8 Explanation in the Social Sciences and in Life Situations

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The skills of an applied and a pure scientist are characteristically different . . . . The pure scientist . . . deliberately excludes, whereas the applied scientist seeks always to accommodate, the effects of intervening levels of reality. —ROY BHASKAR, 1975

Roy Bhaskar’s quote concerning the nature of applied social science follows directly from his realist view of the philosophy of social science (1975, 1978, 1979, 1982). Bhaskar’s synthesis rests on many sources, including those associated with the Kuhnian view as well as those of its critics. Contributing most directly to the new synthesis is the work of Harré (1970, 1972) and of Harré and Madden (1975). This new philosophy of science has profound implications for the social sciences, as described by Manicas and Secord (1983). Unlike most other critiques of positivist approaches to science, the greatest strength of this realist view lies in its constructive nature. In this paper, I will emphasize this aspect, especially as applied to solving social problems. Let me hasten to add that, as in any philosophical analysis, no specific methodological prescriptions are given for how to do research. Nevertheless, Bhaskar’s analysis is highly suggestive of new ways of interpreting research and provides a way of structuring the relationship between the social sciences and applied problem situations.

Most behavioral scientists explicitly or implicitly see their discipline within what has been called the “standard view of science” (Scheffler 1967; Manicas & Secord 1983). Under this view, psychologists see individual behaviors as a function of a multiple set of variables or conditions, and their task as that of identifying these determining variables. Just as a machine functions in a certain way under certain conditions, persons behave according to their pre-
sent physical and psychological state and the stimulus context in which they are situated. This leads immediately to a dilemma. If the behavior of individuals is subject to variation in the presence of myriad different conditions, it is easy to see that the set of conditions quickly reaches a point beyond which they cannot be dealt with. Logically, the conditions that control behavior would extend to include the entire universe. Thus it seems a hopeless task to try to specify such conditions in any detail. Worse yet, statisticians readily recognize that the interpretation of multiple regression designs involving more than just a few variables are extraordinarily prone to guesswork and error (Blalock 1982).

Bhaskar’s new synthesis mutes the effect of this dilemma. He points out that this kind of thinking results from the following misconceptions of the nature of science:

1. Universal laws expressed in terms of regular concomitances between events are the basis of all science.
2. Explanation of a particular phenomenon is accomplished through deduction from universal laws that are applicable to the phenomenon.
3. Causal sequences are inferred from regular concomitances of antecedents and their consequences.
4. The main task of the behavioral sciences is to discover the regularities in behavior.

These propositions sound reasonable, but each of them can be demonstrated to create both logical and practical problems for scientists. Key elements of Bhaskar’s contrasting view of science include the following.

1. The standard view of science misconceives the world, which is radically open. Closed systems rarely occur, and then only in the laboratory.
2. The standard view of science confuses observable regularities with the abstract entities of science.
3. Laws are about behavioral tendencies of entities that stem from their nature. They operate in both closed and open systems, although their effects may not be observable in open systems. Laws do not describe the patterns or legitimate the predictions of kinds of events. Rather they set limits and impose constraints on the types of action possible.
4. The task of science is to discover the nature of entities, their powers, liabilities, and effects. Powers and liabilities can be attributed to entities even though these properties may never be expressed.
5. Causes are found in the nature of things, in their structural properties that create powers or liabilities. Locating causes in the nature of entities greatly attenuates the problem of generalizing from closed systems to open systems. Their nature remains the same but is often not evidenced or activated in the open world. Clearly (in our everyday experience) we are able to identify causes operating in open systems, because our actions depend upon this ability. These notions of causal powers suggest that the task of science must be sharply separated from the task of explanation in the open world.

6. Social phenomena are stratified; they consist of complex objects having different levels. Because of the complexity of the internal structure of persons, an antecedent state description does not enable prediction of behavior: persons may behave differently in the same external circumstances because of being in different internal states.

The radical nature of this critique of conventional science, as well as its constructive features, is apt to be missed unless I concentrate on several vital points. Scientific laws are to be conceived as causal principles or tendencies rooted in the natures of the relevant entities and not as reflecting regular concomitance between events. The Humean regularity conception of cause is replaced with the notion of causal powers that have their origins in the dynamic structures of the entities under study. And finally, a radical distinction is drawn between the open world of everyday life and the closed system in the science laboratory.

**Science as Universal Laws Based on Regularities**

Much of the recent lament concerning the lack of progress in the social sciences during the twentieth century takes as its evidence an inviolable comparison with the natural sciences. None of the critiques, however, take a hard look at the natural sciences; usually only physics is considered, and the other natural sciences are ignored. More appalling is that not even physics is fairly represented. Usually the field to be held up as a model is Newtonian physics. Indeed, it is not uncommon to identify only a single characteristic of that field—its universal laws—as the chief characteristic of the natural sciences in general! This is sometimes a background assumption, taken for granted, and sometimes explicitly put forward. With that assumption, the progress of the human sciences is evaluated in terms of the extent to which they have managed to establish universal laws. The conclusion follows neatly: obviously the social “sciences” have failed to achieve real status as science.

Before accepting that conclusion, the line of possible argument
should be examined more carefully. What is sometimes not noted is that Newtonian physics applies to a closed system; moreover, laws are taken to be best exemplified in terms of the regular associations between events, for example, the regular movements of heavenly bodies in relation to one another. We can legitimately ask whether this kind of science constitutes an appropriate model for the social sciences. If this doubt is not shared, we can turn to a far more important consideration. Almost any physicist will tell you that despite its preeminence in history and in the popular mind, Newtonian physics is an anomaly—other areas of physics have little resemblance to it. Consider the variety in just a few illustrative areas, such as atomic or particle physics, plasma physics, atmospheric physics, molecular physics, the physics of structural materials, of surface tension, and so on. What the appropriate science model is for these areas is by no means clear, and it could be argued that the model is apt to bear little resemblance to that for Newtonian physics.

Doubt concerning the appropriate natural science model increases by an order of magnitude when we turn to the other natural sciences. Biology, in particular, may readily be conceived of in terms of an alternative model based on causal powers. Bhaskar follows Harré and Madden in conceiving of scientific laws as causal principles or tendencies rooted in the natures of the relevant entities and not as regular concomitance between events. What is emphasized, in direct contrast to Humean skepticism concerning the reality of cause, is the idea that the structures and properties of an entity, under the appropriate conditions, give it the power or capacity to act in particular ways. Glass shatters easily because of its particular crystalline structure; dynamite explodes when detonated because of its physicochemical structure. In this view, expounded in detail by Harré and Madden (1975), causes are natural necessities and not mere logical necessities or psychological illusions. Following this view, the essence of scientific investigation is tracing the actions of entities (whether material objects or living organisms) to their origins in the natures of the entities and the prevailing conditions of action.

Causal Powers
The following brief report of a decade-long research program on cancer metastases conducted by Isaiah Joshua Fidler and his colleagues nicely illustrates the concept of causal powers in the realist account of science discussed here. The report is abridged and paraphrased from a longer one (Rodgers 1983). The objective of the program was to obtain a better understanding of how and why tumor cells metastasize and to find a way to combat this process. In this account, I italicize all of those words that fit the realist view—words associated with generative mechanism, causal powers, and the origins of these processes in the nature of the entity.

Metastasis is a well-known phenomenon: cells originating in a tumor travel to other parts of the body and destroy normal cells in those parts. Yet of cancer cells injected in mice, only .1 percent survive. Survival could be some sort of random process in which all cells have equal survival chances, but this seemed unlikely to Fidler, who set out to prove that the cells that survived had a special nature. His hunch was supported when he took cells from a metastatically generated tumor and injected them. These cells produced eighty times more metastases than did cells from the original tumor. Fidler concluded that cells in a tumor are incredibly diverse and that there is something special about cells that successfully metastasize—they have different natures.

To test his hypothesis, Fidler grew a tumor in the laboratory and split it in two. One half was left to grow in the original culture. From the other half, seventeen individual cells were isolated and cloned to produce seventeen different lines of cells, each line containing identical ones. The effects of the cells taken from the original culture were compared with the effects of the cloned cells by injecting them into laboratory animals. Cells from the original culture tended to produce about the same number of metastases—they were roughly equal in their power to produce tumors. But, as anticipated, the cloned cells were much more varied in their effect. One set of identical clones might produce very few tumors, while another set of different identical clones were all highly effective in producing tumors. This supported Fidler’s hypothesis that cells with the capacity to metastasize had special properties. Other research supported this idea. Metastatic cells from the same cancer were found to vary enormously in their sensitivity to drugs used to treat cancer: some were remarkably resistant and had the capacity to adapt.

These findings suggest that current methods of treating cancer, aimed at killing a majority of the cancer cells with radiation and chemotherapy, is misguided. Such treatment is apt to be ineffective if the cells with the capacity to metastasize are resistant. In further research, Fidler worked with macrophages, white blood cells that specialize in engulfing and digesting bacteria, trying to find a way to turn them on so that they would attack and destroy cancer cells. At this point he has had partial success in activating macrophages.

I present this account, not as evidence to support the realist view
and its causal language, but rather to illustrate how such language is apt to play a crucial role in the way the scientist thinks. Scientists do not approach a problem by starting with an established universal law from which they can make deductions; they are much more apt to think in terms of active causal mechanisms and their enabling conditions. The essential concepts guiding this sort of research, which is certainly not atypical or unusual, are that living organisms have certain active powers or capacities that must be recognized and described. But doing that is only the first step—a kind of promissory note. What follows is the effort to specify what structures in the organism's nature account for that power, along with the specification of internal states and external conditions that enable and activate the power.

Interwoven throughout this account are cause-and-effect relationships, notions of actions and mechanisms that produce particular effects. Notice that Filler has recognized the distinctive behavior of metastatic cells and has moved directly to finding ways of combating them. The realist view described here, however, would require as an ultimate goal that the physicochemical nature of these cells be specified in a manner that explains their special powers. Again, this account is not presented as supportive evidence but merely as illustrative of the uses of the concept of causal powers. It could be argued that this particular account is of discovery rather than confirmation or that other scientists do very different sorts of things. But it illustrates nicely the particularistic nature of science in contrast to the ideal of universalism constructed by philosophers.

More needs to be said about causal powers, as argued in detail by Harré and Madden in their book on the topic (1975). The concept is a radical departure from views prevailing in the philosophy of science during the past two centuries, views largely originating with David Hume (1748). This paper is hardly the place to review these arguments: rather, my aim is to make clear how differently science may be approached from this causal perspective. Causal powers are not merely inferred from regular concomitances of antecedents and consequents; instead, through appropriate experimentation, the scientist sees that the structure of the substance or entity or process under investigation, along with certain necessary conditions, accounts for the power or capacity to perform or behave in the observed fashion. Instead of being content with recording the antecedents that precede certain consequences, the scientist zeroes in on the nature of the active substance or agent that, under the right conditions, accounts for the performance. This shifts the causal focus to the structural natures of entities or processes with contextual conditions as a background factor.

Capacities are not necessarily activated in the open world; it is clear that they may be present for years even though the individual does not enact them. One of the great advantages of this focus on causal powers as stemming from the nature of a person is that it allows knowledge to be cumulative. The idea that the structural properties of an entity endow it with certain capacities makes it possible for focused inquiry to accumulate a database and gradually to gain a better understanding of phenomena. Research programs can collect more and more information concerning the conditions under which a particular capacity is facilitated or activated, as well as those conditions inhibiting or constraining its exercise.

An extensive independent analysis of psychological explanation provides considerable insight into how the notion of causal powers might be applied to psychology. Cummins (1983) notes that the received doctrine for scientific explanation is subsumption under law, as exemplified in the covering law model, and further, that this doctrine has been more influential in psychology than anywhere else, even though in his opinion psychological explanation makes little sense as subsumption. He treats psychological phenomena as manifestations of capacities that are explained by analysis and contrasts several forms of analysis with subsumption under law.

Cummins observes that changes in the state of a system are most often explained in terms of subsumption under causal laws, but that such explanations are often inadequate. He identifies this kind of law as a transition law and differentiates it from causal laws that seek to explain the properties of a system (as opposed to the transition of the system). But in addition to causal laws, other laws that are not causal statements are of great importance in science and are often crucial to explanation. These include the following:

1. **Nomic correlations.** An example is the law correlating thermal and electrical conductivity. Nomic correlations are predictive rules but have no explanatory role.
2. **Nomic attributions.** These are lawlike statements to the effect that all entities of a certain kind have a certain property.
3. **Instantiation laws.** These are lawlike statements indicating how a property is instantiated in a particular type of system.
4. **Composition laws.** These are lawlike statements that describe patterns or compositions of a specified system.
The last three types of laws enter into explanatory analyses that are common in science. Cummins identifies what he calls property theories and describes the strategy for explaining the properties of a system. He calls attention to the fact that many scientific theories are intended, not to explain changes of state, but to explain properties. Whereas covering law explanations may seem plausible for dealing with changes of state, they produce only banalities if they are used to explain properties. Property theories are not intended to explain the acquisition by system S of a property P (e.g., how did organisms acquire the capacity to pass on traits to offspring?) but rather what it is about system S that accounts for its displaying that property (e.g., what is there about S that enables it to pass on traits to offspring?).

The strategy for answering such questions is succinctly stated by Cummins: It "is to construct an analysis of S that explains S's possession of P by appeal to the properties of S's components and their mode of organization" (1983, 15). When the property is complex, a prerequisite step to explanation involves the analysis of a system into discrete properties, which in turn can be explained through componential analysis. Although the principle is intuitively obvious when a very simple property is explained, the analysis of more complex properties (e.g., a personality disposition) can be very complicated, and Cummins takes pains to point out some of the confusions that might be engendered.

Among the most important stipulations is the necessity for independently specifying the elemental components of the property being explained; that is, no use may be made of the property itself in explaining how the components function to produce the property. The explanation hinges on knowledge of the properties of the components of the property to be explained and of the way in which these components are organized. These components must be identified through nomic attributions, "lawlike statements to the effect that all x's have a certain property P" (ibid., 7). It is this latter requirement that provides force to the explanation. In explaining a complex property, it may be necessary to analyze the properties of the components into still simpler properties; however, an infinite regress is avoided because at some level only nomic attributions are required.

Explanation through instantiation should not be thought of as necessarily reductive. The property to be explained is often not to be identified with the components of the property as, say, in reducing chemistry to physics. Reduction of psychological properties typically fails because these properties can be realized or instantiated in more than one way, just as adding up numbers can be one kind of process in a mechanical calculator and another in a computer. The analysis of a property into its components does not always provide an adequate explanation of the property. The componential or compositional analysis often is not sufficient to explain more complex properties. The most interesting properties are dispositions or capacities, and to explain them, what Cummins calls "functional analysis" is used. A functional analysis does not provide a complete explanation of a disposition. For this, the details of how the disposition is instantiated in a larger system must be known. The greater the contrast between the capacity to be explained and the elemental units entering into the functional explanation, the more powerful the explanation.

A form of functional explanation central to cognitive psychology is termed "interpretive analysis." We might think of the input and output of a computer program at a low, descriptive level involving the electronic signals occurring. In contrast, at the interpretive level, input and output are described symbolically. Here the gap between the symbolic level and the electronic level is very great, and it becomes difficult to see how specification of more elemental properties constitutes explanation. Cummins argues that only when some isomorphism of structure between the information-processing program and some program couched in descriptive terms can be spelled out do we have an adequate explanation. This example reveals the difficulty and complexity of functional analysis, but also suggests that, when successful, it has great explanatory power.

Both material objects and persons can readily be regarded as having causal powers. Dynamite has the power to explode, seeds have the power to germinate, and persons have the power of speech. Harré and Madden regard the concept as useful for both the natural and social sciences, although unfortunately their book is almost exclusively devoted to natural science examples. At the same time, however, the concept does provide an important point of differentiation between the natural and social sciences. This difference lies in how the powers or capacities of inanimate and animate objects are conceptualized and identified. To make the distinction, the doctrine of "actualism," the idea that "nothing ever has the power to do what it does not actually do," must be refuted (Ayers 1968, 89). Ayers argues successfully that the necessary conditions for actualizing a potentiality are different from the necessary conditions for the existence of a potentiality. The potentiality,
capacity, or power of a thing depends upon its nature, and it must be sharply separated from extrinsic or situational conditions. An automobile designed and constructed to go one-hundred miles per hour has the capacity to do this, whether or not it has a driver and a road to drive on.

The difference between inanimate and animate objects is this: when all of the extrinsic conditions required by material objects are met, their powers are invariably actualized in performance. The necessary conditions are sufficient to produce the action; it is not possible for the automobile to do otherwise. Not so for the human case. From the fact that an individual has the capacity and opportunity to act, we cannot predict that he or she will act. Opportunity cannot be assimilated to external circumstances as in the case of a thing. Many of the confusions in the arguments over determinism stem from the failure to make this distinction. Moral character is sometimes conflated with capacity. This is a mistake; moral character is irrelevant to what a person can do. The possibilities of action are logical ones that stem from a person's capacities, and of course they may run counter to moral character. All but the most helpless invalid are capable of committing murder, although only a few will ever exercise that capacity. To say "he is not capable of committing murder; it's not in his character" only appears to be a statement about capacity. In reality, it is a statement about character and motivation, a way of saying that he's not likely to commit murder.

The rejection of actualism is not a mere abstract idea; it has important implications for social psychological experimentation and the concept of attitude. Many social psychological studies yield results describing "normative" behavior; that is, they ask the question "How do most of the subjects behave under the experimental conditions?" Confusion arises if the experimenters take their results to predict what subjects will do in nonlaboratory circumstances. The only legitimate way that they can be taken is to conclude that subjects have the capacity to behave in the way that was demonstrated under the laboratory conditions. Moreover, from Bhaskar's realist perspective, the well-conducted experiment warrants the inference that they also have this capacity in the nonlaboratory world. Thus the demonstration in an experiment that the presence of a gun among other objects causes aroused subjects to behave more aggressively in the laboratory certainly demonstrates that the sight of a gun can lead individuals to behave more aggressively, but it does not warrant the conclusion that individuals will be more aggressive when a gun is visible (Berkowitz & Donnerstein 1982; Secord 1982, 1984). How could it? Clearly a gun pointed at you and held by a robber is more likely to frighten you and lead to compliance with his demands, although some foolhardy individuals might grapple for the gun.

Attitude has been a central concept in the history of social psychology, but from time to time it has been attacked on the grounds that attitudes have low correlations with behavior. Such assertions typically are supported by empirical studies yielding low or zero correlation between attitude scale measures and behavioral criteria. But the requirement that attitudes be expressed or activated in actual behavior falls into the trap of actualism. The demand for direct correlations is sheer nonsense: an employee can dislike his boss and yet continue to behave courteously toward him because he likes his work and wants to keep his job. Powers or capacities need not be expressed to be valid, and their exercise depends upon the presence of certain activating conditions, of both an internal and external kind.

Similarly the famous experiments by Stanley Milgram (1974) demonstrate clearly that individuals can be induced to behave in a surprisingly cruel fashion toward an innocent victim, but the extent to which they would behave similarly outside of the laboratory is far from clear. Nevertheless, these experiments have value; they extend our knowledge of the limits of human behavior beyond what might be thought credible. Yet, by themselves, they do not enable us to predict behavior in the social world.

**Social Phenomena and Ceteris Paribus Clauses**

One difference between physical science and social science theories is that physical theories specify the conditions under which relationships hold, whereas social theories have open ceteris paribus clauses. This has happened largely under the aegis of logical positivism and is a chief reason why much psychological and social theory is vacuous. Making the psychological experiment central to psychology as a science has also played a central role. Why this is the case needs discussion. In their desire to develop psychology as a real science, psychologists chose the experiment as their principal method because they rightly saw that the experiment was central to the success of the physical sciences. Why, then, didn't psychology achieve the same success?

In a limited sense, the tens of thousands of experiments carried
out during the century-long history of experimental psychology did produce a science. The sagacity of our foremost experimental psychologists led them to limit their efforts to certain kinds of experiments, experiments that did indeed produce a large amount of valid psychological knowledge. But this knowledge is subject to severe limitations (Manicas 1982). Critical views of psychology from outside the profession and self-criticism within it zero in on its limited usefulness for understanding or controlling behavior in the social world. The hard-earned knowledge from experiments is difficult to apply. Why application is more difficult in the social than in the physical realm needs elaboration.

Central to the explanation of this problem is Bhaskar’s idea that a radical distinction must be made between the (partially) closed world of the experiment and the radical openness of the natural world. To the extent that the scientist succeeds in controlling relevant variables, the experiment can be viewed as a closed system that enables testing hypotheses or propositions. But the outside world is an open system, stratified into different levels or structures that intersect in unpredictable ways. Although there is a gap between laboratory and nonlaboratory phenomena in the natural world as well, the gap is far greater in the social sciences.

One of the contrasts is between the behavior of material objects, structures, and processes of the natural sciences and the behavior of living organisms. In the physical sciences, the difference between the behavior of material objects in the laboratory and outside of it is less radical than in psychology and the social sciences. In many instances, the behavior of a material object in the laboratory and outside of it is the same. Critical problems of application arise when conditions prevailing in the laboratory cannot be created outside of it; nevertheless, in the natural sciences, this becomes an engineering problem that is usually solvable with sufficient effort. Some applications are immediate, while others require considerable work. It took a long time to produce the first atomic bomb even though the theoretical principles of producing energy by splitting the atom were well known. In large part, this was because of the technical problems of creating the unusual conditions under which atoms would split. A key reason why application is possible in the natural sciences is that, there, the ceteris paribus clause is spelled out. That is, all of the relevant conditions under which the target reaction will occur are known and their parameters specified. Often effects occur only within the limits of certain parameters, and typically the experimenter takes care to control the conditions accordingly. Because of this, it becomes technically possible for engineers to attack the problem of creating the parametric conditions outside of the laboratory.

Psychology and the social sciences have not been approached in the same way, and the reasons why are instructive. Despite repeated assertions to the contrary, it is not that human behavior is more complicated than physical processes. Consider, for example, the enormous complexity of organic chemistry, whether it be analysis of some complex substance or whether it be discovering a method of synthesizing some natural substance. Neither psychology nor the social sciences currently deal with phenomena that match this complexity. They do not have theories that are of comparable complexity in their abstract structures. In the few instances where such theories have been generated, the theories fail to make any connection with behavior. Similarly, the gap that must be bridged between theory and observation in particle physics is awesome compared to psychological and social phenomena. So it seems that difficulties in applying behavioral science knowledge lie elsewhere than in complexity.

A central factor is that experiments in psychology use ceteris paribus clauses differently from the natural sciences. Typically, psychologists use them to state in a more general form a relationship demonstrated only under conditions that prevailed in the laboratory; an “other things being equal” clause is simply added or just implied. Unlike physicists, behavioral scientists do not specify what these conditions are; they remain unknown. Since many parameters are unknown, “control” is often achieved through such means as randomized “equivalent” groups. In effect, this requires the use of statistics with error terms that in part represent unmeasured parameters.

There are, however, good reasons for this difference. Physics deals with material objects and their structures, while the behavioral sciences deal with living organisms. By and large physics is generalizable outside of the laboratory because physical objects seldom change appreciably under laboratory conditions (obviously there are exceptions). This is so because physical objects are much less responsive to environments than organisms. But a rat, pigeon, or human is confronted with vastly different environments in laboratory and in life situations. When this is combined with the lack of specification of relevant parameters, the upshot is that experimental psychologists have no warrant for extending their results to behaviors outside of the laboratory. They can, however, as noted
earlier, legitimately assume that capacities demonstrated in the laboratory potentially extend to the everyday world.

Consider the century that has been spent in laboratory research on learning and learning theory. And, to take just one example, consider further how little of this huge output contributes to an explanation of how children learn in a classroom. That classroom learning remains largely unexplained should not be a surprise. Throughout most of its history, experimental research on learning has been conducted on rats and pigeons as often as on humans and, with humans, has mainly used nonsense syllables and individual words. The experimental paradigm, moreover, has forced subjects to behave in a less-than-human, unnaturally constrained manner. Thus only knowledge of more primitive learning capacities has been acquired. Participants in such experiments, including rats and pigeons, have not been allowed to use their native intelligence and pragmatic skills. So it follows that we could learn very little of how these higher-level learning capacities are employed in the classroom or in other, freer situations. This traditional approach to studying the learning process has guaranteed that how parameters affect learning outside of the laboratory will simply remain a mystery.

Actually, experimental psychologists have unwittingly followed the realist paradigm in their selection of topics for laboratory study. Perception, learning, and cognition in general are powers or capacities that lend themselves to experimental study. In this respect, the approach parallels that of natural scientists, who study the powers of material objects. But there is a critical difference here. The powers of material objects remain much the same inside and outside of the laboratory, and for many physical processes, but not all, controlling the conditional parameters is not outrageously more difficult outside than inside the laboratory. Controlling the behaviors of living organisms inside and outside of the laboratory is vastly different, however, and it is this which makes application to life situations difficult. I have already called attention to the mistaken doctrine of actualism, which in one sense denies that generalization is a problem. One factor accounting for differences in laboratory and nonlaboratory behavior is motivational, and I will discuss this shortly. But first, let’s look briefly at an ecological perspective.

The work of James J. Gibson (1979) and his colleagues (Shaw & Bransford 1977) is especially congruent with the realist view of science outlined here, although his notion of direct realism should not be confused with our realist view. His work can be seen as focusing on the characteristics of the environment that enable organisms to activate their perceptual powers. Certain types of visual perceptions are made possible by the presence of invariances in the environment. Visual perception is especially dependent on the invariances to be found in ambient light. As long as laboratory studies were limited to visual discriminations not involving ambient light, the laboratory approach could not discover these vital “external” conditions. Gibson’s concept of “affordances” attempts to extend this thinking beyond the visual senses (for an application to social behavior, see McArthur & Baron 1983). Affordances refer to properties of the environment that play a vital role, not only in visual perception, but in influencing a wide variety of behaviors. Certain types of objects “afford” particular behaviors; for example, a chair affords sitting, a swimming pool invites swimming. The idea is not unlike Kurt Lewin’s concept of “valence” or “demand characteristics” (1935), although Gibson’s emphasis is more on the objective properties of objects that account for the affordance. His work can be seen as a search for the external conditions that enable or activate the exercise of powers.

Ordinary Language and the Use of Accounts

The use of ordinary-language accounts of behavior is essential to applying the social sciences to life situations. Two major issues arise concerning everyday language. One is the use of ordinary language to describe behavior, and the other, the use of ordinary language in accounting for one’s own behavior. Rosenberg (1983) has called attention to the problem created by the dependence of social science on the use of ordinary language in the description of social phenomena—one of the prominent controversies in the massive philosophical literature on the topic. In essence, he sees ordinary language as hopelessly subjective and as unsuitable for science, and this is one of the reasons why he looks to sociobiology as a possible way out. At the same time, he notes correctly that sociobiology, like the other social sciences, states its aims concerning human behavior in a way that implies the use of ordinary language, and he hopes that this will somehow be corrected.

My position, however, is quite different. Following Harré and Secord (1972), I argue that any application of social science must necessarily involve ordinary language. Harré and I see several types of languages as essential to a fully developed social science. The nature of organisms are described in a mix of physiological
and psychological terms, but their behaviors or actions must be described in ordinary-language terms simply because there is no other language. Although ordinary language is occasionally enriched by adopting established psychological terms, no comprehensive scientific language for describing behavior has emerged. The dismal record of stimulus-response language, popular in psychology at one time, dampens further any enthusiasm for such a language. That a scientific language could be developed is not conceivable, although it is unlikely. But the point is that even then, in order to communicate with the users of applied research, it would be necessary to translate behavior descriptions into ordinary language. Social problems are described in ordinary-language terms—at least the behavioral aspects of the problems are (cf. Fiske, chap. 3, this volume). The very fabric of society, as well as the meanings of our everyday behaviors, is woven into ordinary language, and it is this feature of language that makes it “subjective.” But it also makes it relevant. It is difficult to see how a “scientific” language could avoid this involvement and still describe the same behavior in the same social context.

Closely related here is the question of whether reasons can be causes, another controversy producing volumes of philosophical literature. During the last two decades, this has shaken down and a moderate consensus has been achieved on the conclusion that reasons can be causes. In Bhaskar’s scheme (1979), they are usually proximate causes. Nothing in this way of thinking precludes a search for antecedent causes that account for the person having the reason; character dispositions, early experiences, and so on, are not excluded as more remote causes. But often such searches are fruitless in the face of the massiveness of every individual’s experiences and the paucity of biographical data. Moreover, some kinds of reasons are in a sense “explanation stoppers,” such as the statement “I don’t know why I like that painting, I just like it!”

The underlying issue here is that of consciousness or reflective awareness. Individuals can sometimes give valid accounts of their behavior, and social science is enriched if such accounts constitute a part of the research data. This is not to say that individuals cannot be wrong in the accounts they give for their actions, nor is it to say that investigators are always right in their version of accounts given by clients. It is only to argue that knowledge is incomplete in the absence of such accounts. Social scientists interested in application can scarcely afford to ignore accounts; if they do, they risk making fools of themselves. Psychological investigations involving inter-

viewing or other techniques of obtaining personal accounts, when competently done, contribute legitimate knowledge to understanding human behavior.

At the same time, it is important to recognize that this type of application, for example, in psychotherapy, takes the behavioral scientist far afield, compared to laboratory research. But it is worth emphasizing that there is no way of bridging the gap; the chasm is there because that’s the way the world is. The applied scientist must be more than a scientist. He or she must have considerable knowledge and experience relevant to the application, and must draw upon biographical, historical, and contextual knowledge if the application is to be effective. This is true because of the nature of our world, and not because of the immaturity or wrongheadedness of social science.

Tacit Knowledge and Unconscious Motivation

Accounts often include intentions and motives of various sorts; indeed, reasons are often motives. But few would dispute that we often do not have an explanation of why we behave as we do, and that much of our knowledge is tacit and often our own motives are not accessible to us. These are serious problems, especially for psychologists, and Bhaskar’s realist theory does not provide any special assistance here. To the extent that motives are biologically based, they fit well into the realist scheme, which emphasizes different levels and structures interacting to produce particular effects. But capacities or powers are only a part of the story; beliefs and feelings are important to understanding behavior. The recent explosion of cognitive psychology overemphasizes cognition at the expense of feeling, as Zajonc (1980) has noted.

Capacities can be affected by motives and feelings. A commonplace symptom of neurosis is an inability to exercise one’s capacities, and sometimes this incapacitation may be so severe as to totally camouflage their existence. Within this realist scheme, such incapacities are as important as capacities and are termed “liabilities.” In this instance, the emotional disturbance constitutes an internal condition preventing the exercise of one’s capacities. In connection with either powers or liabilities, it is helpful to think of enabling and constraining conditions. Some conditions facilitate the exercise of powers, and others constrain them. Liabilities appear mainly to be a function of internal conditions that constrain the capacity from being exercised or that facilitate the activation of a capacity that is potentially damaging (e.g., drug abuse). Else-
where I have suggested that, in effect, what behavior therapists do is to manipulate constraining and enabling conditions in order to change behavior, and that their beloved concept of "reinforcement" is often incoherent when applied to the situations in which they work (Secord 1977). The systematic study of enabling and constraining conditions in connection with the exercise or facilitation of capacities or the elimination of liabilities would seem to hold promise for bringing about changes in behaviors. Recently I have been impressed by the extensive use made by golf professionals in their tutoring and writing of very explicit, specific instructions to take certain actions (e.g., keeping your left arm straight), as well as various cognitive schemes (e.g., imagine a straight line from the ball to the target, consider the ball only as an object that gets in the way, think of swinging through the ball not to the ball). I do not know how effective these techniques are, but because they are widely used in golf instruction, I suspect that they have some merit. No doubt there are similar enabling conditions for other types of capacities or skills, including psychological or social skills, and systematic research on both enabling and constraining conditions for various social behaviors might well pay off (cf. Cronbach Chap. 4, this volume).

A natural tendency is to think of powers or liabilities as biologically based and, indeed, some certainly are. But possibly some capacities are psychological in the sense of having been established through experience, and certainly some liabilities are acquired in that way. It is easy to see Sigmund Freud's thinking as very compatible with an interpretation of science in terms of causal powers and liabilities. Glynour (1974) has argued that Freud's use of case studies is very much like scientific thinking, in this case like the use of Kepler's laws of astronomy! But Freud's analysis of incapacities can very readily be assimilated to the realist scheme. Freud traces certain types of liabilities—guilt complexes, obsessions, compulsions—to certain types of characteristic experiences accompanied by standard sets of conditions that trip off certain types of reactions in persons. Thus he seems to provide some good examples of psychologically based liabilities. Finally, tacit knowledge, unconscious motivation, and enabling and constraining conditions have important links to social structure, and that topic is discussed next.

Social Structures
Any attempt to apply psychology to a social problem is apt to fail if it does not take the social setting or context into account. Social structures play an important role in maintaining or changing behavior. Recent contributions from Bhaskar (1982), Giddens (1976, 1982), and Manicas (1980) have clarified some of the persistent complexities concerning social structures. They are fundamentally different from natural structures, in that they are constituted by the active doings of persons and thus do not exist independently of the agents' conceptions of what they are doing. Yet at the same time, agents do not necessarily grasp what the social structures are. Moreover, social structures are real in that they have real effects. They precede the individual, they are outside of the individual, and they are external facticities; thus they have coercive power over individuals.

The fact that "social structures are both constituted by human agency and yet at the same time are the medium of this constitution" (Giddens 1976, 121) has often led social scientists to choose either persons or structures as primal causal agents. Thus, Max Weber (1947) has emphasized voluntarism and agency in creating social structure: insofar as social structure affects behavior, it does so because it has a subjective reality in the minds of the actors. This in a sense subordinates structures to their construction by persons (Berger & Luckmann 1967) and robs them of any independent causal status. Emile Durkheim (1951), on the other hand, gave primacy to social structure. His most famous example concerned the effect of anomic in a society on suicide rates, and he emphasized that this relationship did not require psychological analysis of the individuals who took their own lives.

The resolution of this apparent dilemma lies in the key insight that although individuals do indeed influence social structures by their actions, these structures are partly independent in that they precede the individual; moreover, they are rarely shaped by rational means. Virtually all structures result at least in part, and sometimes wholly, from the unintended consequences of human actions. For example, the activities of a great many different individuals and agencies in the service of their own needs may create a complex structure that we refer to as economic recession. None of these individuals or other units intended to create this structure, yet it establishes certain constraints on their lives. It is important to see that social structures can have causal efficacy and yet not deny similar efficacy to the actions of individuals. Structures precede the entrance of individuals into society, and individuals act within them as a medium. The example of a recession depends upon a preexisting economic structure within which the actions of
individuals and other units take place, even as these actions inadvertently bring about changes in the structure itself.

The concept of social structure is often used in an empirical, descriptive sense. Thus social institutions are often thought of as structures. It is important to recognize, though, that only structures that are abstract and theoretical have any explanatory power. Conceptually, they are forms of relationships among people, and it is their form that gives them explanatory force. As Giddens (1976) observes, institutions are not structures, but they have structures. An example that distinguishes the empirical from the conceptual is the use of size as a structural variable. The size (of a group, for example) is an empirical measure and is not in itself a structural component. Groups of different sizes, however, may have real structural differences in virtue of their difference in size, as when, for example, communications among members assume different structural patterns in the differently sized groups.

**Types of Structures**

The realist position outlined here suggests that we need much more theoretical development concerning the nature of behavioral and social structures and the ways in which they persist or change. It may well be, as Cummins (1983) suggests, that the dominance of the covering law model of science has inhibited the development of alternative forms of psychological theory. Certainly the focus of psychologists on the experiment is in part responsible for the inattention to structure. Experiments are typically limited to short duration and deal with behaviors taken out of their natural context. In any case, it seems apparent that concepts of time and succession, as well as structural relations between persons, are poorly developed in the behavioral and social sciences.

Psychologists and other social scientists have at times emphasized the structure of relations among people, although ideas proposed so far have seldom had a lasting impact on social science theory. Nevertheless, it is quite possible that sufficient theoretical development and more sophisticated technology would produce structural theories of lasting value. We do know that many relationships among people are extraordinarily stable, and that other relationships change in systematic ways. An adequate structural language for describing these relationships and their changes would advance our science. Worthy of emphasis is the point that we need concepts for many forms of structure. One is temporal, as, for example, in the beginnings and endings of episodic relationships (Albert & Kessler 1976). Other structures might be described as different forms of "connectedness" in relationships among people. Although we might expect a great variety of such forms, one hopes that only a small finite set of these will prove important to social science.

**Applying Social Science to Life Situations**

Because of the radical openness of social phenomena, which contrasts markedly with the controlled nature of scientific investigation, the behavioral sciences are only partly applicable to real-world problems. But this should not be misunderstood. Under the new perspective, application is easier in one sense, although the limitations of application are more clearly recognized. Application is easier because, instead of requiring universal laws, which are impossible to achieve, its laws are tendencies based upon the natures of persons and of social structures that underlie behavioral relationships. These natures account for the causal powers of persons or of social structures. Persons are a mix of physiological and psychological attributes that generate powers under the relevant extrinsic and intrinsic conditions.

This emphasis on causal powers makes generalization from the laboratory easier because powers and liabilities may be presumed to be potentiated by the same conditions whether inside or outside of the laboratory. This does not mean that they will necessarily be manifested outside of the laboratory, for other overriding conditions may prevent their occurrence. It is this last proviso that identifies the task of applied psychologists: they must identify the extralaboratory conditions that are relevant to the exercise of the powers identified through laboratory study. Emphasizing causal powers also sharpens the task of both the pure and the applied scientist: their investigations can be more focused and can exclude an enormous variety of irrelevant considerations. The traditional requirement that science be couched in terms of universal laws, on the other hand, involves the scientist in the dilemma of *ceteris paribus* clauses and the difficulty of generalizing from the laboratory.

The realist perspective gives direction to applied research by underscoring the importance of gaining knowledge of the participants and the social context. Application cannot be a wholly scientific procedure, and the basic scientific knowledge that is used in application must be augmented by competent human judgments. These judgments concern the biographical history of the participants and relevant subjective features of the social context. There is no substitute for the wise use of such judgments in solving
social problems. Again, lest I be misunderstood, I am not arguing against the use of psychometric instruments to gain some understanding of persons, but simply indicating that by themselves they are not enough. Moreover, so little progress has been made in assessing social structures (e.g., in organizational settings) that global judgments by behavioral scientists experienced in application must be heavily used in appraising and solving social problems.

Of further importance for applied social science is that both persons and social phenomena are stratified into many different levels. They can be seen as having physical, biological, psychological, and sociological levels, for example, although of course other categorizations are possible. Because persons are complex entities with internal states, they must be treated holistically; moreover, they behave within a set of complex social structures. These properties of the world demand that application be interdisciplinary—no one discipline can aspire to explaining social behavior in real-world settings.

The interconnectedness of social life means that various behaviors are facilitated or discouraged by social structural factors that must be taken into account. If they are not dealt with in the process of attempting to bring about social change, the effort is largely wasted. Examples are legion. Consider the case of individual psychotherapy. Hundreds of thousands of people are seeing psychotherapists who limit their contacts to the troubled individual despite the fact that most social behaviors, including undesirable ones, are supported by the significant other people with whom the patient interacts. Worse yet, insufficient attention has been given to developing systematic theory as to how to change such support systems, either through dealing with the patient or with significant others.

Attempts to bring about social change on a larger scale often suffer in a similar way from a narrowness of vision. For example, educational research firmly established that one of the most important factors in the educational achievement of children is the quality of the peers that surround them in the classroom. From this conclusion, the inference was made that busing low-performing black children to better schools would improve their performance. Such a conclusion is unfortunately a simple-minded extension of a valid social science discovery, because it does not take into account the ramifications of the action intended to create social change. These include the attitudes and behaviors of the peers toward the newcomers, the reaction of the bused children to their new situation, the actions of white parents in withdrawing their children from such schools by moving to the suburbs, the opposition or support provided by government officials or community leaders, and so on.

Again it is important to reiterate that applied scientists must be more than scientists, they must have considerable knowledge and experience relevant to the application and must draw upon biographical, historical, and social structural knowledge if the application is to be effective. They need to obtain probing accounts of how the affected individuals see the projected social change, they need to understand how these accounts support the behavior that is to be changed, and they must grasp how existing social structures relate to the target behavior. It is easy to see how an adequate application requires the efforts of psychologists and sociologists and sometimes anthropologists, political scientists, or economists, in addition to the wisdom gained through intimate knowledge and experience with the situation in which social changes are to be brought about. The major role in all of this for social science itself is the development of social theory as it pertains to the involvement of social structures and individuals in the process of social change. Just as physicists, in order to make possible the creation of an atomic bomb, had to spell out in precise detail the conditions under which an explosion would occur, and then engineers had to find practical ways to create those conditions, social scientists need both to be able to describe the social structures and individual changes in behavior necessary to produce a planned change and to find a way to create those structures and changes.

In conclusion, the social scientist wishing to deal with an applied problem must search for and describe the structural enablers and constraints that play a part in creating the problem, and he must find a way of transforming these elements to create the kind of situation desired. Often there will be little guidance from social science in general, for each applied problem has many unique features. In fact extensive experience in similar settings may well provide more guidance than the formal body of social science knowledge. Progress in social science toward a better understanding of structures will eventually alleviate the situation, but applied research will inevitably and always require knowledge and experience of the particular target situation that goes beyond theoretical knowledge.
References


