Rethinking Culture and Personality Theory

Part I:
A Critical Examination of Two Classical Postulates

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...it is time to abandon the assumption (so prevalent till now) that everything is glued together; perhaps it is time to seriously entertain the hypothesis that nothing is glued together until proved otherwise (Mischel 1971:23, paraphrasing Bem 1972b:57)... The heuristic advantage of this strategy is not guaranteed, of course. But the difference in morale if +.30 correlations continue to come in is itself worth considering (Bem 1972b:57).

Most of the postulates of the culture and personality school were worked out in the 1940s and 1950s (e.g., Gorin 1943; Kardiner

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In preparing this essay I have benefited from the comments of a number of scholars on an earlier version entitled "Culture and Personality Theory: Is It Fit For Survival?" These scholars are Donald W. Fiske, J. David Greenstone, Robert A. LeVine, Ruth H. Munroe, Melford E. Spiro, Thomas S. Weisner, and John W. M. Whiting. I wish to note that this expression of appreciation in no way implicates them in my particular approach to explanation and understanding in the social sciences.


I gratefully acknowledge the financial support of the Spencer Foundation.
1945; Benedict 1946; Wallace 1952: Whiting and Child 1953). A small set of universal generalizations were advanced which seemed to make it possible to connect and interrelate so much—e.g., the past and the present, motives and institutions, parents and gods. In this essay I examine those bold generalizations of the 1940s and 1950s and discover that they do not weather well under empirical and conceptual scrutiny. When properly qualified they lose much of their predictive power and parsimony and turn out to be severely restricted and context-dependent in their implications. Much less is "glued together," much less is integrated, than most of us have imagined.

Four postulates are examined in this essay: (1) the search for global traits, or the postulate that stimulus generalization has precedence over stimulus discrimination; (2) the search for childhood origins, or the influence of the past on the present; (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 "objective" environment. Postulates 1 and 2 are examined in Part I of the essay; Postulates 3 and 4 are examined in Part II.

The following conclusions are drawn: (1) Individual differences in conduct are narrowly context-dependent and do not widely generalize across comparable contexts. Extant evidence for cultural integration is not compelling (see Postulate 1). (2) Early child care practices per se do not have predictable consequences for adult character (see Postulate 2). (3) Situational comparability is inversely related to cultural variation; hence an individual difference approach to cultural differences is, for the most part, inapplicable (see Postulate 3). (4) "Objective" conditions, reinforcers and other "external" stimulus events do not guarantee the accommodation of an organism to its environment. Unless we already know a good deal about a person's goals, preferences, beliefs, ethics, and cultural conventions, most of our universal generalizations have little predictive power (see Postulate 4). The two-part essay concludes with a discussion of the inverse relationship between parsimony and predictive utility in social science theory. Although much of the essay is critical of current assumptions within culture and personality theory, it would be a mistake to interpret this exercise in "rethinking" as a renunciation of the discipline. It is precisely because the canons of science are so deeply entrenched among culture and personality
researchers that it is possible to seriously appraise, and fruitfully
debate, the adequacy of our assumptions.

TWO CLASSICAL POSTULATES
OF CULTURE AND PERSONALITY THEORY:
ARE THEY FIT FOR SURVIVAL?

Typically, investigators of culture and personality have sought
linkages between social institutions and symbolic systems, on the one
hand, and personality variables on the other (see, e.g., Wallace
1952; Whiting and Child 1955; Whiting, Kluckhohn, and Anthony
Whiting and Whiting 1975). In recent years the links have weak-
ened, in large part because a pattern of evidence and theory has
emerged that seriously questions the relevance of the concept "per-
sonality" as it has been applied in culture and personality research
(see, e.g., D'Andrade 1965, 1973, 1974; Mischel 1968, 1973;
1978; Ross 1977; Ross, Amabile, and Steinmetz 1977; Shweder and
D'Andrade 1979). The implications of this pattern of evidence and
theory are often negative and sometimes severe, especially for any
approach to behavior that emphasizes "genesis" (origins in early ex-
perience) and/or "typology" (the classification of individual or
cultural differences). These implications are explored below with
special reference to two of the classical postulates of culture and per-
sonality theory.

POSTULATE 1: THE SEARCH FOR GLOBAL TRAITS,
OR THE POSTULATE THAT STIMULUS GENERALIZATION
HAS PRECEDENCE OVER STIMULUS DISCRIMINATION

Postulate 1a: The Search for Global Personality Traits

... the attitudes of the child to his father and mother, and to a lesser degree,
towards his siblings will become the prototypes of his attitudes towards all subse-
quently met people (Gorer 1945).

... rituals—techniques for interacting with and influencing the super-
naturals—correspond to and are generalizations from modes of interaction used by
children to influence their parents (Spiro and D'Andrade 1958).
POSTULATE 1b: THE SEARCH FOR GLOBAL CULTURAL TRAITS

...patterns of superordination and subordination, of deference and arrogance, will show a certain consistency in all spheres from the family to the religious and political organizations; and consequently the patterns of behavior demanded in all these institutions will mutually reinforce each other (Gorer 1945).

Religious dogmas, economic practices, and politics do not stay dammed up in neat, separate little ponds but they overflow their supposed boundaries and mingle inextricably with one another... I started from the premise that the most isolated bits of behavior have some systematic relationship to each other... A human society must make for itself some design for living... Men who have accepted a system of values by which to live cannot without courting inefficiency and chaos keep for long a fenced-off portion of their lives where they think and behave according to a contrary set of values (Benedict 1946:11–12).

Any attempt to describe either personality or culture must address two basic questions: (1) How widely do the thoughts, emotions, and actions of a person or a people generalize across diverse stimuli, contexts, or domains? (2) To what extent can the thinking, feelings, and doings of a person or a people be sorted into a limited number of descriptive categories? The two questions are interrelated. If behavior is widely generalized it can be described with fewer categories, and vice versa.

Postulate 1 and all typological or global trait theories claim that individual or cultural differences in behavior are widely generalized and can be described with a relatively small number of categories (e.g., extroverts vs. introverts, Apollonians vs. Dionysians). Obviously, there are no absolute standards for deciding how wide is wide, or how small is small. Nonetheless, to the extent that parsimony is a criterion of adequacy for assessing a theory, disagreements over questions of relative degree can be important.

Benedict (1934), for example, portrays a world in which a great deal hangs together. She believes that knowledge of a people's ritual conduct warrants inferences to political, economic, and military conduct. Thus she argues that people who participate in depersonalized rituals also scorn political office, avoid excess, and devalue heroism (her Apollonian type). Mischel (1968), on the contrary, portrays a world that is neatly "dammed up into neat, separate little ponds." For Mischel, knowledge that a person hates his or her father does not warrant an inference to feelings about his or her boss, and knowledge that a child literally "clings to its mother's apron strings" implies little about other dependency measures such as seeking help.
or seeking attention. Along the continuum between a world in which all things are systematically interrelated and a world in which no things are, there is plenty of room for disagreement about just how widely behavior does generalize. The disagreement tends to focus on the single crucial question, namely, how much context must be written into the definition of one’s descriptive categories or hypothesized traits? It is important to recognize that the relevant question is not "Do traits exist?" but rather, "How context-dependent are traits?" Individual and cultural differences clearly exist, but how generalized are they across situations?

**Global Personality Traits:**
**In Search of a Missing Phenomenon?**

For years, personality psychologists have searched for generalized consistencies in the way people differ from one another in their feelings and social behavior across diverse contexts. What they have discovered is that method variance is greater than person variance (see e.g., Burwen and Campbell 1957; Campbell and Fiske 1959; Burton 1970). Distinguishable qualities of character, e.g., "autonomy" and "ascendancy," typically show higher within method associations than parallel across method associations. For example, if "autonomy" and "ascendancy" are measured using two methods, a projective test (e.g., T.A.T.) and a clinical interview, "autonomy" and "ascendancy" will more positively correlate within the projective test data than "autonomy" correlates with itself across the two methods. In general, features of personality measuring instruments (the clinical interview situation, the projective test stimulus and context) have been found to be more stable than features of the people measured. Single method research usually tells us more about social science’s methodological artifacts than anything else.

Personality psychologists have also discovered that hypothesized global trait dispositions (e.g., "dependency," "dominance," "friendliness") typically account for no more than 9 to 15 percent of the diversity of individual differences over naturally occurring situations (see Mischel 1968, 1973 and Fiske 1978, for an overview; also Newcomb 1929, 1931; Raush, Dittmann, and Taylor 1959; Raush, Farbman, and Llewellyn 1960; Sears 1963; Hunt 1965; Endler and Hunt 1966, 1969; Moos 1968, 1969; Argyle and Little 1972;
Shweder 1972, 1973). The more assertive child at the breakfast table is not the more assertive child in the playroom. The child who seeks help more than others is not the one who is more inclined to seek physical nearness. The man who is more likely to express his emotions to his wife is not the one who is more likely to express his emotions to his friends. The person who gets angry when contradicted is not the person who is more likely to get angry when cut in front of in line. The adult who is more hostile to a parent is not typically the adult who is more hostile to a boss, nor is he or she the one who is typically less hostile to a boss. Individual differences in the one context do not predict individual differences in the other. Different situations, stimuli or domains seem to affect different people differentially.

Consider, for example, the relationship among the three behavioral indicators of nurturance displayed in Table 1. The correlations (Pearson r) in Table 1 come from a research report by Longabaugh (1966:10). They are derived from the average proportion of each behavior in the behavioral profiles of 24 Indian Rajput children as observed by Leigh Minturn (see Shweder 1973:536). A striking feature of Table 1 is, e.g., that knowing which children are more likely to offer help to others tells you very little about which children are more likely to offer support and approval (r = .17) or which are more likely to make responsible suggestions (r = -.25). The relationships among the three items are quite weak. This is typical of behavior observational findings.

Cross-cultural evidence from the Whiting and Whiting (1975:163) study of children’s social behavior does not provide encouraging support for Postulate 1. In a comparison of the behavior of 134 children from six cultures (including the Rajputs discussed

<table>
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<th>TABLE 1</th>
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<tr>
<td><strong>CORRELATIONAL RELATIONSHIP AMONG 3 INDICATORS OF NURTURANCE IN BEHAVIORAL OBSERVATIONS OF 24 RAJPUT CHILDREN</strong></td>
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<td>1 Offers help</td>
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<td>2 Offers support and approval</td>
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<td>3 Makes responsible suggestions</td>
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*Data from Longabaugh (1966:108-110).
above) the highest reported level of consistency for a system of behavior (in this case “prosocial” behavior) across a set of comparable contexts (behavior to infants, to peers, and to adults) is .29 (Pearson r). For nurturant behavior the consistency coefficient is .05. Knowing that a child is relatively more nurturant than other children to his parents tells you nothing about whether he will be relatively more nurturant to his peers. Table 2 presents summary evidence on the degree of consistency of individual differences for each of six systems of behavior across three types of situations (behavior to an infant vs. a peer, to a peer vs. a parent, and to an infant vs. a parent) in the Whiting and Whiting (1975) study. The degree of generalization of individual differences is not impressive.

These failures to find impressive support for highly generalized traits of character are not idiosyncratic. Burwen and Campbell, for example, initiated a study expecting to find “generalized attitudes towards authority derived from previous encounters with authority, especially in the early family situation” (1957:29). In the face of “apparatus factors” (method variance) and low consistency coefficients they were forced to conclude that although “stimulus generalization is appropriate to the autistic thought processes in normal dream states and in the waking states of extreme neurotics and psychotics, it does not interfere with the waking perceptions of normal individuals such as constituted the test population for the present study” (Burwen and Campbell 1957:30). Piaget (1962) seems to have reached a similar conclusion. He argues that broad stimulus generalizations seem to operate only “in certain exceptional situa-

### Table 2

| Average Correlations (Pearson r) for the Individual Difference Scores of 154 Children from Six Cultures Across Three Types of Situations (Behavior to Infants vs. Peers, Peers vs. Parents, Infants vs. Parents) for Six Types of Behaviors*
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<td>Pro-social behavior</td>
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*Adapted from Whiting and Whiting (1975:163).*
tions, such as children's play, the dreams of both children and adults, and sometimes in states of completely relaxed thought [e.g., during a psychoanalytic session]. All these are situations in which assimilation [i.e., stimulus generalization] either takes precedence over accommodation [i.e., response differentiation] or even entirely supplants it" (Piaget 1962:211).

The absence of impressive support for generalized or global traits of character is surprising. Most of us, social scientist and layperson alike, share certain intuitions or everyday personality theories which suggest that certain items or traits of behavior go together (e.g., "smiles easily" and "introduces himself to strangers"; "gentle" and "good-natured") or are opposed (e.g., "aggressive" and "friendly"; "gregarious" and "reserved") (see Brown 1965; D'Andrade 1974). Many personality psychologists and most laypersons interpret these "everyday" personality theories or trait and type concepts as inductive generalizations. They are held to arise out of observational experience and accurately summarize or encode "relative frequencies of joint occurrences of various personality attributes and behavioral dispositions in other persons" (Passini and Norman 1966; also, see Brown 1965). Recent evidence, however, challenges this view.

It now appears that everyday personality theories express widely shared preexisting notions of what is like what or what is conceptually related to what (see e.g., Mulaik 1964; D'Andrade 1965, 1973, 1974; Shweder 1972, 1975, 1977a, 1977b, 1977c, 1977d; Ebbesen and Allen 1977). Moreover, these theories often have very little relationship to immediately scored co-occurrence likelihoods among everyday behavioral events (see, e.g., D'Andrade 1973, 1974; Shweder 1975, 1977a, 1977c). Finally, research suggests that everyday personality theories, themselves in large measure theories about conceptual relationships, systematically distort observational reports on standard personality assessment instruments (see, e.g., D'Andrade 1974; Shweder 1975; Shweder and D'Andrade 1979).

It is troubling that almost all the extant evidence in support of abstract trait and type concepts (e.g., "dependency," "ego-strength," "introversion") in the personality psychology literature has been collected from interpersonal rating forms, questionnaire interviews, and personality inventories (see, e.g., Cattell 1957; Norman 1963; Smith 1967; Lorr and McNair 1963; Sears, Maccoby, and Levin 1957; Block 1965). Rating forms, questionnaire interviews, and inventories turn out to be quite demanding inferential
tasks in which "magical" thought processes are likely to intrude. What seems to happen is that conceptual affiliations (e.g., "smiles easily" and "likes parties") and conceptual exclusions (e.g., "gentle" and "managerial") dominate the judgmental process (see e.g., D'Andrade 1973, 1974; Shweder 1975, 1977a; Ebbesen and Allen 1977). Items alike in concept are judged to go together even when, as is typically the case, they do not behaviorally co-occur. Thus, the data gathered from interpersonal rating forms, questionnaire interviews, and personality inventories lends illusory support to the mistaken belief that individual differences can be described in a language consisting of context-free global traits, factors, or dimensions.

Consider, for example, the pattern of intercorrelations among the five indicators of the hypothesized trait "talkativeness" display in Table 3. The data comes from a study by Newcomb (1929) concerning introversion and extroversion in the behavior of boys at a summer camp. Newcomb's study is rather special in that it is possible to compare what goes with what in behavior as immediately recorded.

**TABLE 3**

**INTERCORRELATIONS AMONG BEHAVIORAL INDICES OF "TALKATIVENESS" FOR 30 BOYS AT CAMP DERIVED FROM IMMEDIATE SCORINGS OF BEHAVIOR (ITALICIZED UPPER HALF-MATRIX), AND FROM SUBSEQUENT OBSERVER RATINGS (LOWER HALF-MATRIX)*

<table>
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<th>Behavioral indices of &quot;talkativeness&quot;:</th>
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<tr>
<td>1. Tells of his own past, and of the exploits he has accomplished.</td>
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<td>2. Gives loud and spontaneous expressions of delight or disapproval.</td>
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<tr>
<td>3. Goes beyond only asking and answering necessary questions in conversations with counselors.</td>
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<td>4. How is the quiet hour spent?</td>
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<td>5. Spends a lot of time talking at the table.</td>
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<td>3</td>
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*Adapted from Newcomb (1929:42, 48).
with what goes with what in behavior as summarized and judged on an interpersonal rating form by those who had kept the immediate records of behavior.

In Table 3 the data derived from immediate scorings of behavior suggest that there is little warrant for drawing inferences among the five indicators of "talkativeness." The behavioral evidence is consistent with the notion that individual differences do not widely generalize across situation. For example, knowledge of whether or not a child talks more than others during quiet hour tells you little about whether or not he will talk more than others at meals (r = .16).

Perhaps the most striking feature of Table 3, however, is the discrepancy between what goes with what in the immediate scorings of behavior and what goes with what in the ratings of those who originally kept the immediate records. For example, in the ratings, the correlation between talking during quiet hour and talking at table is .75! Raters seem to draw inferences about what goes with what that are not warranted by behavioral experience. What this suggests is that the neat package of intercorrelations among trait indicators in the personality psychology literature does not arise out of behavioral experience but rather represents a confusion of "propositions about the world with propositions about language" (D'Andrade 1965:215).

**Parsimony versus Consistency: Too Much Context Is Not a Good Thing**

How might one react to all the failures to find impressive support for generalized or global traits of character? As a result of such repeated failures (see Burwen and Campbell 1957; Mischel 1968, 1973; Endler and Hunt 1966, 1969) the recent emphasis in personality psychology has been on processes of discrimination and differentiation, not on processes of generalization. Unfortunately, this "divide and conquer" strategy of splitting up trait concepts to accommodate person-situation interaction effects creates its own special difficulty, the problem of the unparsimonious proliferation of theoretical categories.

A trait splitting strategy is possible for personality theorists because the relevant question in research on individual differences is not whether individual differences generalize. Rather the relevant
question is, across how many and across which particular contexts are the generalizations that exist to be found? For example, suppose we discover (as we have: Sears 1963) that the global trait concept “dependency” cannot be used to consistently describe children’s social behavior; that is, we find that children who seek attention a lot are not the ones who cling to their mother’s apron strings. A consistency criterion tells us to simply rewrite the original trait concept dividing it into two independent concepts. We no longer speak of “children who are dependent.” Instead we speak of “children who seek attention” and “children who seek physical nearness.” If we subsequently find that children who seek attention from their mothers in the playground are not the ones who seek attention from their mothers at home, we again rewrite our trait concepts including more context. We speak of “children who seek attention from their mothers in the playground” and “children who seek attention from their mothers at home.”

Consistency is not a hypothesis. It is an incorrigible presupposition or criterion. It is the criterion by reference to which trait theorists either admit or exclude a particular trait concept. Consistency dictates how much context we must write into our definition of a hypothesized trait. Since, logically, trait consistency can always be preserved by writing more and more context into our trait concepts, it follows that it is not “consistency” we are in a position to assess but rather the “degree of generalization” of a trait. What we strive to do is find a level of description where our behavioral indicators are internally consistent or homogeneous. With this in mind, the implication of Mischel’s (1968) evidence becomes clear. With consistency as our criterion, our trait concepts will have to get very complex and context-dependent, e.g., “children who ask their mothers to watch them build things when other children of the same sex are watching,” or perhaps “cultures in which people generalize at the expense of detail but only when talking about interpersonal situations during a clinical interview” (see Inkeles, Hansmann, and Beier 1958:11). And, we will have to employ a great many such complex concepts to account for behavioral diversity (although hopefully not as many such complex concepts as there are behaviors to describe). If we are permitted to introduce enough context into the specification of our trait concepts we can always discover consistencies; children who ask their mothers to watch them build things when other children of the same sex are watching may do it
regardless of the time of day, regardless of the number of same sex peers watching, regardless of the object being built, etc. We can always discover (some kind of) consistencies, but at a great cost, the sacrifice of parsimony.

How much context should we be allowed to write in to our definition of our trait categories? How many trait categories should we be permitted to contrive? There seems to be no logical way to decide between two possible answers to these questions: (1) The Nomothetic Scientist’s Response: Write in only as much context and utilize only as many categories as parsimony permits; (2) The Idiographic Clinician’s Response: Write in as much context and use as many categories as are needed to discover the consistencies in behavior.

There is nothing inherently wrong with the clinician’s response. Clinical insight has always involved the appreciation of context-person interactions, and clinical methodology often produces long and complex lists of the ways in which specific situations affect a specific person in a special way. In fact, as noted above, there is considerable evidence that it is “idiosyncratic” or “interactive” effects, “the particular ‘meaning’ that a particular situation” has for a particular person, that are the major determinants of behavior (Raush, Farbman, and Llewellyn 1960; also Endler and Hunt 1966; Moos 1968, 1969; Argyle and Little 1972). The man who is more likely than others to express his emotions to his wife is not typically the man who is more likely than others to express his emotions to his friends. The rub is that such listmaking just does not bring us any closer to the construction of a general explanatory scheme. When complexity is applauded instead of simplified, the pursuit of universal explanatory theory has been abandoned. Postulate 1 will not lead us to parsimony. At the personality level, it does not enable us to sufficiently reduce the diversity of ways individuals differ from one another.

**Other Reactions to the Absence of Global Traits**

There are a number of other ways to react to the pattern of evidence discussed above. One is to suggest that better methods will one day produce evidence more consistent with a global trait approach to personality.

LeVine (1973), for example, in a discussion of the problems of studying personality cross-culturally, acknowledges that extant
evidence in the personality literature is not encouraging. He notes that "different methods of measuring the 'same' disposition correlate poorly or not at all, yielding differing distributions of results for the same group of individuals . . . "(1973:173). He remarks that " . . . behavior measured in psychological experiments and testing situations is strongly, sometimes overwhelmingly, influenced by the interactive settings in which observation and measurement take place (1973:175)." He grants that " . . . the behavior that personality psychologists regard as symptoms of person-specific dispositions have not shown the expected trans situational generality" (1973:176), and that dispositional theories of personality have not produced "accurate predictions as a purely inductive approach to individual behavior" (1973:177).

It is LeVine's view that dispositional theories of personality "have made little headway towards confirmation" (1978:178), in large part because evidence collected in laboratories, classrooms and clinics is restricted and artificial (1973:182-183). LeVine raises the hope (1973:184) that a return to the clinical methods of psychoanalysis (which he characterizes in terms of the logic of "physiological," "ecological," and "embryological" research) will make it possible to identify unities and underlying mechanisms behind the diversity of situation-specific responses (1973:178). He concludes that it would be "premature" to reject a "dispositional view" of the person (1973:177).

LeVine's perspective is quite challenging, although it seems to me that there is nothing in the evidence on the absence of trans situational generality of individual differences (i.e., across tests, situations, or common trait indicators) that requires that we reject a "dispositional" view of the person. Such a rejection would not only be premature, it would leave us with no way to make sense of those individual differences in comparable contexts that do occur. What is required is that we reject the view that dispositions are widely generalized. The main problem is that dispositions may have to be defined too narrowly with reference to context, test situation, etc. (e.g., the disposition to get angry when a big car cuts in front of one in line at a gas station; the disposition to give whole card responses, W, on the Rorschach test, etc.), so narrowly defined that one might well ask: Of what theoretical worth are such dispositions? In reply to LeVine, I would also note that the evidence that dispositions are narrowly context-dependent does not come only from tests and con-
trived laboratory observations. Some of the most convincing and damaging evidence is "ecological" in LeVine's sense (see, e.g., Newcomb 1929; Shweder 1973; Whiting and Whiting 1975; and Tables 1, 2, and 3 above). Finally, I would be encouraged if some small set of underlying transformational principles (e.g., inversion, displacement, projection), when consistently applied, actually permitted us to theoretically equate apparently diverse individual differences. The rub is that the adult who hates his or her parent does not typically have a predictable affectual relationship with a boss (by inversion or extension).

Another type of reaction to the context-specificity of individual differences is to explicitly adopt an idiographic approach to personality assessment. Thus, Bem in a paper entitled "On Predicting Some of the People Some of the Time" has persuasively argued that one can "predict certain behaviors across certain situations for certain people but not beyond that" (Bem 1974:513). (Also see Bem 1972a).

It is tempting to assume that "consistency" itself is a normally distributed variable (see, e.g., Newcomb 1929). Given any trait (e.g., extroverted, dependent, egotistical) most people will mix together behavior items that should not go together from the point of view of the trait category; at the tails of the distribution a few people will display noticeable consistency. It may also be the case that for any person there is some feature of this behavior that will tend to be consistent, although there will be no way of knowing what feature of behavior this will be for any person without having already observed the consistency itself. The main difficulty with Bem's approach is the one discussed above in connection with the "clinician's response." One can predict too few of the people too much of the time.

Cultural Integration

At the cultural level of analysis, Postulate 1 is more difficult to assess. There is very little systematic evidence on the extent of thematic generalization across domains such as the domestic, the economic, the political, the religious, etc. It is certainly the impression of some ethnographers (e.g., Geertz 1973:406-407) that within any culture for every theme there is its "subdued opposite." And it is certainly the impression of this writer that the portrait one gets of a
culture is intimately related to the methods one uses to study the culture (in this regard compare Benedict 1946 with Triandis et al. 1968 for two very different views of the extent to which Japanese and American cultures are alike). Once in a while there are even intimations in the national character literature that the way cultures differ from one another cannot be generalized from one domain to another (see, e.g., Inkeles, Hansmann, and Beier 1958:11, comparing Russia and America). Nonetheless, impressions and intimations do not a science make. At the moment the degree of thematic generalization at the cultural level cannot be judged with confidence and one can only look forward to more systematic research on cultural integration. There are, however, two studies worth mentioning.

Cancian (1975:114-134) sets out to discover the “norms” of Zinacantecos in Chiapas, Mexico. She introduces the notion of a “norm hierarchy,” i.e., the relative importance of rules concerning religious versus economic versus political versus kinship behavior. Cancian tries to infer a Zinacantecos’s “norm hierarchy” using two different methods. Zinacantecos have well developed ideas about the extent to which actions such as sending one’s children to school, renting farmland in the distant yet fertile lowlands, or using a Western doctor, are consistent with their religious, economic, political, or kinship norms. For example, the use of a Western doctor (in addition to a shaman) is thought to be inconsistent with Zinacanteco religious norms. Cancian discovers that the “norm hierarchy” one would be led to infer from the way people make decisions about whether to use a Western doctor, send children to school, etc., is entirely different from the “norm hierarchy,” which one would discover by asking people which norms are important to them. How one studies a people’s norms seems to be decisive for what one finds. Method variance may well be decisive for an ethnographer’s construction of cultural reality.

A second relevant study is Costanzo’s (1974) reanalysis of some of the data in Whiting and Child’s classic volume Child Training and Personality (1953). Child Training and Personality presents evidence on thematic generalization across two cultural domains: socialization customs, on the one hand; and beliefs about illness, on the other. Cultural differences in socialization customs are shown to consistently relate to cultural differences in beliefs about the causes
of illness. Whiting and Child’s most dramatic finding is the significant cross-cultural correlation between customs promoting oral socialization anxiety in childhood (e.g., early and severe weaning from the breast) and beliefs that the causes of illness are “oral” (e.g., “it must have been something I ate”). Whiting and Child also discover a statistically significant relationship between anxiety-producing socialization customs and beliefs about the causes of illness in the area of aggression. Three other socialization and illness explanation systems (the anal, the sexual, and the dependent) tend to respectively converge, but fall short of statistical significance.

This pattern of findings is complicated in interesting ways by Costanzo’s multi-trait/multi-method reanalysis of the Whiting and Child data (see Campbell and Fiske 1959 for a discussion of the multi-trait/multi-method form of analysis). Costanzo’s findings are reproduced in Table 4. The table shows the degree of correlation (Yule’s Q) both within and between all possible pairs of the five socialization systems and the five parallel illness explanation systems.

For example, Costanzo discovers that while oral socialization anxiety is significantly related to oral explanations of illness (.49), it is more highly associated (too highly associated) with anal and sexual explanations of illness (.60 and .67 respectively). Moreover, oral socialization anxiety predicts anal and sexual illness explanations (.60 and .67 respectively) better than these illness explanation systems are predicted by anal and sexual socialization customs (.45 and .33 respectively).

The notion of cultural integration receives only equivocal support from the evidence in Table 4. The results are too mixed although they are more encouraging than the results of other multi-trait/multi-method analyses I have seen. On the one hand every socialization system is positively related to its parallel illness explanation system (.49, .45, .33, .25, .47). On the other hand, there are just too many instances in which nonparallel systems are more highly correlated. For example, dependent socialization anxiety is positively correlated to dependent illness explanations (.25) but it is more highly associated with every one of the four other illness explanation systems (.37, .37, .31, .30, respectively). Also, on the average, the intercorrelations among the five different socialization systems are as highly correlated with each other as any of them is correlated with its equivalent illness explanation system. For exam-
TABLE 4
MULTITRAIT-MULTIMETHOD ANALYSIS
OF FIVE SOCIALIZATION ANXIETY SYSTEMS
AND FIVE ILLNESS EXPLANATION SYSTEMS*

<table>
<thead>
<tr>
<th>Socialization Anxiety</th>
<th>Illness Explanation</th>
</tr>
</thead>
<tbody>
<tr>
<td>Oral</td>
<td>Anal</td>
</tr>
<tr>
<td>Oral</td>
<td>—</td>
</tr>
<tr>
<td>Anal</td>
<td>.45</td>
</tr>
<tr>
<td>(49)</td>
<td>(54)</td>
</tr>
<tr>
<td>Sex</td>
<td>.45</td>
</tr>
<tr>
<td>(54)</td>
<td></td>
</tr>
<tr>
<td>Dep</td>
<td>.75</td>
</tr>
<tr>
<td>s (55)</td>
<td>s (55)</td>
</tr>
<tr>
<td>Agg</td>
<td>.27</td>
</tr>
<tr>
<td>(58)</td>
<td>(47)</td>
</tr>
</tbody>
</table>

| Illness explanation   | Oral    | Anal | Sex    | Dep    | Agg    | Oral | Anal | Sex | Dep | Agg |
| Oral                  | .49     | .12  | .40    | .57    | .47    | —    | —    | —   | —   | —   |
| s (65)                | (49)    |      | (61)   | (57)   | s (64) |       |      |      |      |      |
| Anal                  | .60     | .45  | .45    | .57    | .58    | —    | —    | —   | —   | —   |
| s (65)                | (49)    |      | (61)   | (57)   | s (75) |       |      |      |      |      |
| Sex                   | .67     | .10  | .55    | .51    | .40    | .52  | —    | —   | —   | —   |
| s (65)                | (49)    |      | (61)   | (57)   | (64)   |        |      |      |      |      |
| Dep                   | .09     | .04  | .51    | .25    | .06    | .09  | .08  | —   | —   | —   |
| s (65)                | (49)    |      | (61)   | (57)   | (64)   | (75)  | (75) |      |      |      |
| Agg                   | .27     | .02  | .01    | .30    | .47    | .08  | .16  | .27 | .15 | —   |
| (65)                  | (49)    |      | (61)   | (57)   | (64)   | (75)  | (75) | (75) |      |      |

*From Costanzo (1974). Reanalysis of Whiting and Child's (1953) intermediate and high confidence data. Coefficient of correlation = Yule's Q (for dichotomous ordinal data). s = significant at the .05 level (two-tailed). Numbers in parentheses are cell base frequencies.
ple, the "oral socialization anxiety"-"dependent socialization anxiety" coefficient (.75) is greater than either the "oral socialization anxiety"-"oral illness explanation" coefficient (.49) or the "dependent socialization anxiety"-"dependent illness explanation" coefficient (.25). The case for cultural integration in Whiting and Child's data is probably debatable. Prothro (1960:152), for example, in a factor analytic study of Whiting and Child's variables concludes that cross-cultural child rearing norms "cannot be described as permissive or nonpermissive. Rather they must be described as more or less permissive on orality-sexuality, on anality-independence, and on aggression. It is not even accurate to refer to the 'permissiveness for dependency' in a given culture, for dependence is not unifactorial." Nonetheless, Prothro does discover some clustering of variables around an orality-sexuality factor and anality-independence factor. Inspecting Costanzo's results (Table 4), some may wish to argue that when the people of a culture think a lot about the human body they are more scrupulously concerned with it when they raise their children. This interpretation has some plausibility but should probably be viewed with caution. As Prothro (1960:153) remarked with regard to a possible "hypochondria" factor in his study, it "might represent ethnographer's generalized concern and lack of concern with illness rather than the culture's concern or lack of it." The question of cultural integration will be examined further in my discussion of the motivational integration of culture (see Part II of this essay).

POSTULATE 2: THE SEARCH FOR CHILDHOOD ORIGINS, OR THE PAST IN THE PRESENT

... habits established early in the life of the individual influence all subsequent learning, and therefore the experiences of early childhood are of predominant importance (Gorer 1943).

In 1971, Barry and Paxson published information on what it's like to be an infant or young child in each of 186 societies (also see Barry et al. 1977). Barry and Paxson's numerical codes concerned 34 parameters of childhood experience including, for example, the night time sleeping proximity of mother, father, and infant, the identity of principle caretakers, the amount of body contact between infants and their caretakers, the type of infant carrying device
and carrying position, the age of weaning and "elimination control," the age at which motor skills develop, and so forth. Barry and Paxson introduce their codes with the following justification: "Infancy may be of special interest to some analysts of culture and personality because of the widely accepted belief that experiences early in life have important influences on the development of adult character" (1971:466).

Barry and Paxson are, of course, correct. Most studies in culture and personality ultimately try to explain adult personality characteristics by reference to the causal influence of specific child care practices. In fact, many of these studies (e.g., Ayres 1967; Erikson 1950; Shirley and Romney 1962; Whiting and Child 1953; Whiting 1959, 1964, 1971) focus on exactly the kinds of childhood variables that Barry and Paxson code.

To what extent does the past influence the present? Postulate 2 is widely endorsed, but how well does it stand up in the face of evidence on the longitudinal stability of individual differences in conduct? The question is difficult to answer, primarily because of the lack of good observational evidence on behavior, in the developmental literature. What little evidence there is, however, is not encouraging.

For example, Caudill and Schooler (1973) report dramatic differences between Japanese caretakers and their children, and American caretakers and their children, for behavioral events having to do with activity level, affect display, and dependency. Like most culture and personality theorists, Caudill and Schooler imagine there to be early and significant happenings in the experiential world of the child (in this case the behavior of caretakers) that produce distinctive underlying qualities of character. These qualities of character are supposed to account for the anticipated longitudinal stability of individual behavioral differences.

Caudill and Schooler perform a very sensible test of Postulate 2. They first try to predict within each culture a child's behavior at age six years from information about his caretaker's behavior in infancy. They find fewer significant correlations than would be expected by chance. Then they relax their test and try to predict the child's behavior at age two and one-half years from the caretaker's behavior in infancy. The test fails dreadfully. Finally, they try to predict the child's behavior at age six years from his own behavior at age two and one-half years. Again, nothing.
Dismayed by their results, Caudill and Schooler note that "although even with the most valid measures one would not necessarily expect complete consistency in the individual's behavior over time, the level of individual inconsistency in the present data is unbelievable" (1973:337). Caudill and Schooler do not reexamine Postulate 2, but they are puzzled by their failure to validate their intracultural predictions. They offer two speculations: (1) Perhaps the data are unreliable. (2) Perhaps there are threshold effects that render measures of monotonicity inappropriate. Caudill and Schooler may be right, but this failure to find confirmation for Postulate 2 should probably not be dismissed, especially since it is not unique for studies that utilize systematic and detailed behavior observational techniques.

Escalona and Heider (1959), for example, try to predict the behavioral characteristics of 31 preschool children (ages 32 months to 66 months) from observations of their "reaction tendencies" in infancy. In one aspect of this study (1959:132-141), the longitudinal stability of individual differences is assessed by applying the same set of 134 rating scales (e.g., "impulse control," "speed or tempo," etc.) to summary descriptions of each child's behavior at the two age periods. The authors then intercorrelate the two sets of ratings separately for each child. Across the 31 children, the average correlation between the two time lagged descriptions of their behavior is .24 (uncorrected for attenuation). The range is from -.17 to .51. Perhaps even more startling is the finding that random comparisons of children across the two sets of behavioral ratings (e.g., Child 1 in infancy compared to Child 2 at preschool age) are correlated on the average at .18. The standard deviation is .30. As Escalona and Heider point out, "this means that the five percent level of the distribution is .65. None of the predictions can be said to be better than chance by this criterion" (1959:138). They reject the criterion. For a further discussion of the problematical status of Postulate 2, see Chess, Thomas, and Birch (1959). Chess, Thomas, and Birch examine the consequences of early child care practices in the area of feeding, discipline, toilet training, sleeping, and instructional modes. In general they discover that children show "differing responses with parents whose approaches have been similar" and "similar responses with parents whose approaches have differed" (1959:798). They conclude that "the available published data do not confirm the hypothesis that the pattern of child care practiced
by the parent in the child's early life has a clear-cut, consistent effect on the personality of the older child or adult" (1959:793). (Also see Kagan and Klein [1973] and Kagan [1976].)

("Rethinking Culture and Personality Theory, Part II: A Critical Examination of Two More Classical Postulates" will appear in the next issue of Ethnos. The essay examines the problem of cross-cultural comparability and the limits of three explanatory principles: rational choice, natural selection, and the law of effect.)

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