Rethinking Culture and Personality Theory

Part II:

A Critical Examination of Two More Classical Postulates

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This essay is the second part of a critical examination of the classical postulates of culture and personality theory. In Part I of the essay (see Ethos 7:3) two postulates were examined: (1) the search for global traits, or the postulate that stimulus generalization has precedence over stimulus discrimination; and (2) the search for childhood origins, or the postulate that the past influences the present. It was concluded that individual differences do not generalize widely across comparable contexts, and that early child-care practices may not have predictable consequences on adult character. It was also concluded that the extant evidence for "cultural integration" is not compelling.

In the present essay, two other postulates are appraised: (3) the search for comparable situations, or the individual difference model of cultural differences; and (4) explanation by reference to consequences, or the postulate of adaptive accommodation to an "objec-
tive" environment. The essay focuses upon the failure of theories in the social sciences to arrive at parsimonious and universally valid explanatory generalizations. It is argued that the parsimony of a social science theory is inversely related to its predictive success and vice versa.

TWO MORE CLASSICAL POSTULATES OF CULTURE AND PERSONALITY THEORY: PARADOXES AND LIMITATIONS

POSTULATE 3: THE SEARCH FOR COMPARABLE SITUATIONS, OR THE INDIVIDUAL DIFFERENCE MODEL OF CULTURAL DIFFERENCES

Personality refers to more or less stable internal factors that make one person's behavior consistent from one time to another, and different from the behavior other people would manifest in comparable situations (Child 1968:83, emphasis added).

The individual difference concept of personality has dominated culture and personality research. The literature is rich in such formulations as, the Tuscarora modal personality type displays a fear of rejection and punishment by the environment and a strong urge to become passive and dependent (Wallace 1952:75) or the Burmese monk lacks self-confidence and has a more than average fear of female and mother figures (Spiro 1965). Nevertheless, despite its popularity, the individual difference conceptualization of personality may be inappropriate for cross-cultural research. Its application to cross-cultural materials seems to lead to confusion and paradox.

What Is Personality Theory About?

What is it that psychologists have tried to explain by means of the individual difference concept of personality, and are the conditions for applying the concept likely to be met in cross-cultural research? Child's definition is a useful starting point. By Child's account the concept "personality" refers to a set of internal factors that makes one person's behavior consistent over time and different from the behavior other people would manifest in comparable situations. Let us examine the implications of this element in Child’s definition,
To understand the personality concept, one must recognize that not everything a person does is personality relevant. There are many true statements about behavior that are simply irrelevant to personality theory. Personality theorists are not interested in explaining why people talk more in debating clubs than in libraries, or touch more in bedrooms than in kitchens. They are not interested in explaining why the pupil of a person's eye contracts more in sunlight than in a dark room.

Among the things that are not grist for the personality theorist's mill are behaviors "demanded by the situation" and behaviors "that any rational person would do under the circumstances." The point is fundamental but often overlooked. Differences between the anxiety level of one Trukese man who is having a familiar sexual experience with his brother's wife and that of another who is sailing a canoe on high seas in a gale may tell us only about differences between sailing and sex (or sex with a brother's wife, or familiar sex with a brother's wife). Similarly, one would not want to make personality claims about differences in the altruism or helping behavior of two cultures if those cultures also differed in the number of bystanders who typically witness "altruistic occasions." Helping behavior is to a large extent "demanded by the situation." The greater the number of bystanders, the less likely it becomes that anyone will help (Latané and Darley 1970).

A characteristic difference in behavior is not necessarily paralleled by a characteristic difference in character. A people's behavior can be altruistic without their having corresponding altruistic personalities. The same can be said for authoritarianism or any other type of behavior. For example, Barker (1971) explains the social behavioral differences between an American and English town without reference to personality at all. He presents evidence that levels of participation and responsible social behavior are higher in cultures in which behavior settings are relatively deficient in personnel. He then goes on to claim that "people in the Midwest have to participate to a greater degree than in Yorkdale," and if the English were imported to the Midwest they would "have to participate also, and this would occur immediately with no learning period involved" (1971:30). As Mischel notes (1969:1016), "dispositional theories try to categorize behaviors in terms of hypothesized historical psychic forces that diverse behaviors serve; but it is also possible to categorize behaviors in terms of the unifying evoking and maintaining con-
ditions that they jointly share." The two types of categorizations should not be confused.

Personality theory has a subject matter. But, to even begin a personality analysis one must show that the behavioral differences one is trying to explain are not "demanded by the situation" or "what any rational person would do under the circumstances." In effect this means that one must be able to demonstrate that the relevant behavioral differences have been observed in the same or equivalent situations. It is with reference to the equivalence or comparability of the situations in which individuals from different cultures are observed that doubts arise about the relevance of the individual difference concept of personality to cross-cultural studies.

**How Comparable Are Situations Across Cultures?**

Anthropologists can rarely treat a difference in the conduct of two peoples as if it were an indication of a difference in personality. Spiro was fully aware of this as early as 1955. He commented upon the role of Rorschach testing in psychological anthropology by noting that the Rorschach test provides us with a yardstick against which genuine personality variables, in contrast to patterned-response variables, can be measured. This proposition requires some explanation. A culture, psychologically viewed, consists of a configuration of stimulus situations and the customary responses to those situations which have been learned by the members of a given society. Hence, in demonstrating, as we have successfully done, that emotional and behavioral differences are to be found within different socio-cultural contexts we have not demonstrated the existence of personality difference at all. We have merely shown that different stimuli evoke different responses. What we must demonstrate, if we are to show personality differences, is that peoples reared in different socio-cultural contexts respond differently to the same stimuli. We must, in short, be able to observe the responses of different peoples to a stimulus that is identical for all of them and to which they have not already learned a culturally patterned response (Spiro 1955:257).

Child and Spiro seem to agree. To talk of differences one must first demonstrate likeness or equivalences. To talk of personality differences one must observe behavioral differences in equivalent situations. (I hasten to add that to talk of personality factors one must observe consistent differences over a set of equivalent situations. See Postulate 1). The crucial question then becomes, How are we to decide that the differential responses we observe are in fact dif-
ferential responses to an equivalent set of stimuli? How are we to establish the "equivalence" or "comparability" or "alikeness" of the situations or contexts in which our observations of differences are made?

There is another way to ask this question. With respect to which particular descriptive components must stimuli (situations, contexts, environments) be shown to be equivalent? How many descriptive components are to be written into our specification or definition of a behavioral situation before we test for situational equivalence? From a logical point of view there are a number of conceivable answers.

At one extreme we have Spiro's 1955 proposal that stimulus or situational equivalence is established whenever the, for lack of a better term, "objective" properties of the stimulus situation are identical from the point of view of the outside observer. For example, Spiro suggests that the verbal responses of people around the world can be treated as comparable because they have been asked to interpret the same 10 "ink blots." Occasionally, although not characteristically, LeVine (1973) argues in the same way. He suggests that one of the reasons "bureaucratic institutional structures" such as schools and hospitals can be used as a universal framework for the detection of personality differences is that schools and hospitals are organized around similar physical settings, in particular classrooms, clinics, and wards (1973:247).

I believe we should feel uneasy with Spiro's proposal. Newell and Simon's (1972) discussion of the difficulty of defining a task environment in human problem-solving research suggests why. Newell and Simon ask us to imagine an experiment in concept attainment.

A sequence of stimuli is presented to a subject, who is asked to classify each as an instance, or noninstance, of a concept (as yet unknown to him). He is informed whether each reply is right or wrong, . . . if the subject refuses to try the task, is obviously inattentive, or undertakes to spoof the experimenter—we would not regard it as an experiment in concept attainment. We would also reclassify the experiment if it turned out that the subject could not discriminate among the stimuli because the light was too dim or because his eyes could not resolve the differences among them. In these cases, we would call it an experiment in visual sensation, or possibly, in perception (1972:54).

Newell and Simon's very simple example reminds us that a situation (environment, context, setting) is more than its physical properties
as defined by an outside observer (also see Labov 1970; Cole and Bruner 1971). It is a situated activity defined in part by its goal from the point of view of the actor. "What any rational person would do under the circumstances" depends upon what the person is trying to accomplish. Two individuals moving about in what we call a "boxing ring" are not in comparable situations if one of them is Jack Johnson and the other is Muhammad Ali, or if one of them is a "boxer" and the other a "referee"; and differences in their behavior may tell us only about the difference between trying to win a fight in the early twentieth century and doing so in the late twentieth century, or the difference between trying to enforce the rules of boxing and trying to exploit the rules to one's advantage. "Boxing" and "refereeing" are two distinct forms of activity and only behavioral differences within each activity can be used to make inferences about distinctive features of the boxer, or referee (in contrast to inferences about "boxing" versus "refereeing").

This is probably the reason many culture and personality theorists (e.g., LeVine 1973:247) have postulated certain universal goals (e.g., controlling sexual and aggressive impulses, defending the ego against anxiety) as a framework within which to study cross-cultural variations in personality. One difficulty with a "universal goals" strategy is that it restricts our investigations to a very small subset of the goals actually pursued by any people, most of which are not universal. A more important difficulty is that once we concede that our definition of a behavioral situation must include the actor's goals and that two situations are equivalent only if the actors in the respective situations share the same goals, we are on a very slippery slope. If our answer to the question, What kind of situation is this person in? must make reference to the actor's goals, then certainly it must also make reference to the means that the actor views as potentially admissible (and thinkable?) to accomplish these purposes.

Allow me to pursue the boxing example a bit further. Imagine a test of boxing skill. A contestant is placed in a boxing ring for a fifteen-round match against Muhammad Ali. The goal is the usual one in contemporary boxing: outpoint your opponent or by your own agency (slips don't count) cause him to remain 10 seconds "off his feet." Our contestant is knocked down in the first round, but his friends, relatives, and supporters rush into the ring and help him to his feet. In the second round our contestant comes out of his corner
with a sledge hammer, and Ali is left unconscious on the mat for the rest of the day. How do we assess our contestant's behavior? Clearly we do not view it as an instance of his boxing skill. Boxing is a form of activity defined not only by its goals, but also by a detailed (even if implicit) list of ways in which the goal can be legitimately accomplished. Thus, we must write what we usually call conventions and norms into our very definition of a behavioral situation. Since "forms of activity," "goals," "conventions" and "norms" constitute a large part of what we mean by "culture," it would seem to follow that two actors are in "comparable" or "equivalent" situations only to the extent they are members of the same culture! Who is the more skillful boxer, Muhammad Ali or Jack Johnson? The question seems unanswerable; there is no yardstick that permits comparison since the two men fought under dissimilar conditions. Ali had the option to outpoint his opponent; Johnson could win only by knockout. Furthermore, in Johnson's day there was no limit on rounds, audience interference was not tabooed, and defensive dancing was unthinkable. The very things that make the two actors members of different activity subcultures (their goals, conventions, techniques, etc.) make it impossible to place them in comparable contexts. 

It would seem to follow that the difference in the anxiety level of two Kikuyu boys both about to be circumcised after having been masked, marked, secluded, and hazed in an initiation ceremony might warrant a personality analysis. But what of the difference in the anxiety level of the Kikuyu adolescent about to "go under the knife" and the Jewish 13-year-old about to read from the Torah? 

The individual difference concept of personality creates an irresolvable dilemma for cross-cultural researchers. If we force situational comparability by simply writing very little into our definition of a behavior situation (e.g., we claim comparability by pointing at those 10 ink blots, or by noting that all people go through rituals of status transition, or by pointing out that "every human being lives in a world in which there are others who are also seeking food and water" [Sears 1961:449]), it becomes nearly impossible to discriminate between personality-relevant behaviors and the behavior "any rational person would do under the circumstances" (given that culture's definition of the situation). The concept of "culture" and the concept of "personality" become hopelessly blurred and we, as theorists, do little more than personify cultural differences. We must recognize that descriptions like "the Dobu are
more competitive and less cooperative than the Zuni” (Benedict
1934:91, 112, 154) or “the extremest of Russian atheists is on better
speaking terms with God than are the devout of other lands, to
whom God is always something of a mystery” (Sapir 1924) are merely
reports of differences between two or more populations. They are
mute concerning the causes of such differences—e.g., the history of
a people, the situation of a people, the character of a people. The
personification of cultural differences is not what culture and per-
sonality are about. Moreover, it is a hazardous business. As has been
demonstrated (Shweder 1973) there is no reason to expect that the
ways individuals differ from one another within any or all cultures
have anything to do with the ways cultures differ from one another.

On the other hand, if we write a great deal into our definition of a
behavioral situation (e.g., the actor’s goals, the actor’s understand-
ing of the possible means, the actor’s evaluations of the likelihood
that each available means will accomplish the desired goal, etc.)
then whatever the actor does will seem to be demanded by the situ-
tion. It is what any rational person would do given that perception
of the circumstances. The actor’s personality will seem irrelevant
and disappear from our analysis. We will have actions without ac-
tors.

Personality psychologists resolve this dilemma, the choice between
vacuous commonalities and incomparable differences, quite
pragmatically. They participate in the same culture as the people
they study. Thus, they can sometimes reasonably assume that their
notion of the actor’s situation approximates the actor’s notion. Un-
fortunately for cross-cultural researchers the individual difference
model of personality (Postulate 3) presupposes the common cultural
background of those whose behavior is being compared. It does not
lend itself to cross-cultural comparisons.

A final remark concerning Postulate 3: cross-cultural situational
comparability is a “problem” because it is a prerequisite for carrying
out a personality or character analysis of two or more peoples. It
should not be confused however, with another “problem,” that of
identifying transcultural variables. Sears (1961), for example, asks
what are the criteria for “defining both a street fight and the telling
of malicious gossip as indices of aggression?” There are many possi-
ble answers to this type of question—e.g., that the two indices have
face validity, go together in behavior, and have similar empirical
relationships to relevant external variables. Sears lists four criteria of
his own (e.g., that the same antecedent-consequent relations be demonstrable in all cultures). It is somewhat depressing that Sears' criteria have rarely (if ever) been met in cross-cultural research, but that is not the main point I wish to make. The main point is that it is possible for a variable (e.g., aggression) to have transcultural universality without it being a personality variable (see Shweder 1973). The incidence of street fights and gossip may rise and fall together across neighborhoods, cities, states, or whole nations. We may even want to talk about some situations eliciting more aggression (viz., fights and gossip) than other situations. We can do this without ever being tempted to use the variable to describe individual differences across comparable situations.

POSTULATE 4: EXPLANATION BY REFERENCE TO CONSEQUENCES, OR THE POSTULATE OF ADAPTIVE ACCOMMODATION TO AN "OBJECTIVE" ENVIRONMENT

... the effect of an act, including its rewards or non-rewards, is to change the organism that produced the act in such a way that the effect of the act is included among the future causes of the organism (E. L. Thorndike, The Law of Effect).

There are two kinds of events that culture and personality theorists typically explain by reference to consequences: (1) the development of modal personality types, and (2) the motivational integration of culture. "Consequences" are introduced into their explanations by invoking either (1) the concept of "rational choice," (2) Darwin's notion of "natural selection," or (3) the "law of effect" (see Gorer 1943; Barry, Child, and Bacon 1959; LeVine 1966, 1975:115-135). In fact, one or the other of these three explanatory schemata plays a part in most explanations in the social sciences. In this section I examine the "logic" of these three forms of explanation. What I conclude is that the concept of "rational choice" and the "law of effect" must be contextualized and subjectivized to such an extent that they lose their parsimony and predictive power while the notion of "natural selection" begs many of the questions that concern social scientists the most. In the light of this examination I discuss the limitations and potential of social scientific understanding.

THE DEVELOPMENT OF MODAL PERSONALITY TYPES

One of the ways culture and personality theorists explain the formation of modal personality types is by applying established learn-
ing theory generalizations about the functional relationships between instrumental acts (operants) and their consequences (positive and negative reinforcements). The assumption is that "human behavior is predominantly learned" and that learning takes place "by differential reward and punishment chiefly meted out by other members of the society" (Gorser 1943). Thus, for example, Barry, Child, and Bacon (1959; also see Barry, Bacon, and Child 1967) in a discussion of "low accumulation economies" (e.g., hunting and gathering societies) argue that the frequency, degree, consistency, and immediacy of reward with respect to self-reliance and achievement "shape children into venturesome, independent adults who can take initiative in wrestling food daily from nature." The claim is that one can predict an act's likelihood of occurrence from information about its attendant positive and negative consequences. Personality formation is referenced to the "objective" constraints of the social environment.

**The Motivational Integration of Culture**

After culture and personality theorists have explained the formation of individual character by reference to "objective conditions," the sociocultural domain is then analyzed as though it were fashioned to correspond either directly or inversely to features of the modal personality of societal members. Thus, for example, Gorser (1943) assumes that "when childhood wishes and frustrations are shared by a majority of a population, social and cultural institutions will be evolved to gratify them, and existing social and cultural institutions and those borrowed from other societies will be modified to congruence with these wishes" (emphasis added). Similarly, Spiro and D'Andrade (1958) assume that "unless the personalities of the members of the group are consonant with the various traditions of the group, they will not in the long run be motivated to learn and/or transmit the traditions" (emphasis added).

Culture and personality theorists frequently introduce person variables (e.g., wishes, frustrations, conflicts, motives) as prior objective constraints that select for or against sociocultural institutions and thereby influence their evolution. Thus the persistence of a cloistered monastic tradition in Burmese Buddhism is interpreted as an accommodation to the passive dependency, homosexuality, and excessive fear of female figures latent in the Burmese male character
Thus the belief in benevolent gods responsive to prayer is interpreted as an adaptation to motivational tendencies established in childhood with respect to nurturant parents who were responsive to verbal and non-verbal solicitations such as crying, whining, and begging (Spiro and D’Andrade 1958). Thus, adolescent initiation rituals emphasizing aggressiveness, martial skills, and other “hyper-masculine” virtues are interpreted as a defensive adjustment to the gnawing persistence throughout childhood of anxious concerns over whether one’s sex identity is male or female. The ritual ends all doubt (Whiting 1964).

The historical or ontogenetic process producing an adaptive accommodation between personality variables and sociocultural institutions is not always made explicit in culture and personality studies. Nevertheless there are only three possible ways to explain behavior by reference to consequences: (1) the concept of “rational choice”; (2) Darwin’s notion of “natural selection”; and (3) the “law of effect.”

**Rational Choice**

The concept “rationality” refers to any choice “whose probability of success is not exceeded by that of any available alternative”; it refers to any behavior that on the basis of available information “offers optimal prospects of achieving its objectives” (Hempel 1962).

A rational orientation is one of calculation. It involves assessing alternative means to determine which will most efficiently accomplish one’s ends — i.e., maximize one’s benefits and minimize one’s losses with reference to some goal. J. Harsanyi (quoted in Allison 1971:31) argues that the concept of rationality is important to social scientists because

if a person acts rationally, his behavior can be fully explained in terms of the goals he is trying to achieve. When we say that Napoleon’s strategy in a particular battle was rational, this means that his strategy choice can be explained essentially by pointing out that this was the best strategy for him to choose in terms of his military objectives at the time.

Mischel (1973) seems to have turned to rational choice analysis as an alternative to a global trait approach to personality. In various studies on “self-control” behavior (e.g., Mischel and Staub 1965) he argues that choice is a function of the expectation that it will lead to
an outcome and the actor’s evaluation of that outcome. Mischel demonstrates that delays in gratification are related to the actor’s expectation of success under various experimentally controlled contingency conditions. Delay times are highly manipulable under experimental conditions that alter either the actor’s expectations about chances for success or the actor’s evaluations of the outcomes. When children are provided with differential information about such success likelihoods, it is not difficult to alter their “self-control” behavior.

One of the difficulties with the concept of rational choice is that its application depends upon numerous qualifications that substantially erode its predictive power. For example, Simon (1957) has noted that, from the point of view of an omniscient observer, human rationality is a “bounded rationality” severely limited by cognitive and symbolic constraints. As Simon argues (1957:199),

The intended rationality of an actor requires him to construct a simplified model of the real situation in order to deal with it. He behaves rationally with respect to this model, and such behavior is not even approximately optimal with respect to the real world. To predict his behavior we must understand the way in which this simplified model is constructed, and its construction will certainly be related to his psychological properties as a perceiving, thinking and learning animal (emphasis added).

Thus, upon examination it turns out that one cannot “fully explain” or predict the behavior of a rational actor by reference to his or her goals alone. One must also know, for example, the order in which admissible alternatives will be searched out and considered. This is because actors do not consider all the alternatives and then pick the one with the optimal prospects for goal attainment. Rather they select the first alternative that is “good enough,” or “satisfices” (Simon 1957; Allison 1971). One might argue that there is a “cost to searching among alternatives.”

Rationality is not a hypothesis. It is presupposition or criterion. (In this respect the concept of rational choice is like the concept of consistency in Postulate 1.) The relevant consistency question is not, Is behavior consistent? but rather, Where are the consistencies that exist to be found? Similarly, the relevant rationality question is not, Is behavior rational? but rather, With respect to what can the behavior be seen as rational by an outside observer? We assume that behavior is rational and then use the concept of rationality as a
criterion to decide how much we need to know in advance of our explanations. For example, it seems that Simon's (1957) account of "satisficing" is not really an abandonment of the notion that an actor selects an alternative that "offers the optimal prospect of achieving its objects." Rather Simon seems to ask, With respect to what is the actor optimizing? It is the criterion of rationality that leads him to introduce notions like the actor's "simplified model of the situation." Similarly, Freud was led to introduce the notion of "unconscious goals" by his commitment to the concept of rationality, as well as concern for the question, With respect to what can phobic, compulsive, and hysterical behavior be seen as rational? This presuppositional status of the concept of rational choice is most apparent in the following remark by Mischel and Mischel (1976):

Even the noblest altruism [i.e., apparently acting against one's interests for the benefit of others] supported by the "highest" levels of moral reasoning [e.g., it is "right" to help others regardless of the personal consequences] still depends on expected consequences, although the consequences are often temporally distant, are not in the immediate external environment, are not easily identified, and reside in the actor himself rather than in social agents.

The relevant question, then, is not whether behavior is rational. The relevant question is, How bounded is that rationality and what specifically are the boundaries? The boundedness of the concept of rationality will be inversely related to its parsimony and directly related to its uselessness as a predictive principle (although even if it is useless as a predictive principle it will still have utility as a criterion for discovering the limits of rational choice such as the sacred, the conventional, the unconscious, etc.). The more we have to know about an actor before we can predict his or her behavior, the less helpful is the concept of rationality.

How bounded is the concept of rationality? How many "givens" must we introduce before it can be applied? How much must we already know about an actor and his or her behavioral context before we can go about predicting behavior? We can answer these questions by explicating the way rational choice operates as an explanatory scheme. (The scheme is often referred to as "practical inference" or "practical reason"; see Von Wright 1971; Sahlins 1976a).

Rational choice is, perhaps, the concept most frequently employed in explaining the behavior of everyday life (see Lewis
1978). Question: During his recent trip to Europe, why was Jimmy Carter received so warmly in Poland? Answer: Poland has a sluggish economy and its leaders want to expand trade with the United States (CBS Evening News Report, December 29, 1977). Why does a particular individual take a particular course of action? In answering this question, we usually assume that the individual in question had some goal towards which the action was an optimal means (see Allison 1969). More explicitly, a rational choice explanation takes the following form (see Hempel 1962; Von Wright 1971).

Step 1. People are rational agents, i.e., their actions are under voluntary control (which, as we shall see later, is not equivalent to being under conscious control; we must admit the possibility of unconscious decisions). If we had reason to believe that a particular individual was in the throes of an epileptic seizure, or in a hypnotic trance, or under the influence of drugs, or in any other way coerced into acting, we would not try to explain that person's behavior in terms of the rational choice concept. When it comes to knee jerks, the concept of rationality is out of place (Fitzgerald 1973).

Step 2. A particular person, a rational agent, is in a situation of type S. A description of the situation must include the following: (1) the person's goals (N.B., The concept of rational choice will not help us to explain behaviors that are not goal directed, are done for their own sake, or are ends in themselves or merely matters of habit, e.g., doodling and perhaps certain forms of ethical conduct [see Weber 1958; Black 1975].); (2) the person's perceived means, i.e., a list of the behaviors thought to be sufficient to bring about the desired goal, one of which the person believes must be selected if the goal is to be attained (If there is no such list there can be no rational choice.); and (3) the person's calculations concerning the efficiency and relative cost of each possible method for attaining the desired goal.

Step 3. In a situation of type S anyone who is a rational agent will do such-and-such, i.e., select the behavioral alternative which "on the basis of available information" and beliefs seems to offer the optimal prospect of success (see Hempel 1962).

Step 4. Therefore, the person in question did such-and-such.

There are two noteworthy features to this explanatory pattern. The first is that Step 3 (the claim that in a given situation all rational agents would select the alternative that offers optimal prospects of success) is not a generalization based on experience. It is an assump-
tion that we must make because if we were to deny it we would have no way to make sense of the behavior of rational agents. In fact we often use the assumption implicit in Step 3 to ascertain an individual's goals, perceived means, etc., (Step 2); and we typically do this in the light of what that person has already done (Step 4).

For example, imagine we observe a man proceeding out into a cold winter's night to catch something to eat. He tells us he is in search of a rabbit, that he will certainly be gone for several hours, and that he may in fact come back empty handed, cold, and hungry. On his way out the door he discovers a dead rat in a trap; he promptly tosses it into his garbage heap. As observers, what interpretive use do we make of such behavior? Do we reject Step 3? Do we argue that the man has failed to select an alternative that he believes offers optimal prospects of achieving his goal? I think not. What we do is revise our understanding of his goals. "Something to eat" does not include rats. In fact, it seems to me we would be willing to do more than simply revise our notion of his goal; we would postulate unconscious goals before we would doubt the assumption expressed in Step 3.

The second noteworthy feature to the rational choice form of explanation is that to use it to predict an individual's behavior we must already know an enormous amount about that individual. We must know which alternatives are actually going to be considered. All people typically fail to consider potentially optimal alternatives. They may fail to do so because the order of search among admissible alternatives has already produced a satisficing solution (after we have found a "decent" candidate for a job, how long should we go on searching for the "perfect" candidate?). A particular individual may also fail to consider a potentially optimal alternative because s/he is unaware of it or because s/he holds erroneous beliefs about its efficiency. S/he may fail to consider a potentially optimal alternative because s/he classifies it as "sacred" and hence not as something to be considered as a means to an end (a wooden cross is not "firewood," at least not for some freezing Catholics), or because the alternative violates conventional rules about how goals are to be attained (one does not use a sledge hammer to fell one's opponent in boxing, or eat rats to satisfy one's hunger).

To predict behavior by means of the concept of "rational choice" we must already know the actor's state of mind, goals, sources of information, beliefs, ethics, and cultural conventions. Given that the
actor is not under duress, given that s/he has a goal in mind, given that s/he has this particular goal in mind, given that s/he defines these particular alternatives as "alternatives," given that s/he considers these "alternatives" in this particular order, given all that, then the actor's behavior can be "fully explained." The parsimony and predictive power of the concept of "rational choice" are severely limited by all these historical, clinical, and ethnographic "givens" that it presupposes.

This seems to be what Sahlins (1976a:171) has in mind when he argues that

The "opportunity costs" of our economic rationality are a secondary formation, an expression of relationships already given by another kind of thought, figured a posteriori within the constraints of a logic of meaningful order. The food taboo on horses and dogs thus renders unthinkable the consumption of a set of animals whose production is practically feasible and which are nutritionally not to be despised. Surely it must be practicable to raise some horses and dogs for food in combination with pigs and cattle. There is even an enormous industry for raising horses as food for dogs. But then, America is the land of the sacred dog.

There is a final, and curious, limitation upon the usefulness of the concept of "rational choice" as a predictive principle: although we must know an enormous amount about a person before we can predict behavior, we usually cannot, as was hinted at above, ascertain this information without previous knowledge of what the person has done (e.g., discarded that nutritious rat).

**Natural Selection**

"Rational choice" is a prerogative of actors; "natural selection" is a prerogative of environments. Both processes have been championed as ways of explaining behavior by reference to consequences. We have just seen that the concept of "rational choice" inevitably leads us to a detailed historical, clinical, and ethnographic "subjectivization" of decision making. A "rational choice" analysis ultimately must examine the unparsimonious intricacies of the actor's point of view if it is to explain behavior.

The concept of "natural selection," on the other hand, explains solely by reference to the "objective" conditions of an environment. Subjective categories such as "goal," "awareness of alternatives," "evaluation of outcome likelihoods," and "preference orderings"
have no proper place in a Darwinian account (although see Popper and Eccles 1977:11–14 for the view that evolutionary processes become more understandable if one makes reference to an animal's "subjective aims and purposes"—animals, it is argued, choose, alter, even construct their own selective environments). The causative influence of consequences is direct. The only consequences that are theoretically relevant are those that threaten the survival of the organism and its ability to reproduce or duplicate its kind. Viability or persistence is the only criterion for assessing the adequacy of an alternative; by definition only "fit" alternatives survive. For a Darwinian, the expression "this species is 'fit' but has a very limited reproductive potential" is a contradiction in terms.

Darwinians restrict their concerns to only those consequences that reduce an organism's ability to reproduce. By doing this they avoid the need for "subjective" categories. This is certainly one of the appeals of Darwinism, but one surrenders "subjective" categories at a price. That price is silence on a central issue for most social scientists, viz., of all the conceivable viable alternatives, why this one and not some other (see Sahlins 1976a, 1976b)?

An explanation by reference to the process of "natural selection" tells us about the limits of what is possible. It tells us which of a large class of occurrences could not persist. Bateson (1967) has aptly termed it "negative explanation."

Explanation by elimination can only go so far. "Natural selection" is a theory of persistence; it is concerned with regeneration, not generation. It tells us nothing about why certain events occur; its only answer to the question. Why this viable alternative and not some other viable alternative? is an appeal to "chance" or "randomness." At its limits Darwinism is indeterminate.

Natural selection is a variety of functional explanation, and its indeterminate nature is typical of such. Functional explanations have the following form (see Hempel 1959):

1. There is some entity (e.g., a population of honey bees) that functions adequately (e.g., perpetuates its kind) in some environment.

2. This entity (e.g., those bees) functions adequately (perpetuates its kind) in that environment only if it is able to satisfy some necessary condition (e.g., locate food resources).

3. If such-and-such a trait (e.g., a language for communicating the location of pollen) was present in this entity (e.g., bees), then the
necessary condition (e.g., locating food resources) would be satisfied.

4. Hence, the trait is present in the entity.

There are two difficulties with functional explanations of this type, both discussed by Hempel. The first is the tacit assumption that systems are self-regulating, i.e., develop appropriate traits for meeting their needs. "That adaptation has occurred seems obvious. That it does so most of the time or even very often is completely unclear" (Lewontin 1976:21). (See Gould et al. 1977, where it is argued that the order apparent in evolutionary trees is merely apparent and can be simulated by a random stochastic system.)

The second difficulty is that functional explanations commit the "fallacy of affirming the consequent." If such-and-such a trait (e.g., the dance language of honey bees) would satisfy some necessary condition (e.g., locating food resources) it does not follow that if some necessary condition is satisfied (e.g., food resources are routinely located) it must be satisfied by that trait ("if p → q" does not imply "if q → p"). (In fact, honey bees typically rely on smell, not dance languages, to locate food resources [see Gould 1975].) It is rare indeed that there is only one trait that nature can "design" to satisfy a necessary condition for survival.

Bateson (1967), however, disputes this point. He believes that, by a process of elimination, Darwinism can provide a uniquely determinate account of what persists (i.e., "only if p → q"). Bateson likens "natural selection" to the form of logical proof by reductio ad absurdum. In this species [sic] of proof, a sufficient set of mutually exclusive alternative propositions is enumerated, e.g., "P" and "not P," and the process of proof proceeds by demonstrating that all but one of this set are untenable or "absurd." It follows that the surviving member of the set must be tenable within the terms of the logical system.

Bateson's analogy is certainly an eye-opener, but I do not find it compelling. Unlike the "events" that occur within logical or mathematical systems, the events that concern empirical scientists cannot typically be partitioned into exhaustive, mutually exclusive alternatives (e.g., one cannot always tell whether a particular organism is a vertebrate or an invertebrate, a plant or an animal [see Rudd 1954]); alternatives overlap with one another. Moreover, there are no rules or algorithms for specifying or interdefining all the possible events that can occur "within the terms of" the system.
(see Cavell 1969 [Chapter 1]; Ziff 1972 [Chapter 4]; also see Toulmin 1971; Shweder and LeVine 1975). "Natural" systems, unlike logical and mathematical systems, are not tautological.

Finally, Bateson's analogy disguises the indeterminate nature of Darwinian explanations and seems to encourage "the fallacy of an a priori fitness course" (Sahlins 1976b:82–84). Sahlins identifies the fallacy by reference to various Darwinian explanations of the spawning behavior of the Pacific salmon. In the process of a "long and dehabilitating swim upstream" the female salmon "undergoes certain organic changes that optimize her egg-bearing capacity, such as atrophy of the digestive system." These changes also guarantee the salmon's death. According to Sahlins, a typical Darwinian account of the salmon's spawning behavior argues that "if a female salmon laid only one or two eggs, the reproductive effort, consisting primarily of the long swim upstream, would be very high. To lay hundreds more eggs entails only a small amount of additional effort." To this Sahlins points out the hidden indeterminacy of the "natural selection" process:

If selection will go so far as to atrophy the digestive tract in favor of a single reproductive explosion that also kills the organism, why should it not as easily effect structural changes that will allow the salmon to spawn twice or more to the same fitness effect, as for instance sturgeons do?

Facetiously, Sahlins remarks:

The problem is this course or some other was precluded not by a natural selection but by an analytic one. The salmon was taken as an a priori limited being with only one possible solution to the evolutionary problem of resource allocation to fitness, by a premise not motivated in the nature of evolution itself. The salmon is going to have only one chance to lay eggs, and that at very considerable cost. Once this set of conditions is taken as given, all other evolutionary possibilities to the same net fitness effect may be conveniently ignored.

Hence we have "the fallacy of the a priori fitness course."

Recently, neo-Darwinian theorists (e.g., Campbell 1965; Toulmin 1972; LeVine 1973) have argued that Darwinism is more than just a theory of organic evolution. These theorists advance it as an explanatory scheme of such generality that it can account for organic evolution, trial-and-error learning, the evolution of science, and the development of personality.

In an important paper, Campbell (1965:26–27) argues that "the
most exciting contribution of Darwin is in his model for the achievement of purposive or ends-guided processes through a mechanism involving blind, stupid, unforeseeable elements." The specifications of the model are as follows. Given three kinds of hypothetical events, an accommodation of a system to the objective constraints of its surrounding environment becomes inevitable:

1. **The occurrence of haphazard variations.** Campbell points to the mutation process in organic evolution and the exploratory process in trial-and-error learning.

2. **The differential elimination of variants as a result of the selective pressures of the environment.** Campbell points to the differential survival of certain mutants in organic evolution and the differential reinforcement of certain responses in trial-and-error learning.

3. **The reproduction of those variants that are retained.** Campbell points to the duplication process of the chromosome-gene system in plants and animals and the memory system in learning.

What Campbell has given us is an abstract formulation of the "law of effect" while at the same time arguing that it is the psychologist's analogue to Darwin's notion of "natural selection." Behavioral outcomes or consequences (e.g., rewards and punishments) are viewed as selective criteria that either differentially propagate or differentially eliminate certain behavioral variants, thereby "shaping" behavior to environmental constraints. Campbell's attempt to equate "natural selection" with the "law of effect" is provocative, but, even at this high level of generality, I do not think it succeeds. "The law of effect," unlike the concept of "natural selection" but very much like the concept of "rational choice," can only "explain" by reference to "subjective categories"; it does not guarantee the adaptation or accommodation of an organism to the constraints of its environment, as we shall see.

**The Law of Effect**

The "law of effect" states that "the effect of an act, including its rewards or non-rewards, is to change the organism that produced the act in such a way that the effect of the act is included among the future causes of the organism." The general claim is that behaviors are controlled by the events or stimuli that follow them. More specific claims concern the degree and manner of control of subse-
quential stimuli over preceding behaviors. For example, one principle states that the greater the delay between behavior and consequence, the less the consequential influence over future behavior. Another principle (the principle of variable-ratio intermittent reinforcement) states that the more random the occurrence of a consequence around a designated average number of consequences of that particular kind per some fixed number of behaviors of a particular kind, the more likely the behavior will persist even when the consequences fail to occur.

The "law of effect" is not as easy to understand as it first appears. Interpretive difficulties emerge as soon as one asks, Is the "law of effect" a law-like statement? Is it an empirical generalization capable of disproof? There is reason to believe it is not. One reason is that if the "law of effect" is an empirical proposition, as Thorndike (1933) seems to have believed, then the case against it is overwhelming; it would have to be rejected.

The consequences of an act often do not alter the future likelihood of occurrence of the behaviors that precede them. For example, Garcia and Koelling (1966; also see Seligman and Hager 1972) tried to train laboratory rats (90-day-old Sprague-Dawley males) to avoid drinking water. Two kinds of drinking water, "tasty water" and "bright and noisy water," were paired with two kinds of aversive consequences of water drinking, X-ray induced nausea and electrical shock. The experimenters discovered that their rats were unable to associate the occurrence of electrical shock with the "tasty water" and were also unable to associate the occurrence of nausea with the "bright and noisy water." The rats failed to link the occurrence of shock with their efforts to drink "tasty water" even when act and outcome were nearly simultaneous. The rats suffered shock and came back for more. Similarly, there is evidence that the pecking behavior of pigeons persists even if it costs the pigeon grain (Williams and Williams 1969) and that the locational preference of rats for black over white areas is not reversed by the experience of shock in the black area (Allison, Larson, and Jenson 1967).

In addition, there is evidence that the particular way in which the effect of a behavior influences that behavior, when there is an influence, is unpredictable from the effect alone. For example, in rats and pigeons, the larger a food reward the more rapid the extinction of the behavior. In turtles and goldfish the relationship between amount of reward and rate of extinction is just the opposite (Bitter-
man 1975:701). Similarly, partial or intermittent (as opposed to consistent) reward produces greater resistance to extinction in rats but not in African mouth breeders (Bitterman 1975:703). The extent and type of influence that a consequence will have on the preceding behavior cannot be predicted from only knowledge of the consequence itself.

One could go on enumerating the evidence that the actual consequences of an act are independent of its future course (see the papers edited by Seligman and Hager 1972). The effort, however, would be misguided. The evidence has no bearing on the "law of effect" because the "law of effect" is not an empirical generalization; it is a definition of what is to count as a consequence (or reinforcer) of a behavior. Consequences (or reinforcers) are by definition those subsequent events that alter the future probability of the occurrence of preceding behaviors.

The "law of effect" simply specifies what class of subsequent events (those that change the future probability of occurrence of preceding behaviors) we should be interested in studying. Like any definition the relationship between its terms is circular and tautological (see Chomsky 1964). The rewards or non-rewards (i.e., reinforcers) following an act alter its future probability of occurrence. How are we to know whether some subsequent event is a reinforcer? It is a reinforcer only if it changes the future probability of the occurrence of the preceding act. Thus evidence that the drinking behavior of rats is unaffected by subsequent shock or that food decrements do not alter pecking behavior in pigeons merely indicates that shock and food are not reinforcers in those instances.

At first glance it seems perplexing that the "law of effect" is either wrong or circular. One way out of this interpretive conundrum is to recognize that not all circularities are vicious ones. Once we recognize that the "law of effect" is merely a definition of a reinforcer, we can turn away from the misguided question. Is the "law of effect" true or false? (definitions are neither true nor false) to the more relevant question, Is the "law of effect" useful?

How can we assess the usefulness of the "law of effect"? The answer seems to be that we must see if we are able to predict which subsequent events will serve as reinforcers without having to carry out a detailed contextual analysis of this particular organism's behavior in this particular environment. Are we in fact able to predict reinforcers in this way? Unfortunately the answer seems to be no. Knowl-
edge of which events will reinforce is knowledge of a great deal more
than simply what event will follow an act.

Premack (1965), for example, has mounted an impressive case
that there are no intrinsic reinforcers. The subsequential occurrence
of, for example, shock, food, or intracranial stimulation may or may
not influence the future likelihood of the occurrence of preceding
behaviors. Sometimes shock suffering, food ingesting, and intra-
cranial stimulation are reinforcers; sometimes they themselves can
be reinforced. It all depends. What it depends upon, although very
simple, is highly contextual and profoundly subjective.

To identify what can be reinforced and what will serve as a rein-
forcer one must first discover the organism’s preferences or goals (as
measured by duration of activity selection under free choice condi-
tions). These preferences vary from organism to organism, situation
to situation, and time to time. Premack presents considerable
evidence in support of the following claims:

1. For any pair of responses the more probable response will rein-
force the less probable response.

2. Reinforcement is a relative property. The most probable
response of a set of responses will reinforce all members of the set;
the least probable will reinforce no member of the set.

3. The reinforcement relationship is reversible. If the probability
of the occurrence of two responses can be reversed in order, so can
the reinforcement relationship between the two responses.

For example, if a rat would rather explore its cage than drink
water (as measured by the duration of time it freely spends in each
activity) then the subsequential opportunity to explore can be used
to increase the future probability of occurrence of water-drinking
behavior but not the reverse. But, if the rat would rather drink than
explore its cage, it is now water drinking that can be used to rein-
force exploration. It would seem that even hard-nosed behaviorists
must sneak in the actor’s point of view before behavior can be ex-
plained; the concept “reinforcer” is ultimately a subjective category.
Reinforcers are not independent objective features of an external
environment.

The selective criteria with respect to which an organism (or per-
son) adapts its behavior turn out to be the organism’s (or person’s)
own goals. Behaviors that enable the organism to attain its goals are
repeated. Gain leads to repetition. All this sounds like rational
choice cloaked in a different idiom.
It seems that the "law of effect" is either wrong or tautological. To the extent it is a tautology it is not the kind of tautology that enables us to make powerful context-free predictions about behavior. Its usefulness is limited by the fact that one cannot specify what will be a reinforcer until one already knows all about the actor's preferences. This can only be learned by a detailed contextual analysis of the actor's behavior.

Note that nothing I have said denies the existence of a reinforcing relationship between particular events in everyday life. That reinforcement occurs seems obvious. That it does so most of the time or even very often is entirely unclear (see quote above from Lewontin 1976:21). Of course, all members of a society (and most learning theorists) acquire a good deal of context-dependent knowledge about what subsequential events will have what effects on what behaviors under what circumstances for which class of persons (or species of organisms). But the knowledge is context dependent. Whatever predictive validity one achieves presupposes that one already knows all about the subject's preferences and the special conditions that affect them. There are no context-free reinforcers. The reinforcement relationship is a "secondary formation."

**Duty and Desire Reconsidered:**
**The Congruence of the Individual and Social**

We are now in a position to reconsider the claim that culture and personality are consonant (congruent, integrated, isomorphic, etc.) and the related formulation that sociocultural institutions are constrained by, and ultimately adapt to, prior motivational variables. The status of the claim is not unlike the "law of effect." For example, if we treat the "motivational integration of culture" as an empirical generalization it is probably wrong. On the one hand, Rorschach test evidence suggests that with respect to personality variables "individuals within cultures vary much more among themselves than they do from individuals in other cultures" (Kaplan 1954:16). On the other hand there is evidence that affective variables (including "anxiety") are not global traits and display a specificity like the personality traits discussed under Postulate 1 (see Endler and Hunt 1966, 1968). The person who gets angry when contradicted is not the person who gets angry when pushed aside in a ticket line. There seems to be too much specificity and overlap of
personality variables across populations to systematically account for the striking differences that exist between populations in their social and cultural institutions (see LeVine 1966:32-33 for the view that "cross-population differences, accounting for a small proportion of the total variance 'can' have a significant impact on sociocultural systems and processes"; and Spiro 1961:115-116 for the apparently opposite view that "it is possible for different modal personality systems to be associated with similar social systems, and for similar modal personality systems to be associated with different social systems").

Finally, the postulate of shared motivations is not always required to account for those instances of cultural integration that do occur. Earlier I discussed the complex pattern of results that emerges from Costanzo's (1974) reanalysis of Whiting and Child's (1953) data on socialization anxiety and explanations of illness (see Table 4 in Part I). In the light of Costanzo’s results one is tempted to reconsider a position entertained and then dismissed by Whiting and Child (1953:310) (I will call it the "null hypothesis"): "[cultural] integration might be directly between 'projective systems' (which would then not merit the name) and child training practices, personality characteristics being quite irrelevant." A people that thinks a lot about the human body may be more scrupulously concerned with it when raising children. As far as I can tell, the "null hypothesis" has yet to be rejected (also see Benedict 1928 for a "cognitive set" theory of cultural integration).

However, claims concerning the "motivational integration of culture" need not be interpreted as empirical generalizations. Rather they can be interpreted as definitions of individual-social congruence. What do we mean when we say that a belief (e.g., monothelism) or an institution (e.g., adolescent initiation ceremonies) is congruent (consonant, isomorphic, etc.) with a group's personality? The best definition I can offer runs as follows: One's personality is congruent with the beliefs of one's culture and the institutions of one's society to the extent that one's motives can be satisfied by means of those beliefs and institutions.

This definition of individual-social congruence (or integration) directs our attention to the class of motives that is satisfied by any particular belief or institution. The relevant theoretical question for culture and personality theory becomes, To what extent can we predict the motive that is satisfied, exclusively from knowledge of
the belief or institution? Unfortunately, here as elsewhere, our predictive successes are rather limited.

The relationship of motives to beliefs and institutions is many-to-many. Any particular motive can be satisfied by a wide (although not infinite) range of beliefs or institutions. Any particular belief or institution is compatible with diverse motives. It is because of this many-to-many relationship that Bendix (1952) argues that normative behavior is psychologically unrevealing. He notes that it is, of course, possible for a theorist to construct "an analogous psychological syndrome" for any cultural belief or social institution, e.g., the compulsive character type of psychoanalytic lore has been advanced as a personification of German Nazi values emphasizing hard work, discipline, sacrifice, devotion, and will power. But, Bendix points out, the psychological syndrome and the cultural institution do not require one another and empirically go together only as an exception, not as a rule.

In a similar vein, Spiro (1961:115) argues that

a knowledge of a person's social roles would not even lead to an accurate prediction of those aspects of his personality that are caught up in their performance ... (1) different drives may be canalized by the same goal, which is attained by the performance of the same role; (2) the same drive may be canalized by different goals, which are attained by the performance of different roles; and (3) different drives may be canalized by the same goal which is attained by the performance of different roles.

Sahlins (1976b:7-8) formulates the many-to-many relationship this way:

There is no necessary relation between the phenomenal form of a human social institution and the individual motivations that may be realized or satisfied therein... Men may be moved to fight out of love (as of country) or humaneness (in light of the brutality attributed to the enemy), for honor or some sort of self-esteem, from feelings of guilt, or to save the world for democracy ... compassion, hate, generosity, shame, prestige, emulation, fear, contempt, envy, greed—ethnographically the energies that move men to fight are practically coterminous with the range of human motivations.

Sahlins and Spiro seem to concur: there is no reason to expect that one can predict an actor's particular motive from what s/he does (or what s/he believes).
RETHINKING CULTURE AND PERSONALITY THEORY:
THE BREAKDOWN OF THE ACTION SCHEMA

Men may violently disagree about the purposes behind a given act, or about the
color of the person who did it, or how he did it, or in what kind of situation he
acted; or they may even insist upon totally different words to name the act itself.
But be that as it may, any complete statement about motives will offer some kind of
answers to these five questions: what was done (act), when or where it was done
(scene), who did it (agent), how he did it (agency), and why (purpose) (Burke
1969:XV).

In a unit act there are identifiable as minimum characteristics the following: (1) an
end, (2) a situation, analyzable in turn into (a) means and (b) conditions, and (3) at
least one selective standard in terms of which the end is related to the situation. It is
evident that these categories have meaning only in terms which include the subjective
point of view, i.e., that of the actor (Parsons 1968:77).

I believe most culture and personality theorists would agree with
both Horton (1967:51) and Hart (1961:184): (1) To construct a
scientific theory is to elaborate "a schema of forces or entities (of a
limited number of kinds and governed by a limited number of
general principles) operating 'behind' or 'within' the world of com-
mon sense observation" (emphasis added); and (2) "In the last
resort, a scientific theory's claim to forward our understanding of
nature is dependent on its power to predict what will occur, which is
based on generalizations of what regularly occurs" (paraphrased).
That is, a good theory is both parsimonious (it explains a lot and
does it with relatively few categories and principles) and valid (it has
predictive utility). A major conclusion of my critical examination of
culture and personality theory is that parsimony and validity cannot
be simultaneously achieved in the social sciences. What typically
seems to happen is that the predictive success of one's theory is in-
versely related to its parsimony and vice versa.

With predictive success as a criterion, none of the parameters of
Burke's (1968, 1969) or Parsons' (1968) action schema seem to lead to
a parsimonious theory of human conduct. As we have seen, an
"agent-act" emphasis (an emphasis on "personality" or "the cor-
respondence between a man's character and the character of his
behavior," [Burke 1968]) leads us into complexity, highly specific
act-context-person interaction effects, and the recognition that "it
all seems to depend" (see Postulates 1 and 2). An "agency-purpose"
emphasis (an emphasis on "rational choice" or the principles gov-
erning the selection of means to accomplish ends) leads us to the
limits of rationality and to the details of the historical, cultural, and clinical idiosyncrasies of the actor (see Postulate 4 above). An "act-act" emphasis (an emphasis on the "law of effect" or the principles governing the way certain events have subsequential control over preceding events) suffers the same fate (see Postulate 4 above). Finally, a "scene-act" emphasis (an emphasis on "circumstances," the "situation," or the way "objective conditions" regulate conduct via "natural selection") leads us into non-comparability on the one hand (see Postulate 3 above) and indeterminacy on the other (see Postulate 4 above). In summary, human conduct does not readily lend itself to description in terms of a universal scheme of forces or entities "of a limited number of kinds and governed by a limited number of forces."

There is a striking similarity and convergence between this conclusion and the one drawn by certain ethologists, ethnologists, and psychologists concerned with social behavior. Jones and Konner (1976:347), for example, doubt the very possibility of a general theory of animal behavior. They suggest that the best one can do is "simply to know a lot about each animal." Somewhat the same position is adopted by Geertz (1973:26). He argues that theory building in cultural anthropology should be aimed at making "thick description" possible instead of either generalizing across cases or trying to "subsume them under a governing law." (Perhaps Geertz should have said "universal law": a generalization within cases is still a governing law.) Explicitly endorsing the logic of clinical inference, he believes that generalizations are only possible within cases, i.e., that generalizations in cultural anthropology are restricted in their scope and limited in their power. What this means is that to adequately understand human conduct one must engage in the unparasimonious proliferation of context-dependent insights; that is the conclusion that is demanded by all the evidence on interaction effects. Raush, Dittmann, and Taylor (1959:371) provide us with an illustration from children's social behavior.

On the whole, 54 percent of all responses produced by children towards adults were coded as hostile. One of the children, Frank, exhibited 60 hostile out of a total of 161 interactions with adults, an average of 37 percent hostile responses. However, in the arts and crafts setting, a teaching situation, only 9 percent of his behavior towards adults was hostile in orientation; this was in contrast with an average of 28 percent hostile responses by all children toward adults in arts and crafts. At meal time, the case was reversed. There, 48 percent of Frank's behavior towards adults was hostile in orientation in contrast to the lower group average of 29 percent.
Raush, Dittmann, and Taylor conclude that the effects of individual differences on social behavior are "considerably enhanced" when they are examined separately within each setting. Different settings affect different children in different ways. This clinical recognition of interaction effects anticipates the retreat from universal explanatory theory reflected in the work of Jones and Konner (1976), and Geertz (1973) (also see Cronbach 1975 for the view that "generalizations decay," and Campbell 1972:XI for the view that "for the social and psychological issues that concern the students of culture, higher-order interactions are the rule, and main effects, ceteris paribus generalizations, the rare exception. Complex interdependencies, highly contingent relationships and context dependencies characterize the relationships among the aspects of culture and personality."). I happen to find this retreat disturbing (I much prefer "lumpers" to "splitters"), yet I do not see how it can be avoided. Nonetheless there are retreats and there are retreats.

Those on the quest for universal explanatory theory that is both parsimonious and valid may soon arrive at an unexpected and disquieting fork-in-the-road. The signpost on one road reads: "Anything can be explained; little can be predicted." The signpost on the other road reads: "Anything can be predicted; little can be explained." Neither choice is acceptable.

The first road has been well traveled by Geertz's proverbial peasant (1973:26). He shoots holes in fences and then paints bull's eyes around them. Much that passes for social science is little more than the promiscuous generation of interpretations within the grammar of some unassailable world view (e.g., Darwinian, Marxian, Freudian, Behaviorist, Structuralist, etc.) for events that have already happened. There is a certain satisfaction that comes from being able to explain any possible outcome, but it should be severely tempered by Hart's (1961:184) reminder. Our respect for a theory is related to its predictive success, not its post-dictive ability to translate any occurrence into its flexible idioms.

Down the second road, successful prediction becomes a non-explanatory activity. One can predict anything, and with enough prior information about event likelihoods one can often do it reasonably well. All one needs to assume is that "behavior at [time] t + 1 will be [only] marginally different from behavior at the present time" (Allison 1969:702). Not infrequently, the best prediction of behavior at t + 1 is simply t. For example, D'Andrade (1974: 181-185) has shown that the best prediction one can make about
what a group or individual is going to do is to guess that it will do what it has done most frequently in the past (also see Mischel 1968:106). Of course to do this one must already know all about how the thing predicted has previously behaved. T + 1 predictions are very costly; moreover they typically fail to explain the phenomenon or render it theoretically intelligible. At the fork-in-the-road, I would turn back.1

REFERENCES


1 Not all readers will want to shift theoretical emphasis with me, and those who do will probably have quite diverse agendas for culture and personality research. My own ideas for a positive program, "Rethinking Culture and Personality Theory. Part 3: From Genesis and Typology to Hermeneutics and Dynamics," will appear in Ethos 8:3.


