finally emerged is presented in Ginzberg, Miner, Anderson, Ginsburg, and Herma (1959).

This work on the determinants of emotional disorders and performance failure drew upon my background in clinical psychology and it was from that perspective that we prepared several articles during my stay at Columbia (Miner and Anderson 1958a; 1958b). However, what proved to be much more important in the long run was the insight this work gave me into the dynamics of ineffective performance and the human resource man-
agement issues involved. Here I was on completely new ground, and it was a business school environment that provided it.

**Role Motivation Training at Atlantic**

Shortly after my arrival at Atlantic the training director asked me to teach a course for managers in the Research and Development (R&D) Department, with the objective of arousing their interest in managing. Many had been promoted into management based on their scientific accomplishments and continued to give more attention to the professional aspects of their work than the managerial. Could I stimulate them to become better managers?

While at Columbia it had occurred to me that the schema we had created might be converted to a diagnostic framework that managers could use to determine what factors were contributing to the ineffective performance of their subordinates. Once these operating factors were identified, a determination could be made as to which could be changed, and a plan for corrective action to restore effective performance established. Basically this involved transferring the diagnosis and treatment model of clinical psychology (and medicine) to my newly found world of human resource management.

Faced with the challenge presented by the training director, the possibility of teaching this diagnostic approach to supervision to the R&D people began to form in my mind. The problem-solving approach involved might appeal to them as scientists. Furthermore, they would be cast in the role of manager, responsible for those assigned to them and for getting the work accomplished as well. My thought was that we could construct a role model for them, make that role model attractive, and give them the tools to perform in accordance with what the model prescribed. In the terminology of psychoanalysis, this meant helping the managers develop a particular type of ego ideal. This was my approach to dealing with the upside of the problem. On the downside was the possibility that some managers had avoided managing because it created anxiety. By extending the model of ineffective performance and its causes to them personally it might be possible to get them involved in diagnosing and correcting their own performance failures. In particular, it seemed that attention should focus on sources of phobic reactions to the managerial situation. By providing the managers with what amounted to psychoanalytic interpretations of their unconscious motives and how these might yield anxiety on the job, perhaps some diminution in negative affect could be achieved.

This was the very rudimentary theory that drove the creation of a course administered to seventy-two R&D managers at Atlantic. Evaluations conducted on this course indicated that managerial motivation was in fact increased, and thus support for the theory was obtained (Miner 1960a). This was also the first research involving managerial role motivation theory, and we will consider how that theory entered in shortly. Note that the theory underlying managerial role motivation training as initially developed was strongly influenced by my psychoanalytic background. However, it was both more than and less than a full psychoanalytic statement.

**Formulations Dealing with Ineffective Performance**

The schema of strategic determinants of ineffective performance first developed at Columbia has gone through several iterations. The Atlantic course, with variations, has been taught many times. The course was initially written up in book form in Miner (1963), and the
version of the schema presented there has held up reasonably well. Table 16.1 presents the most recent statement. Some of the questions that training of this kind elicits are discussed in Miner (1986); the need for such training clearly continues today (see Tyler 2004).

To a large extent these formulations regarding the schema have been more a product of my psychological training than of my business school experience, at least since Columbia. However, there is one aspect of this work where input from the business schools was crucial. After auditing a course in accounting shortly after joining the University of Oregon faculty, I became quite interested in the concept of control. This led me into the literature on the subject and particularly to a book by Jerome (1961). Gradually my writing and teaching began to incorporate aspects of the control model into discussions of the nature of ineffective performance and methods of dealing with it. The approach that finally emerged is depicted in Figure 16.1.

**THE MANAGERIAL (HIERARCHIC) ROLE MOTIVATION THEORY**

The training director at Atlantic brought home to me the importance of managerial motivation, later called “motivation to manage” and designated by others using terms such as “the will,” the desire,” and “the need to manage.” A search of the literature of the time (1957) yielded little on the topic. Accordingly, there seemed to be no alternative but to try to develop a measure myself, validate it, and apply it to evaluate role motivation training to see if the training raised levels of managerial motivation.

The Theory Applied to Atlantic

Initially, we tried to develop a measure labeled Supervisory Interest from the Kuder Preference Record, in part because we had considerable data on that instrument. The research utilizing this homegrown measure yielded significant validities, but the correlations were low (Miner 1960b). Ultimately, the conclusion was that we should continue our search in the hope of finding a better measure.

The next step was to develop a sentence completion instrument using item stems (beginnings of sentences) and a scoring system that adhered closely to the local definition of “the good manager” as held at the Atlantic Refining Company, particularly at corporate headquarters. My sources of information were partly participant observation and partly a very close reading of a large number of management appraisal documents. The theory that resulted was in the grounded theory tradition (see Chapter 2), although this was not apparent to me at the time.

The theory-development process involved writing possible items for the test, paring these down through a “quick and dirty” item analysis, and then abstracting the implicit theory behind this measure after the fact. Thus, theory construction was a highly inductive process. Clearly, the items were all intended to relate to managerial motivation at Atlantic from the beginning, but the components of that motivation emerged from a conceptual grouping of these items after we learned how people responded to them. Subsequently, it also became evident that I had been influenced to some degree by certain sources in the psychological literature such as Kahn and Katz (1953).

Furthermore, the idea behind the components was that managers were evaluated relative to informal role prescriptions inherent in the Atlantic situation, prescriptions that managers either met (behaved in accordance with), and thus were highly valued (judged effective), or did not
Table 16.1

**Schema of Strategic Factors That May Contribute to Ineffective Performance**

Intelligence and Job Knowledge Problems
- Insufficient verbal ability
- Insufficient special ability
- Insufficient job knowledge
- Defect of judgment or memory

Emotional Problems
- Frequent disruptive emotion: anxiety, depression, anger, excitement, shame, guilt, jealousy
- Neurosis: with anxiety, depression, anger predominating
- Psychosis: with anxiety, depression, anger predominating
- Alcohol and drug problems

Motivational Problems
- Strong motives frustrated at work: pleasure in success, fear of failure, avoidance motives, dominance, desire to be popular, social motivation, need for attention
- Unintegrated means used to satisfy strong motives
- Low personal work standards
- Generalized low work motivation

Physical Problems
- Physical illness or handicap, including brain disorders
- Physical disorders of emotional origin
- Inappropriate physical characteristics
- Insufficient muscular or sensory ability or skill

Family-Related Problems
- Family crises: divorce, death, severe illness
- Separation from the family and isolation
- Predominance of family considerations over job demands

Work-Group Problems
- Negative consequences of group cohesion
- Ineffective management
- Inappropriate managerial standards or criteria

Organizational Problems
- Insufficient organizational action
- Placement error
- Organizational overpermissiveness
- Excessive spans of control
- Inappropriate organizational standards and criteria

Society-Related Problems
- Application of legal sanctions
- Other enforcement of societal values, including the use of inappropriate value-based criteria
- Conflict between job demands and cultural values: equity, freedom, moral and religious values

Problems Related to the Work Situation
- Negative consequences of economic forces
- Negative consequences of geographic location
- Detrimental conditions in the work setting
- Excessive danger
- Problems inherent in the work itself

meet, and thus were judged ineffective. This was a purely psychological approach to role behavior predicated on individual differences. The more sociological view that roles mandate individual behavior did not come to my attention until several years later (Faris 1962). For a treatment of the current status of role theory in organizational behavior see Ashforth (2001).

**Components of Managerial Motivation**

The components of managerial motivation that were identified in the items were:

- Favorable attitudes to superiors
- Desire to compete
- Desire to exercise power
- Desire to perform in the masculine role, and thus to assert oneself
- Desire to be distinct and different
- Desire to perform routine duties responsibly
To the extent scores on the items in each grouping were consistently positive, the person was assumed to possess the motive (or motive constellation) involved. However, many people gave negative responses, often a number of them. This was not merely indicative of a lack of the particular type of motivation; it reflected a desire not to do these things, to avoid such behavior. My theory, following psychoanalytic theory, was that this occurred because the type of motivation aroused by the role requirement had become associated with anxiety and guilt. Consequently, the motives were driven to the level of the unconscious in order to avoid recognizing them. Yet this attempt may be only partially successful, and the negative emotion persists.

The underlying dynamics involved here are described in more detail in Miner (1975). The point is that informal role requirements involved in managerial work of the kind performed at Atlantic can elicit phobic reactions to the managerial situation. My theorizing in this respect derived primarily from counseling managers whose performance had proved unsatisfactory.

When this theory, as operationalized in what came to be called the Miner Sentence Completion Scale (Form H), was put to test at Atlantic it proved to have considerable validity (Miner 1965). Correlations with success criteria as high as the low .40s were obtained, and a majority of the components of managerial motivation were supported. Furthermore, when the test was used in a pretest-posttest design with experimental and control groups to evaluate managerial role motivation training, it proved capable of identifying the hypothesized changes.

The Theory Applied to Bureaucracies Generally

For some time my impression was that the theory we had developed and supported was simply applicable to Atlantic, or perhaps the oil industry. After all, it had been created to model the Atlantic situation and all of the testing had been done there, too. Then, after moving to a business school, I began to be exposed to the sociological literature on organizations, not just the psychological, and ultimately to read Weber and those who followed him. It soon became apparent to me that this was what I had been talking about all along. Atlantic was not just Atlantic; it was a bureaucracy. This seems simple now, but it was not for me then. It was a revelation.

The result was a long series of studies to test out the theories in new contexts. The very first of these are described in Miner (1965). Many more have been compiled in Miner (1977). Initially most of the research was my own, but gradually it spread to joint authorships, studies by doctoral students, and completely independent investigations. The theory was expanded to state that managerial motivation would explain and predict the success of managers in bureaucratic organizations with multiple levels of hierarchy, but it would not work with nonmanagers, and it would not work in structures other than the bureaucratic.

Gradually it came to my attention that people in the business world and in the business schools were using the managerial motivation construct as an explanatory tool in their writings. Some of these views are included in Miner (1975)—the writings of Marvin Bower (from McKinsey and Company), Sterling Livingston (from the Harvard Business School), Henry Mintzberg (from the business school at McGill University), and Thomas Patten (from the labor school at Michigan State). Again, as with the finding of bureaucracy, the result of these writings was to encourage my faith in the widespread applicability of the theory that began at Atlantic. Without my move into a business school environment, it seems very unlikely that the program of research that emerged would ever have existed; the research would not have expanded at all, and would have remained an Atlantic-specific phenomenon.
Sometime in the mid-1970s, the limitations of the hierarchic theory began to bother me. That theory was restricted to the domain of bureaucratic organizations, and there were other organizational forms to which it did not apply. My attempts to wrestle with this problem settled first on a concept of control hypothesized to operate broadly through various organizational systems (see Miner 1977, ch. 28). Later, the idea of inducement systems was substituted (Miner 1980a), and specific role prescriptions and their motivational bases were identified. Finally, research hypotheses were specifically stated:

**Hypothesis 1.** In hierarchic systems, managerial motivation should be at a high level in top management and it should be positively correlated with other managerial success indexes; managerial motivation should not differentiate in these ways within other types of systems.

**Hypothesis 2.** In professional systems, professional motivation should be at a high level among senior professionals, and it should be positively correlated with other professional success indexes; professional motivation should not differentiate in these ways within other types of systems.

**Hypothesis 3.** In group systems, group motivation should be at a high level among emergent leaders, and it should be positively correlated with other group-determined success indexes; group motivation should not differentiate in these ways within other types of systems.

**Hypothesis 4.** In task systems, task (achievement) motivation should be at a high level among task performers (entrepreneurs, for example), and it should be positively correlated with task success indexes; task motivation should not differentiate in these ways within other types of systems. (Miner 1982, 298)

**Professional Theory**

The professional system did not arise out of test items as the hierarchic theory did. Specific informal role prescriptions were developed and motivational patterns that fit them were then stated. The source of these formulations was in part my own professional experience as a psychologist and university professor. However, substantial input came from the existing literature, primarily of a sociological nature (Etzioni 1964; Satow 1975; Vollmer and Mills 1966; Sorensen and Sorensen 1974; and others). Again, exposure to this literature came only from being in a professional school. Had my experience been limited to the disciplinary confines of psychology, the existence of this literature almost certainly would have remained forever hidden.

The components of professional motivation thus identified were:

Desire to learn and acquire knowledge
Desire to exhibit independence
Desire to acquire status
Desire to help others
Value-based identification with the profession

A measure of the motives involved was constructed in much the same manner as for the hierarchic theory, except that the five motive patterns were known at the outset and
this fact guided the selection of items. The resulting instrument, the Miner Sentence Completion Scale (Form P), was used in a subsequent validation study among faculty members of the Academy of Management. Correlations with professional success criteria were consistently high; in contrast, Form H correlations with these same criteria were rarely significant. The theory appeared to have considerable support in this sample consisting almost entirely of business school professors (Miner 1980b). It has been supported in other studies as well.

**Task Theory**

Task theory is basically an adaptation of McClelland's (1961) work on achievement motivation, work that I had been following since my graduate student days. Thus, the theory itself is essentially psychological. However, the research we have done in the entrepreneurship area to test the theory has been strongly influenced by Norman Smith, a marketing professor at the University of Oregon. We have been collaborating on research since the latter 1960s, and Smith's intimate knowledge of entrepreneurship has been a major factor in whatever success our studies have achieved; he has taught me a great deal. A nother plus for business schools.

Initially, we set out to test the hypothesis that entrepreneurs found companies to have something to manage, and thus managerial motivation should be the major source of entrepreneurship accomplishment. This hypothesis had only limited success at best (Smith and Miner 1983). From there we turned to task theory using McClelland (1961) as a guide and influenced by the marked success achievement motivation has shown over the years as a predictor of entrepreneurial accomplishment. Johnson (1990) reports on twenty-three studies conducted since 1960, twenty of which demonstrate some relation to entrepreneurial activity.

The components of our theory were as follows:

Desire to achieve through one's own efforts
Desire to avoid risk
Desire for feedback on performance
Desire to introduce innovative solutions
Desire to plan and establish goals

A measure was constructed using the sentence completion format—the Miner Sentence Completion Scale (Form T). This in turn was employed in a major study of high-tech entrepreneurs. Support was obtained for the theory as a whole, and for all five of its components (Miner, Smith, and Bracker 1989). Subsequent work has continued to indicate the validity of the theory in the entrepreneurial domain. The marketing professor, Norman Smith, was instrumental in locating almost all of our research samples.

A related program of theory and research further serves to emphasize the extent to which being in a business school setting has facilitated my work. In this instance, data were derived from an entrepreneurial leadership development program and from an MBA course in entrepreneurship—both of which were then unique to the business school context. The theory sets forth a typology of entrepreneurs consisting of personal achievers, real managers, expert idea generators, and empathic supersalespeople (Miner 1997). Although the Miner Sentence Completion Scales are used in this research, the overall theory itself is not a role motivation theory.
GROUP THEORY

Group role motivation theory exists as a theory, but the motives involved have not been operationalized and the theory has not been tested, as of this writing. The theory is almost completely a consequence of the literature on autonomous work groups, sensitivity training, and organization development. My own experience with organizational systems of this type is limited, in contrast to hierarchic, professional, and entrepreneurial organizations, where my experience has been substantial. Thus, input from personal exposure is a lesser factor here. The literature that exerted the most influence is in the tradition of Kurt Lewin—from the Massachusetts Institute of Technology, the University of Michigan, and the Tavistock organization in England (see Chapter 8). It is basically a behavioral humanist literature and there have been a great many contributors.

The components of the theory derived from this literature are:

- Desire to interact socially and affiliate with others
- Desire for continuing belongingness in a group
- Favorable attitudes toward peers
- Desire to have cooperative/collaborative relationships
- Desire to participate in democratic processes

ORGANIZATIONAL TYPES

The previous discussion, and the research described, have focused at the level of individual motivation utilizing primarily the various Miner Sentence Completion Scales. But the theories are meso theories dealing as much with organizational forms, and the informal role requirements they posit, as with intrinsic motive patterns. In the early years, we made certain assumptions about the type of organizations we were dealing with and thus the types of motive patterns that should and should not prove appropriate. We appear to have been correct in these assumptions most of the time. Otherwise, the predictor-criterion relationships found would not have been in evidence.

Given that the theories predict particular types of person-organization fit, if role behavior and role requirements are to mesh, there is a clear need for a measure of the four organization types. This became evident in the late 1970s as the three organizational forms beyond the hierarchic were developed. As a result, Oliver (1982) created an instrument, the Oliver Organization Description Questionnaire, to differentiate the four organizational forms. In this instance, the measurement expertise needed was psychological in nature, and in fact we called upon a psychometrician in the psychology department to help us in this regard. But writing the items also required a broad knowledge of organizational types and their characteristics. This is not an instrument that would have been generated at the time within a psychological context.

We have used Oliver’s questionnaire extensively in research we have conducted (see Miner, Crane, and Vandenberg 1994; Wilderom and Miner 1991). It has proved very helpful in identifying organization-wide and unit structures, and thus the particular theoretical domain involved.

MORE RECENT THEORETICAL HISTORY

The role motivation theories are very much creatures of social science in the business schools and the development of organizational behavior. Theory, research, and the milieu of the time...
are all intricately intertwined. Certainly, for all its conflict and inherent uncertainty, the business school environment served as a catalyst in this instance for whatever creative contribution was achieved. There is every reason to believe that the same stimulus to intellectual output operated in the case of many other theories as well.

The various role motivation theories have received considerable attention over the years since their development; they continue to do so up to the present (see Yukl 2002; Shane, Locke, and Collins 2003; Vecchio 2003). The group theory remains underdeveloped, although some research has appeared that seems to bear on the theory’s hypotheses in a favorable manner (Taggar, Hackett, and Saha 1999; Shaw, Duffy, and Stark 2000). Role motivation theorizing has been extended into the leadership domain and also used to deal with aspects of career choice (Foti and Miner 2003; Miner 2002). Although there have been certain criticisms over the years, these have been considered at length in Miner (1993b), and there is no need to elaborate here.

My intent has been to show how, as organizational behavior survived and grew in its early years, role motivation theory also grew; in large part, this was a consequence of the facilitating effects of the business school context. I have not attempted to discuss this process as it occurred after the first twenty-five years of organizational behavior’s history in any detail. However, one additional episode may serve to emphasize the point that the business school setting continued to make a significant contribution.

Shortly after my arrival at the State University of New York at Buffalo, it came to my attention that our business school had an exchange relationship to develop business instruction at Dalian University of Technology in China. As a consequence of this relationship, it proved possible to carry out a research program to investigate the relevance of hierarchic role motivation theory in China. Ultimately, we were able to extend the theory beyond the borders of the United States and thus to define the bureaucratic form more broadly (Miner, Chen, and Yu 1991; Chen, Yu, and Miner 1997). Clearly, the business school context continues in its catalytic role.

CONCLUSIONS

There is repeated evidence that organizational behavior was born in a context of high uncertainty and instability. Predicting the future was difficult, if not impossible. The long period of unfreezing created a conglomeration of disciplines and ideas, many of which had never been in close proximity to one another before. For many people exposure to this environment was very stressful; the number who escaped to alternative careers was not insignificant.

Yet others found the opportunities, the swirl of ideas, and the rewards attractive. Not only that, but a substantial creative outpouring occurred from these people. In presenting my own experience, I hope to have contributed a better understanding of what happened during this period—as organizational behavior came into being.

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PART IV

THE IMPORTANCE, VALIDITY, PRACTICAL USEFULNESS, AND INSTITUTIONALIZATION OF ORGANIZATIONAL BEHAVIOR THEORIES

Drawing on the thrust of the previous chapter, Part IV seeks first of all to provide an indication of how theories of various types and vintages have led the development of organizational behavior over the past fifty years. At various points in their histories, scientific disciplines such as organizational behavior need to take stock of their progress and to use the information thus marshaled as feedback to readjust their goals and approaches. An assessment of a field’s existing theories provides an ideal opportunity to accomplish this goal. That is what I attempt to do here. Specifically, the objective is to take stock of seventy-three established theories of organizational behavior broadly defined, and thus to determine how organizational behavior has fared in terms of this core aspect of any discipline.

My objective in this part is the same as that sought in an earlier evaluation of much the same kind conducted more than twenty-five years ago (see Miner 1984, 1990). Then, existing theories were surveyed as to their importance, and additional data on validity and practical usefulness were incorporated, to provide an overall assessment of the field from a theoretical perspective. Now, I attempt to do the same thing, drawing heavily on an interim report (Miner 2003). This is an update on the previous analyses, which takes into account the growth and other changes that have occurred in organizational behavior over the years and extends the analysis in several new directions. Yet again, as previously, my intent is simply to determine whether organizational behavior has succeeded in developing a set of “good” theories to lead the field’s scientific advance.

Some time ago, Kurt Lewin (see Chapter 8) indicated that “nothing is so practical as a good theory.” This statement is often treated as something of a dictum; it has continued to hold force over the years (Lundberg 2004). However, if “practical” is considered as meaning that which is useful in an applied setting to accomplish some goal and “good theory” refers to theory of a kind that yields valid scientific knowledge, then Lewin’s statement may be treated as a testable hypothesis. Testing a hypothesis of this kind is a second objective of Part IV. What follows, then, is essentially a review of theoretical positions in the organizational behavior field, and of research and opinion related to their importance and validity, to include research and opinion bearing on their practical usefulness. Thus, it bears on the posi-
tion taken by Christensen and Raynor (2003) in their recent Harvard Business Review article favoring the practical relevance of theory.

A third contribution to which this part aspires is to provide information on the extent to which consensus prevails and institutionalization operates with regard to organizational behavior’s theories, and thus, to determine whether a solid knowledge base exists within the field. A widespread belief prevails to the effect that the degree of consensus among knowledgeable scholars that marks a mature science and produces positive consequences for members is lacking in organizational science (see Pfeffer 1993). My goal here is to test this hypothesis and accordingly to provide information on the legitimacy of organizational behavior as a field.

Fourth, I attempt to compare data from the past, and in particular from Miner (1984), with the current scene. What has changed? Has there been any improvement in the status of our theories? Is organizational behavior moving toward or away from maturity? How do comparisons over time contribute to the development of mandates for the future?

A final objective is to provide assistance to those who wish to learn about organizational behavior, and to teach the subject, in selecting which theoretical content to emphasize. My goal in this instance is to reduce the number of theories to be learned to a manageable number, and to concentrate on those that are most worthy.

As a prelude to the actual presentation of the methods used in this analysis and of the findings, I need to place the seventy-three theories in historical context. In what time period did these theories originate, relative to the initiation of organizational behavior in the mid-1950s? Previous discussions have indicated that a burst of theoretical activity occurred as organizational behavior came on the scene, but exactly how these new theories were distributed over time remains to be established. How, indeed, did bringing previously unrelated ideas and subject matters together in the business schools actually affect theoretical output? The time of origin of the seventy-three theories was as follows:

<table>
<thead>
<tr>
<th>Time Period</th>
<th>Percentage</th>
</tr>
</thead>
<tbody>
<tr>
<td>Preorganizational behavior</td>
<td>10%</td>
</tr>
<tr>
<td>1940s</td>
<td>3%</td>
</tr>
<tr>
<td>1950s</td>
<td>14%</td>
</tr>
<tr>
<td>1960s</td>
<td>31%</td>
</tr>
<tr>
<td>1970s</td>
<td>29%</td>
</tr>
<tr>
<td>1980s</td>
<td>12%</td>
</tr>
<tr>
<td>1990s</td>
<td>1%</td>
</tr>
</tbody>
</table>

These are initial publication dates, and almost all of the theories continued to yield contributions over a number of years—many of them up to the present. The median date of origin is in the 1960s. In general, these data confirm that a real burgeoning of theoretical output did occur in the 1960s and 1970s, shortly after organizational behavior emerged. However, the drop-off in recent years should be assessed taking into account the fact that it takes a number of years for theories to be tested via research, or at least to be given an opportunity for adequate testing. Thus, very recent theories are unlikely to find their way into a listing of this kind; a period of at least ten to fifteen years seems to be required.

One additional point should be noted as well. Of the seventy-three theories, forty are considered to be essential and are treated in previous volumes of this series (Miner 2005, 2006). The remaining thirty-three are not in the essential category and have not been updated as recently; they are discussed only in Miner (2002). A comparison of the dates of
origin of these two groups of theories indicates no significant differences; the median date for both is in the 1960s. Nevertheless, the largest differential, amounting to 10 percent, occurs in the preorganizational behavior period, with essential theories being less frequent then and theories not considered essential being the more likely. This is consistent with the fact that in this period organizational behavior had not yet become a science.

REFERENCES

IMPORTANCE OF ORGANIZATIONAL BEHAVIOR THEORIES
CHAPTER 17

METHODOLOGY OF THE IMPORTANCE, VALIDITY, AND USEFULNESS ANALYSES

Establishing the Theories to Be Studied
Establishing Judges to Assess the Theories
Establishing Key Variables to Be Measured
   Rated Importance of Theories
   Failure to Rate a Theory
   Estimated Scientific Validity
   Estimated Usefulness in Practice
   Thoughts on Measuring Validity and Usefulness
Establishing That the Group of Judges Is Representative
Establishing That the List of Theories Studied Is Complete
Differences Between Groups of Judges
   Organizational Behavior versus Strategic Management
   Fifty Years of Age and Over versus Under Fifty

This chapter and the next take up the importance, validity, and usefulness analyses with the methodology considered here and the findings in Chapter 18.

ESTABLISHING THE THEORIES TO BE STUDIED

The theories that judges were asked to evaluate were accumulated in several different ways. A number had been evaluated previously in Miner (1984) where the selection process was guided by a survey of knowledgeable scholars of the time who nominated theories for inclusion. To this list was added an even larger group of theories that also met the criteria that (1) the author(s) had produced substantial theoretical work; (2) this theoretical work is identified with the field of organizational behavior (OB); and (3) the theory is recognized as significant within organizational behavior. The third point was addressed by drawing on various published sources, including Bedeian (1992–98), Pugh and Hickson (1993), Pollard (1974, 1978), Wren and Greenwood (1998), Tosi (1984), Mathur (1990), and Donaldson (1995).

These theories may be divided with regard to content into categories as preorganizational behavior (seven), motivation (and perception) (fifteen), leadership (eighteen), organizational decision making (six), systems concepts of organization (ten), bureaucracy-related concepts (twelve), and sociological concepts of organization (five). Thus, among those theories that extend beyond the more general preorganizational behavior period, thirty-three are predominantly micro in nature and thirty-three are more macro—an even division that was not planned, but rather emerged out of the theory selection process. The seventy-three theories are listed by name and author(s) in Table 17.1. Some theories have different authors; some authors have more than one distinct theory. Of these theories, seventeen have substantial ties to countries
other than the United States, consistent with the fact that the published sources used to select theories were in a number of cases written by authors from outside the United States as well. The essential theories, considered to be more worthy of attention, noted previously, are forty in number. In Table 17.1 they are the following:

Preorganizational behavior—numbers 3 and 5
Motivation (and perception)—numbers 8 through 17
Leadership—numbers 23 through 30
Organizational Decision Making—numbers 41 through 45
Systems Concepts of Organizations—numbers 47 through 52
Bureaucracy-related Concepts—numbers 57 through 60
Sociological Concepts of Organization—numbers 69 through 73

ESTABLISHING JUDGES TO ASSESS THE THEORIES

The sources of the judges were essentially the same as those used in the prior analysis some twenty-plus years before—past presidents of the Academy of Management, past editors of the Academy of Management Journal, past editors of the Academy of Management Review, and editorial (review) board members from both of these publications (in this instance for the years 1999 and 2000). This process produced 226 individuals who were contacted by mail, of whom 95 provided usable responses for a 42 percent response rate; this contrasts with a 35 percent response in the earlier study (Miner 1984). Two rounds of mailings were involved, with the first round yielding sixty-seven judges who replied from November 2000 through January 2001, and the second round twenty-eight judges who replied during February and March 2001.

The age range of the judges was from thirty-two to seventy-four years, with a mean of 48.1 years. The sex breakdown was 78 percent male and 22 percent female. There were seventeen who resided outside the United States in nine countries. All appear to have held doctorates. Data were obtained on the subject area of the doctorate as well as on areas of research specialization, teaching specialization, and consulting specialization. From this information, judges were assigned to groups as having primary expertise in either strategic management or organizational behavior. The strategic management group numbered twenty-four, with 67 percent having degrees in that field and another 17 percent in management. The seventy-one organizational behavior specialists had 49 percent of their degrees in OB, 21 percent in psychology (industrial/organizational or social), and 15 percent in human resources or industrial relations. There was some, but quite limited, overlap across groups, in that 29 percent of the strategic management group indicated some OB activity and 13 percent of the organizational behavior group indicated some strategic activity. Nevertheless, these two groups were clearly distinct; they were the only distinct groups of any size that could be identified. Thus, strategic management, in addition to organizational behavior, was studied because the data available made it possible to do so.

ESTABLISHING KEY VARIABLES TO BE MEASURED

Rated Importance of Theories

Each judge was asked to rate each of the seventy-three theories on a seven-point scale of importance from low (1) to high (7). A theory was defined as including models, definitional
Table 17.1

Theories Included in Study Sample and Evaluation Data for Each

<table>
<thead>
<tr>
<th>Theories evaluated</th>
<th>Mean importance rating</th>
<th>Percent who did not evaluate</th>
<th>Estimated usefulness in practice</th>
<th>Institutionalization score</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>organizational behavior (N = 71)</td>
<td>strategic management (N = 24)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Preorganizational Behavior</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>1. Conceptualizations derived from the Hawthorne Studies (Elton Mayo, Fritz Roethlisberger, William Dickson)</td>
<td>4.65</td>
<td>4.08</td>
<td>0</td>
<td>2</td>
</tr>
<tr>
<td>2. The Functions of the Executive Concepts (Chester Barnard)</td>
<td>4.41</td>
<td>4.09</td>
<td>5</td>
<td>2</td>
</tr>
<tr>
<td>3. Social Psychological Views of Leadership and Change (Kurt Lewin)</td>
<td>5.31</td>
<td>4.60</td>
<td>7</td>
<td>4</td>
</tr>
<tr>
<td>4. Social Philosophy and Prophetic Statements on Management (Mary Parker Follett)</td>
<td>3.28</td>
<td>2.76</td>
<td>25</td>
<td>2</td>
</tr>
<tr>
<td>5. Theory of Bureaucracy (Max Weber)</td>
<td>5.90*</td>
<td>5.26</td>
<td>2</td>
<td>4</td>
</tr>
<tr>
<td>6. General and Industrial Management Formulations (Henri Fayol)</td>
<td>3.73*</td>
<td>2.76</td>
<td>13</td>
<td>2</td>
</tr>
<tr>
<td>7. Scientific Management Formulations (Frederick Taylor)</td>
<td>4.63</td>
<td>3.95</td>
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<td>2</td>
</tr>
<tr>
<td>Motivation (and perception)</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>8. Achievement Motivation Theory (David McClelland)</td>
<td>5.15*</td>
<td>4.00</td>
<td>5</td>
<td>4</td>
</tr>
<tr>
<td>9. Motivation-Hygiene Theory (Frederick Herzberg)</td>
<td>3.81</td>
<td>3.67</td>
<td>5</td>
<td>2</td>
</tr>
<tr>
<td>10. Job Characteristics Theory (Richard Hackman, Edward Lawler, Greg Oldham)</td>
<td>5.61*</td>
<td>4.10</td>
<td>5</td>
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<tr>
<td>11. Expectancy Theory—Work and Motivation (Victor Vroom)</td>
<td>5.96*</td>
<td>4.55</td>
<td>3</td>
<td>5</td>
</tr>
<tr>
<td>12. Expectancy Theory—Managerial Attitudes and Performance (Lyman Porter, Edward Lawler)</td>
<td>5.41*</td>
<td>4.71</td>
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<td>5</td>
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<tr>
<td>13. Operant Behavior and Reinforcement Theory (Clay Hamner)</td>
<td>4.25</td>
<td>3.35</td>
<td>12</td>
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</table>

(continued)
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<tr>
<th>Theories evaluated</th>
<th>Mean importance rating</th>
<th>organizational behavior (N = 71)</th>
<th>Percent who did not evaluate</th>
<th>Estimated validity</th>
<th>Estimated usefulness in practice</th>
<th>Institutionalization score</th>
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<tbody>
<tr>
<td>14. Organizational Behavior Modification (Fred Luthans, Robert Kreitner)</td>
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<td>2.75</td>
<td>13</td>
<td>4</td>
<td>4</td>
<td>+4</td>
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<tr>
<td>15. Equity Theory (Stacy Adams)</td>
<td>5.93*</td>
<td>4.26</td>
<td>6</td>
<td>4</td>
<td>3</td>
<td>+13*</td>
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<tr>
<td>16. Goal Setting Theory (Edwin Locke, Gary Latham)</td>
<td>5.97*</td>
<td>4.19</td>
<td>4</td>
<td>5</td>
<td>4</td>
<td>+22*</td>
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<tr>
<td>17. Attribution Theory—Managerial Perceptions of the Poor Performing Subordinate</td>
<td>4.18*</td>
<td>3.38</td>
<td>12</td>
<td>4</td>
<td>2</td>
<td>-3</td>
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<tr>
<td>(Terence Mitchell, Stephen Green)</td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>18. Need Hierarchy Theory (Abraham Maslow)</td>
<td>4.14</td>
<td>4.17</td>
<td>2</td>
<td>2</td>
<td>2</td>
<td>-1</td>
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<tr>
<td>19. Existence, Relatedness, and Growth Theory (Clayton Alderfer)</td>
<td>3.58</td>
<td>2.76</td>
<td>15</td>
<td>3</td>
<td>1</td>
<td>-3</td>
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<tr>
<td>20. Psychoanalytic Theory Applied to Organizations (Harry Levinson)</td>
<td>2.84</td>
<td>2.40</td>
<td>23</td>
<td>3</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>22. Theory of Behavior in Organizations (James Naylor, Robert Pritchard, Daniel Ilgen)</td>
<td>3.94*</td>
<td>3.00</td>
<td>28</td>
<td>3</td>
<td>1</td>
<td>-1</td>
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<tr>
<td><strong>Leadership</strong></td>
<td></td>
<td></td>
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<td></td>
<td></td>
</tr>
<tr>
<td>24. Contingency Theory of Leadership (Fred Fiedler)</td>
<td>4.33</td>
<td>3.81</td>
<td>5</td>
<td>3</td>
<td>3</td>
<td>+1</td>
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<tr>
<td>25. Vertical Dyad Linkage/Leader Member Exchange Theory (George Graen)</td>
<td>4.69*</td>
<td>3.13</td>
<td>16</td>
<td>5</td>
<td>3</td>
<td>-3</td>
</tr>
<tr>
<td>26. Information Processing Theory of Leadership (Robert Lord)</td>
<td>3.84*</td>
<td>2.36</td>
<td>27</td>
<td>4</td>
<td>1</td>
<td>-2</td>
</tr>
<tr>
<td>27. Substitutes for Leadership (Steven Kerr)</td>
<td>4.46*</td>
<td>3.39</td>
<td>15</td>
<td>3</td>
<td>3</td>
<td>0</td>
</tr>
<tr>
<td>28. Role Motivation Theory (John Miner)</td>
<td>4.05</td>
<td>3.71</td>
<td>25</td>
<td>4</td>
<td>4</td>
<td>-3</td>
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<tr>
<td>29. Charismatic Leadership Theory (Robert House)</td>
<td>4.76*</td>
<td>3.30</td>
<td>8</td>
<td>4</td>
<td>2</td>
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<tr>
<td>No.</td>
<td>Theoretical Model</td>
<td>Importance</td>
<td>Validity</td>
<td>Usefulness</td>
<td>Methodology</td>
<td>Notes</td>
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<td>30.</td>
<td>Transformational and Transactional Leadership Theory</td>
<td>5.06*</td>
<td>3.52</td>
<td>5</td>
<td>4</td>
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<tr>
<td></td>
<td>(Bernard Bass)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
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<tr>
<td>31.</td>
<td>Theory X and Theory Y (Douglas McGregor)</td>
<td>4.39</td>
<td>3.65</td>
<td>1</td>
<td>1</td>
<td>3</td>
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<td>32.</td>
<td>Consideration and Initiating Structure (John Hemphill, Ralph Stogdill, Carroll Shartle)</td>
<td>4.60*</td>
<td>3.17</td>
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<td>Managerial Grid Theory of Leadership (Robert Blake, Jane Mouton)</td>
<td>2.98</td>
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<td>34.</td>
<td>Situational Leadership Theory (Paul Hersey, Kenneth Blanchard)</td>
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<td>3.18</td>
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<tr>
<td>35.</td>
<td>Path-Goal Relationship Theory (Martin Evans)</td>
<td>4.11</td>
<td>3.50</td>
<td>19</td>
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<td>36.</td>
<td>Path Goal Theory of Leader Effectiveness (Robert House)</td>
<td>4.58*</td>
<td>3.62</td>
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<td>37.</td>
<td>Leadership Pattern Choice Theory (Robert Tannenbaum, Warren Schmidt)</td>
<td>3.02</td>
<td>2.67</td>
<td>37</td>
<td>2</td>
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<td>38.</td>
<td>Influence Power Continuum Theory (Frank Heller)</td>
<td>2.71</td>
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<td>Cognitive Resource Theory (Fred Fiedler, Joseph Garcia)</td>
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<td>2.76</td>
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<td></td>
<td>Organizational Decision Making</td>
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<td>41.</td>
<td>Theory of Administrative Behavior/Organizations (Herbert Simon, James March)</td>
<td>5.81</td>
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<td>Behavioral Theory of the Firm (Richard Cyert, James March)</td>
<td>5.43</td>
<td>6.08*</td>
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<td>Garbage Can Model of Organizational Choice (Michael Cohen, James March, Johan Olsen)</td>
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<td>Systems Concepts of Organizations</td>
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<td>47.</td>
<td>Control Theory Using the Control Graph (Arnold Tannenbaum)</td>
<td>3.58</td>
<td>3.07</td>
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<td>Theories evaluated</td>
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<td>Percent who did not evaluate</td>
<td>Estimated validity</td>
<td>Estimated usefulness in practice</td>
<td>Institutionalization score</td>
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<tr>
<td>48. Social Psychology of Organizations (Daniel Katz, Robert Kahn)</td>
<td>5.33</td>
<td>4</td>
<td>3</td>
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<td>+7</td>
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<tr>
<td>49. Sociotechnical Systems Theory (Eric Trist, Fred Emery)</td>
<td>5.09*</td>
<td>8</td>
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<td>+5</td>
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<td>50. Sociological Open Systems Theory—Organizations in Action (James Thompson)</td>
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<td>5</td>
<td>3</td>
<td>1</td>
<td>+6*</td>
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<tr>
<td>51. Mechanistic and Organic Systems (Tom Burns, G.M. Stalker)</td>
<td>5.42*</td>
<td>2</td>
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<td>1</td>
<td>+6*</td>
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<tr>
<td>52. Contingency Theory of Organization—Differentiation and Integration (Paul Lawrence, Jay Lorsch)</td>
<td>5.39</td>
<td>2</td>
<td>3</td>
<td>4</td>
<td>+4*</td>
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<tr>
<td>53. Theory of Systems 1 to 4 and 4T (Rensis Likert)</td>
<td>3.66</td>
<td>14</td>
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<td>54. Group Focused Systems Theory (Ralph Stogdill)</td>
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<td>39</td>
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<tr>
<td>55. Technological Determinism (Joan Woodward)</td>
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<td>1</td>
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<tr>
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**Bureaucracy-related Concepts**

<table>
<thead>
<tr>
<th>Theories evaluated</th>
<th>Mean importance rating</th>
<th>Percent who did not evaluate</th>
<th>Estimated validity</th>
<th>Estimated usefulness in practice</th>
<th>Institutionalization score</th>
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<tbody>
<tr>
<td>57. Theory Undergirding the Aston Studies (Derek Pugh, David Hickson, Christopher Hinings)</td>
<td>4.28</td>
<td>12</td>
<td>4</td>
<td>3</td>
<td>-2</td>
</tr>
<tr>
<td>58. Theory of Differentiation in Organizations (Peter Blau)</td>
<td>4.31</td>
<td>13</td>
<td>4</td>
<td>3</td>
<td>-2</td>
</tr>
<tr>
<td>59. Goal Congruence Theory—Personality and Organization (Chris Argyris)</td>
<td>4.38</td>
<td>7</td>
<td>3</td>
<td>3</td>
<td>-2</td>
</tr>
<tr>
<td>60. Theory of Organizational Culture and Leadership (Edgar Schein)</td>
<td>4.85*</td>
<td>8</td>
<td>2</td>
<td>4</td>
<td>-3</td>
</tr>
<tr>
<td>61. Structural Contingency Theory (Lex Donaldson)</td>
<td>4.33</td>
<td>14</td>
<td>3</td>
<td>2</td>
<td>0</td>
</tr>
<tr>
<td>62. Dysfunctions of Bureaucracy (Victor Thompson)</td>
<td>3.38</td>
<td>2.71</td>
<td>27</td>
<td>3</td>
<td>3</td>
</tr>
<tr>
<td>63. Compliance Theory (Amitai Etzioni)</td>
<td>3.95</td>
<td>3.53</td>
<td>23</td>
<td>3</td>
<td>1</td>
</tr>
<tr>
<td>64. Theory of Organizational Learning and Defensive Routines (Chris Argyris)</td>
<td>4.23</td>
<td>4.10</td>
<td>12</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>65. Theory of Bureaucratic Demise (Warren Bennis)</td>
<td>2.75</td>
<td>2.41</td>
<td>32</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>66. Grid Organization Development (Robert Blake, Jane Mouton)</td>
<td>3.03</td>
<td>3.00</td>
<td>17</td>
<td>1</td>
<td>3</td>
</tr>
<tr>
<td>67. Process Consultation Theory of Organization Development (Edgar Schein)</td>
<td>4.02</td>
<td>3.50</td>
<td>16</td>
<td>2</td>
<td>3</td>
</tr>
<tr>
<td>68. Alpha, Beta, and Gamma Change in Organization Development (Robert Golembiewski)</td>
<td>3.77*</td>
<td>2.31</td>
<td>31</td>
<td>3</td>
<td>3</td>
</tr>
</tbody>
</table>

*Sociological Concepts of Organizations*

| 69. External Control of Organizations—Resource Dependence Perspective (Jeffrey Pfeffer, Gerald Salancik) | 5.29 | 5.50 | 3 | 5 | 3 | +7 |
| 70. Neoinstitutional Theory—Institutional Environments and Organizations (John Meyer, Richard Scott) | 4.79 | 4.83 | 17 | 5 | 3 | +1 |
| 71. Neoinstitutional Theory—Institutionalization and Cultural Persistence (Lynne Zucker) | 4.51 | 5.00 | 20 | 4 | 2 | +6 |
| 72. Neoinstitutional Theory—Institutionalism in Organizational Analysis (Walter Powell, Paul DiMaggio) | 5.22 | 5.35 | 23 | 3 | 2 | +9 |
| 73. Organizational Ecology and Demography (Michael Hannan, John Freeman, Glenn Carroll) | 4.88 | 4.96 | 3 | 4 | 2 | +1 |

* Significantly greater (at p < .05 or better) than comparison sample.
systems, analytical schemata, and powerful constructs. The criteria indicated to evaluate a theory as important were that (1) the theory should have proved useful in understanding, explaining, and predicting the functioning of organizations or the behavior of people in them; (2) the theory should have generated significant research; and (3) the theory should have clear implications for practice and application in some area of management or organizational functioning. However, respondents were asked to utilize any other criteria of importance that they might consider useful as well. Thus, the importance ratings were expected to subsume strongly held values and institutionalization processes in addition to the three criteria specifically noted. Importance was selected for rating because it had been measured in the prior study (Miner 1984) and because it has the potential for incorporating values and institutional processes (Miner 1990).

The mean importance ratings given by the organizational behavior component \((N = 71)\)—those considered to be the most knowledgeable scholars with regard to a sample of organizational behavior theories—and by the strategic management judges \((N = 24)\) are presented in Table 17.1. There the organizational behavior means range from 2.71 to 5.97 and the strategic management means from 2.00 to 6.08.

Clearly, the ratings do discriminate well among the various theories. But do they discriminate in the same manner as the nominations used in the earlier study did? This is an empirical question that can best be answered by correlating the frequency of nomination for the thirty-four theories considered in 1977 with the mean rating given by the organizational behavior judges in 2000–2001 to the same theories. There were few judges with a strategic management specialization included in the early group, and accordingly, this is the appropriate comparison. The correlation is a highly significant .49**.

This is not to say that certain theories have not changed in the importance rankings over the years. In particular, Weber’s theory of bureaucracy (no. 5), Locke’s goal setting theory (no. 16), Graen’s vertical dyad linkage theory (no. 25), and Burns and Stalker’s theory of mechanistic and organic systems (no. 51) have increased their status, while Maslow’s need hierarchy theory (no. 18), Fiedler’s contingency theory of leadership (no. 24), Evans’s version of path goal theory (no. 35), and Likert’s theory of system 1 to 4 and 4T (no. 53) have declined the most. Yet, the nomination process and the rating procedures used in the present study seem to have maintained a substantial relationship to one another, even over twenty-plus years, thus setting a lower bound for the reliability of rating. Further evidence on the reliability of the importance ratings comes from the correlation of the mean of the first-round ratings of each theory (the first sixty-seven judges) with the mean for the second-round subjects (the last twenty-eight). This test-retest value across roughly ten weeks is .92**.

Failure to Rate a Theory

It was recognized from the beginning that some of the judges would not be able to rate certain theories, and that this failure-to-rate variable might yield useful information in its own right. Accordingly, after each theory there was a line that read:

**Cannot assess** ___________ Reason? ______________________________

Although not all who checked “Cannot assess,” and thus did not rate, provided a reason, most did. These reasons were as follows:
Not familiar  Unfamiliar  Haven’t studied carefully
Never heard of  Don’t know enough  Not too familiar
No knowledge of  Don’t remember  Unknown
Not sure of  Lack of familiarity  Outside area
Do not know  Unaware

It is apparent that checking “Cannot assess” characteristically meant a lack of knowledge of the particular theory. In addition, there were instances where the item was simply skipped; in such cases, there was less basis for attributing causation; but knowledge deficiencies probably were involved here, too.

Of the ninety-five judges, twenty actually rated all seventy-three theories. From there, the failure-to-rate variable rose to as high as fifty theories, with the mean being 9.78 per judge overall. Of the total possible ratings, 13.4 percent were not made—.7 percent involving skipping and 12.7 percent “Cannot assess.” There was substantial variation from theory to theory on the failure to rate factor, ranging from zero for the Hawthorne studies (no. 1) to forty-five (47%) for Heller’s influence power continuum theory (no. 38). Other theories with close to universal ratings were Porter and Lawler’s version of expectancy theory (no. 12), McGregor’s theory X and theory Y (no. 31), Simon and March’s theory of administrative behavior/organizations (no. 41), and Cyert and March’s behavioral theory of the firm (no. 42). Being in existence for a long time often seems to mean a theory is more likely to be rated. Information on the percentage who did not evaluate each theory is provided in the third column of numbers in Table 17.1.

Estimated Scientific Validity

Estimated scientific validity was a rating made by the author intended to indicate whether “good theory,” and thus improved understanding and prediction, had been attained. The extent of logical consistency and other criteria of “good theory” were invoked here (see Chapter 2), but the key consideration was the extent to which true research tests of the theory had indeed been carried out, and if they had, whether they supported the theory. A five-point scale was used, ranging from (1) at the low end (where the research evidence was either nonsupportive or did not exist to a sufficient degree, in spite of the fact that adequate time had elapsed to permit studies to be conducted) to (5) at the high end (where substantial segments of the theory had been supported by a sizable body of subsequent research). These ratings were based on many years of study of the individual theories and related research; they were made in late 2000 and at that time were not informed by the results of the importance rating procedure. Later (in 2004), these results were reviewed and several upgrades incorporated. The rationales underlying these validity estimates are spelled out in considerable detail first in Miner (2002) and then with even more specificity in the “Conclusions” sections of the chapters of Miner (2005) and Miner (2006). Thus, specific sources exist providing documentation for each rating. Although made by one person, these ratings incorporated the views of many others, both critics and enthusiasts.

All five points on the validity scale were in fact utilized; the mean rating (as revised in 2004) was 3.08 and the distribution was essentially normal. Evidence of the lack of bias inherent in such ratings derives from their relationship to similar ratings provided by others for overlapping theories. Locke and Henne (1986) published data on eight motivation theories that overlapped; the correlation with my early ratings was .94. Lee and Earley (1992) provided validity
data obtained from a survey of 127 scholars on thirteen motivation and leadership theories that overlapped; the correlation with those same ratings was .75. On this limited evidence, it appears that the ratings for estimated scientific validity were themselves valid. Evidence of reliability for the theories rated in 1977 was calculated by correlating these earlier ratings with those given in 2000–2001. This lower bound test-retest value across a twenty-plus year interim, within which much new input to the rating process was absorbed, was .89**.

**Estimated Usefulness in Practice**

The estimated usefulness ratings were also made by the author on a five-point scale extending from (1) at the low end (where the theory clearly had not contributed to practice in any meaningful way, either because applications were not generated or because research and/or experience had proved them essentially useless) to (5) at the high end (where one or more highly viable applications had been generated and shown by research to produce the intended results). These ratings were made in late 2000; they were reviewed in 2004 with the result that several upgrades occurred. A gain, while made by a single person, they utilized the views of many others who had published regarding the theory. These usefulness ratings are documented in Miner (2002) and in much greater detail in Miner (2005, 2006).

A gain all five points on the scale were utilized; the mean rating (as revised in 2004) was 2.52. There was, however, a heavy weighting toward the low end of the scale (1 and 2) and a deficit on the high end (4 and 5). Questions have been raised regarding this type of analysis (Brief and Dukerich 1991), and indeed, comparative data involving theory ratings provided by others are lacking. Nevertheless, I believe that data on the potential for practical application of our theories are needed, and that tests of Lewin’s hypothesis should be carried out periodically. The test-retest lower bound value, obtained in the same manner as for the validity estimate, was .83**.

**Thoughts on Measuring Validity and Usefulness**

The validity and usefulness measures described suffer from the possibility that they are biased in that they derive from the judgments of a single person. In that respect, this review is similar to other literature reviews; the views of the author, as to both selection of the underlying literature and interpretation of that literature, are paramount. The ratings made here derive from extensive study of the writing (critique, research, meta-analyses, etc.) surrounding each theory. This is typically what happens in any review process.

One reason for using this measurement procedure was to replicate the earlier (Miner 1984) study at a twenty-plus year interval. The advantages of this approach in permitting comparisons over time should be evident from the previous discussions. But there were also reasons for not using alternative measurement approaches. Meta-analyses of the research surrounding a theory could have been relied upon, except that this would have severely restricted the number of theories that could have been considered; many theories lack meta-analyses. Furthermore, meta-analyses do not necessarily cover any more studies than a thorough literature search, often fail to weight the better-conducted studies appropriately, and neglect many of the findings from a given study because of the independence requirement. There are even instances where different meta-analyses of the research on a given theory reach conclusions that are at variance with one another (see Miner 2005, 2006). In any event, theory usefulness is rarely the subject of meta-analysis.
Another measurement possibility is the use of citation counts. Unfortunately, however, as research indicates, publications that perform particularly well on these counts do so less because of their perceived quality, or because of their usefulness to practitioners, than because of their usefulness to scholars in carrying out professional tasks (research methodology, etc.). This does not appear to be the kind of measure that would yield the type of information desired (Shadish 1989). However, measures that correct for this source of bias could be developed. Even so, citation counts have invariably raised a great number of objections when used in other disciplines (see Hébert 2004). This is a very rocky road to follow.

This brings us back to some type of rating procedure, perhaps using knowledgeable practitioners to provide input as to usefulness in application. Yet, on the evidence, even well-educated practitioners seem to be lacking in any real understanding of organizational behavior theories (Priem and Rosenstein 2000). Thus, this approach comes up short insofar as providing a truly informed group of judges to assess validity and usefulness (reading textbooks and practitioner publications is not enough).

This same problem of possessing adequate knowledge of theories themselves, the research on them, and the relevant literature plagues other approaches to rating theory validity and usefulness, as opposed to the much more global importance ratings. It is quite possible to find specialists who can evaluate motivation or leadership theories in this manner. This has been done, and the results compare well with my own ratings. Generalizing from these samples to other theoretical content domains within organizational behavior seems entirely justified. The added contributions of domain specialists do seem to support at least the validity part of the equation.

Rating all the theories across domains is another matter. In his review of Miner (2002), Ed Locke says, “It must have taken about ten years to put this book together.” Counting the input from various earlier versions, this estimate is not far from the truth. The point is that reviewing all the evidence (including meta-analyses) to make meaningful ratings of the validity and usefulness of seventy-three widely distributed theories at the present time is not something we in the field would do often. I did it only to write a number of books, and then only in my retirement. It is too much to ask that others do the same, and as Locke says, “I doubt anyone will again for the next twenty years.”

For all these reasons, I believe the type of quantitative (but still personal) review I have settled upon is the most feasible approach to evaluating the theories of organizational behavior at the present time. For those who remain skeptical, I ask only that they read the documentation on which the ratings are based, particularly that in Miner (2005, 2006), and then reevaluate their position.

**ESTABLISHING THAT THE GROUP OF JUDGES IS REPRESENTATIVE**

Evidence on the probability of nonresponse bias in surveys such as this (where 58 percent did not respond) may be obtained by comparing the responses on the study variables related to individuals of the first-round respondents (numbering sixty-seven) with those of the second-round respondents (numbering twenty-eight). These are the samples used in the test-retest analysis. If differences are minimal, it is likely that no differences would be found comparing the respondents and nonrespondents either. If differences are found from the first round to the second round, this trend should be perpetuated into the nonrespondent group.
and the representativeness of the respondent group of judges comes into serious question (Rogelberg and Luong 1998).

Comparisons between the first- and second-round respondents were made for the mean importance ratings for each theory, the frequency of failure to rate for each theory, and for several other study variables. These latter included the mean overall importance rating given by the individual, the proportion of strategic management versus organizational behavior respondents, the proportion of respondents fifty years or older versus those under fifty years, and for the proportion of respondents who proposed additional theories beyond the base seventy-three when given a chance to do so versus those who did not.

Of these 150 comparisons between the first and second rounds, 6 yielded significant results, all at \( p < .05 \). Five of these were on the importance ratings (theories 9, 16, 19, 33, and 66 in Table 17.1) and one on failure to rate (theory 44). In all 6 cases, the mean or frequency was greater in the second round. By chance alone, one would expect to find 7.5 differences at \( p < .05 \) in 150 comparisons. Thus, the evidence supports the representativeness of the group of judges, and appears to rule out nonresponse bias. Note also that the mean theory ratings across rounds correlated .92.

**ESTABLISHING THAT THE LIST OF THEORIES STUDIED IS COMPLETE**

After responding to the seventy-three items dealing with particular theories, the judges were asked the following question:

"Are there any other theories that should have been included in this list? Please indicate the importance of each theory you nominate using the (1) to (7) scale."

Responses obtained at this point represent the proposed additional theories alluded to in the prior section. Of the ninety-five judges, fifty-two (55 percent) left this section blank. Among those who did respond the mean number of theories nominated was 3.0. Names of theory authors were provided in only 47 percent of these cases, and importance ratings were given 77 percent of the time.

Most frequently nominated were strategic management theories—resource-based (twelve nominations), agency (ten nominations), transaction costs (ten nominations), industrial organization economics (six nominations). Of these nominations, 79 percent came from strategic management judges. There was a scattering of other strategic management and economics theories as well, typically nominated by the strategic management judges, and when rated, given high ratings (of 6 and 7), as were the other more frequently nominated strategic management theories. The problem here is that these are not organizational behavior theories, and thus do not meet the specified criteria of the analysis.

When consideration is given to the nominations of appropriate, organizational behavior theories, three emerge with more than two nominations; all with six nominations, almost exclusively noted by organizational behavior judges, and typically given high ratings. These three theories are labeled organizational justice, network, and identity, but with little consistency as to the authors specified. No other theories have any meaningful numbers at all, and these three receive nominations from only 6 percent of the judges; 8 percent of the organizational behavior group. On this evidence it seems appropriate to conclude that the original seventy-three theories represent a reasonably complete listing. Note also that nominations by judges from outside the United States unearthed few new theories of an international nature and did nothing to change this conclusion.
DIFFERENCES BETWEEN GROUPS OF JUDGES

Organizational Behavior versus Strategic Management

At various points in the preceding discussion, the strategic management-organizational behavior differential among judges has arisen. However, Table 17.1 presents the issue most forcefully. There, if one compares the mean importance ratings for the two groups of judges, certain patterns become manifest. The results of such a comparison indicate that there are seventeen instances where the organizational behavior means are higher at \( p < .01 \), and another seven at \( p < .05 \). In contrast, only one strategic management mean is higher and that is for the Cyert and March behavioral theory of the firm, a theory that has a strong affinity with economics and an economist as its primary author. The mean of means across all theories is 3.80 for the strategic management group and 4.40 for the organizational behavior judges (\( t = 9.12^{**} \)). Clearly, the strategic management raters give lower scores to most organizational behavior theories.

In Table 17.1, the theories favor the organizational behavior group and often are significant throughout the preorganizational behavior, the motivational, and the leadership categories until one gets to the organizational decision-making theories, where the only significant finding is the one reversal, and the general pattern is more balanced. The systems concepts, again, have higher importance ratings among the organizational behavior judges, as do the bureaucracy-related theories, but in these instances the effect is more muted than previously and significance is rarer. Within the sociological concepts of organizations, the predominance swings back to strategic management, but never to the point of significance.

Data were also obtained on the failure-to-rate factor. One might expect that organizational behavior judges would rate more of their own theories and strategic management judges would feel less capable of making these ratings of theories outside their field. Indeed, that is what happens. In seventeen cases, the organizational behavior raters evaluate the theory more frequently (ten at \( p < .01 \), seven at \( p < .05 \)); the reverse occurs only once (at \( p < .05 \)). The same pattern of findings across content areas noted for the mean importance ratings is found for number of ratings (or failure to rate) as well. The strategic management group feels less able to rate most of these organizational behavior theories, except for those dealing with decision making and certain theories of an organization-wide nature.

One result of the pattern noted above is that while twenty theories earn a really good rating (a score of 5.0 or higher) from the organizational behavior judges, only ten do so when the strategic management judges are involved. Furthermore, the theories so rated by the strategic management group are, without exception, of a decision-making or organization-wide nature; the good theories as rated by the organizational behavior group are more balanced, and include a number of micro formulations.

Given all of the evidence, and the logical facts of the situation, it seemed best to concentrate on the organizational behavior group of judges as being the truly knowledgeable scholars here, and it is their data that are used in the following analyses. Including the strategic management data would appear to be creating an apples-and-oranges problem, and to incorporate a less-informed group, when the theories to be judged are all of an organizational behavior type. Strategic management, with its close affiliation with economics, is apparently a distinct entity as opposed to organizational behavior. Accordingly, one might expect that were strategic management theories under consideration, the same tendencies to fail to rate and to rate lower would appear among organizational behavior judges. A by-product of this
analysis is the strong recommendation that publications be evaluated (peer reviewed) only by those whose disciplinary orientations fit the material; otherwise, the possibility of rejecting manuscripts that make a substantial contribution is high.

Fifty Years of Age and Over versus Under Fifty

In the same ways that strategic management and organizational behavior judges were anticipated to differ, a similar differential was expected between judges fifty years and over and those under fifty. Specifically, the younger judges were believed likely to rate older theories (originated before the mid-1970s) lower and to fail to rate them more frequently; the same tendencies should exist when older judges rate newer theories. This analysis does not overlap with the previous one because, although strategic management judges occurred somewhat more often in the younger group, the relationship involved was not close to being significant ($x^2 = 2.63$, NS; $df = 1$).

Comparisons of mean importance ratings by this age-based grouping of the raters in fact yielded only one significant difference (at $p < .05$); that was on Beach’s image theory (no. 45) — a newer theory that was indeed rated higher by the younger raters. In spite of this dearth of individual theory results, the older mean of 4.32 was above the younger mean of 4.23 ($t = 2.79**$), presumably because there were more older theories and because the ratings of the two age groups were highly correlated ($r = .94$).

Insofar as the number of ratings is concerned, the picture is somewhat more differentiated, all involving preorganizational behavior or older theories that were rated significantly more frequently by older judges. There were six such instances—numbers 2, 4, 20, 35, 49, and 66—in Table 17.1.

Overall, 7 of the 146 comparisons are significant at the .05 level or better (2 of these at $p < .01$). All fit theoretical expectations, but the numbers involved are not above chance levels. Certainly, there is no basis here for choosing one set of ratings over another, as was the case in choosing the organizational behavior judges over those of a strategic management nature; the differentiation in terms of the age of the group doing the judging is not that strong. It is for that reason that the actual means for the age groups are not included in Table 17.1.

This concludes the methodology part of this presentation on theory. The following chapter contains the actual findings of our research and the “Conclusions” for both chapters are to be found at the end of Chapter 18.

REFERENCES

CHAPTER 18

FINDINGS FROM THE IMPORTANCE, VALIDITY, AND USEFULNESS ANALYSES

Importance Versus Validity Relationship
Importance Versus Usefulness Relationship
Validity Versus Usefulness Relationship
The Validity-Usefulness Matrix and Theory Content
The Validity-Usefulness Matrix and Professional Degree Source
Defining Essential Theories in Terms of Importance, Validity, and Usefulness
Theory Building in the Content Areas
  Motivation (and Perception)
  Leadership
  Decision Making
  Systems
  Bureaucracy
  Organizational Sociology
Consensus
Conclusions

The results to be presented are modeled after the approaches taken in the prior study (Miner 1984), thus permitting certain comparative evaluations over time. In a number of respects, however, the results themselves are substantially different.

IMPORTANTNESS VERSUS VALIDITY RELATIONSHIP

Whereas before there was no evidence of any relationship between importance and validity, now such a relationship is apparent. The chi-square reaches the \( p < .05 \) level (see Table 18.1). When the full range of values are brought to the analysis, the correlation with importance emerges as \( .46^{**} \). Thus, we now have evidence that the two are significantly related, although as of 1977, we did not; it appears that a real change has occurred in the interim in this regard.

IMPORTANTNESS VERSUS USEFULNESS RELATIONSHIP

The organizational behavior ratings of importance exhibit a significant relationship to estimated usefulness in practice (see Table 18.2), not strong, but a real improvement over what was indicated in the Miner (1984) report. Yet the correlation using the full range of variables is only \( .22 \) (\( p < .10 \)), still better than the \( p \) value reported previously, but lacking in significance. It appears overall that the usefulness factor does contribute to the importance ratings, and certainly this relationship has improved over the years, but it is still not very pronounced.
VALIDITY VERSUS USEFULNESS RELATIONSHIP

While the Lewin hypothesis was not supported in the Miner (1984) analysis, it is now. Perhaps what is needed to confirm an hypothesis of this type is a more mature science. The chi-square value in Table 18.3 is significant at $p < .02$ and the correlation obtained is .28*.

Eighteen theories are higher on usefulness than on validity. In these cases, the applications appear to have detached themselves from the underlying theory, developing a life of their own, presumably through a process of trial and error (Weick 1987). This process appears to be particularly characteristic of theories having a tie to organization development in some form. Of the eighteen theories involved, twelve have such a tie. In addition, three are of a pre-OB nature, where practical applications have often come to outdistance the validity of the underlying theory (numbers 1, 6, and 7).

Even more frequent are cases in which a theory’s validity outdoes its usefulness; there are a number of good theories that have not proved very practical. In all, thirty-eight theories were considered to have higher validity than usefulness. Of these, fifteen were relatively more recent in nature, and only five such theories had an equal balance or had a validity below their usefulness ($x^2 = 5.83, p < .02; df = 1$). It appears from these data that the recent period has stressed the validity of its theorizing, at the expense of practical application. The need for a new breed of “application theorists” who can take the good theories of others and extend them into the world of practice appears to be accelerating, or perhaps the greatest need is for theorists who concern themselves primarily with practice.

### Table 18.1

**Estimated Scientific Validity in Relation to Mean Organizational Behavior Importance Rating**

<table>
<thead>
<tr>
<th>Estimated scientific validity</th>
<th>Low (2.71–3.99)</th>
<th>Medium (4.00–4.99)</th>
<th>High (5.00–5.97)</th>
</tr>
</thead>
<tbody>
<tr>
<td>High (4 and 5)</td>
<td>3</td>
<td>12</td>
<td>11</td>
</tr>
<tr>
<td>Mixed (3)</td>
<td>9</td>
<td>8</td>
<td>7</td>
</tr>
<tr>
<td>Low (1 and 2)</td>
<td>10</td>
<td>11</td>
<td>2</td>
</tr>
</tbody>
</table>

$x^2 = 10.95; p < .05; df = 4.$

### Table 18.2

**Estimated Usefulness in Practice in Relation to Mean Organizational Behavior Importance Rating**

<table>
<thead>
<tr>
<th>Estimated usefulness in practice</th>
<th>Low (2.71–3.99)</th>
<th>Medium (4.00–4.99)</th>
<th>High (5.00–5.97)</th>
</tr>
</thead>
<tbody>
<tr>
<td>High (4 and 5)</td>
<td>0</td>
<td>4</td>
<td>7</td>
</tr>
<tr>
<td>Questionable (3)</td>
<td>10</td>
<td>13</td>
<td>5</td>
</tr>
<tr>
<td>Low (1 and 2)</td>
<td>12</td>
<td>14</td>
<td>8</td>
</tr>
</tbody>
</table>

$x^2 = 10.55; p < .05; df = 4.$
Table 18.3

Relationship between Estimated Usefulness in Practice and Estimated Scientific Validity

<table>
<thead>
<tr>
<th>Estimated usefulness in practice</th>
<th>Estimated scientific validity</th>
</tr>
</thead>
<tbody>
<tr>
<td>Low (1 and 2)</td>
<td>High (4 and 5)</td>
</tr>
<tr>
<td>1. Mayo et al.</td>
<td>49. Trist and Emery</td>
</tr>
<tr>
<td>6. Fayol</td>
<td>52. Lawrence and Lorsch</td>
</tr>
<tr>
<td>7. Taylor</td>
<td>3. Lewin</td>
</tr>
<tr>
<td>9. Herzberg</td>
<td>8. McClelland</td>
</tr>
<tr>
<td>Questionable (3)</td>
<td>16. Locke and Latham</td>
</tr>
<tr>
<td>6. Fayol</td>
<td>(Normative)</td>
</tr>
<tr>
<td>24. Fiedler (Contingency)</td>
<td>28. Miner</td>
</tr>
<tr>
<td>27. Kerr</td>
<td>30. Bass</td>
</tr>
<tr>
<td>6. Fayol</td>
<td>N = 1</td>
</tr>
<tr>
<td>24. Fiedler (Contingency)</td>
<td>20. Levinson</td>
</tr>
<tr>
<td>27. Kerr</td>
<td>5. Weber</td>
</tr>
<tr>
<td>9. Herzberg</td>
<td>11. Vroom (Expectancy)</td>
</tr>
<tr>
<td>38. Heller</td>
<td>59. Argyris (Congruence)</td>
</tr>
<tr>
<td>53. Likert</td>
<td>13. Hamner</td>
</tr>
<tr>
<td>64. Argyris (Learning)</td>
<td>7. Taylor</td>
</tr>
<tr>
<td>65. Bennis</td>
<td>24. Fiedler (Contingency)</td>
</tr>
<tr>
<td>66. Blake and Mouton (OD)</td>
<td>11. Vroom (Expectancy)</td>
</tr>
<tr>
<td>Low (1 and 2)</td>
<td>6. Fayol</td>
</tr>
<tr>
<td>2. Barnard</td>
<td>19. Alderfer</td>
</tr>
<tr>
<td>32. Hemphill et al.</td>
<td>35. Evans</td>
</tr>
<tr>
<td>33. Blake and Mouton (Leadership)</td>
<td>36. House (Path Goal)</td>
</tr>
<tr>
<td>34. Hersey and Blanchard</td>
<td>39. Fiedler and Garcia</td>
</tr>
<tr>
<td>37. Tannenbaum and Schmidt</td>
<td>(Cognitive)</td>
</tr>
<tr>
<td>44. Weick</td>
<td>42. Cyert and March</td>
</tr>
<tr>
<td>51. Burns and Stalker</td>
<td>43. March</td>
</tr>
<tr>
<td>55. Woodward</td>
<td>46. Cohen et al.</td>
</tr>
<tr>
<td>56. Perrow</td>
<td>48. Katz and Kahn</td>
</tr>
<tr>
<td>61. Donaldson</td>
<td>50. J. Thompson</td>
</tr>
<tr>
<td>N = 11</td>
<td>54. Stogdill</td>
</tr>
<tr>
<td>N = 16</td>
<td>72. Powell and DiMaggio</td>
</tr>
<tr>
<td>N = 7</td>
<td>73. Hannan et al.</td>
</tr>
</tbody>
</table>

\( x^2 = 12.79; p < .02; \text{df} = 4. \)
The validity-usefulness matrix of Table 18.3 may be completed not with specific theories but with the contents, or areas, of theory formulation (see Table 18.4). The high-high theories here deal in a majority of instances with motivation, but include a number in the leadership area as well. When the net is extended more broadly in one direction or the other, the mix of theory content is expanded considerably, even though the motivational factor continues to add numbers. Since in the Miner (1984) study, motivation theories clearly dominated this analysis, holding all of the positions in the high-high category and yielding a very significant chi-square, the significance of this finding was tested once again using the more recent data. The results at the bottom of Table 18.4 indicate a similar situation in 2000–2001. Motivation continues to hold a highly significant position, even though forced to share its dominance with theories of other kinds. If one wishes to create a highly valid theory, which is also constructed with the purpose of enhanced usefulness in practice in mind, it would be best to look to motivation theories, often with a more limited domain, for an appropriate model.

Table 18.4

<table>
<thead>
<tr>
<th>Estimated usefulness in practice</th>
<th>Estimated scientific validity</th>
</tr>
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<tbody>
<tr>
<td>low (1 and 2)</td>
<td>mixed (3)</td>
</tr>
<tr>
<td>High (4 and 5)</td>
<td>Bureaucracy (60)</td>
</tr>
<tr>
<td></td>
<td>Systems (49, 52)</td>
</tr>
<tr>
<td>Questionable (3)</td>
<td>Bureaucracy (64, 65, 66, 67)</td>
</tr>
<tr>
<td></td>
<td>Pre-OB (1, 6, 7)</td>
</tr>
<tr>
<td></td>
<td>Leadership (31, 38)</td>
</tr>
<tr>
<td></td>
<td>Motivation (9)</td>
</tr>
<tr>
<td></td>
<td>Systems (53)</td>
</tr>
<tr>
<td>Low (1 and 2)</td>
<td>Leadership (32, 33, 34, 37)</td>
</tr>
<tr>
<td></td>
<td>Systems (51, 55, 56)</td>
</tr>
<tr>
<td></td>
<td>Pre-OB (2, 4)</td>
</tr>
<tr>
<td></td>
<td>Motivation (18)</td>
</tr>
<tr>
<td></td>
<td>Decision making (44)</td>
</tr>
<tr>
<td>Validity-usefulness</td>
<td>Areas of theory formulation</td>
</tr>
<tr>
<td>(4 and 5, 4 and 5)</td>
<td>other than motivation</td>
</tr>
<tr>
<td>(3, 4 and 5)</td>
<td>motivation</td>
</tr>
<tr>
<td>(4 and 5, 3)</td>
<td>12</td>
</tr>
<tr>
<td>Other combinations</td>
<td>45</td>
</tr>
</tbody>
</table>

\[ x^2 = 7.57; \ p < .01; \ df = 1. \]

THE VALIDITY-USEFULNESS MATRIX AND THEORY CONTENT

The validity-usefulness matrix of Table 18.3 may be completed not with specific theories but with the contents, or areas, of theory formulation (see Table 18.4). The high-high theories here deal in a majority of instances with motivation, but include a number in the leadership area as well. When the net is extended more broadly in one direction or the other, the mix of theory content is expanded considerably, even though the motivational factor continues to add numbers. Since in the Miner (1984) study, motivation theories clearly dominated this analysis, holding all of the positions in the high-high category and yielding a very significant chi-square, the significance of this finding was tested once again using the more recent data. The results at the bottom of Table 18.4 indicate a similar situation in 2000–2001. Motivation continues to hold a highly significant position, even though forced to share its dominance with theories of other kinds. If one wishes to create a highly valid theory, which is also constructed with the purpose of enhanced usefulness in practice in mind, it would be best to look to motivation theories, often with a more limited domain, for an appropriate model.
THE VALIDITY-USEFULNESS MATRIX AND PROFESSIONAL DEGREE SOURCE

A different type of overlay for Table 18.3 may be obtained by entering into the matrix the department or program from which the highest professional degree of the theorist(s) was obtained. When there are multiple authors, the predominant discipline among them is used; thus the number of entries equals the number of theories. The most frequent disciplinary origin among the theories is psychology with 52 percent; second is sociology with 12 percent, followed by organizational behavior at 10 percent and political science at 8 percent. No other discipline extends beyond 4 percent, although there are a number of fields represented.

The Miner (1984) findings using this matrix indicated a highly significant dominance of psychology among the various disciplines represented. In fact, psychology held almost all of the top spots. However, this is no longer true in 2000–2001, and many more theories set forth by psychologists are further down in the validity-usefulness matrix. The result is that psychology no longer occupies a significantly strong position. I suspect that this represents a trend into the future, and that psychology’s hold on theoretical dominance in the field will continue to shrink.

DEFINING ESSENTIAL THEORIES IN TERMS OF IMPORTANCE, VALIDITY, AND USEFULNESS

At several points previously, I have referred to certain theories as “essential” in that they meet specified criteria for that purpose. The essential designation was also used to determine which theories would be treated in the prior volumes of this series (Miner 2005, 2006). It is now possible to be more specific regarding the criteria incorporated in the essential terminology. For this purpose, we must return to Table 17.1.

The cutting point used to determine whether a theory was considered important by the organizational behavior judges was 5.00. Any theory with a mean score at or above this level was considered to have met this criterion; there are twenty such theories. Thus, 27 percent of the theories qualified.

The cutting point for estimated validity was 4, and consequently any such score of 4 or 5 met this criterion; twenty-six theories are identified in this way. Insofar as estimated validity is concerned, 36 percent of the theories equaled or exceeded the criterion—25 percent with 4s and 11 percent with 5s.

The cutting point for estimated usefulness in practice was again 4 and once more a score of 4 or 5 was sufficient; eleven theories are considered to have met this criterion. Thus, for estimated usefulness only 15 percent of the theories qualified—12 percent with 4s and 3 percent with 5s.

In addition to the above criteria, another approach was employed that brought theories into the essential category on the basis of their being almost good enough on all three factors—importance, validity, and usefulness. Thus a theory with an importance rating in the 4.00s, a validity of 3, and a usefulness of 3 would qualify. A total of three theories met this criterion—4 percent of the theories.

Applying these considerations to the seventy-three theories, and recognizing that in many instances theories met multiple criteria, forty theories emerged as sufficiently worthy to warrant the essential designation. Only five, however, qualified on all three criteria—Lewin (no. 3), McClelland (no. 8), Hackman et al. (no. 10), Locke and Latham (no. 16), and Bass (no. 30). All of these are in the motivation/leadership content area.
My objective in establishing these criteria in the first place was to select theories worthy of inclusion in the previous volumes of this series (Miner 2005, 2006). But an alternative goal was to set some standards for theorists of the future. These are the kinds of criteria to which theorists should aspire. They are not easily accomplished, and yet they need to be met if theorists of the future are to make a major contribution to organizational behavior as an applied science.

THEORY BUILDING IN THE CONTENT AREAS

Given this assessment of the current state of theory in organizational behavior, and drawing upon the data of Table 17.1, it is possible to specify something about the potential for theory building in one area as opposed to another. Where are the opportunities for theory development greatest? In what areas has theory advanced the farthest?

In considering these questions I make two assumptions: (1) the importance ratings, now related to both validity and to a degree of usefulness, provide the most helpful guidance for theory building; (2) theories judged to have greater importance can be used effectively as models for building new theories and also to establish a framework from which new theory can be extrapolated, thus contributing to both knowledge acquisition and increased consensus. This latter point emphasizes a strategy in theory development that a number of successful theorists have used in the past, growing their theories in both scope and the number of variables considered over a substantial period of time (Edwin Locke, Karl Weick, and Paul Lawrence all provide examples). This type of expansion from an existing theoretical base is most commonly carried out by the original theorists themselves, but as has been the case with House (see theories 29 and 36), it can be undertaken by a new theorist (perhaps of a subsequent generation). Such building on what is already known in a given area presents itself as an ideal approach to creating new theory, although certainly not the only approach.

Motivation (and Perception)

Motivation theory has not only exhibited considerable validity and usefulness over the years, but it generates a mean organizational behavior importance rating for its fifteen theories of 4.62 (5.06 for the essential theories). Certainly, motivation, and personality theory more broadly, are not engaged in a “great disappearing act” as some have claimed (Nord and Fox 1996). A much more positive characterization of the area, as reflected in the views of Latham and Pinder (2005), now seems warranted. In fact six of the fifteen such theories are rated at 5.00 or above in importance. A number of these latter theories have increased in importance over the past twenty years including McClelland’s achievement motivation theory, Hackman and Oldham’s job characteristics theory, Adams’s equity theory, and in particular Locke’s goal setting theory. The declines have been primarily in some of the more humanistic theories.

All of the above theories represent candidates for extension and new theoretical breakthroughs, as do the Vroom, and Porter and Lawler, versions of expectancy theory. In fact, equity and expectancy theory have already experienced such extrapolations at the hands of other than the original authors. However, further extensions in these areas seem warranted.

Leadership

Leadership theory is in ferment at the present time, probably because there are so few dominant positions. The mean organizational behavior importance rating for the eighteen theories
is 4.00 (4.45 for the essential theories); only Bass’s transformational theory is rated above 5.00. The latter is closely followed by House’s charismatic theory and Graen’s leader member exchange theory, which has moved up substantially in importance over the years. Yet, the decline of path goal theory in its various forms and of Fiedler’s theorizing has left something of a void in the leadership area.

This situation creates a major opportunity for new theorizing in the leadership area, perhaps by moving up from theories previously designated as motivational, as has happened in the past. A recent example of this kind is the Miner (see theory no. 28) extrapolation of role motivation theory from motivation to the leadership domain. Yet, further extensions of the Bass, House, and Graen theories also seem warranted, either by the current authors or by others with similar interests.

**Decision Making**

There are only six entries on our list of organizational decision-making theories, but they are impressive. The mean organizational behavior importance rating is 4.98 (essential theories 5.46) and four theories are rated at 5.00 or above. These four are the work in the 1940s and 1950s of Simon and March, the Cyert and March behavioral theory of the firm, March’s views on organizational learning, and Weick’s theories of organizing and sensemaking. What is needed here is research to build an adequate empirical base. Given that, it should be possible to carry these theoretical frameworks forward. Yet, the genius of people like March and Cyert and Weick, and even Simon, has been in their theorizing, not their research. It may take some time for new theory firmly embedded in research to catch up.

**Systems**

Macro theories of organizational structuring and functioning cover a wide range from organization development to systems concepts, to bureaucracy-related views, to the more recent positions such as neoinstitutional theory. Across the twenty-seven such theories (twenty-eight if one includes Weber himself) the mean importance rating is 4.38 (4.43 with Weber); seven achieve values of 5.00 or above (eight with Weber). However, if one focuses on the ten theories that use a systems approach, the mean importance rating from organizational behavior judges is 4.62 (5.07 for essential theories); five such theories have an importance rating above 5.00. These include two organization development-generating theories—Trist and Emery’s sociotechnical theory and Lawrence and Lorsch’s contingency theory of organizations. Also above the 5.00 level on importance are the Katz and Kahn theory, James Thompson’s theory, and the Burns and Stalker view of mechanistic and organic systems. While these theories have held positions in the over 5.00 category, other systems theories, such as Likert’s systems 1 to 4 and Woodward’s technological determinism, have declined sharply. This says a good deal about where this field might best devote its future energies.

Whether systems theory, which has probably passed through the period of its greatest popularity, will provide a basis for further breakthroughs in the future remains something of a question. However, the continuing thrust of organization development practice will almost certainly drive a reemergence of theory building in that area, perhaps working from the frameworks provided by sociotechnical theory and the Lawrence and Lorsch position. Alternatively advances in the organization development area may emerge out of bureaucracy-related theories, rather than from the systems approach.
Bureaucracy

Turning now to the bureaucracy theories, of which there are thirteen when Weber is included, the mean organizational behavior importance rating is only 4.09 (with essential theories at 4.74). Interestingly, none of the bureaucracy-related views (other than that of Weber) achieve the top category on importance, although Schein’s theory of culture and leadership comes close. This is an area that is clearly lacking in powerful theorizing. Perhaps the best guide for future theorists is the Schein work, but the subject is wide open right now. My own belief is that some type of culture theory where the variables are operationalized, and thus capable of research study, is most likely to bring about a breakthrough, and thus, a theory that draws at least partially on the Schein thinking. In any event the bureaucracy-related area is one of the content aspects of organizational behavior most in need of theoretical assistance.

Organizational Sociology

Of the five theories dealing with sociological conceptualizations of organization, two have had high mean importance ratings as determined by organizational behavior judges, and all five meet criteria for the essential designation. The mean importance value here is 4.94. The two theories in this category that exceed the 5.00 importance value are the Pfeffer and Salancik resource dependence theory and Powell and DiMaggio’s version of neoinstitutional theory. Both of these latter appear ripe for further developments, and indeed, all of the recent institutional approaches seem to have moved beyond the “neo” stage to a fully established identity of their own (Dacin, Goodstein, and Scott 2002).

What I am arguing throughout is that organizational behavior is now mature enough (given the findings reported) to warrant building much of its new theory on the base already established over the past fifty years. In most areas, a solid framework exists; now what is needed is to develop this framework to its full potential. The theories rated most important tell us where best to focus our efforts, what thrusts to take into the future, both as to research and for theory development.

Consensus

The range of importance ratings given to the various theories suggests a substantial lack of consensus (as discussed in Chapter 4 of this volume), something that has been widely bemoaned, and occasionally extolled, in the organizational behavior field in the past (see, for instance, Roberts, Weissenberg, Whetten, Pearce, Glick, Bedeian, Miller, and Klimoski 1990). When all ninety-five judges are invoked, 86 percent of the theories have all rating points from one to seven filled and another 12 percent have six points filled.

When the analysis is limited to the seventy-one judges in the organizational behavior group, as is appropriate, there are only 71 percent of the theories where all seven points are filled, but another 25 percent where six scale points are occupied—not a great improvement, but some. This analysis suffers, however, from the fact that a number of theories have outliers with only a single judge’s rating at one or both of the extremes. When these instances are eliminated, the organizational behavior judges’ figures fall to 41 percent with all seven scale points filled and 41 percent with six filled. Applying a goodness-of-fit analysis to these data, with the figures for the total sample of ninety-five supplying the expected values, a significant difference is obtained ($x^2 = 16.07**$, df = 2). There is clearly much greater consensus
IMPORTANCE OF ORGANIZATIONAL BEHAVIOR THEORIES

among the organizational behavior raters than for the total group, as might be hoped. This position is confirmed by the smaller average standard deviation of the organizational behavior ratings than is found in the total group ($t = 3.84^{**}$).

Any such evaluation is confounded, however, by the variation in standards apparently applied by different judges. The average rating by an organizational behavior judge was spread across 3.66 scale points; for the total group this spread was 4.48. Quite evidently, some judges consistently apply rather negative standards and others are consistently quite positive—25 percent of them restrict their ratings to from three to five scale points. What we do not know is to what extent these judges with apparent strong response tendencies would continue to exhibit the same behavior on other rating scales with quite different content, thus demonstrating the use of stable differences in standards. Thus, the research as currently conceived does not tell us to what extent differences in standards exist and thus might serve to exaggerate the apparent lack of consensus. Yet it seems almost certain that some such differences exist; consensus clearly is greater than the current data indicate.

Furthermore, comparison data utilizing the key theories of other disciplines and appropriate raters from those disciplines are lacking. Are fields such as economics or political science, or the physical sciences, which are often characterized as possessing high consensus, really more homogeneous in their theory ratings? We do not know. Thus, in certain respects, with regard to consensus, this analysis represents a pilot investigation serving more to unearth design needs and key variables than to provide definitive answers. Further research is definitely needed.

CONCLUSIONS

The Miner (1984) report concluded with the following summary:

> Overall, in spite of pockets of substantial success, the picture presented by this stock taking of organization theories is not highly positive. The feedback is at least as negative as it is positive, sufficiently negative so that a readjustment of goals, paradigms, and basic processes appears worth considering. (303)

Now, however, this negative picture appears to have changed substantially; the feedback is much more positive, consistent with a more mature science. Organizational behavior is clearly differentiated from strategic management. Validity and usefulness are contributors to perceptions of theoretical importance. Lewin’s dictum (hypothesis) regarding the tie between theory and practice receives solid support. Psychology has come to share its dominant position in the validity-usefulness matrix with other disciplines, thus creating a more diversified knowledge base. A consensus regarding the theoretical knowledge possessed by the field appears to be emerging, although it is not clear how strong this consensus is. Our newer theories, and a number that have survived from the previous generation, are of high validity.

All this bodes well for the future of organizational behavior. A call for a readjustment of goals, paradigms, and basic processes no longer seems warranted. Yet, we have to a degree lost sight of the usefulness criterion and the matter of practical application; perhaps some will believe that we have become too academic. Still, this may well be a phase as organizational behavior establishes its credentials and solidifies its position in the academic world, a position that fifty years ago was far from well established. In any event, I do not wish to argue from the results reported here that organizational behavior should be
satisfied with what it has accomplished and cease to develop. There certainly is much more to accomplish.

In concluding this chapter, I want to say a word about the process of conducting surveys of this kind. My impression is that the current approach represents a major advance over what was done more than twenty years earlier. There are areas in which further improvements could be made, especially with regard to consensus measurement. I have argued that theoretical advance tends to be and should be evolutionary, and that would appear to be equally true of the conduct of surveys such as this one. Nevertheless, given the importance of periodic stocktaking for theory development, organizational behavior seems to have reached a point where it might be practical to institute some type of ongoing, systematic procedure for surveying the importance of its theories. Perhaps such an endeavor could be undertaken by our professional societies.

A final point on the results presented in this chapter is that they do not always match perfectly with those reported in Miner (2003), although the disparities are not large. This differential is not attributable to the correction of prior errors; rather, it arises from the fact that those theories found to be essential, and presented in Miner (2005) and (2006), were reevaluated in certain respects and thus the evaluation process for these theories was upgraded to consider the literature until 2004.

The appraisal of the current state of theory in organizational behavior that represents the objective of Part IV is not yet done with this treatment in Chapters 17 and 18. We still need to take up the matter of the extent to which these theories have become institutionalized—the subject represented by the number in the last column of Table 17.1. Accordingly, we move to this topic in the following chapters.

REFERENCES


Chapter 19

Background and Hypotheses for the Institutionalization Analyses

An Introduction to Institutional Theory from Miner (2006a)

Focusing in on the Institutionalization of Theories
Selected Statements from the General Theory
Selected Statements with Regard to the Professional Domain
The Role of Theory in Institutional Thinking

Conformity Theory
Origins of Conformity Theory in Projective Measurement
Correlates of the Conformity Measures
Parallels to Institutional Theory

Conclusions

The objective in this and the next two chapters is to apply neoinstitutional theory to the theories of organizational behavior to determine which among them, if any, have become institutionalized, and thus to indicate something about the maturity level of organizational behavior at the present time. To achieve this objective, it has proved necessary to develop a new approach to measuring the institutionalization of the theories of the field. This chapter presents the background in institutional theory and in conformity theory needed to understand that approach. In the process, it provides rationales for hypotheses as stated in the “Conclusions” section.

In concluding their review of institutional theory, Tolbert and Zucker (1996) argue forcefully for the need to develop more direct measures of institutionalization; they suggest that this might be accomplished through the use of survey research, and the application of questionnaire methods grounded in standard psychometric techniques. At the same time, they are aware that developing such measures is likely to be a contentious task, akin to the creation of indexes of such concepts as uncertainty (see Miller and Shamsie 1999, for instance). The present discussion takes its lead from the Tolbert and Zucker (1996) arguments, and indeed, follows their suggestions into the domain of questionnaire design and psychometric methodology. In the process, it will no doubt elicit its fair share of contentiousness as well.

The approach to measurement taken has its origins in personality theory and the specification of a variable variously described as popularity or conformity motivation (Miner 1965a). This chapter develops this construct, reports research on it, and shows how it relates to institutional theory. But first, the nature of institutional theory needs to be explained, as does the meaning of that theory in the present context.

An Introduction to Institutional Theory from Miner (2006a)

As an introduction to institutional theory I draw upon excerpts from a previous extended presentation in Chapter 20 of Miner (2006a):
As it originally emerged, neoinstitutional theory was a fragmented array of positions with some common ground but many differences as well. (Scott 1987)

Neoinstitutional implies that there was some type of institutional emphasis preceding, and there was. This "old institutionalism" spread across many of the social sciences in a rather loose form, but within the study of organizations it is best manifested in the writings of the sociologist Philip Selznick (1949, 1957), who continues to reassert his position (Selznick 2000). That this old version is not entirely dead or displaced is evident from a recent attack on the new upstarts as impoverished by Arthur Stinchcombe, a student of Selznick's at Berkeley. His views will provide a useful backdrop against which to present the theoretical ideas to follow—

[T]he trouble with the new institutionalism is that it does not have the guts of institutions in it. The guts of institutions is that somebody somewhere cares to hold an organization to the standards and is often paid to do that. Sometimes that somebody is inside the organization, maintaining its competence. Sometimes it is an accrediting body . . . [S]ometimes that somebody . . . is lacking, in which case the center cannot hold, and mere anarchy is loosed upon the world. (Stinchcombe 1997, 17–18)

Neoinstitutional theory began with an article by Meyer and Rowan (1977).

[There] organizational structures are said to develop in highly institutionalized contexts. Thus, they are influenced to take on the practices and procedures that are defined by prevailing rationalized ideas about organizational work held in society. When they do this they increase their legitimacy and their chances of survival. However, these societal expectations are really myths and may well conflict with criteria of efficiency. Formal structures and the rules that govern them are in fact reflections of the institutional environment. These institutional effects are quite apart from the effects produced by networks of social behavior and relationships within and around a particular organization. Examples of institutionalized processes are professional rules, business functions, and established technologies. Following these approaches is viewed as being appropriate and displays responsibility and avoids charges of negligence.

These . . . arguments lead to three theses for research attention—

1. Environments and environmental domains that have institutionalized a greater number of rational myths generate more formal organization.
2. Organizations that incorporate institutionalized myths are more legitimate, successful, and likely to survive.
3. Organizational control efforts, especially in highly institutionalized contexts, are devoted to ritual conformity, both internally and externally. (Meyer and Scott 1983, 26–44)

Institutions are defined as cultural rules giving collective meaning (in terms of the collective purposes of progress and justice) and value to particular entities and activities, integrating them into the larger schemes. Institutionalization, accordingly, is the process through which a given set of units and a pattern of activities come to be normatively and cognitively held in place, so that they are taken for granted to be lawful (either as a result of formal law,
custom, or knowledge). In this view, action is not a matter of individual choice but of broad social scripts; individualism loses out in large part to “the massive institutional features of the social system.” These features in turn are part of the culture.

*****
The processes through which behavior is shaped . . . may be specified as follows—

• Representational rules that involve shared logics or modes of reasoning that help to create shared understandings of reality that are “taken for granted”
• Constitutive rules that create social actors—that is, identities linked to specified behaviors and action routines
• Normative rules that stipulate expectations for behavior that are both internalized by actors and reinforced by the beliefs and actions of those with whom they interact
• Enforcement mechanisms, both formal and informal, involving surveillance, assessment, and the application of sanctions rewarding conformity and punishing deviance. (Scott and Meyer 1994, 67)

Working from this perspective institutions are now defined as symbolic and behavioral systems containing representational, constitutive, and normative rules together with regulatory mechanisms that define a common meaning system and give rise to distinctive actors and action routines. To this is added somewhat later that, institutions operate at a variety of levels, and their elements can be embodied in and carried by cultures, regimes, and formal organizations.

*****
A discussion of the role of cognitive, normative and regulative structures and activities in influencing social behavior provides an addition to what was said previously on this score. This discussion is summarized in Table [19.1]. (Miner 2006a, 372–78)

FOCUSBING IN ON THE INSTITUTIONALIZATION OF THEORIES

With this introduction in hand, we need first to specify from the general theory certain selected statements that have applications to the institutionalization of theories; then to narrow down to what neoinstitutional theory has to say about the professional domain; and finally to focus directly on the subject of scientific theory.

One problem with neoinstitutional theory at present is that it possesses a substantial degree of ambiguity (Tolbert and Zucker 1996). The constructs of the theory are stated in very general terms, often without any concern about operationalization. This situation is what instigated the present attempt to develop a measure of institutionalization. Yet institutional theory represents the leading perspective among organizational sociologists in the United States (Mizruchi and Fein 1999). Many of its central propositions amount to very powerful theory. It has achieved this status in spite of its inherent ambiguity.

Selected Statements from the General Theory

To the extent that technologies are uncertain and goals are ambiguous in a field (such as organizational behavior) institutionalization will be perpetuated (DiMaggio and Powell 1983). This was in fact the situation that existed in the early days of organizational behavior (see Chapter 15 in this volume). Thus, ambiguity fosters institutionalization and it can be assumed as well that ambiguity fosters its manifestation.
Institutions are taken-for-granted, and thus typically require little by way of conscious thought (Powell and DiMaggio 1991; Scott 2001). Thus, much of the institutional process is hidden from consciousness, and is in fact unconscious.

Institutional theory is mobilized in somewhat different forms in economics, organizational behavior, political science, history, law, anthropology, and sociology (Ingram and Clay 2000; Scott 2001). None of these listings includes psychology; sociological perspectives tend to dominate at present and are accordingly the focus of the treatment here.

Institutionalization is a gradual process that typically takes years to become fully established. One should not look for signs of institutional forms and processes in new phenomena (Tolbert and Zucker 1983). Although aspects of the surrounding environmental context may predict institutionalization in the early years of the process, these relationships disappear later on as adoption becomes expected and automatic, even if not functional (Zucker 1983). Thus, an analysis of the adoption of total quality management programs by hospitals indicated that while early adopters tended to customize the approach to meet their particular efficiency needs, later adopters accepted the programs in standard form to gain legitimacy (Westphal, Gulati, and Shortell 1997). Only when this late-stage mimicry becomes widespread can institutionalization be said to have occurred. That typically takes a number of years.

The pursuit of legitimacy is a major motivating force behind the process of institutionalization. Legitimacy is defined as “collective recognition of and orientation to binding rules” (Stryker 2000, 192). Legitimacy may or may not be synonymous with reputation; the former emphasizes the social acceptance resulting from adherence to social norms and expectations, whereas reputation places emphasis on comparisons across organizations (Deephouse

### Table 19.1

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Cognitive</th>
<th>Normative</th>
<th>Regulative</th>
</tr>
</thead>
<tbody>
<tr>
<td>Basis of compliance</td>
<td>Taken for granted</td>
<td>Social obligation</td>
<td>Expedience</td>
</tr>
<tr>
<td>Mechanisms</td>
<td>Imitation</td>
<td>Normative</td>
<td>Coercive</td>
</tr>
<tr>
<td>Logic</td>
<td>Orthodoxy</td>
<td>Appropriateness</td>
<td>Instrumentality</td>
</tr>
<tr>
<td>Indicators</td>
<td>Prevalence</td>
<td>Certification</td>
<td>Rules</td>
</tr>
<tr>
<td></td>
<td>Isomorphism</td>
<td>Accreditation</td>
<td>Laws</td>
</tr>
<tr>
<td>Basis of legitimacy</td>
<td>Culturally supported</td>
<td>Morally governed</td>
<td>Legally sanctioned</td>
</tr>
<tr>
<td>Carriers</td>
<td>Cultures</td>
<td>Values</td>
<td>Rules</td>
</tr>
<tr>
<td></td>
<td>Categories</td>
<td>Expectations</td>
<td>Laws</td>
</tr>
<tr>
<td>Social structures</td>
<td>Isomorphism</td>
<td>Regimes</td>
<td>Government systems</td>
</tr>
<tr>
<td></td>
<td>Identities</td>
<td>Authority systems</td>
<td>Power systems</td>
</tr>
<tr>
<td>Routines</td>
<td>Performance programs</td>
<td>Conformity</td>
<td>Protocols</td>
</tr>
<tr>
<td></td>
<td>Scripts</td>
<td>Performance of duty</td>
<td>Standard procedures</td>
</tr>
</tbody>
</table>


Institutions are taken-for-granted, and thus typically require little by way of conscious thought (Powell and DiMaggio 1991; Scott 2001). Thus, much of the institutional process is hidden from consciousness, and is in fact unconscious.

Institutional theory is mobilized in somewhat different forms in economics, organizational behavior, political science, history, law, anthropology, and sociology (Ingram and Clay 2000; Scott 2001). None of these listings includes psychology; sociological perspectives tend to dominate at present and are accordingly the focus of the treatment here.

Institutionalization is a gradual process that typically takes years to become fully established. One should not look for signs of institutional forms and processes in new phenomena (Tolbert and Zucker 1983). Although aspects of the surrounding environmental context may predict institutionalization in the early years of the process, these relationships disappear later on as adoption becomes expected and automatic, even if not functional (Zucker 1983). Thus, an analysis of the adoption of total quality management programs by hospitals indicated that while early adopters tended to customize the approach to meet their particular efficiency needs, later adopters accepted the programs in standard form to gain legitimacy (Westphal, Gulati, and Shortell 1997). Only when this late-stage mimicry becomes widespread can institutionalization be said to have occurred. That typically takes a number of years.

The pursuit of legitimacy is a major motivating force behind the process of institutionalization. Legitimacy is defined as “collective recognition of and orientation to binding rules” (Stryker 2000, 192). Legitimacy may or may not be synonymous with reputation; the former emphasizes the social acceptance resulting from adherence to social norms and expectations, whereas reputation places emphasis on comparisons across organizations (Deephouse
Legitimacy is the more important construct because its loss may result in a major decline in market share.

Much of the work on legitimacy has recently involved new ventures. One proposition from this context is that new ventures can acquire legitimacy by conforming to regulations, rules, standards, and expectations introduced by governments, credentialing associations, professional bodies, and the like; in this process they tend to gain access to more resources, and thus a greater potential for growth (Zimmerman and Zeitz 2002). On the evidence from new high-tech ventures this appears to be true (Deeds, Mang, and Frandsen 2004). Yet not all things institutionalized in new ventures provide favorable outcomes. Honig and Karlsson (2004) found that the institutionalization of written business plans in such firms in Sweden was unrelated to either profitability or survival.

This raises a question as to what is the position of neoinstitutional theory on the outcomes to be expected from institutionalization. The evidence is incontrovertible that, over the long run, institutionalized entities can survive for extensive periods of time even though they possess clear inefficiencies that should logically produce failure and demise (Meyer and Zucker 1989). In the short run, survival under such conditions is much less likely. Institutionalization can certainly yield a greater effectiveness in eliciting resources, but not necessarily greater efficiency (Friedland and Alford 1991). In contrast, there are also claims that institutionalization can contribute to greater efficiency and effectiveness (Orrù, Biggart, and Hamilton 1991).

Once institutionalization has occurred, the survival and prosperity of the form involved depends less on its performance than on the reassuring sense of order conveyed. Thus, survival chances can be at their height when by most measures task performance is declining rapidly (Brint and Karabel 1991).

Once minimal standards are met, institutionalization is likely to bring about a continuing pursuit of the esteem of external judges. This pursuit of reputation is a means to gaining legitimacy, and significant changes in reputation may well drive the institutional basis of behavior (Staw and Epstein 2000).

Selected Statements with Regard to the Professional Domain

Much of the theory development with respect to institutionalization has occurred in the context of organizational structures and bureaucratic systems. Thus, professional organizations and the professions, although mentioned, are not given a central role in the preceding discussions. Yet, if we are to get to theories, the necessity of understanding institutionalization within the scientific professions, of which theories are a part, becomes crucial. In fact, neoinstitutional theory, and its older version as well, have a good deal to say about the professions.

One such statement is that the professions fall under the normative pillar of institutions—compliance by social obligation, binding expectations, normative mechanisms, the logic of appropriateness, certification and accreditations as indicators, and legitimacy grounded in morality (Scott 2001). This normative emphasis is often associated with the earlier brand of institutional theory, and carries with it a highly prescriptive, evaluative, and obligatory dimension.

Also associated with this earlier normative view is the idea that institutionalization is a variable, with different degrees possible, such that where goals are well defined and technologies precise, institutionalization is likely to be minimal (Scott 2001). Ambiguity, on the other hand, fosters institutional development and manifestation.
Training programs in business organizations and education generally, being professionally controlled, are often institutionalized, and thus only loosely linked to goals and objectives (Scott and Meyer 1991). Since theories are often taught in such courses, this has implications for theory evaluation as well. Professional education also provides members with a common, institutionalized culture, in that common definitions of problems and common methods for handling these problems are taught (Tolbert 1988). Thus, the use of theory in solving problems, and beliefs as to what are good theories for handling what problems, have the potential for institutionalization as a function of professional training.

Overall, the greater the degree of reliance on academic credentials and the more professional networks and associations characterize a field, the more institutionalism is to be expected, with no necessary gain in efficiency to be anticipated (DiMaggio and Powell 1983).

New institutions arise and become established when institutional entrepreneurs bring their political efforts to bear to accomplish their ends, while fulfilling their values; thus, institutional entrepreneurship is a primary means of change (Dorado 2005). Developing or creating an environment to enact their claims, including a constituency, is the key task that institutional entrepreneurs face in carrying out a project (DiMaggio 1988). This description of how self-interest operates in institutionalization projects applies to professional efforts and the activities of theorists just as it does in other instances. Such efforts tend to be expensive and require access to substantial resources. They also require extensive collaboration and high levels of involvement across often changing networks (Lawrence, Hardy, and Phillips 2002).

Theorization within institutional theory is the means by which new ideas are introduced as local deviations from prevailing conventions. The term indicates the development and specification of abstract categories, and the establishment of cause-effect chains; it formulates ideas into understandable and well-argued formats (Greenwood, Suddaby, and Hinings 2002). Theorization is a source of change, just as any new theory is, and it is part of the institutional entrepreneurship process.

Within institutional entrepreneurship, aspects of technologies are built into common patterns among members of a collective as new standards. This process is full of problems, and the standards involved are inherently fragile; the creation of standards in institutions is inevitably manipulative, devious, and political (Garud, Jain, and Kumaraswamy 2002). All of these characteristics would appear to be present when a theory is being espoused for institutionalization.

The introduction of organization behavior as a science within the business schools may be cited as an instance of institutional change that occurred along the lines specified above. In discussing this change and her research on it, Goodrick has the following to say:

Using an institutional perspective, I analyze the historic circumstances surrounding the shift from a management as a vocation model to one that is scientifically based. I argue that prior to World War II, the management education field was fragmented but dominated by a vocational model in which specific trade practices and skills were taught. I trace how the institutional field shifted to embrace a model of management education that was tightly linked to empirical research. Using the Academy of Management Journal articles as a marker for this paradigm shift, I test hypotheses about the diffusion pattern of science-based concepts of management education. (Goodrick 2002, 649)

Similar gradual shifts to institutionalize science as the core ingredient of a field have been described for other disciplines as well. A recent example is Schofer’s (2003) treatment of the historical emergence of geological science.
The Role of Theory in Institutional Thinking

Theory in the scientific professions may be viewed as a social technology that is standardized in various contexts (Sampat and Nelson 2002). At least in organizational behavior, theory as a technology has gradually become institutionalized from a point where a high proportion of the writing in the field was outside the scientific domain, and more appropriately labeled as philosophy, to the present situation where scientific theory is accepted as the taken-for-granted approach to solving professional problems (see Chapter 5 in this volume). Confirmation of the current institutionalized status of theory in organizational behavior is inherent in statements by leading journals to the effect that it is the means by which we move to further research and improved practice. Manuscripts that do not contain adequate theory are said to have limited value. Further evidence on the point comes from various journal forums and special issues dealing with the theory topic— not specific theories, but theory generally, as a social technology in organizational behavior (see Sutton and Staw 1995; Elsbach, Sutton, and Whetten 1999; Ghoshal 2005). A discussion of the nature and role of special issues may be found in Olk and Griffith (2004).

One characteristic of social technologies is that it is often difficult to determine whether they work. A belief that a social technology is effective can persist, incorrectly, because adequate feedback is not available (Sampat and Nelson 2002). Within scientific theory, we handle this problem by demanding extensive validation research, performed according to very precise rules. But in actual practice, we still accept theories as “good” when practically no research has been performed on them, or even ignore research that provides clear negative feedback. To the extent this is true, institutionalization would appear to often prevail.

No actual measure of the institutionalization of organizational behavior theory in general is developed or provided here. I take that as given. However, if theory in general is institutionalized, one ideally should be able to point to specific theories that fit this mode. That I will attempt to do. Thus, by indirection, arguing from multiple instances of the specific to the general, this book does provide some evidence on whether theory in organizational behavior is an institutionalized technology.

Another issue focuses on the matter of the direction, positive or negative, of the institutionalization process. Is it good theory or bad theory that becomes institutionalized as such, or sometimes one and sometimes the other? Occasionally in the literature, one finds instances where both positive and negative features are considered a basis for institutionalization, but not with regard to theories. In fact, there is very little literature to provide guidance with regard to the conduct of research on the institutionalization of theories. The closest approximation to guidance of this kind comes from the Staw and Epstein (2000) article dealing with the institutional process as it surrounds the adoption of popular management techniques in the corporate world. Since these techniques are often espoused by various organizational behavior theories, there is reason to follow whatever guidance comes from this source.

Staw and Epstein (2000) set forth fourteen hypotheses for investigation. All but two of these are stated so as to consistently link the possession or adoption of popular management techniques with positive outcomes, and thus, positive directionality. One exception does not speak to direction at all; the other presents two antithetical hypotheses (a and b), one positive and the other negative. Thus, there is only half a hypothesis where negative factors are investigated in the research. Following this lead, the present research adopts the precedent of placing primary emphasis on the positive tail of the evaluation distribution, taking the posi-
tion that it is only when a theory is rated as important that it becomes a source for institutionalization, not when it is described as unimportant. This position is consistent with the preponderance of the writing in the field. It is also supported by the institutional entrepreneurship concept, which indicates espousal and promotion of the particular technology involved. Evidence testing the appropriateness of using this type of positive directionality with the data of this study is presented in Chapter 20.

**CONFORMITY THEORY**

Within institutional theory, the concept of conformity is typically used to refer to the adoption of organizational structures and processes mandated by the external environment, and thus becomes a means to organizational survival (see, e.g., Powell 1991; Scott 2001). In this instance, the term is used at the organizational level, not the individual level. The idea that conformity can be internally motivated within the individual is not at variance with institutional theory, and there are references to the idea that with institutionalization comes strong pressure on individuals to conform and to avoid any deviance (Scott and Meyer 1994). Yet the theory rarely moves to the individual level and individual differences are almost never mentioned. A central tenet of institutional theory is that individuals have a preference for certainty and predictability in organizational culture (DiMaggio 1988), but that certain people may prefer ambiguity and uncertainty (see McCasky 1982) is not considered. It is a fact that all kinds of individual differences do operate, and not everyone behaves in accordance with institutional forces (not even laws).

Conformity theory is psychology’s version of institutional theory. Generally, the two approaches do not talk to each other, or even recognize each other. Yet I come to institutional theory via conformity theory, and my work with evaluative distributions, work that ultimately brought me to the measurement approaches described in Chapter 20, derives from a conformity source. Furthermore, I believe that conformity theory, insofar as it emphasizes norms and their functioning, has considerable insights to contribute to institutional theory. In its early period, normative processes were central to institutional theorizing; within neo-institutional theory, however, norms are considered primarily as related to the professional domain. I believe that norms are indeed central to the professional theory. What follows is a proposal to feature them within institutional theorizing and to demonstrate that insights from the psychological level can contribute to the development of a more comprehensive theory.

With this in mind, I launch upon a treatment of conformity theory and research that tracks not only my own personal exposure but also the route to my developing the measurement approach of Chapter 20.

**Origins of Conformity Theory in Projective Measurement**

Conformity theory, like its institutional cousin, is not one. There are numerous versions, each with its own measure, and although some relationships among these measures have been found, the predominant pattern is one of fragmentation. Thus, construct validity for conformity in a comprehensive sense does not appear to exist. One broad thrust has come from the study of social influence processes within social psychology (Cialdini and Goldstein 2004). Another thrust, in this instance from clinical psychology, provides the approach that underlies the measure of institutionalization considered here. Foster (1968) offers evidence
that a measure of this kind is positively related to intelligence, and also to a desire to avoid recognition, to hide in the crowd; thus, there is the suggestion that conformity is learned, and that those who wish to remain indistinguishable from their peers are attracted to conforming behavior. It is also apparent that considerable variation on the conformity-deviance dimension is to be expected.

Turning now to the origins of conformity theory in clinical psychology (and in projective measurement), the first tentative statements can be found in Rorschach’s writings in the early 1920s (English translation 1949). There it is reported that certain responses to the Rorschach test cards that occur only once in 100 tests, and thus are original (labeled O), as well as those that are given by 1 in 3 normal subjects, and thus are common (labeled V for vulgar), have diagnostic value. The latter are described as representing a person’s share in the collective manner of perceiving things; they also are said to have limited value in determining the specific individual strivings of a person (i.e., little psychoanalytic meaning). The resort to a statistical definition of the vulgar response, more recently called popular, has remained part of Rorschach nomenclature ever since.

We took this statistical concept and applied it to another projective instrument, the Tomkins-Horn Picture Arrangement Test (Tomkins and Miner 1957), often referred to as the PAT. The PAT requires a subject to select the particular arrangement of a set of three pictures (plates), placed at 120 degree angles to one another, that the person feels makes the best sense. There are a total of twenty-five such arrangements to be selected. The stimulus pictures represent everyday life, and over half deal with aspects of the work situation. There are six possible arrangements for each plate. Data based on the administration of the PAT to a representative sample of the U.S. population via survey research permit attaching percentage frequencies to each set of six arrangements (see Tomkins and Miner 1957).

An average frequency of response for each subject may be computed by totaling the percentages from the U.S. data for the twenty-five arrangements the person selects, and then dividing by twenty-five. The higher this figure the more the overall conformity to norms derived from the U.S. data. Much closer to Rorschach’s approach was a second measure called adherence to social norms, which defined a norm as a response given by 50 percent or more of the U.S. sample; there are nine such arrangements in the range 51 percent to 86 percent. This latter measure presumably comes closer to institutional theory, but the two conformity indexes correlate at $r = .89$. Both of these measures yield adequate reliability.

It proved possible to compute similar measures based on samples of organizational or reference group members, rather than the U.S. sample, and then to compare these findings with the U.S. data to determine if a high or low degree of norming was present and how this norming matched up with the U.S. norms. For the average frequency measure, the correlations were generally in the .80s and .90s; for the social norms, many moved down into the .60s and .70s. There is a lot of commonality here across organizations and groups with the overall U.S. culture. But typically these samples exhibited significantly more norming than the overall population; a few, however, mostly prisoners and long-term hospital patients, exhibited less norming. The commonality with the U.S. data and increased norming extended to other groups outside the United States, in Italy and Pakistan, for example (Miner 1965a).

**Correlates of the Conformity Measures**

A key factor in relating findings in the conformity area to institutional theory is the correlation between the conformity measures and outcomes such as management level, compensa-
tion, performance ratings, and individual success. Miner (1965a) reports on a small-sample study of managers in one business organization where sizable positive correlations with potential ratings were obtained, but actual performance ratings were significant at only \( p < .10 \).

In a bank management group studied by Michael Conklin and published in Miner (1965b), significant negative correlations were obtained with performance and potential ratings, but no relation to grade level was found. Success as indicated by a Who's Who in America listing was not related to conformity among groups of senior business executives and intellectual leaders (university professors) (Miner 1962). Stoess (1973) was not able to establish any correlation between conformity and organizational grade level among managers from eight Nevada companies.

In four school districts where substantial norming was present, no significant relationships with either administrative grade level or compensation were found. Using performance ratings, six of twenty-eight correlations were significant in one of the districts, but none in the other three; all six values were negative, indicating that nonconformity (deviance) was associated with better performance (Miner 1967).

The positive association between conformity and intelligence and the negative relationship with desire for recognition have already been noted. In the U.S. sample, conformity rose with age into the late teens, then stabilized, and started to decrease in the mid-thirties on average. This increasing decline with age appears to reflect a degree of disenchantment with what group relationships and compliance with group norms have to offer (Miner 1965a). Conformity levels in more highly educated groups tend to be elevated, and we have found instances where specific college courses have produced some increase (Miner 1963; 1965b), but the exact dynamics of the education findings remain unclear. It is also uncertain what is operating to cause the wives of business managers to score lower on conformity than the managers themselves; perhaps it is the conformity-producing pressures on the husbands of the employing organizations, but that is not certain (Stoess 1973). Note that in this study, typical of the times, the husbands worked and the wives did not.

**Parallels to Institutional Theory**

The parallels between findings from institutional theory and conformity theory are substantial, including most notably the fragmentation existing in both theoretical domains, the concern with social norms in both, and the lack of consistent positive relationships to outcomes such as performance. It is tenable to hypothesize that professions and organizations operate as individuals do in seeking the security of conforming-institutionalized behavior. There may well be many additional generalizations found to extend across these two domains, if and when more research is conducted for that purpose. In any event, the important point to be noted here is that the analysis of evaluative distributions undertaken in this chapter grew out of the earlier work on average frequencies of response and adherence to norms using similar distribution data. The selection of one of six possible arrangements on the PAT represents a judgment of preference, in many cases demonstrating a desire to meld with the majority, which, when aggregated across individuals, results in an evaluative distribution.

The Foster (1968) analysis, which involved correlating a number of conformity indexes including the PAT measures, scores from a social influence situation, and various self-report scales, produced only a limited number of relationships. At the time, I interpreted this as evidence of a lack of construct validity. However, in the interim, it has become apparent that the PAT measures, being projective in nature, would not necessarily be expected to correlate
well with self-report scales (see Miner 2006b). In addition, evidence now exists that the social influence situation that Foster (1968) used “may be better described as tactfulness or social sensitivity . . . its descriptions as conformity may not be . . . appropriate” (Bond and Smith 1996, 126). Accordingly, given what is known now, my earlier concerns about construct validity may well have been misplaced. The PAT data using evaluative distributions may be a much better indicator of conformity to be applied to institutionalization contexts than was apparent some thirty-five years go.

That my own conformity research terminated when it did was partially attributable to my reading of the Foster (1968) results, but there were other considerations as well. In the late 1950s and 1960s, there was a blossoming of interest in conformity (possibly somewhat institutionalized) occasioned by the publication of William Whyte’s book The Organization Man (1956), and also by a number of his articles in Fortune (see Miner 1962). The topic took hold within organizational behavior as well (see Berg and Bass 1961; Walker and Heyns 1962), prompting me to devote my energies to work in the area. But by the 1970s, conformity as a field of study, at least in organizational behavior, was clearly running out of steam, and my interests shifted to other seemingly more promising research areas. To my knowledge, no one else has picked up on these beginnings in the projective stream of conformity research. Perhaps, if I had then been more aware of developments within institutional theory and more foresighted as well, I would have continued on, but I was neither. I now believe that the potential in this brand of conformity theory is considerable, and that it could well contribute to an expanded understanding of institutionalization processes.

CONCLUSIONS

In the context of the previous discussion, and recognizing the extent of the evidence I am able to bring to bear, the following hypotheses for the subsequent research are proposed:

1. Drawing on the importance ratings by organizational behavior scholars considered in Chapter 17, indexes to measure the institutionalization of organizational behavior theories should prove capable of creation using these evaluative distributions; accordingly, these indexes should serve to identify certain theories that have become institutionalized.

2. Theories thus identified as institutionalized should have an earlier year of origin to reflect the time needed for institutionalization to occur and become established; recent theories should not be institutionalized.

2a. As the earliest theories and formulations directly associated with the field, the preorganizational behavior theories should exhibit more extensive institutionalization than theories categorized as being of a truly organizational behavior nature.

2b. Because a major thrust in theory development during the early years of organizational behavior was empowerment and humanism (Wall, Wood, and Leach 2004), theories of this kind should exhibit more extensive institutionalization than theories lacking in such a humanistic orientation.

3. The null hypothesis should apply in that the institutionalization of theories should be unrelated to outcome factors dealing with efficiency and performance.

3a. In view of the above, institutionalization should not relate to estimated validity indexes.
3b. In view of the above, institutionalization should not relate to indexes of estimated usefulness in practice.

4. Indexes to establish scores that measure institutionalization using evaluative distribution data for individuals who rate organizational behavior theories should prove capable of creation; accordingly, these indexes should serve to identify individuals who are institutionalizers and thus exhibit conformity motivation. Individual differences should be clearly manifest through this process.

The methodology for testing these hypotheses is set forth in the next chapter.

REFERENCES


Chapter 20

Methodology of the Institutionalization Analyses

Obtaining the Institutionalization Scores
  Testing for Institutionalization’s Main Effects
  Deriving an Institutionalization Score for Individual Theories
  Testing for the Significance of an Institutionalization Score

Reliabilities for Measures

Measures Beyond the Institutionalization of Theories
  Decade of Origin, Humanism, Validity, and Usefulness
  Institutionalization of Individual Judges

Construct Validity

Conclusions

The base data used to establish the institutionalization of the seventy-three theories were the importance ratings considered in Chapter 17. These were exactly the same figures as were described previously; the ninety-five judges, accordingly, were the source of the data. However, now the focus becomes the evaluative distributions.

Obtaining the Institutionalization Scores

A key consideration in the development of an institutionalization index was that a modal elevation should appear on the positive side of the evaluative distribution of importance ratings, reflecting the impact of those theories that had become in fact institutionalized. No such modal elevation should be apparent within the negative tail. Thus, the negative side of the distribution should follow the normal curve rather closely, but the positive side should not. Whether any such tendency to bimodality might bring about a significant deviation from the normal distribution would depend upon the extent to which theories had become institutionalized in organizational behavior. The discipline is relatively new among the social sciences, having at best achieved its fiftieth birthday, and it may not as yet have had time to institutionalize many, or even any, of its theories.

Testing for Institutionalization’s Main Effects

Table 20.1 presents the results of an attempt to test this hypothesis regarding the shape of the evaluative distribution. Two distributions were employed— one involving the mean importance ratings obtained for the seventy-three theories, and the other consisting of the percentage of these theory ratings falling in the 5–7 grouping in the positive tail above the mean of means. There should be an elevation in frequency in this set of ratings if sufficient institutionalization is operative; thus, a significant departure from the normal distribution should be identified.
In Table 20.1, the statistical technique employed is the Kolmogorov-Smirnov (K-S) test. In such instances, where goodness of fit is to be determined, the frequent procedure is to compare the observed distribution against figures expected based on the normal curve, and to do this using chi-squares. However, several considerations argue for substituting the Kolmogorov-Smirnov statistic $D$:

The advantages of the K-S test over the $x^2$ test for goodness of fit are twofold: the K-S test is applicable for $N$ smaller, and it is a more powerful test than the $x^2$ test. The latter advantage means that departure from normal form is more apt to be detected by the K-S test. Stated differently, compared to the $x^2$ test the K-S test is less apt to mislead us into accepting the hypothesis of normality of distribution. (McNemar 1969, 271)

In Table 20.1, no significant deviations from the normal curve appear in either distribution. However, the largest observed-expected departures occur in the 5.00-5.49 and 70-79 percent categories, both in the positive tail and both with the observed values exceeding the expected. Furthermore, the negative ends of both distributions extending downward from 4.00-4.49 and from 40-49 percent do not show any similar departures and appear to follow the normal curve rather closely. Overall, these results suggest that some limited institutionalization of organizational behavior theories may exist, but not on a major scale.

However, when data for the thirty-nine essential theories as described in Chapter 17 (based on the importance, validity, and usefulness measures) are substituted in Table 20.1 for the observed values, a quite different result is obtained. For the mean importance ratings, the largest difference score occurs once again in the .500 to 5.49 range and the result is:

$$D = .304 \quad p = .01 = .261; \quad p < .01$$
For the percentage of ratings falling in the 5–7 zone, the largest difference score also occurs in the same place as in Table 20.1 (in the 70–79 category) and the result is:

\[ D = 0.304 \quad p = 0.01 = 0.261; \quad p < 0.01 \]

**Deriving an Institutionalization Score for Individual Theories**

On this evidence, we move from the more global analyses to the individual theories themselves. To accomplish this, the analysis attempted to identify any instances where in the positive end of the evaluative distribution for a theory the frequencies increased, rather than declining as the normal curve would require. Thus, the examination focused on determining which theories might have contributed to the modal elevation noted previously. The result was the institutionalization score given in the last column of Table 17.1, where plus values indicate increasing departures from the normal distribution of a kind suggesting more and more institutionalization and minus values indicate declining frequencies and thus movement on a vector consonant with the normal curve. The specifics of calculation are as follows:

Compare the frequencies for a rating of 5 with those of 6, 5 with 7, and 6 with 7. If any of these comparisons yield an increase, note the largest such difference and assign a plus (+) to that value. If there is no increase on any of these three comparisons, determine the smallest difference in the 5–7 range and assign a zero (0) or minus (−) to that value. Zero (0) frequencies at the extremes of a distribution should be treated as appropriate to the calculations noted above, but only one zero should be included.

Examples are:

**For theory no. 69 in Table 17.1—Resource Dependence Theory (Pfeffer, Salancik)**

<table>
<thead>
<tr>
<th>Rating</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>None</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>0</td>
<td>5</td>
<td>6</td>
<td>6</td>
<td>14</td>
<td>21</td>
<td>16</td>
<td>3</td>
</tr>
</tbody>
</table>

Comparing 5–6: +7

Comparing 5–7: +2

Comparing 6–7: −5

Institutionalization score = +7

**For theory no. 36 in Table 17.1—Path-Goal Theory of Leader Effectiveness (House)**

<table>
<thead>
<tr>
<th>Rating</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>None</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>1</td>
<td>6</td>
<td>9</td>
<td>13</td>
<td>17</td>
<td>17</td>
<td>4</td>
<td>4</td>
</tr>
</tbody>
</table>

Comparing 5–6: 0

Comparing 5–7: −13

Comparing 6–7: −13

Institutionalization score = 0

As can be seen in the last column of Table 17.1, this process produced thirty-four theories with a plus (+) score, thus indicating some movement in the direction of institutionalization. This institutionalization score is a rough measure, in that it does not express an actual deviation from the normal curve values. However, it should be a sufficient approximation to such an index. In testing the hypotheses noted at the end of Chapter 19, this score was used to determine relationships. The problem in doing this is that the scores, ranging from +22 to −8 as they
do, tell us nothing about where in this range institutionalization starts. Quite clearly, it starts somewhere in the range of +1 or above, but a method of precisely determining which among these theories should be identified as significantly institutionalized is needed. Random fluctuations in the plus (+) scores must be culled out.

**Testing for the Significance of an Institutionalization Score**

In order to accomplish a precise determination of which theories were actually institutionalized, the frequencies observed in the 5–7 rating range were set against those in the 1–3 range; thus, comparing the positive segment of the distribution where institutionalization was presumed to be operative with the negative part, where it was not; a chi-square value was then calculated, focusing on the frequencies in the 5–7 range that yielded the positive institutionalization score and then comparing these with their counterpart in the 1–3 range. The result was a 2 x 2 chi-square.

This process may be illustrated using the data for theory no. 69 in Table 17.1:

<table>
<thead>
<tr>
<th>Rating</th>
<th>5</th>
<th>6</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>14</td>
<td>21</td>
</tr>
</tbody>
</table>

\[ x^2 = .73 \]

This value is not significant at \( p < .05 \); thus, institutionalization is not presumed.

In some cases, the comparable frequencies in the 1–3 range were too small for the calculation of a chi-square. In these cases, the comparison cells to be used were moved upward on the rating scale (usually by one point) until sufficient frequencies were obtained. Thus:

For theory no. 51 in Table 17.1—Mechanistic and Organic Systems (Burns, Stalker)

<table>
<thead>
<tr>
<th>Rating</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>None</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>1</td>
<td>0</td>
<td>4</td>
<td>11</td>
<td>16</td>
<td>22</td>
<td>15</td>
<td>2</td>
</tr>
<tr>
<td>Institutionalization score = +6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Rating</td>
<td>5</td>
<td>6</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Frequency</td>
<td>16</td>
<td>22</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

\[ x^2 = 4.20 \]

This value is significant at \( p < .05 \); thus, institutionalization is presumed.

(Since the Ns in 3 and 2 were insufficient.)

The institutionalization scores in Table 17.1 marked with an * are those that, like no. 51, survived this process and achieved at least a \( p < .05 \) significance level. There are eleven of them. These theories were compared with those that did not manifest evidence of significant institutionalization to test the hypotheses.

**RELIABILITIES FOR MEASURES**

Table 20.2 contains reliability data for various measures. For the moment, the major concern is with the figures reported for the importance ratings and institutionalization scores for theories. The Spearman-Brown formula has been applied where the N involved has been reduced in order to carry out the reliability calculations, and thus needed to be corrected to bring it back up to the level inherent in the original sample. The reliabilities for the theories
measures are generally satisfactory, or, in the case of the mean importance rating, actually quite superior. The one exception is the institutionalization score for the strategic management group. There, the value of .42 is entirely unacceptable and precludes using this measure in any of the analyses.

We will take up the other reliability data in Table 20.2 in the following sections.

**MEASURES BEYOND THE INSTITUTIONALIZATION OF THEORIES**

The hypotheses stated in Chapter 19 require measures above and beyond those involving the institutionalization of theories. These concern us in this section.

**Decade of Origin, Humanism, Validity, and Usefulness**

To date the beginning of each theory, I used the decade in which it occurred. Pinpointing this date any more specifically is not only unnecessary but also rather perilous. Did a theory originate with the first reference to an aspect of it in a footnote or a section of an article on a somewhat different topic, or should we wait until the definitive book is published? These are hard questions to answer, questions that do not usually arise when the appropriate decade is used to establish the date of origin. The midpoint of the decade can be used in this instance if a specific year is needed.

A related measure needed to test hypothesis 2a was the identification of preorganizational behavior theories and formulations. This has been done in Part II, chapters 6 through 11, of this volume, and with the first seven theories of Table 17.1. In the same manner, to test hypothesis 2b, a measure of the humanism inherent in theories was required.

This humanistic value orientation of a theory was rated by the author. This is the predominant value position that was evident in theories of organizational behavior throughout the early history of the field, and determining whether it continued on to drive the institutionalization process is important. Ratings on this score were made initially in connection with the analyses reported in Miner (1990). In addition, any theory in Table 17.1 that was not assessed in this earlier analysis was judged on the same basis for the present purposes.

Humanistic theories (++) are those that clearly promote humanism—a commitment to

---

**Table 20.2**

**Reliabilities for Variables Used in the Research**

<table>
<thead>
<tr>
<th>Groups involved</th>
<th>Mean importance rating</th>
<th>Institutionalization score</th>
<th>Estimated scientific validity</th>
<th>Estimated usefulness in application</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>theories*</td>
<td>raters†</td>
<td>theories*</td>
<td>raters†</td>
</tr>
<tr>
<td>Organizational behavior group</td>
<td>.95</td>
<td>.97</td>
<td>.68</td>
<td>.76</td>
</tr>
<tr>
<td>Strategic management group</td>
<td>.87</td>
<td>.99</td>
<td>.42</td>
<td>.75</td>
</tr>
<tr>
<td>Total sample</td>
<td>.96</td>
<td>.97</td>
<td>.71</td>
<td>.76</td>
</tr>
</tbody>
</table>

*Test-retest over a ten-week interval with the Spearman-Brown formula applied.
†Internal consistency with the Spearman-Brown formula applied.
‡Test-retest over a twenty-three-year interval for thirty-four theories.
human welfare, dignity, and growth to full potential, which may extend above and beyond organizational concerns. These theories may contain an alternative form or approach, but this alternative is either largely ignored or it is handled as a straw man. Partially humanistic theories (+) tend to contain humanistic and nonhumanistic alternatives, but the various contingent components or moderated relationships are taken more seriously. As a result, the theory does contain a full-blown nonhumanistic component. Finally, nonhumanistic theories (0) do not contain a humanistic alternative as a major aspect, and do not serve to promote humanism. In most cases they are simply concerned with other matters. Although such a theory could be intendedly antihumanistic, this is not typically the case. Of the thirty-two theories rated for the 1990 publication, eight were fully humanistic, another ten were partially humanistic, and fourteen were nonhumanistic.

Testing hypothesis 3 in its various forms required the use of ratings as given in Table 17.1 for estimated validity and estimated usefulness in practice. The way in which these ratings were developed is described at length in Chapter 17 as well. Table 20.2 contains reliability data for these indexes. The validity and usefulness figures in the table are clearly underestimates, given the time interval involved. In view of the nature of the Miner (1984) sample, reliabilities for other than the organizational behavior group could not be obtained. In any event, the data clearly indicate that the reliability of these two performance measures for the theories are more than adequate. On the same evidence, similar findings should apply to the humanism measure.

Institutionalization of Individual Judges

The findings involving raters, not theories, were obtained from measures developed for the purpose of showing how individual differences might contribute to the results obtained. Were there not just institutionalized theories but institutionalizing (conforming) judges as well, and if so, how widespread was this phenomenon? These were the data used to answer hypothesis 4. The ninety-five evaluative distributions used for this purpose were composed of the ratings given to the seventy-three (or often fewer) theories by each of the ninety-five judges. Institutionalizing processes were established as with the theories. Thus, for example:

For an organizational behavior rater (hypothetical)

<table>
<thead>
<tr>
<th>Rating</th>
<th>1</th>
<th>2</th>
<th>3</th>
<th>4</th>
<th>5</th>
<th>6</th>
<th>7</th>
<th>None</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>0</td>
<td>0</td>
<td>14</td>
<td>23</td>
<td>6</td>
<td>12</td>
<td>17</td>
<td>1</td>
</tr>
</tbody>
</table>

Institutionalization score = +11

Rating 5 _ _ _ _ _ _ 7 Comparing the frequency values:

<table>
<thead>
<tr>
<th>Rating</th>
<th>5</th>
<th>6</th>
<th>17</th>
</tr>
</thead>
<tbody>
<tr>
<td>Frequency</td>
<td>6</td>
<td>17</td>
<td></td>
</tr>
</tbody>
</table>

\[ x^2 = 20.49 \]

This value is significant at \( p < .01; \) thus, an institutionalizer is presumed.

In Table 20.2, the reliabilities for these rater institutionalization scores (like those for the theories, excluding the strategic management group) are adequate, but not high. They appear to have been depressed by the fact that, unlike the mean importance ratings, they utilize only part of the evaluative distribution. In future research with these scores, it would be desirable to use larger samples, and thus somewhat more stable data. The institutionalization scores obtained from the judges’ distributions produced forty-five plus (+) scores, essentially the same proportion as with the theory distributions (47 percent).
The construct validity of the institutionalization scoring was assessed initially using a social norms measure taken from the evaluative distributions as discussed in Chapter 17. This is not an independent measure, but it needs to be related to the institutionalization scoring to determine how well the two are coordinated. Should there be any problem on that score, the fact needs to be identified and steps taken to deal with it. The idea was to provide an index comparable to the adherence to social norms index obtained in the conformity studies discussed in Chapter 19. From the conformity work with the Picture Arrangement Test, one would expect a relationship in the .80s.

The objective in creating this social norms index was to establish norms operating in the 5–7 rating zone where at least 50 percent of the judges responding selected a particular response. There were no such norms for any of the individual ratings—5, 6, or 7. However, when ratings of 6 and 7 were combined, fifteen such norms were identified with frequencies extending from 50 percent to 73 percent. The question here was to establish whether this index correlated positively with the other institutionalization measures, thus duplicating the conformity analyses and contributing to construct validity.

This social norm index was computed as well from the individual evaluative distributions numbering ninety-five. Again fifteen norms with combined frequencies (on ratings of 6 and 7) of 50 percent or more were identified—in the range of 51–95 percent. As with the theory norms, these data were obtained to provide evidence as to construct validity, but in this instance with regard to the institutionalizing processes of individuals (thus individual differences).

Construct validity (Bagozzi, Yi, and Phillips 1991) in the ideal instance requires convergence from an independent source, and thus something beyond the social norms comparison is needed. A source of this kind that has the potential to provide added evidence is the Bedeian and Wren (2001) survey of the Fellows of the Academy of Management dealing with influential books in the field, that is, those that had a major impact on management thinking at the time of their publication. By comparing the results of this survey with the institutionalization scoring, it should be possible to establish more conclusively whether that scoring does possess construct validity. Such an analysis was indeed carried out.

**CONCLUSIONS**

I have been able to obtain nine reviews of various drafts of the material contained in this chapter. Some of these reviews are more laudatory than others. This is a new approach and thus those who were willing to provide their comments were dealing with a new type of analysis that required them to move back and forth between micro and macro subject matters. Not surprisingly, a number of misinterpretations of my procedures occurred, and I have attempted to expand upon what was said in earlier versions here to forestall similar failures of understanding. As Tolbert and Zucker (1996) indicated, the type of approach I have taken is likely to arouse a good deal of controversy, and indeed, it appears to have done that. In what follows, I will focus on these contentious comments and attempt to indicate what I have done to deal with them.

Of the greatest significance is what the reviewers had to say with regard to the various calculations and statistical procedures contained in this chapter. Overall, those mentions were few in number; criticisms on this score were minimal, and those that were raised have been treated in the foregoing. Thus, a concern with the use of the Kolmogorov-Smirnov test
has produced an explanation of why this test was used. On balance, the reactions to this aspect of my methodology seem to represent a solid endorsement. My calculation approaches and statistical procedures appear to have been viewed as quite satisfactory, although alternative statistical methods are mentioned on occasion.

Some reviews failed to reflect an understanding of my points with regard to the relation between conformity and institutionalization. Accordingly, this issue has been treated at much greater length here. It is hoped the result has been a considerable clarification.

The matter of construct validity was also raised as a concern, although not usually indicated using that term. I have added a section of my methodology for dealing with this issue. Yet, I am sure that for the skeptical this will not be enough. Some will doubt that I am truly measuring institutionalization, and they are unlikely to be convinced on this point without much further research. This is part of the contentiousness that Tolbert and Zucker (1996) envisioned. All that can be said at this point is that an effort to handle construct validity is included in the methodology. How much is enough will have to be determined over time, but I believe that a needed start to deal with the issue has been made.

On the evidence from these reviews, the approaches to methodology presented in this chapter would seem to meet the requirements for good science. Next, then, we need to consider the findings that they produced; these are presented in Chapter 21.

REFERENCES


CHAPTER 21

FINDINGS FROM THE INSTITUTIONALIZATION ANALYSES

Findings
  Hypothesis 1
  Hypothesis 2
  Hypothesis 3
  Hypothesis 4
  Construct Validity—Social Norms
  Construct Validity—Influential Books

Discussion
  Contributions to Institutional Theory
  Contributions to Learning
  Contributions to Essential Theories

Conclusions

The findings to be presented will be considered in terms of the numbered hypotheses as indicated at the end of Chapter 19. The discussion that follows the findings attempts to place these results in the context of neoinstitutional and conformity theory; at the same time, we will look into the meanings that the findings have for the maturing of organizational behavior as a science.

FINDINGS

Hypothesis 1

Eleven theories, of the seventy-three studied, meet the criteria for institutionalization in that they have positive institutionalization scores and significant chi-squares. This is not a large number, given that the seventy-three theories were selected so as to have a sizable chance of institutional status in the first place. Nevertheless, these eleven, as indicated in Table 21.1, give credence to hypothesis 1; it is indeed possible to create methods that work to identify theories that have become institutionalized within the field of organizational behavior.

All of these theories have positive institutionalization scores, while among the sixty-two theories that do not evidence institutionalization, there are twenty-three such positive scores and thirty-nine with scores of zero or negative scores ($x^2 = 14.98; p < .01; df = 1$). Although the institutionalization scores in Table 21.1 are among the highest such scores obtained, they do not include all such high scores (see Table 17.1). The correlation between the importance rating means provided by the organizational behavior judges and institutionalization scores is $.65, p < .01.$
Taken as a whole, these findings add up to substantial support for hypothesis 1. Yet they also indicate a need for construct validity evidence; this will be provided subsequently.

Hypothesis 2

The data also support hypothesis 2, although not clearly either 2a or 2b. The most evident differentiating characteristic of the institutionalized theories is their decade of origin. As indicated in Table 21.1, the institutional theories date from the beginning of the 1900s through the 1960s, with a mean of 1954.1 (s.d. = 19.2). This compares with a noninstitutionalized mean date of 1966.9 (s.d. = 16.4), more than a decade later. The t is 2.34, p < .05. When one computes a chi-square from these data, setting the pre-1970 period against the period from the 1970s on, a median split, the value is 8.96 (df = 1), p < .01. The correlation between the institutionalization score and the date of origin is –.22; thus, the more the institutionalization, the earlier the date. This figure does not quite reach significance, but is consistent with the other findings. The really distinctive result here, as indicated in Table 21.1, is that there are no institutionalized theories in the period since 1970; newness is not characteristic. The chi-square analysis points this up best. Yet, more than 50 percent of the theories that have not become institutionalized stem from the 1960s decade and before; there is no guarantee that just because a theory had an early date of origin it would have become institutionalized. These results testing the overall statement of hypothesis 2 are in accord with the literature in general on neoinstitutional theory, as reflected, for instance, in the Tolbert and Zucker (1983) research. Although the period of time taken for each of these theories to achieve institutionalization remains unknown, no theories from the 1970s, of which there are nineteen, have as yet achieved that status; thus, assuming that some of these theories will, institutionalization must take at least thirty years.

With regard to hypothesis 2a, only two of the seven preorganizational behavior formula-
tions treated in Part II achieve institutional status—some 29 percent. This contrasts with a 14 percent figure for the theories of organizational behavior itself. Although the proportion of preorganizational behavior institutionalization is larger, the differential is not sufficient to reach significance. Only number 3 (Lewin) and number 5 (Weber) have become institutionalized, and none of the other institutionalization scores rise above +3. Quite apparently, and consistent with previous results, the mere fact of being old does not work to produce certainty for institutional status. That these particular two theories rise above the others in this regard is a matter for speculation, and we will return to this topic later in this chapter.

Table 21.1 also provides data bearing on hypothesis 2b, which states that humanistic values should prevail in theories that become institutionalized because theories of this type characterized the early years of organizational behavior. During the 1960s and 1970s, this was the prevailing value thrust of organizational behavior theory. Yet, as of the year 2000, it appears to have little to do with institutionalization in the field. Comparing the institutionalized theories with the others that have not been institutionalized as to the extent of humanistic theories in each grouping, the result is a nonsignificant chi-square. This occurs both when the total sample of seventy-three theories is used, and when the sample of theories on which data are available from 1977 (Miner 1990) is employed as well. There simply is no relationship in evidence. What this seems to mean, at least from the seven theories in Table 21.1 that are not humanistic, is that scientific values have come to prevail in organizational behavior. There certainly are other value orientations operating in the field, but there is no evidence that any of these have risen to the point of influencing institutionalization processes.

In support of this position that a relationship does not exist, note that the institutionalization scores of the four humanistic theories in Table 21.1 range from +3 to +6 with a mean of 4.5. The nonhumanistic theories range from +6 to +22 and their mean is 14.3. This finding seems to indicate that within this group of theories institutionalization is associated with a nonhumanistic value orientation. However, when the data for all seventy-three theories are brought to bear, and humanism is related to the level of the institutionalization score (+ versus 0 or −), the finding is once again not significant.

Hypothesis 3

The validity analysis comparing the eleven values in Table 21.1 with the sixty-two for noninstitutionalized theories yields a mean of 3.64 (s.d. = 1.12) in the first instance and of 2.98 (s.d. = 1.08) in the second. This difference has a t-value of 1.84 (two-tailed) with a p of .07—not significant by most standards, but close to being so. Considering validity as an index of performance, this result is consistent with hypothesis 3 and with the prevailing conclusion in the institutionalization literature; the null hypothesis holds.

The usefulness index of performance, however, does not produce anything approaching a significant finding at all. The mean for the institutionalized theories in Table 21.1 is 2.73 (s.d. = 1.10) and for the noninstitutionalized 2.48 (s.d. = 1.02) with a t of .72 (N.S.). Still the trend of the data, as with validity, is toward the institutionalized theories having somewhat better performance.

When chi-squares are computed relating the fact of institutionalization to the estimated validity and usefulness measures, splitting both to set values of 4 and 5 against those of 1 to 3, nothing even approximating a significant result is obtained. This is the analysis that pits the essential theory criteria against those criteria considered not to indicate essential
theories; accordingly, it would appear to be most relevant in testing the hypothesis under investigation.

When the institutionalization score as given in Table 17.1 is correlated directly with the performance measures, without involving the tests to determine significance, the results are not as consistent with the null hypothesis. The validity correlation is .43, \( p < .01 \) and the usefulness correlation is .23, \( p = .05 \). Thus, in these instances the prior trend toward a positive relationship between institutionalization and performance achieves significance, although the usefulness finding remains marginal.

All in all, I believe that the data support the null hypothesis, and thus hypothesis 3, but that a distinct tendency exists among the institutionalized theories to exhibit a relationship with performance, especially validity. In all likelihood, this relationship manifests itself most with those particular theories that are in the early stages of institutionalization, consistent with the findings of Zucker (1983) and Westphal, Gulati, and Shortell (1997).

**Hypothesis 4**

Table 21.2 presents the results when indexes to establish scores that measure institutionalization using evaluative distribution data for individual judges are introduced. From these findings, hypothesis 4 is supported; certain individuals do exhibit institutionalization or conformity in their individual evaluative distributions. There are twenty-two significant cases,
twice as many as with the analysis of the theories. Of these, eighteen are organizational behavior judges and four strategic management judges. The proportion of institutionalizers is higher in the former instance, but not significantly so. The institutionalization scores are high (from +2 to +34), yet there are numerous scores in that range that do not appear to produce institutionalization. Sex, age, and serving as an editor or president within the Academy of Management do not significantly differentiate the institutionalizers. In fact, no factor could be identified that distinguished these institutionalizers from those who were not.

**Construct Validity—Social Norms**

When the data on theoretical norms are considered, it appears that nine of the theories that have achieved normative status (the 50 percent frequency level) in their 6–7 rating category have become institutionalized as well; six theories with norms have not. The relationship between institutionalization and norming is pronounced with \( x^2 = 29.13, p < .001 \). The phi coefficient is .64, which translates into a Pearson \( r \) at least .20 higher (.84 or above)—see Breaugh (2003). The two institutionalized theories that do not yield norms are number 9 Herzberg (+3) and number 52 Lawrence, Lorsch (+4), both humanistic in orientation and with usefulness ratings higher than their validity. There is some suggestion in this that these two theories may be deinstitutionalizing. On the other hand, the six theories with norms but no significant institutionalization tend to have higher institutionalization scores (+3 to +10, mean +7) and may be approaching full institutionalization. These hypotheses with regard to institutional change, however, require longitudinal data to test, and thus are beyond the scope of the present research. In any event, evidence supporting the construct validity of the institutionalization identification process is inherent in this normative analysis, although it is limited by the lack of independence of the two indexes.

Applying this same type of normative analysis to the evaluative distributions of the individual judges (and thus across theories) also produces strong results. Some fifteen individuals have norms contained in their evaluative distributions, and twelve of these are institutionalizers. The chi-square in this instance is 22.44, \( p < .001 \) and the phi-coefficient .55 (again with at least a .20 addition appropriate to bring this value up to a Pearson \( r \)).

The ten institutionalizers who do not yield a norm, and are thus under 50 percent in the 6–7 rating zone, have institutionalization scores ranging from +2 to +11, mean +7.4, in contrast to a range of from +6 to +34, mean 13.8, for those whose evaluative distributions manifest both institutionalization and norms. There are only three individuals who yield norms, but are not institutionalizers; the range in these cases is diverse, from -1 to +22, mean 10, on the institutionalization score. Hypotheses regarding deinstitutionalization and increased institutionalizing do not seem warranted in these instances, given that age differences are not found within the age range involved. Individual differences of this nature should be quite stable. Once again, the data support construct validity, and appear to indicate that institutional processes may develop with the strong support of only a limited number of institutionalizing individuals (in this case some 25 percent of those judging).

An apparent limitation of the research reported here is that it is cross-sectional. It tells us where we are at a point in time, but it says little regarding the longitudinal processes that operated to get us there. Some hope on this latter score comes from the 1977 data (Miner 1984). Unfortunately, however, these data derive from nominations and thus do not permit the construction of evaluative distributions; accordingly, comparison with the present findings on institutionalization is impossible.
However, norms can be derived from the 1977 data, although there are not many of them. The theories thus identified match with number 11 Vroom, number 12 Porter, Lawler, number 24 Fiedler, number 41 Simon, March, number 42 Cyert, March, and number 52 Lawrence, Lorsch in the current analysis. Of these, numbers 11, 41, 42, and 52 are now institutionalized. Only number 24, Fiedler's contingency theory of leadership, is no longer on either list. Overall, this suggests some, but not perfect, stability for at least the normative measure over time.

**Construct Validity—Influential Books**

Although the preceding analysis involving social norms adds to confidence in the construct validity of the institutionalization measurement process, or, perhaps better, fails to subtract from one's confidence on this score, more evidence is definitely needed. This evidence comes from the Bedeian and Wren (2001) survey dealing with influential books in the management field, as noted in Chapter 20. These books have been identified as the “great books” of the field by leading historians of management thought (Duncan 2004; Van Fleet and Wren 2005). Thus, their relevance to institutionalization is apparent.

The Bedeian and Wren (2001) listing contains twenty-five such books, plus four others that garnered enough votes to deserve an “honorable mention” designation. Of the twenty-five, three were clearly outside the field of organizational behavior (from strategic management and production). Two more were written (both in the preorganizational behavior period) by individuals not included in the list of seventy-three theories studied. Of the twenty remaining books, eight presented theories identified here as institutionalized covering seven different theories. An additional such theory was on the honorable mention list. The institutionalized theories not listed at all were number 3 Lewin, number 15 Adams, and number 16 Locke, Latham; all of these theories were presented initially (and over a period of some time) in articles and book chapters, not in books written by the theory’s authors. Thus, they could not have had their construct validity verified by inclusion in this list of “great books.” That this analysis is concerned with institutionalization is further documented by the fact that the mean institutionalization score for books dealing with the seventy-three theories of Table 17.1 on the Bedeian and Wren (2001) list is +4.14, while the mean for theories from the seventy-three presented similarly, primarily through books, and not on the list, is –.63 (t = 2.94, p < .01). The chi-square value here, comparing these factors and setting positive institutionalization scores against those of 0 and negative, is 6.64, p = .01, df = 1. Positive institutionalization scores are clearly found more frequently among the “great books” and negative scores are more frequent among those books not considered to be of such high status.

Furthermore, support for the institutionalization of certain theories is inherent in the findings derived from testing hypotheses 1–4, particularly hypothesis 2, dealing with the decade of origin, and hypothesis 3, dealing with performance relationships. These findings are what would be expected from prior theory and research on the institutionalization process, and thus add to our confidence that institutional factors are being measured.

**DISCUSSION**

Given the early dates on which the institutionalized theories arrived, the fact that only two of the preorganizational behavior positions considered in Part II achieve institutional status is somewhat surprising. These two (no. 3 Lewin, with a score of +5, and no. 5 Weber, with a
score of +15) have continued to generate research, and major subsidiary theories as well, whereas the other, comparable views have been characterized by a lesser impact of this kind on organizational behavior; perhaps this is the source of the difference. Yet, number 1 Mayo, Roethlisberger, Dickson, number 2 Barnard, number 6 Fayol, and number 7 Taylor are all reflected in the Bedeian and Wren (2001) listing; their institutionalization scores, however, are -3, -1, +3, and +2, respectively, while number 4 Follett has a score of +3. Those theories that have become institutionalized do stand out on the basis of their scores. Furthermore, there is reason to believe that the noninstitutionalized theories may well deal with wisdoms that no longer apply (Harvey and Buckley 2002).

Another unexpected finding relates to the content areas of the institutionalized theories. Those theories that meet this standard are from all over the organizational behavior spectrum. The authors are spread over psychology, sociology, political science, economics, and organizational behavior in terms of their origins. Some are micro theorists and some macro; some are living and some deceased. Yet there are no leadership theories in the institutionalized group, and no descendants of Weber’s bureaucratic theory either. These omissions would appear to require further explanation to establish their cause.

Previously in this volume and in a prior publication (Miner 2002), I have suggested bringing constructs from psychology and micro-organizational behavior into institutional theory. Conformity motivation and a desire to avoid conformity would fit this description. It seems to me that some individuals will easily embrace newly forming institutions in their early stages while others will reject institutions to the bitter end. These patterns should be just as characteristic of individuals as organizations. What was found here with regard to institutionalizers, and those who are not, fits individual difference conceptions of this type. Yet the present study only broaches this topic. A great deal more needs to be learned, and considerable research needs to be done. All we know for certain on this score is that individual differences exist with regard to scientific theories of organizational behavior.

Contributions to Institutional Theory

Evidence is available from the present study to the effect that the relationship between institutionalization and performance or effectiveness is positive when significance is obtained, but that such significance is often lacking. With individual theories, institutionalization apparently can contribute to substantial effectiveness, or none at all. Extrapolating this finding to the case studies or qualitative research that often typify the institutional field, one could well find almost any result, with the findings just as widely dispersed as individual theories are. Needed now are large sample studies, or perhaps meta-analyses if sufficient data are available, that would move well beyond what is provided in the present research.

The data of this investigation offer indirect support for the institutionalization of theorizing in general, at least within organizational behavior. Theory appears to be a taken-for-granted social technology of the field, widely perpetuated by professional education. Yet it would be helpful to have more direct evidence on this score, and to be able to make comparisons with other fields as well.

Although little used in institutional research, theory studies such as this one offer the prospect of revealing a great deal about the institutionalization process. Theories have their genesis in scientific domains characterized by high uncertainty and ambiguity. It appears that it is just such domains that tend to foster institutionalization, and on the evidence from
the current research, that is exactly what happens. Theories are capable of study through survey methods, which can produce the needed evaluative distributions.

A further consideration favoring the use of theory studies in institutional research relates to institutional entrepreneurship. Theories that have become institutionalized, as well as a number that have not, are typically promoted extensively by their authors and proponents through the medium of research and via persuasive communications. This is fertile ground for learning about the processes of institutional entrepreneurship and for determining how institutions develop or change. The research reported here provides little direct insight on this topic, but it does highlight an approach that might be used in the future to gain such insight.

In short, in spite of its limitations, this research does open up new ground, both for the scientific study of meta theory and for institutional theory itself. Whether this ground will prove productive in generating understanding is a matter only the future can determine. However, from a practical perspective, certain gains seem apparent. Based on the research evidence (Deephouse 2000; Staw and Epstein 2000), to the extent that an organization’s or theory’s reputation is associated with institutionalization, gains in this regard should enhance the judged performance of whatever is institutionalized. Institutional status really can have a very real practical effect, although not always enough to salvage organizations or theories from serious problems.

Contributions to Learning

As indicated earlier, a major goal of this research has been to develop a set of organizational behavior theories that can be said to have become institutionalized, and presented as such. If there is a core body of knowledge in organizational behavior, these theories represent that core at the present time. Students need to know about them because without this understanding they cannot pass the test of being literate in the field of organizational behavior.

This means that Lewin’s social psychology and Weber’s theory of bureaucracy should be included not only in history courses, but in any type of introduction to organizational behavior. Any course that is intended to cover motivation, as its primary topic or as a major segment, should deal with Herzberg’s motivation hygiene theory, Vroom’s expectancy theory, Adams’s equity theory, and the Locke/Latham formulations on goal setting. Similarly, any course concerned with macro-organization theory should consider James Thompson’s open systems theory, the Burns/Stalker view on mechanistic and organic systems, and the Lawrence/Lorsch contingency theory of organizations. Organizational decision making is often considered separately, and if this is the case, then the Simon/March theory of administrative behavior and of organizations as well as the Cyert/March behavioral theory of the firm should be taken up there. Where decision making is incorporated with the macro area (as recommended in Miner 2003), then these institutional theories should be included there. Any introductory treatment at any level that presumes to present a comprehensive view of organizational behavior needs to consider all eleven of these theories, if it is to produce graduates who will be viewed as literate in the field.

Although there is considerable overlap between the recommendations made previously (see Miner 2003) and those made here, it is important to recognize that the present recommendations stem from a very different source. The theories advocated for inclusion before were identified on grounds other than institutionalization; the institutionalized theories rec-
ommended here are significant because knowledgeable individuals would expect a student who has had instruction in organizational behavior to know about them.

However, I believe that teaching these eleven theories requires not that a literate learner come away with an understanding of just the theories themselves, but of what it means to be institutionalized. The theories need to be presented in a context of institutional theory. Without this, it makes no sense to teach a theory such as Herzberg’s, which is seriously lacking in validity, or James Thompson’s, which pays no attention at all to usefulness in application.

Much of what might be required to provide an institutional theory context has been included previously. In addition, I would recommend the treatment in Miner (2006, ch. 20), and the special research forum in the Academy of Management Journal (Dacin, Goodstein, and Scott 2002). Following the publication streams provided by the references in these sources should allow for a comprehensive coverage of institutional theory.

**Contributions to Essential Theories**

The astute reader may well recognize that adding the institutionalization criterion to the list of requirements for a theory to be labeled as essential brings only one new theory (no. 9, Herzberg’s motivation-hygiene theory) into the fold, beyond what was established in Chapter 18. However, as I have attempted to indicate above, institutionalization is of significance not so much for its marginal contribution to the number of theories considered essential as for the insight it provides into the reason for selection. Institutionalization tells us why the judges considered certain theories to be important, and thus goes well beyond the more ambiguous importance ratings to pinpoint a specific cause. With the fact of institutionalization, we are able to establish something about these eleven theories that we otherwise would not have known, something that extends understanding to a substantial degree.

**Conclusions**

Institutional status not only helps to improve understanding of individual theories; it makes a contribution to our understanding of the current position of organizational behavior itself. Knowing that certain theories have become institutionalized tells us that the field has developed to a point where its level of maturity at least matches the status of other much older and well-established social science disciplines. Thus, it provides a benchmark for the progress of the field as a whole.

With this benchmark achieved, in Part V, we now move from a consideration of where organizational behavior has been to where it might be going.

**References**


In Part V, I present an attempt to extend the time horizon for organizational behavior out into the future, thus completing the contextual package. This represents something of a changing of gears; the emphasis is now strongly on “the future” aspect in the title of this book, and on what organizational behavior’s historical origins have to say with regard to its possible development over time. In doing this, I hope to provide a more comprehensive picture of the framework within which the field is now operating as well.

With this in mind, I include here a brief overview of what other authors have said on the foundations and origins of organizational behavior and their implications for subsequent generations, if indeed such implications are treated at all.

Greiner (1979) provides a chapter for an edited volume that deals primarily with the intellectual history of the field, often from an organization development perspective. There is some discussion of the short-term future of the field.

French (1982) writes a short article, adapted from his book on organization development, that presents the history of the latter field, but in the process extends well beyond organization development into much of organizational behavior as a whole. The emphasis is on the intellectual leaders of the period up to 1960. Future developments are not considered.

Lawrence (1987) includes a chapter in the Handbook of Organizational Behavior providing wide-ranging coverage of organizational behavior’s emergence, development, methodology, and theory, as well as certain defining characteristics. This chapter contains particularly good coverage of the Harvard contributions in the early period. Little is said regarding the future of the field.

Roberts, Whetten, Pearce, Glick, Bedeian, Miller, and Klimoski (1990) base a journal article on interviews conducted with major contributors to the field. These interviews cover origins and evolution, definitions, characteristics of the field, influences on the interviewees, sources of ideas, contributions, and public image. They reflect a wide variation in opinion. Minimal attention is given to the future of the field.

Strauss (1993), an autobiographical essay, contains a substantial amount of material presented from the perspective of an observer of organizational behavior over the years of its history. The future of the field is not a major issue considered.

Blood (1994) contributes a chapter to an edited volume providing a brief history and focusing on the multidisciplinary origins of the field. The problems and challenges facing the field in the future are a subject for considerable discussion.

Goodman and Whetten (1998), in a chapter for an edited volume, utilize interviews and
analyses of journal articles from the 1950s to the 1990s to develop historical information. The chapter discusses the future of the field in terms of issues and choices.

Pfeffer (1998) provides a handbook chapter that emphasizes a wide range of content issues and deals primarily with the more recent history of organizational behavior. The perspective is essentially sociological. There is some treatment of new directions for the future in terms of content areas, and an assessment of how well the field is doing that stresses both areas of success and sources of challenge.

Finally, Porras provides some useful historical insights in an interview conducted by Bradford (2004). Here, again, the focus is on organization development, its changing procedures, and the values that have permeated that field from humanism on. The future is considered only in the sense that what must be retained from the current organization development repertoire is discussed, and set against what should be eliminated.

Looking at these sources yields some interesting conclusions. Concern with the scientific foundations is minimal, presumably because this is taken for granted. No real effort is made to assess the theories of the field in terms of the extent to which they have proved valid and made useful contributions, thus having the potential to guide us effectively into the future. Although challenges, issues, and choices facing the field in the future are considered by a few of the authors, especially in the more recent years, no comprehensive agenda for dealing with the future is attempted; no strategic plan or vision is stated either. The diversity of organizational behavior as a discipline is both lauded and bemoaned, but little is offered as to how this diversity might be handled, either reduced or perhaps used to advantage. What becomes apparent is that the field appears to be stuck dead-center in something of an identity crisis, a crisis that might benefit from looking more closely at what our historical origins have to say about the subject; we need to learn who we are.

In this final part of the book, I seek to deal with these matters. Because they have received only limited attention elsewhere, this inevitably means breaking some new ground. As organizational behavior moves away from its first generation, with many of that generation’s members either deceased or no longer active in the field, and now into a new century, assessing where and what we have been with a view to contributing to the understanding, prediction, and control of the future seems entirely appropriate. Even more, given the pessimism about the field evidenced by some of the more recent writings on the previous list, it seems badly needed.

REFERENCES


FROM GENERATION TO GENERATION
THE HISTORICAL CORE AND INHERENT IDENTITY OF ORGANIZATIONAL BEHAVIOR

On the Nature of Identity
Identity as Dynamic
Applying the Identity Construct to Organizational Behavior
Contributions from Scientific Foundations (Part I)
Commitment to Science
Philosophic Undercurrents
Contributions from Multidisciplinary Origins (Part II)
Contributions from the Conditions of Historical Development (Part III)
Rapid Growth
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Fuzzy Boundaries
Contributions from Theory (Part IV)
Positive Answers
Negative Answers
Conclusions

Organizational behavior (OB) is a profession with all the accoutrements of that status, and accordingly, it is institutionalized. Consistent with most definitions of institutionalization, it has the potential to possess a professional culture, and I believe it does indeed possess such a culture, of a nature that we will need to consider (see Hatch and Schultz 2004; Pratt and Rafaeli 2004). It also has a history, as the chapters of this book testify and as the publications just listed document as well. Certain of the aspects of this history are more salient, constituting a core and serving to frame our decisions regarding the identity of organizational behavior.

ON THE NATURE OF IDENTITY

Identity is one of those words in and about our field that I would prefer not to use—because of its ambiguity and the variety of ways in which it is invoked. Yet it has a tradition in our science and I know of no other effective means to express what I intend. It is important to have

[An] internalized cognitive structure of what the organization stands for and where it intends to go—in short a clear sense of the organization’s identity. A sense of identity serves
as a rudder for navigating difficult waters . . . because identity is problematic—and yet so
critical to how and what one values, thinks, feels, and does in all social domains, including
organizations . . . the dynamics of identity need to be . . . understood. . . . Identity and
identification explain one means by which individuals act on behalf of the group or the
organization. (Albert, Ashforth, and Dutton 2000, 13–14)

When questions of identity are posed, an adequate statement would need to meet the
following criteria:

1. The answer points to features that are somehow seen as the essence of the organiza-
tion: the criterion of claimed central character.

2. The answer points to features that distinguish the organization from others with
which it may be compared: the criterion of claimed distinctiveness.

3. The answer points to features that exhibit some degree of sameness or continuity
over time: the criterion of claimed temporal continuity.

For purposes of defining identity as a scientific concept, we treat the criteria of
central character, distinctiveness, and temporal continuity as each necessary, and as
a set sufficient. (Albert and Whetten 2004, 90)

Organizational identity is said to become most salient at certain points, including in par-
ticular for present purposes during the formation of the organization and in times of ex-
tremely rapid growth. Organizational behavior’s identity would appear to be rooted in these
times in its history. There is a tendency to become committed to what has existed in the past;
seldom are new identifying characteristics substituted for old ones, they are merely added on
to what existed as a consequence of history.

Identification is the perception of oneness with or belongingness to a group, involving
direct or vicarious experience of its successes and failures. Group identification and favor-
itism tend to occur even in the absence of strong leadership or member interdependency,
interaction, or cohesion. Identification is associated with groups that are distinctive, presti-
gious, and in competition with, or at least aware of, other groups. (Ashforth and Mael 2004,
153–54)

Identity concepts achieve their power from the fact that they “cut across individual, group,
and organizational levels of analysis” (Polzer 2000, 628). In addition to its group and orga-
nizational features as already noted, identity represents a self-categorization (Hogg and Terry
2000), and thus is an image in the mind of the member.

Identity as Dynamic

The typical definition of identity includes an indication that it involves what is core or cen-
tral, distinctive, and enduring about the character of an organization. This is the taken-for-
granted position. However, there is an alternative view arguing that dynamic should replace
enduring in this trilogy (Gioia, Schultz, and Corley 2000). This dynamic view tends to de-
tract from the power of the construct because it gives it less stability or reliability over time,
as well as breaking the ties to an organization’s history. External images of an organization
are seen as exerting a strong influence on identity over rather short periods of time.
This dynamic view receives most of its support from case studies or qualitative research (see, e.g., Corley and Gioia 2004; Humphreys and Brown 2002); thus, the evidence provided tends to be more appropriate to constructing theories than to testing them. In contradistinction to this inherently unreliable qualitative evidence, a review of scientific research in the area “sees identities as generally stable, although sensitive to social context” (Howard 2000, 287). A particularly compelling set of findings to this same effect is provided by Dukerich, Golden, and Shortell (2002) working in the professional domain. Whatever merits of the dynamic position may be for other purposes, the usage assumes that identity is indeed an enduring property. This does not mean that multiple identities cannot develop over time with the introduction of new contextual variables.

Applying the Identity Construct to Organizational Behavior

“In the field of organizational identity, theory development has far outpaced theory testing” (Foreman and Whetten 2002, 618). And again, “very few empirical tests of organizational identification theory” (p. 631) exist. Yet, very recently, a meta-analysis has been conducted that seems to establish the distinctiveness of organizational identity, and its relationship to a wide range of organizational variables including attitudes, behaviors, and context factors (Riketta 2005). Identity as a construct does appear to have established its scientific credibility, and accordingly, I use it here to consider the future of organizational behavior. Others who have done this include Augier, March, and Sullivan (2005).

Organizational behavior’s history is short and for that reason there has been limited time to develop traditions and entrenched characteristics. Yet there is much to be learned about the nature of the field and its potential for the future from this short history. Certain core characteristics do exist. I propose in this chapter to look at this historical core to establish distinguishing features, and then to use this knowledge to develop a strategic plan, or probably better a vision, through which organizational behavior might cope with the threats and opportunities it faces. In this process, I attempt to provide a picture of organizational behavior’s basic nature and to show how this inherent identity provides a source of strength for coping with the future, and thus “a rudder for navigating difficult waters” (Albert, Ashforth, and Dutton 2000, 13).

In approaching the problem of defining the basic nature of organizational behavior, it seems appropriate to break down the characteristics of the field using the framework we have been following throughout this book. Thus, I consider first, what can be learned from the scientific foundations of the field; second, what the multidisciplinary origins contribute; third, what emerges from a consideration of the field in the context of its historical background; and fourth, what role existing theory has and can play.

Contributions from Scientific Foundations (Part I)

The idea inherent in the development of organizational behavior from the very beginning was to introduce science into an area of business school operations that had previously lacked any scientific credentials whatsoever. Thus, that a close tie exists with science and the scientific method has been at the core of organizational behavior from the time the field emerged in the 1950s. This tie was much less in evidence in the preemergence period as reflected in the thinking and approaches of the people considered in Part II.
Commitment to Science

Inherent in this commitment to science is the idea that theories are important, not only in their own right but because they nurture practice. As Lawrence notes, “the strong bond between theory and practice is a crucial feature of organizational behavior. Theory has guided practice, and practice has provided crucial insights to research. Progress has been greatest when the two sides of the field move together” (1987, 9). Certainly organizational behavior’s theories have often suffered from ambiguity, but in many instances this ambiguity has eventually been reduced by painstaking research, which has served to narrow down definitions of both variables and domains.

This emphasis on research has in fact been important for organizational behavior from the beginning. We have used it to test our theories, and in many instances to refine them, as well as to test hypotheses that have very limited theoretical underpinnings. Organizational behavior as a field is very adept, even ingenious, at conducting research studies, in part at least because it has been able to draw upon so many different research strands. This is particularly evident in the creation of measures to operationalize the multitude of variables the field has unearthed in its pursuit of understanding the functioning of organizations, and functioning within them. Organizations were a new and uncharted domain as organizational behavior came on the scene, and few measures applicable to them existed; research since has found many ways to reduce the size of this void.

Philosophic Undercurrents

The scientific foundations of organizational behavior, although often taken for granted, have actually been the driving force in generating theories, research, measures, and applications—indeed, the knowledge base of the field—and this has been true from the beginning. Yet, at the same time, numerous nonscientific, even antiscientific, forces have operated from an early period as well. These include strong values, such as humanism and those tied to disciplinary backgrounds in psychology and sociology, as well as various philosophic persuasions. These undercurrents have varied in content and source over the years, but they have always been there in one form or another.

These specific undercurrents, coming and going as they do, have never become part of the core nature of the field. The schools have faded, at least in the United States. Now, however, a variety of antiscience philosophic positions that have much in common with the schools have emerged out of other parts of the world. It may well be that these movements reflect the fact that Europe in particular did not go through a schools phase in the 1960s and 1970s, as the United States did. Perhaps what is happening is that the schools phase has broken out in other parts of the world that never went through it before, and this process in turn is now reinfecting the United States. Given its relatively recent experience with schools and with the activism of the 1960s and early 1970s, U.S. organizational behavior may well be vulnerable to such reinfection. If this is what is occurring, and I strongly suspect it is, we can expect a continuing outburst that will eventually fade as schools tend to do in the face of scientific advance. In any event postmodernism and its siblings are not part of the long-standing core characteristics of the field; they are a relatively recent addition.

In Part I, the matter of the field’s inability to compare competing theories was considered. This view holds that one cannot decide objectively between competing theories that utilize different languages, different assumptions, and different constructs that reflect totally disparate value systems— theories that are in essence on a different plane one from another.
Proponents of this position hold that this is the situation that organizational behavior faces at present, and accordingly, that the field is one in which theory pluralism prevails and science becomes useless; there can be no consensus regarding the knowledge base of the field. The extended discussion in Part I provides ample evidence that this type of extended theory pluralism is not characteristic of organizational behavior, and thus is not part of the field’s core. However, the point to be emphasized for present purposes is that theoretical anarchy within organizational behavior is in fact a myth.

Speaking, then, to the philosophy-based positions that have attempted to infiltrate and sometimes actually permeated organizational behavior, if there is a core feature in these positions it is that a nonscientific (ranging to contrascientific) undercurrent of some kind has existed in the field from a very early point. Its nature and proponents have varied and so have the apparent functions served, but the fact of some such continuing undercurrent cannot be denied. It may well simply be a part of the diversity of our field, like the multidisciplinary origins. It may even offer some positive benefits in terms of the stimulation of creative contributions, although on most days I find that very difficult to acknowledge.

**CONTRIBUTIONS FROM MULTIDISCIPLINARY ORIGINS (PART II)**

Goodman and Whetten have this to say, “We view OB as a field of inquiry characterized by tremendous diversity. Diversity comes in the form of many disciplinary perspectives (e.g., psychology, sociology, economics, political science) aimed at understanding organizations. The diversity comes in the form of different theories and methods” (1998, 46). Lawrence says, “characteristic of organizational behavior is its sustained capacity to bridge allied areas of study. . . . By drawing in ideas and methods from so many diverse allied areas, OB has kept alive the multidisciplinary tradition of its founders” (1987, 8–9). He notes influences form psychology, sociology, business policy, economics, labor relations, human resource management, political science, and anthropology. There is some disagreement as to whether this disciplinary diversity has worked to the disadvantage or benefit of the field, but that it exists is unquestioned.

Probably this diversity of origins is greater within organizational behavior than in many other fields. Often new fields arise at the border between two disciplines, as for instance between two sciences. But if we look back at the preemergence contributors considered in Part II, only one was clearly a social scientist. The fields represented are medicine, philosophy, psychology, economics, social work, political science, sociology, law, history, management, and engineering. Of these, medicine and engineering are sciences but not social sciences. Philosophy, history, law, and the management of the times were not sciences of any kind. Thus, in this very early period, the social science base of the field was heavily diluted. It was only as organizational behavior actually emerged in the business schools that it achieved the posture of a multidisciplinary social science. Nevertheless, there seems to be no question that multidisciplinarity is at the very core of organizational behavior, distinguishing the field, in the extent of the diversity involved, from other disciplines.

**CONTRIBUTIONS FROM THE CONDITIONS OF HISTORICAL DEVELOPMENT (PART III)**

The matters discussed in Part III are greater in number than those considered in the earlier chapters of this volume, and accordingly, the contributions to organizational behavior’s in-
herent identity are more extensive. In general, these contributions fall under four headings—rapid growth, lack of constraint, orientation to practice, and fuzzy boundaries.

**Rapid Growth**

Organizational behavior has experienced rapid growth on multiple dimensions from the beginning, to the point where it is appropriate to consider this a characteristic of the field (Lawrence 1987). Given the lack of a true professional organization, it is hard to document growth in size precisely, but in the early 1960s something like 300 members appear to have existed. By the mid-1980s, this figure was up to about 1,800 by Lawrence’s estimate and by the mid-1990s it was 3,200 using the same criterion. This growth is associated almost entirely with the expansion in the number of business schools and the size of their faculties. Whether organizational behavior grew because it was in the business schools or the business schools grew because organizational behavior was within them is almost impossible to disentangle; probably both factors were operative. Anyway, the field got bigger very quickly.

A major factor in this growth has been the existence of doctoral programs that taught research and science from the beginning. These doctoral programs, especially at the major schools, have been a real source of strength. They have filled empty positions in the field and have driven membership growth in a variety of ways. Yet the field continues to attract faculty members from the outside as well, particularly psychologists and sociologists. Many of us who had thought our doctoral graduates would quickly come to monopolize the discipline have been surprised by this continuing openness to entry.

Along with the growth in members have come growth in status, power, reputation, and the like. This is well documented in the Porter and McKibbin (1988) report (see also Augier, March, and Sullivan 2005). This process took a while to establish itself amid the internal wars that plagued organizational behavior in the early years, but by the 1970s the change in status had been achieved at most business schools.

This diverse array of sources of continuing growth has not been joined by rapid curricular expansion in recent years. The sizable support for such expansion from the business community has had little impact on the business school administrators and faculties that make these decisions. Intransigence of this kind is in fact characteristic of the business school setting (Oviatt and Miller 1989). Thus, growth, although pervasive in the history of organizational behavior, cannot be assumed on all dimensions. Success often spawns a certain amount of resentment and jealousy; I believe something of that kind is operating in the business schools now, perhaps contributing to the self-questioning and identity concerns that organizational behavior currently is experiencing. When a field has been the wunderkind among its peers for so long, it is somewhat disconcerting to return to the reality of normal development.

This identity crisis is attested to further by Rynes, Trank, Lawson, and Ilies (2003), whose research finds substantial evidence of negative attitudes toward organizational behavior coursework among current students and among corporate recruiters, thus negating certain of the Porter and McKibbin (1988) findings. Certainly generational change is a reality (Hill 2002) and organizational behavior appears to be enmeshed in one result of that change at present. Rynes, Trank, Lawson, and Ilies (2003) offer certain possible reasons for this state of affairs as well as antidotes to correct them. To their list, I would add the fact that organizational behavior possessed an image in the business community that was dominated for many years by humanism and its values (see Chapter 5); perhaps this image is still im-
planted. If this is so, organizational behavior needs to replace this image with that of science, as reflected in the findings of Chapter 21, and to emphasize that rather than attempting to erase hierarchy, the field now accepts it as inevitable (see Leavitt 2005). Thus, we need to become involved in changing our image to what are in fact the true realities of our current situations, if we are to continue to grow as we have in the past.

**Lack of Constraint**

Organizational behavior has long been an area characterized by a lack of constraint on what its members do. Much of this represents a freedom from disciplinary limitations, but there is more involved than that. Lawrence notes that, “a characteristic of the field has been its responsiveness to important issues of the day. . . . Although some deplore this intimate involvement with contemporary values, I applaud it” (1987, 9). Elsewhere, Lawrence (in Lawrence and Lawrence 1993) lauds the problem-solving orientation of the field in other respects as well. All this is about being free to pursue problems wherever they may lead and whatever their nature without having to worry about disciplinary strictures and “political correctness.”

As noted previously, such freedom and lack of constraining structures can lead to uncertainty, and organizational behavior has experienced its share of that throughout its history. Perhaps, over time, this uncertainty has been reduced somewhat as certain constraints have been introduced; this process may well continue. But as long as a problem-oriented focus remains, with its potential for leading to entirely new methodologies and solutions, uncertainty will remain.

Part of this syndrome as well is the potential for new and innovative theoretical ideas. In pursuing problems wherever they may lead, concepts, approaches, even values that had not been in close proximity before are often thrown together, thus yielding the potential for creative solutions. This triumvirate—problem orientation, high uncertainty, innovative theorizing—has been at the heart of organizational behavior ever since the long period of unfreezing began. It tends to attract certain kinds of people who are comfortable with the ambiguity of the field and to drive others away.

**Orientation to Practice**

Closely tied to this freedom to follow problems to their source, and the uncertainty and conceptual innovativeness thus aroused, is a proclivity for valuing practice and the world of application. Certainly, the fact that organizational behavior came into being in the business schools reflects this proclivity. The fact that the theories of the field are typically evaluated, at least in part, in terms of their relevance for practice reflects it also.

For most people in the field of organizational behavior, practice is directly experienced in the role of part-time consultant, if it is experienced at all. Only a few have served in managerial capacities in business organizations. This is consistent with the finding that the strong motives of people in the field are those of professionals, not managers (Miner 1980). Consulting permits the satisfaction of those motives, as would full-time business employment in a professional capacity. However, the latter is something of a rarity at present.

The long-standing orientation to practice of the field has had difficulties finding an appropriate outlet, to the point where some have contended it does not really exist. Yet the very frustration that many of us feel on this score is evidence that the orientation continues to operate. Beyer suggests “that more of the results of organizational research would cross the
gap between science and practice if the linking roles available to assist this transfer were restructured. It is unrealistic to expect organizational research to cross the gap between science and practice unless there are structures and roles dedicated specifically to making that transfer happen” (1992, 472). Full-time consultants specializing in organizational behavior would meet this need, as would practicing organizational behavior specialists who are employed by individual companies. Huff (2000) made suggestions along these lines in her Academy of Management presidential address. In fact, something of this kind has indeed happened in the organization development field, but the somewhat tangential status of organizational development to organizational behavior as a whole at present, and the ups and downs of sensitivity training and the quality of work life movement, make this only a partial solution at best right now.

I will return to this matter of orientation to practice in the next chapter. However, the point to be made here is that the orientation has long-standing status in organizational behavior, and that in spite of the frustrations experienced, it still exists as part of the field’s identity. It is part of the business school setting, and although organizational behavior was brought into the business schools to give them scholarly luster, practical usefulness has remained a strong value from the beginning simply as a result of this context.

Fuzzy Boundaries

The core domain of organizational behavior has always been that of organizations. But within that domain, however it is actually defined, the field has always had rather fuzzy boundaries (Blood 1994); this appears to have occurred, at least in part, because of its multidisciplinary origins. In the early period, the overlap with organizational psychology was pronounced, and there were even moves to fully incorporate industrial/organizational psychology programs into business schools. This did not happen widely, but a certain amount of co-optation has occurred. More recently, the major overlap with organizational sociology has become manifest.

That this fuzzy boundary phenomenon remains a strong characteristic of the field may well be associated with the freedom from constraint value as well as the openness to new entrants that has been apparent since the business schools unlocked their doors to a small group of social scientists who previously had been without an acceptable home. Over time, this openness has become evident in the opportunities extended to women and to organizational behaviorists from outside the United States. There has been a continuing porousness, in the best sense of that word, that goes back to the very beginnings of the field. This porousness in turn has generated new research, new ways of teaching courses, new theories, and new practical applications. On balance, it has been a real asset to organizational behavior, contributing among other things to its rapid growth.

The lack of barriers to entry, on a disciplinary basis and in other respects too, has also created its share of problems. The problems inherent in this diversity are discussed at length by Goodman and Whetten (1998) and by Roberts, Weissenberg, Whetten, Pearce, Glick, Bedeian, Miller, and Klimoski (1990). The consequence appears to be a major contributing factor to the identity crisis noted previously. Among all this diversity, who are we really?

One answer is that we are what our history and traditions say we are, but that is a somewhat fragmented picture. Nowhere is this more evident than on the professional association front. Some years ago Cummings (1978) advocated bringing together the divisions of Organizational Behavior, Organization and Management Theory, Person-
nel and Human Resources, Organizational Development, and Organizational Communication within the Academy of Management to create a true professional organization for organizational behavior. This has not yet happened, and now, with the addition of new divisions, the scope of the new federation would certainly be greater. It nevertheless remains true that if organizational behavior is to fully resolve its identity crisis, it must have its own unified professional organization, either as a subsidiary of an organization such as the Academy of Management or as a stand-alone entity. This, too, is an issue I will return to later, but at this point we need to recognize that organizational behavior’s continuing tendency to keep its boundary lines somewhat muddied over the years has proved to be an asset, as well as on occasion a detriment. We have always had many members who possess other professional identities as well. That need not be a problem, but it becomes one, if at a given point in time a person does not know which hat to wear, and thus lacks any true professional organizational identity at all.

A component of this type of identity crisis derives from the increasing internationalization of the field. In the beginning, organizational behavior was a U.S. phenomenon, largely because that is where the business schools operated and where the Ford Foundation put most of its dollars. With time, however, that has changed and organizational behavior has added many new members. As indicated in Table 22.1, however, these new members, compared to the United States, tend to be disproportionately concentrated in the “almost no research,” or teaching-only institutions. This raises the issue of whether we are returning to our preorganizational behavior identity as a primarily teaching field or maintaining the research identity that got us where we are now (Tung 2005). The Table 22.1 data overall weigh in heavily for an emphasis on research (the first four categories) with a figure of 63 percent, but the disparity between the U.S. value of 73 percent and the international value of 46 percent does suggest a source of identity conflict.

### Table 22.1

<table>
<thead>
<tr>
<th>Research orientation</th>
<th>Total sample</th>
<th>U.S. sample</th>
<th>International sample</th>
</tr>
</thead>
<tbody>
<tr>
<td>Very heavy research</td>
<td>14</td>
<td>22</td>
<td>0</td>
</tr>
<tr>
<td>Heavy research</td>
<td>14</td>
<td>13</td>
<td>15</td>
</tr>
<tr>
<td>Some research</td>
<td>15</td>
<td>17</td>
<td>11</td>
</tr>
<tr>
<td>Little research, but increasing</td>
<td>20</td>
<td>21</td>
<td>20</td>
</tr>
<tr>
<td>Little research</td>
<td>16</td>
<td>15</td>
<td>16</td>
</tr>
<tr>
<td>Almost no research</td>
<td>21</td>
<td>12</td>
<td>38</td>
</tr>
</tbody>
</table>


Note: Percentages exclude institutions for which not enough information to classify is available.

**CONTRIBUTIONS FROM THEORY (PART IV)**

We do not have just a commitment to theory development as part of our scientific foundations, we have in fact a body of theory that constitutes a large part of the knowledge base of
the field. What role do these theories play in the field of organizational behavior? Do we have from this source a stable underpinning for our teaching, consulting and other practice activities, writing, research, and practically everything we do in a professional role?

**Positive Answers**

The data presented in the studies described in Part IV seem to argue convincingly for the value and validity of organizational behavior’s theories. The forty essential theories (Miner 2005, 2006) make this case particularly strong. Furthermore, the fact that a number of these theories have become institutionalized points up a maturity in the field’s theoretical knowledge that it had not been possible to demonstrate previously. If anything, we seem to be getting better at generating theories that prove on subsequent testing to be highly valid (Miner 1984, 2003). In any event, there is ample evidence that out of organizational behavior’s history has come a solid underpinning in the form of theoretical knowledge that gives us much to teach our students as well as to use in the interest of our consulting clients (see, e.g., Murnighan 2002).

As indicated previously, the introduction of usefulness as a criterion for judging theories has its detractors (Brief and Dukerich 1991), but also its advocates (Weick 1987). There has indeed been some ambiguity in the way the term is applied. Yet it seems to me that we need some index of how well we are doing not just in making our customers happy, but in providing them with applications that have the backing of the scientific method. This latter, science-based view is what usefulness is taken to mean here.

On the current evidence, however, organizational behavior is not doing as well with regard to usefulness as it has been with validity; the prior findings on validity are not in evidence. There has been a drop in usefulness to the point where many of the theories give no meaningful attention to practice at all. Organizational behavior’s theories appear to be suffering from a limited but clearly evident decline in this regard. The field’s theories do not seem to be generating prescriptions for practice at nearly the same positive rate that they are generating valid understanding of organizational processes.

There is one consideration here, however, to which Weick has pointed:

> [A] theory can also serve as a catalyst that sets action in motion. But once action is set in motion, the theory can be set aside in favor of trial and error and close scrutiny of tight feedback loops that are sensitive to local conditions. . . . This possibility extends dramatically the number of theories that now become useful and valid. (1987, 108)

In short, not very good theories can activate applications that achieve an identity of their own in the world of practice; even theories that pay little attention to practice can have such an effect on occasion, as consultants, for instance, rephrase them for their own purposes. Thus, the theoretical impact on the business world and on other types of organizations may be much greater than was either intended or thought to be the case.

**Negative Answers**

Although the images emanating from my own work on organizational behavior theory appear to be quite positive, certain other sources seem to provide a somewhat different picture. In particular, we need to consider a recent piece, entitled “Bad Management Theories Are
Destroying Good Management Practices” (Ghoshal 2005), that elicited a total of six commentaries in the same journal issue—several by major organizational behavior theorists.

The Ghoshal (2005) argument is primarily about strategic management theories and the economic theories that are often utilized in that field—agency theory, transaction cost economics, the five forces framework, stewardship theory, game theory, social network analysis, theories of social dilemmas, and the like. These are the really bad management theories, but the net is cast more broadly than that. Some organizational behavior theories are mentioned as sources of the problem, but they play a clearly secondary role in the argument. At points, science is viewed quite negatively, and elements of the doctrine of postmodernism appear on occasion as well. Most of this is outside organizational behavior, and I would be entering a domain beyond my training and learning were I to attempt to provide an answer to it. Insofar as organizational behavior theories are concerned, I find the arguments somewhat tangential and weak, but this is not the major thrust involved (see also Hambrick 2005). Had Ghoshal lived to write more, perhaps he would have provided a stronger attack on organizational behavior theorizing, but he did not—or perhaps he would have avoided the topic completely.

A somewhat related critique of theory is Donaldson’s (2002) article, “Damned by Our Own Theories: Contradictions Between Theories and Management Education.” The same author (Donaldson 2005) wrote one of the commentaries on the Ghoshal (2005) piece. In spite of its title, this article is not as wide-ranging in its condemnation of theory as is the one by Ghoshal (2005), but it does continue to focus on theories in strategic management and economics. Science is not eschewed and postmodernism is not embraced. Certain organizational behavior theories are found to be counterproductive, although these are limited in number; thus, this critique is not the across-the-board attack that its title might suggest. In general, Donaldson appears quite favorably disposed toward organizational behavior theory.

CONCLUSIONS

Quite evidently there are a number of contributions from various sources that represent candidates for aspects of organizational behavior’s identity. The extent to which these can operate effectively, however, is a function of the consensus, the degree of cultural cohesiveness, that impinges upon them. I do not know how much consensus of this kind exists at present. The multidisciplinary nature of organizational behavior’s origins, and thus the multiple identities of its members, combined with the short time span since those origins, would seem to argue against consensus and for a rather fragmented culture and identity.

Yet the core characteristics of organizational behavior add up to more of a body of tradition and knowledge than many might have suspected. In particular, there appears to be some agreement on the values of science and the value of our theories. There is reason to believe that, at least during the 1970s, humanist values operated to confound the evaluation process, serving to reduce the amount of consensus around the theories (Miner 1990). This influence appears to have waned more recently, although other philosophic undercurrents are still at work. Furthermore, with time has come a certain amount of institutionalization, which probably causes many to eulogize certain early theories that have turned out to be not very good (valid and/or useful), but that nevertheless hang on in people’s minds and confound the evaluation process to a degree.

Overall though, it is clear that organizational behavior has a core body of theoretical knowledge going back to the very early years of its development; a body of knowledge on
which most knowledgeable scholars can agree, given that they accept that the relevant criteria derive from scientific research evidence. This theoretical strength of the field is of sufficient magnitude as to be the envy of other disciplines that have had difficulty generating such a theoretical core.

That organizational behavior possesses its own identity and meets criteria established for that purpose is clearly established in a quote from the Augier, March, and Sullivan article:

[T]he field has experienced the gradual creation of a knowledge domain, a knowledge community, and scholarly identity. This process is revealed by two conspicuous features:

First, the field of organization studies has constructed a history of itself, a set of loosely connected stories.

Second, the domain of organization studies has increasingly differentiated itself from other fields and from the social science disciplines.

In the early periods of this history, a scholar’s identity as a member of the organization community was weak compared to identity as a member of some discipline community. Over time it became stronger. By the year 2000, it had become socially meaningful for a scholar to identify with the organization studies field, to publish in journals of that field, to cite other scholars of that field who published in journals of that field, to belong to professional associations connected to that field, to attend professional conferences for that field, and to have a professional title that identified that field. At the same time, the disciplinary identification of organizations scholars appears to have weakened. (2005, 87–88)

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CHAPTER 23

A VISION FOR ENSUING GENERATIONS

Developing Mechanisms for Providing Professional Opportunities

- Opportunities to Learn
- Opportunities to Be Independent
- Opportunities to Achieve Status
- Opportunities to Help Clients (Including Students)
- Opportunities to Develop Professional Commitment

Perpetuating Growth

- Growth and the Business Schools
- Growth Within the Business Schools
- Growth Within the University
- Growth in Training Organizations Outside the University
- Growth Through Professional Practice
- Gaining Access to the World of Practice

Restructuring the Profession

- Industrial/Organizational Psychology and Organizational Sociology
- The Academy of Management and Strategic Management
- Approaches to Restructuring

Conclusions

My original thought, at this point, was to propose a strategic plan for organizational behavior. That, however, involves making accurate forecasts and acting to implement the plan once established, neither of which I am in a position to accomplish. Thus, I settle for simply setting forth my vision for the field, in the hope that, to the extent it includes goals, it will serve to stimulate action along the lines I envisage. My intention in any event is to complete the package of organizational behavior’s context, extending from the past, through the present, to the future of an unknown duration. A vision of this kind clearly can stimulate improved performance (Hauser and House 2000), but it must be applied with caution (Humphreys 2004).

What I propose is to set forth my view of what I believe organizational behavior should become in the future as it moves well into a new century. My intention is not to predict the future, but to help to form it. This is, of course, a personal statement, but it is informed by my experience across the entire history of organizational behavior, by extensive study of the field’s literature, and by practical work experience in many of the field’s domains. Many would probably consider it a wisdom piece.

DEVELOPING MECHANISMS FOR PROVIDING PROFESSIONAL OPPORTUNITIES

Professional role motivation theory (see Chapter 16) posits that five motive patterns are strong among successful professionals and that these motives match the role requirements established
for the work that professionals do. Based on a search of the, primarily sociological, literature, these role specifications and their allied motive patterns provide an indication of what professions need to do to keep their members, or more correctly their successful members, satisfied and committed. This theory has strong support from research extending across the professions, including organizational behavior (Miner 1980). Let us look at organizational behavior from this perspective. What should, and does, the field, as a profession, provide? In the following I assume that organizational behavior remains a profession, despite pressures operating to the contrary (Trank and Rynes 2003).

Opportunities to Learn

A knowledge base does exist in the field in that we know a lot about organizational behavior, and this knowledge can be transmitted. Furthermore, a mechanism for supplementing existing knowledge exists in the form of theory building and research. Organizational behavior is a science, and was from the beginning. As long as this remains true, there will be plenty of opportunities to learn. The field’s multidisciplinary nature is a plus here because it expands the size of the knowledge pool and the number of possible theoretical interconnections. The long-standing freedom from constraints and the problem orientation that characterizes the field should also facilitate new learning.

What is needed in the future is to continue building on these existing strengths, to use our historical identity to the utmost, and, certainly, not to deny its existence. There is nowhere that this is more important than in the conduct of doctoral programs in organizational behavior. These must remain strong and produce a sizable output of qualified professionals if the rapid growth that has been typical in the past is to be maintained. This is where the most concentrated opportunities to learn occur. Everything possible should be done to prevent diversion of resources to other programs—in fact, quite the opposite. Doctoral education should come first because it is the lifeblood of the profession. In my view, existing programs should be expanded and diversified, and more schools should offer doctoral training.

Opportunities to Be Independent

Academic freedom is part of this; also involved is the freedom to act based on one’s professional knowledge without the need to acquiesce to political and other pressures. Organizational behavior’s historical freedom from constraint provides a basis for providing this type of opportunity. Scientific freedom, which organizational behavior values, is an allied force for independence. The innovative theorizing that typified the first generation in organizational behavior was an outgrowth of the opportunity to be independent, and, at the same time, provides a basis for continuing to protect our freedoms.

In this instance, as with the opportunity to learn, organizational behavior seems to have within its historical core identity the means to provide the needed opportunities to its members. However, providing opportunities for independent action is going to be much easier in a growth situation where the field is moving forward, expanding into new areas, and reducing barriers on the scope of its activities. Particularly crucial is that governing bodies such as the American Assembly of Collegiate Schools of Business (AACSB) not develop policies that limit the discretion of individual professionals (see Thompson 2004; also Durand and McGuire 2005), and that the office of the dean serve to foster values of independence, not to thwart them (Bedeian 2002).
Opportunities to Achieve Status

Status is important to professionals because it is often what attracts those who use the services provided (students and clients in the present instance). Anything a field can do to increase the status of the profession as a whole and of individual members serves to provide the needed opportunities for members. Historically, organizational behavior has achieved considerable status within the university setting, and some members have done the same in the business world. There is room for more opportunities here, however. Science, discovery, solving problems, and innovative theorizing are all means to this end. There is much in the core identity of the field that should contribute to the creation of opportunities for status.

An important consideration is that a field must possess sufficient critical mass to be recognized by possible publics and stakeholders. Organizational behavior appears to have achieved a size sufficient for recognition by its immediate publics in the academic community and in many sectors of the business world. Yet there are large numbers of people who have little idea what the field is, let alone sufficient knowledge to accord it status. To overcome this type of barrier, growth is absolutely essential. Furthermore, professional associations can do a great deal to help build the status and recognition of a profession and its members. It is important to be sure that existing professional associations are configured to fill this need.

Opportunities to Help Clients (Including Students)

Organizational behavior is a helping profession. In fact, all professions are helping professions—in some manner, for some purpose, with some people. Thus, providing opportunities for members of the profession to teach, to consult, and to utilize their knowledge in the service of others is essential for organizational behavior. Obviously, the tradition of valuing practice and the application of knowledge is important in this regard. So, too, are the field’s growth status (because it increases the number of clients), the field’s multidisciplinary origins (because they increase the scope of applications), and the field’s fuzzy boundaries (because they increase the range of situations and clients where the field may find a useful niche). These considerations are probably less true of teaching at present than of service in other professional roles, but one still finds professional courses that are very much out of the mainstream, taught by someone with unusual skills to a nonbusiness school audience. Multidisciplinarity and fuzzy boundaries can represent real assets when it comes to innovative helping. But their presence carries with it the perennial high uncertainty levels that have characterized the field in the past.

Creating new opportunities for helping by building on organizational behavior’s core characteristics not only mobilizes uncertainty, it is best achieved, once again, through growth. A growing profession pushes out the boundaries and finds new ways to utilize its potpourri of skills. This is just as true of university teaching as of service to business and other similar organizations. However, there may well be differences between areas of service in their openness to new applications. Certainly, organizational behavior already has a wide range of potential applications available, many of them a direct outgrowth of the field’s theories, but, with growth, new vistas can open up as well.

Opportunities to Develop Professional Commitment

Professional commitment is what keeps members within a profession, working at its tasks, contributing to its welfare, and abiding by its norms. To the extent psychologists or sociolo-
gists, for instance, enter organizational behavior only to leave again and return to the disciplines from which they came, this would be reflective of a lack of commitment to organizational behavior. Certainly, this has happened, sometimes in highly visible ways, although not widely. However, the key point is that a mature, stable, growing profession needs to take steps to create commitment. Within the historical origins of organizational behavior the dedication to science, the growth in status, the freedom from constraint, the orientation to following problems, and the value placed on practice all seem to serve in some way with some people to foster commitment. There are mechanisms in place to foster this end.

However, the identity crisis the field seems to be experiencing suggests this is not enough. The major source for developing commitment in a profession is the professional association the individual utilizes as a membership and a reference group. Organizational behavior has had some problems on this score, thus contributing to questions of the type “Who are we?” My intent in writing this book, at least in part, has been to provide some answers to that question. But beyond that, we need professional structures that will provide opportunities to develop commitment, and we need strategies for getting to those structures. This is not a matter that is necessarily facilitated by growth; in fact, the problem may be compounded by growth, and by diversity, too. Good doctoral training can help to foster commitment, and organizational behavior has that as a strength. More, however, seems to be needed.

I should note as well that this is the place where ethical matters come into the mix. Organizational behavior has, on occasion, had problems in this regard, largely because it has been held culpable in the unethical behavior of its students (see, e.g., for the recent period, Ghoshal 2005; Trank and Rynes 2003; Pfeffer and Fong 2004). We have been accused of not giving sufficient attention to the inculcation of ethical precepts in our students (Mitroff and Swanson 2004). Yet it seems to me that were we to teach more on the subject, we would still face the dilemma of what to teach to truly bring about changes in these students. In short, the need at present is not for more coursework, but for more theory and research in the ethics area. We need to learn a great deal more in this regard, and organizational behavior is ideally positioned to create the needed knowledge. A much expanded research agenda on the pros and cons of various approaches to ethics and justice seems called for; there is so much to learn.

PERPETUATING GROWTH

Much of what is needed to create the opportunities required to perpetuate organizational behavior as a field is inherent in the core characteristics identified previously. These characteristics, in turn, stem from the scientific foundations, the multidisciplinary origins, the conditions of historical development, and the existing theoretical knowledge base of organizational behavior, and thus from the identity of the field. The message is to listen to our history, and this means not backing away from characteristics such as multidisciplinarity, fuzzy boundaries, and high uncertainty that on the surface may seem to be detrimental. It also means that, consistent with the nature of identity, comparisons need to be made with other organizations and disciplines in organizational behavior’s field.

The facet of this identity that has come up repeatedly and that requires particular elaboration is rapid growth. Historically, growth for organizational behavior has occurred hand in hand with growth in the number and size of business schools. Thus, a key question becomes whether the business schools overall may be expected to continue to expand as they have in the past.
Growth and the Business Schools

The issue of business school growth requires some elaboration on the nature of these schools. Those on the faculty primarily hold doctorates and can be assumed to be largely professionals. In carrying out doctoral level education they are also preparing people for work as professionals. This same professional-professional relationship holds for many courses and programs that teach specialists in preparation for careers where their knowledge is the essential ingredient. However, MBA programs that prepare people to become managers in hierarchical systems are another matter. In this instance, the relationship is professional-manager in nature, and the training process becomes much more difficult because the teachers cannot serve as role models for their students. Yet the growth in the number of MBAs produced is what has fueled the expansion of business schools. Other so-called professional schools do not face this type of disparity between faculties and students.

In this connection, I should add that business schools have no control over entry into management positions, unlike the conditions applying with other professional schools, and that the major source of career success in management is a type of motivation that business faculties do not possess and typically do not impart through MBA training (Miner 1993a). What business schools do with the MBA is to provide knowledge about business to students whose undergraduate work did not include information of this kind (i.e., majors in engineering, humanities, education, etc.). Because there are many such students, the MBA has flourished.

Returning to the issue of business school growth, the greatest barriers in this regard appear to be those that have been championed by Jeffrey Pfeffer at Stanford University and Henry Mintzberg at McGill University. Pfeffer’s concerns have been expressed in a number of articles sprinkled through the organizational behavior literature. A major concern is that the attainment of, and performance in, the MBA degree has little to do with subsequent managerial success (Pfeffer and Fong 2002, 2003). Here the suggested solution is for the business schools to model themselves better on the other professional schools; if they do not, their existence will be threatened. These conclusions have since been reiterated, and the student culture of business schools is questioned as well (Pfeffer and Fong 2004). “There is not sufficient professionalization of management and management education” (Pfeffer 2005, 99). In all this, Pfeffer and his associates have repeatedly questioned the role of economics, economic theory, and those who endorse economic theories as these operate in the business schools and in management (Jost, Blount, Pfeffer, and Hunyady 2003; Ferraro, Pfeffer, and Sutton 2005).

Mintzberg (2004) has equally harsh things to say about the current MBA programs. They are given to the wrong people, teach the wrong things, glorify greed, produce corrupt practitioners, are rigidly unchanging, do not yield good managers, and so on. They should be replaced with a different approach, which Mintzberg has helped develop (Mintzberg and Gosling 2002). Not surprisingly, Pfeffer (2004) reviews the Mintzberg (2004) book very favorably.

Taken together, these treatises represent telling criticisms of the MBA programs. And there are others (see, e.g., Langbert’s 2000 critique of MBA programs as preparation for human resource careers). One would think that with all these failures the MBA would be defunct by now, and that the growth in business school enrollments would have ended some time ago.

Yet, in actual fact, business schools continue to proliferate and they are growing at a faster rate than any other part of the university (Starkey, Hatchuel, and Tempest 2004). This is true in the United States and throughout the world. In Europe there is a steady growth in demand. This appears to be true, despite the validity of many of the criticisms, because a continuing supply of students wants to learn something about business before entering upon
business careers, and they have not been able to obtain that learning from their undergradu-
ate studies. This need exists throughout the world. In addition, the evidence that an MBA
education does not pay off is not quite as incontrovertible as has been said (see, e.g., the
study reported by Connolly [2003] for University of Miami MBAs).

It would appear that the negative press being received by the business schools from within
their own ranks may be attributable at least in part to the persistent war that members of one
type of organizational system tend to wage on the members of another to change the latter to
make them over into a more compatible image. In the present case, this involves profession-
als (faculty members) trying to turn their MBA students, and the hierarchies they will enter,
into something more like professionals and professional systems. This is a long-standing
effort, as evidenced by, for instance, Mary Parker Follett’s early attempt to define manage-
ment as a profession (see Chapter 9); it has been repeated many times since. In any event, in
spite of such efforts, the business schools seem likely to continue their growth for some time
to come, and organizational behavior with them.

Growth Within the Business Schools

Another possibility for future growth is to attempt to expand organizational behavior’s con-
tribution within the business schools—more courses, more research appointments, more stu-
dents, more faculty, more programs, more administrators. There is nothing wrong with this
approach, but the degree of power required to bring about a redistribution of resources along
these lines is rarely present today in the organizational behavior components of existing
business schools. We have noted the intransigence that characterizes many business school
faculties. Without bringing a sizable store of new resources to the table, organizational be-
havior is unlikely to expand its domain very much within the business schools, and at present,
new resources on this scale simply do not appear to be in the offing (Rynes and Trank 1999).

Over time, windows of opportunity may open up for organizational behavior’s growth within
specific business schools, but capitalizing on these opportunities is not likely to bring about
anything that could be characterized as representing rapid growth overall for the field.

Growth Within the University

An alternative scenario is to attempt to open up shop in other locations on the university cam-
pus. Some of this has been going on for years, primarily in other professional schools besides
business, but not on a large scale. Opportunities exist in areas such as education, public admin-
istration, dentistry, medicine, agriculture, engineering, journalism, law, and the like. This ap-
proach represents a departure from the historic ties to the business schools, but not necessarily
from the value placed on practice that was so much a part of the business school experience.
Furthermore, these fields have seen a distinct movement from private practice to group and
corporate structures, which makes an understanding of organizations more relevant.

I believe a systematic effort to expand along these lines, going beyond the mere oppor-
tunism that has characterized such efforts in the past, could be of major benefit to other
disciplines as well as to organizational behavior. Fuzzy boundaries make this type of expan-
sion relatively easy. The new positions could be staffed from organizational behavior doc-
toral programs, as well as from doctoral programs in other fields, particularly industrial/
organizational psychology and organizational sociology—thus maintaining the inter-
disciplinarity that is indigenous to the field.
Growth in Training Organizations Outside the University

An opportunity for growth appears to exist outside the universities with the increase in the number and scope of corporate universities, defined as educational organizations that operate as a strategic means designed to help the parent company to achieve its mission by conducting activities that enhance both individual and organization knowledge and wisdom. The rise of these entities appears to be highly correlated with the emergence of corporate chief learning officers (Baldwin and Danielson 2000).

Although the numbers involved tend to get rather fuzzy in their own right, the best data I know of indicate a rise from 400 in 1988 to 1,000 in 1998 to over 2,000 in 2002, with about 40 percent granting or interested in granting an accredited degree (Meister 1998; Allen 2002). This phenomenon is often presented as a major threat to the business schools, but there is no reason that it should not represent a golden opportunity for organizational behavior, if we create the talent to grasp it. In this regard, however, I should note that there are data to suggest that the number of these corporate universities may have peaked and be somewhat on the decline today (Moulton 2004). Moreover, the stage of the transition from a training department to a full-scale university varies tremendously. Finally, although such universities may be staffed with part-time business school faculty members and with others hired away from the business schools, the use of executives is common and has the advantage of fostering manager-manager relationships where the faculty can serve as role models to the students. Nevertheless, a real opportunity for growth exists in the corporate university phenomenon.

Growth Through Professional Practice

An attractive scenario involves full-scale expansion into the world of practice and the world of organizations beyond academe. A growth strategy of this kind involves some break with the past in that the close identification with business schools is broken and the characteristic tendency for organizational behavior’s members to possess little by way of practical business experience is jeopardized. However, other values and features of the field would be fostered including valuing practice, multidisciplinarity, rapid growth, problem orientation, high uncertainty, fuzzy boundaries, and under certain circumstances, the values of science. All this requires a considerable amount of explaining.

I do not envisage a practitioner model that is devoid of research capabilities and theoretical knowledge. You need to know the underlying theories to use their applications effectively. Research has been a part of industrial psychology practice (personnel research) from the beginning, and it has served that field well. Research is part of following problems to a solution and that is what organizational behavior does.

One approach would be to join forces with organization development, which is now less committed to humanism than it once was, and is currently utilizing a broad range of techniques that make practical contributions without regard to their value orientation (Bazigos and Burke 1997; Church, Burke, and Van Eynde 1994). Given that organization development (OD) started within organizational behavior and is in fact its greatest practical extension to date, this makes considerable sense.

Jerry Porras (in Porras and Bradford 2004) says: “OD became more than just human process driven interventions. It became whatever OD consultants were doing” (396); and also “the reason that OD hasn’t prospered and really grown over the years is because it has tried to tell people that ‘Our values are the only right values’ . . . the values were very much humanistic”
This kind of thinking has brought organization development to a current state where it is very much in flux (Bradford and Burke 2004; Worley and Feyerherm 2003). A strong emphasis on data-driven approaches (Waclowski and Church 2002) has begun to take hold. It seems to me that the timing is such that organizational behavior might well join forces with organization development on a much expanded scale to the benefit of both disciplines.

However, I am also suggesting that organizational behavior take on consulting roles, applied scientist roles, and internal professional roles in conjunction with a wide range of other disciplines and groups that are already on the firing line, such as human resource management, economics, public administration, educational administration, industrial/organizational psychology, industrial engineering, labor relations, entrepreneurship, and many more.

This process of joining forces (engaging in co-optation, networking, forming partnerships) with practitioner groups who are already at the interface contains some risk of further loss of identity, but fuzzy boundaries make it easier, too. It entails high uncertainty, but that has been with organizational behavior from the beginning. It can be quite financially rewarding, and that is why the world of practice is so heavily populated in the first place. Yet organizational behavior has many techniques and approaches that provide added value beyond what currently exists; they just need to be expedited.

**Gaining Access to the World of Practice**

How might an organizational behaviorist from academe gain access and acceptance within the various disciplines and groups that populate the world of practice? Again, it may be helpful to look back at the past. Maslow moved from animal psychology to clinical psychology by writing and doing research in his new area. Herzberg moved from the clinical field to industrial psychology in the same manner. McClelland started in experimental psychology and wrote his way into personality theory, and organizational behavior. There are many instances of this in the Management Laureates autobiographies. I myself have done this several times, moving from personality theory to industrial psychology, to organizational behavior, and even for periods of time into strategic management and entrepreneurship (Miner 1993b), simply by writing and/or researching my way in. The point is that by making contributions to the literature of a field one can become recognized in that field, and thus be accepted to perform other functions as well. This is the preferred route to joining a new field for academics with doctoral degrees, prefaced on the assumption that the doctorate prepares you to be able to teach yourself.

Following the expansion into practice scenario, as I have suggested, is a very proactive process. There are those who believe this is not characteristic of organizational behavior. Goodman and Whetten describe the field as typified by “a reactive versus a proactive orientation” and by “strong external and institutional forces that shape much of what we do, and that create both opportunities and stabilities” (1998, 47) But they also note the tremendous diversity of the field and an emphasis on the applied. Both of the latter should facilitate an effective entry into the world of practice. What Goodman and Whetten do not note is that in its origins organizational behavior was extremely proactive as its members streamed into business schools, facilitated by the foundations, true, but in a manner and from disciplines and in numbers that the foundations did not anticipate. There is no reason why this proactive bent cannot continue to manifest itself through a major expansion into practice. We have seen in the business world in recent years that downsizing, concentrating on core businesses, becoming lean and mean, and the like often carry with them many problems; growth strate-
gies create more opportunities and more freedom, and they are a lot more fun to experience. It is hoped that organizational behavior will learn from this.

Something of what I have in mind was expressed in Ann Huff’s (2000) presidential address to the Academy of Management, although it is not clear to me that she envisages quite the degree of movement into the practitioner world that I do. Another related opportunity at present is represented by the widespread use of MBAs by the major consulting firms. Many of these firms would fill these positions with people who possess business doctoral degrees if sufficient talent were available, and indeed organizational behavior is ideally situated for this purpose. The potential inherent in these consulting organizations is quite sizable (see Singh 2001). The starting salaries are at least as attractive as what is offered in the academic world.

Closely related is the increase in employment opportunities in consulting firms of a more specialized nature, particularly in the human resources area. This source has experienced substantial growth with the resort to outsourcing of human resource functions by both the private (Klaas, Gainey, McClendon, and Yang 2005) and the public (Siegel 2000) sectors. Here, the demand for doctoral talent is already considerable.

The type of proactive onslaught on the world outside academe I envisage has much in common with the entrepreneurial push that emanated from industrial psychology during the early 1900s (Van De Water 1997). In that instance, industrial psychology joined forces with engineering to market itself to the business community, and with a great deal of success, thus creating the practice of industrial psychology and a number of consulting firms as well.

These are only examples. There probably are many more such opportunities waiting to be developed. But yes, one does have to be proactive and perhaps a bit entrepreneurial. The kind of vision I describe, however, is certainly not without precedent in other fields. Education schools have been preparing doctoral specialists in many areas such as educational administration and counseling for many years. The physical sciences have sent their doctorates out into research labs for an equally long time. Other examples could be cited. This is an image for the future that is clearly attainable and that would be of benefit to all involved.

**RESTRICTURING THE PROFESSION**

My vision extends as well to the makeup of the organizational behavior profession insofar as its relationships with industrial/organizational psychology, organizational sociology, and strategic management are concerned. This involves some restructuring at both the disciplinary level and with regard to the professional associations.

**Industrial/Organizational Psychology and Organizational Sociology**

One source of the multiple identities that impact on organizational behavior is the fact that many members are psychologists and/or sociologists as well. If these disciplines were to be collapsed into organizational behavior, insofar as their organizational components are concerned, any identity conflicts would be removed, a major source of fragmentation in the study of organizations would be eliminated, and the combined discipline would not only be more powerful but also more effective. I propose a collapsing into organizational behavior for several reasons, the most prominent of which is that in most universities, merging psychology into sociology, or the reverse, is structurally infeasible.

This kind of vision requires a look at the current status of these other fields:
1. Pfeffer indicates that “organization studies . . . has almost disappeared from psychology and political science departments, particularly in the United States, and represents a comparatively small part of the field of sociology. . . . [I]ndustrial and organizational psychology first lost status and then position within the discipline, such that today it exists in relatively few psychology departments and in almost none of the most prestigious. . . . [T]he place of organization studies within sociology is a small one” (1998, 734–35).

2. Augier, March, and Sullivan say that “the micro part of the field retains linkages to psychological journals; the macro part retains linkages to sociological journals; but both parts have become . . . independent of their disciplinary cousins. . . . [D]isciplinary outposts for organizational studies remained in most of the social science disciplines, but the bulk of the research in the field was found in business schools. . . . [D]ifferentiation, elaboration, and institutionalization left disciplinary remnants (e.g., public administration, organizational economics, organizational psychology, organizational and economic sociology) in the disciplines. . . . [T]hey all became somewhat isolated” (2005, 88–91).

3. Scott notes that “the shift in location of organizational sociologists from academic departments to professional schools. . . . [T]he numbers of organization faculty in our academic departments have dwindled during past decades, raising the question: Who will train succeeding generations of organizational sociologists? . . . [P]erhaps we can persuade business schools to subsidize the training of organizational sociologists as a long-term investment in their intellectual capital. A more feasible approach may be to foster joint appointments wherever possible, although given the salary differentials, this too may require some transfer of funds from professional schools” (2004, 16).

A sense of the extent to which industrial/organizational psychology has lost position in the more prestigious psychology departments may be gained from a reading of Zickar’s (2003) biography of Arthur Kornhauser.

On many grounds, the organizational aspects of psychology and sociology represent strong merger candidates. I suggest that this be accomplished by moving the appropriate components from the social science disciplines to the business schools, by starting up such components in the business schools of those universities that currently lack them in the psychology and sociology departments, and by merging the appropriate professional societies into those of the business schools. The coalition would benefit from the strength of industrial/organizational psychology in the practice area and in selection and training among the human resource specialties. From organizational sociology, the theoretical acumen of the field would provide a major advantage, especially in dealing with the macro aspects involved.

Such a merger would not be easy and inevitably would take considerable time. The three cultures have developed in quite different ways, and, as with most mergers, integrating them would almost certainly present problems. Resistance may well develop within the business schools, but the nature of this resistance will vary from school to school. In general, I would expect accounting departments to be supportive because they, too, operate with an extensive repertoire of professional practitioners. Some signs of friction within the triumvirate have emerged from time to time (see, e.g., Judge 2003) and this could represent a barrier as well. However, the most difficult problem, and the most crucial, would involve bringing the professional societies together. A discussion of this factor follows.
The Academy of Management and Strategic Management

Some history of the Academy of Management is required initially. After a minimal effort to establish a discipline-based association of a strictly psychological nature (see Chapter 14), which obviously did not fit the multidisciplinary nature of the emerging field, organizational behavior found a home in the existing Academy of Management. In fact, from the late 1970s to the beginning of the 1990s, it almost became synonymous with the Academy of Management—but not quite. During this period, organizational behavior dominated the governance structure of the Academy and provided a great majority of the articles published in Academy journals (Mowday 1997a). Yet, throughout most of this period, the association was organized as a federation of divisions, some of which fell under the organizational behavior umbrella, and some of which did not.

This did not matter much as long as the governance of the Academy was in the hands of organizational behavior people. There was a superordinate group bringing together the diverse strands of organizational behavior. However, beginning in the 1990s the power structure of the Academy began to change. The key factor was the growth of strategic management in size and in influence. As a result, there was no longer a stable superordinate body above the divisions coalescing the various forces within organizational behavior, and looking out for the interests of the whole. It seems unlikely that the Academy will ever provide this function for organizational behavior in the same way again.

The extent and nature of strategic management’s growth has been phenomenal. Petroff (2005) reports that this division within the Academy of Management has grown by 65 percent in the previous seven years. A large part of that growth has occurred from sources outside the United States. At the same time, the theoretical base of strategic management has shifted from a relatively close alignment with organizational behavior (see Schendel and Hofer 1979) to a very substantial investment in economic theory (see Ramos-Rodríguez and Ruiz-Navarro 2004). This economic thrust has been such as to suggest that it may be coming to dominate aspects of organizational behavior as well (see Rynes 2004). Certainly, the increasing tendency to cite the economics literature has been documented (Pfeffer 1998), but this seems to reflect primarily the influence of the greater economic component within strategic management. At the present time, strategic management theory is dominated by a diverse array of theories based in economics, plus several macro-organizational behavior theories (Hambrick 2004).

The present status of strategic management thus places it at the opposite pole from organizational behavior. Organizational behavior’s history reflects its origins in an effort to provide an alternative to the predominantly economics thrust of the business education of the times (see Chapter 12). Strategic management, with a completely different history and cultural identity (see Hoskisson, Hitt, Wan, and Yiu 1999), is now on the verge of being taken over by economics.

This situation within the Academy of Management presents a major dilemma for organizational behavior. The field needs something more than a set of structurally unrelated divisions if it is to serve in the way I have envisioned, and thus to grow. However, it is thwarted from accomplishing this goal by the existing structure of its major professional organization. There would seem to be no possible way of integrating industrial/organizational psychology and organizational sociology into the existing structure of the Academy of Management. The bipolarity of that structure is illustrated in Figure 23.1, and has been noted by others as well (see Pearce 2003; Rynes 2005).
As Figure 23.1 indicates, the Academy of Management is split between two membership clusters with several divisions in neither cluster. The organizational behavior cluster includes organizational behavior, human resources, and organization development and change from the five divisions specified by Cummings (1978) as having a common identity; the two other such divisions—organizational communication and organization and management theory—are in the no-man’s-land between the two clusters. Thus, there is considerable continuity over time here.

Approaches to Restructuring

One approach to the dilemma thus presented would be to redesign the Academy of Management in such a way as to create an umbrella structure for organizational behavior as a whole, placing the relevant divisions or some realignment of them under a governing body for organizational behavior alone. This body would need to be much more autonomous within the overall Academy than has been any such structure in the past, and certainly more autonomous than any division. Presumably, this configuration would be matched by a similar body for strategic management, if that entity does not opt for a total alignment with economics.

However, I believe that following such a within-Academy option requires something more. First, the name “Academy of Management” carries with it meanings that are inconsistent with the growth concept proposed on several counts. Academy means “academe” to most people, a group of professors gathered to do whatever professors do in a university setting. That does not portray a profession seeking to grow by expanding into the business world and elsewhere. Second, “management” has taken on so many meanings that it means nothing. For some, indeed, it may mean an identification with the long-defunct principles of manage-
A VISION FOR ENSUING GENERATIONS

ment school (see Chapter 5). I am reminded also of a colleague, a labor arbitrator, who was told by one of the unions he served to drop his membership in the Academy because remaining a member put him clearly in the management camp. Caught between his job and his professional association membership, he chose the former. My point is that Academy of Management conveys no single meaningful message at the present time and certainly little that suggests organizational behavior.

There is another concern with regard to the Academy of Management as it is currently functioning. Over the past decade, several presidents of the organization have expressed opinions to the effect that the Academy is somewhat ineffectual, especially in its dealings with its environments (see, for instance, Hambrick 1994; Mowday 1997b). Since my vision for growth requires nothing if not dealing with environments effectively, this becomes a matter that may well require attention. All in all, one has to question whether the Academy of Management is the right vehicle for organizational behavior’s future; certainly, it is not in its present form.

This raises the possibility of establishing an institutional identity for organizational behavior outside the existing structure. Actually, strategic management already has such an organization, and there have been several limited-purpose professional associations within organizational behavior for a long time. Perhaps one of the latter could be reconstituted on a much larger scale to serve the purposes I have in mind. Or a new Association for Organizational Behavior, or some such, might be created. Such freestanding organizations have been emerging quite frequently in the social sciences, and creating one more should not present great difficulties. Such an association must provide its own publications, as the need arises. Most important, it must coordinate and implement the strategies of the profession, and have the resources to do so. Perhaps, with a merger, the present Society for Industrial and Organizational Psychology could be reconstituted along these lines.

The question of how this association relates to the Academy of Management must remain indeterminate. If it can emerge and be effective within the Academy (with a new name), that would save much time and effort. The Academy has been very flexible in the past, and the need for a clear institutional identity for organizational behavior somewhere should be widely apparent. If restructuring within the existing system should not prove feasible, then the new structure will have much to do on its own without devoting its energies to trying to shape the Academy. I see no reason, however, why many members of the organizational behavior discipline would not wish to remain active in the Academy of Management as well, as long as that organization continued to embrace activities that have relevance for our field.

CONCLUSIONS

I have set forth a vision, a set of growth scenarios, and the structural implications inherent in these, for organizational behavior. Others almost certainly would have very different choices for the future. If my proposals do no more than initiate dialogue regarding the future of the field, something will have been achieved, but my hope is that more than that can be accomplished. Whatever develops, the field is clearly at a turning point. It has an identity rooted in its history and this identity has the potential to provide a solid foundation for future building. The need is to be proactive about what is built, not merely to be reactive and thus to build what someone outside the field wishes, or worse still, not to build at all, living only in the foundation.

One implication of my dream that should be reiterated, no matter what direction organizational behavior takes into the future, is that a major investment in doctoral education will be
required. Without that, organizational behavior will suffer the same fate that organizational sociology appears to be experiencing. I do not believe this investment should occur at the expense of the MBA program. The two, in fact, should be made to operate to the advantage of one to each other, thus emphasizing an inherent synergy.

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