Likeness and Likelihood in Everyday Thought: Magical Thinking in Judgments About Personality [and Comments and Reply]

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Likeness and Likelihood in Everyday Thought: Magical Thinking in Judgments about Personality[1]

by Richard A. Shweder

This paper is dedicated to the little boy who tried to prove that spiders hear with their legs. After cutting off a spider's legs, he yelled, "Jump!" The spider did not jump. So the little boy said, "You see, I was right. Spiders hear with their legs."

IN WHAT TERMS SHOULD WE UNDERSTAND the understandings of other peoples and compare those understandings with our own? Reflection on this central issue in the anthropology of thought raises the perplexing secondary question, What are we to make of another culture's apparently false knowledge? For example, how is the student of the Azande to comprehend their attempts

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to cure epilepsy by eating the burnt skull of a red bush monkey or their therapeutic application of fowl's excrement in cases of ringworm (see Evans-Pritchard 1937, Tambiah 1973)? A line of research conducted by Newcomb (1929), D'Andrade (1965, 1973, 1974), Chapman and Chapman (1967, 1969), Tversky and Kahneman (1973, 1974), and Shweder (1972b, 1975a, 1976a, n.d. a, b) suggests one possible answer to this question: that the distinction between likeness and co-occurrence likelihood (contingency) is collapsed in everyday thought.

The absence of a distinction between resemblance and contingency is familiar to anthropological fieldworkers. It is characteristic of magical thought. Evans-Pritchard (1937:487), for example, notes:

Generally the logic of therapeutic treatment consists in the selection of the most prominent external symptoms, the naming of the disease after some object in nature it resembles [e.g., the physical movements of bush monkeys resemble epileptic seizures; ringworm resembles fowl excrement] and the utilization of the object as the principal ingredient in the drug administered to cure the disease. The circle may even be completed by belief that the external symptoms not only yield to treatment by the object which resembles them but are caused by it as well.

The existence of magical thought systems has posed a difficult interpretive problem for anthropological theory. Some scholars (e.g., Lévi-Strauss 1966) have viewed magic as a relatively effective set of procedures for acquiring knowledge and exercising control over one's environment, comparable to scientific canons of enquiry. Others (e.g., Malinowski 1954) have viewed it as analogous to wish-fulfillment, an irrational symbolic attempt to influence uncontrollable events. Still others (e.g., Tambiah 1973) have viewed it as a form of rhetoric, designed to arouse sentiments rather than make truth claims about what goes with what in experience. I shall present an alternative perspective, arguing that magical thinking is an expression of a universal disinclination of normal adults to draw correlational lessons from their experience, coupled with a universal inclination to seek symbolic and meaningful connections (likenesses) among objects and events. Magical thinking is no less characteristic of our own mundane intellectual activities than it is of Zande curing practices.

Magical thinking is an expression of a cognitive-processing limitation of the human mind. It is that form of noncorrelational reasoning utilized when objects and events conceptually (or semantically) affiliate or exclude one another in our minds.

Typically, it involves a confusion of propositions about the world with propositions about language (D'Andrade 1965).[2] Correlation and contingency are relatively complex concepts that are not spontaneously available to human thought (see, e.g., Smedslund 1963, Jenkins and Ward 1965, Ward and Jenkins 1965, Shweder 1976a). Correlation-relevant information is difficult for the human mind to organize into a format that lends itself to correlational manipulation, and correlational reasoning is typically avoided when most adults in all cultures estimate what goes with what in their experience. (Using Piaget's terminology, we might hypothesize that most adults in all cultures are not formal operational thinkers. Correlation and contingency are formal operational concepts; see, e.g., Inhelder and Piaget [1958].) Instead, most normal adults do as the Azande do. They rely on likeness to estimate co-occurrence likelihood (see, e.g., D'Andrade 1965, 1973, 1974; Chapman 1967; Chapman and Chapman 1967, 1969; Tversky and Kahneman 1973, 1974; Shweder 1972b, 1975a, 1976a, n.d. a, b).

I shall examine this cognitive-processing perspective on magical thinking by focusing upon everyday personality judgments. I shall show that the distinction between conceptual affiliation (resemblance, likeness) and co-occurrence likelihood (contingency, correlation) is ignored in everyday judgments about individual differences in conduct. When normal adults make personality assessments, items of conduct that *resemble* one another (e.g., smiles easily, introduces himself to strangers, likes parties) are typically said to covary over people *despite experience and available information to the contrary* (see, e.g., D'Andrade 1974, Shweder 1975a, n.d. a). Magical thinking seems to be no more a feature of Zande beliefs that ringworm and fowl excrement go together than of our own beliefs that self-esteem and leadership do. It is a universal aspect of everyday judgments about what goes with what in experience. My study suggests that anthropologists interested in thought may have mistaken a difference in the content of thought for a difference in mode of thought. Magical thinking does not distinguish one culture from another. Resemblance, not co-occurrence likelihood, is a fundamental conceptual tool of the everyday ("savage?") mind. Most of us have a "savage" mentality much of the time.

INTUITIVE AND NONINTUITIVE CONCEPTS

A useful distinction in the study of human thought is between intuitive and nonintuitive concepts. Concepts can be arranged along a continuum having to do with the relative ease with which they can be attained. More intuitive (so-called spontaneous) concepts are acquired even under highly degraded learning conditions, e.g., without explicit instruction, with a minimal amount of practice, and regardless of a desire to learn or the nature of reinforcements (see Seligman and Hager 1972). Moreover, intuitive concepts seem to be available for use without conscious effort or reflection. In contrast, nonintuitive concepts require special learning conditions for their acquisition, e.g., massive instructional input, an orderly and explicit organization of learning trials, high motivation, etc. These learning conditions are more difficult to arrange. Consequently, nonintuitive concepts are less widely distributed (both within and across human populations) than intuitive concepts; they seem to be associated with deliberate, self-reflective intellectual activities.

Some concepts are so intuitive that they will be attained even under the most degraded of learning conditions and regardless of variations in physical, social, or cultural environment. De-

velopmental psychologists such as Price-Williams (1969) have alerted us to the relative ease with which children in all cultures come to have such concepts as object constancy and reversibility (see Piaget 1953, 1967). It seems safe to conclude that normal adults in all cultures act on the implicit understanding that the external world has an independent existence. They have the concept of object constancy; they make plans, negotiate agreements, predict events, etc. It also seems safe to conclude that normal adults in all cultures act on the implicit understanding that for any event there is an intellectual operation that compensates for it, cancels it, or undoes it. They have the concept of reversibility, e.g., breaches of social expectation are followed by excuses, verbal stratagems designed to reverse and undo the distress caused by misconduct. Normal adults in all cultures easily master the intellectual manipulations characteristic of concrete operational thought (as discussed by Piaget, e.g., 1967).[3]

The anthropological literature on the kinds of relationships expressed in folk definitions (e.g., Casagrande and Hale 1967, Tyler 1969) provides us with other examples of relatively intuitive concepts. It appears that relationship concepts such as class inclusion, function, antonymy, synonymy, exemplification, part-whole, temporality, etc., are relatively easy to acquire and are universally mastered with or without reinforcement, deliberate training, etc., and regardless of variations in culture (also see Shweder and LeVine 1975). Controversy persists over whether concepts such as these are merely elicited by experience (the innateness hypothesis), constructed out of experience (the Piagetian hypothesis), or received from experience (the learning-theoretic hypothesis) (see, e.g., Chomsky 1968, Piaget 1970, Putnam 1975b). Nonetheless, such concepts are easy to acquire; the fact that they are universally attained is some evidence of this relative acquisitional ease.

On the other hand, one can point to many nonintuitive concepts, concepts that are relatively difficult to learn and absent from the thinking of most normal adults. Many statistical concepts having to do with change and probability are nonintuitive. For example, if I were to wager that, in a randomly selected group of twenty-five persons, two would have been born on the same day of the year, the chances would be slightly in my favor (Kemeny, Snell, and Thompson 1966:134–41), yet very few normal adults would refuse my request for heavy odds. For a randomly composed group of forty persons, it would be I who should offer heavy odds (in fact, eight to one) to anyone foolish enough to bet that no two would have been born on the same day of the year.[4] The intuitive mind boggles!

Correlation is a nonintuitive concept (see, e.g., Smedslund 1963, Jenkins and Ward 1965, Ward and Jenkins 1965). It is a second-order relational concept, expressing the relationship between two relationships, and it is generally absent from the thinking of most normal adults. One way to describe the concept of correlation is as a comparison between two conditional probabilities (see Ward and Jenkins 1965). Table 1 is a two-by-two contingency table portraying hypothetical information about the correlation between an abstract symptom (S) and an abstract disease (D). One can estimate this correlation by judg-

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[2] D'Andrade does not mention magical thinking, but his pioneering paper on the biasing influence of semantic structures on the personality-assessment process presents a highly original analysis of the tendency of the human mind to substitute statements about "what is like what" for statements about "what goes with what." Also see D'Andrade (1973, 1974).

[3] Apparently there are a few cross-cultural psychologists who do not accept this conclusion (see, e.g., Dasen's 1972 review of the literature). The denial of the conclusion seems ethnographically naive to me (also see Mehler and Bever 1967.)

[4] A second example of a nonintuitive problem in estimation is as follows: A piece of paper is folded in half. It is folded in half again, and again. . . . After 100 folds, how thick will it be? Most readers will estimate a thickness of a few inches, perhaps a foot, and will be startled to discover (upon reflection) that the thickness of the paper will be greater than the distance between the earth and the moon.

The statistician's notion of chance presupposes the ability to think hypothetically. As Inhelder and Piaget (1958:224) point out, "the probability that an event will occur is nothing more than the relationship between the possible instances of an event and those which actually occur." The concept of chance deals "with what is possible and not only with what is real."

CORRELATION-RELEVANT FREQUENCY INFORMATION ON THE RELATIONSHIP BETWEEN A HYPOTHETICAL SYMPTOM AND A HYPOTHETICAL DISEASE IN 100 SUPPOSED PATIENTS

		DISEASE		
		Present (D₁)	Absent (D₂)	Total
SYMPTOM	Present (S₁)	37 (*a*)	33 (*b*)	70
	Absent (S₂)	17 (*c*)	13 (*d*)	30
	Total	54	46	

ing the extent to which the probability of D₁ (having the disease) given S₁ (having the symptom) *differs from* the probability of D₁ (having the disease) given S₂ (not having the symptom). The judgment reduces to an estimate of $a/(a + b) - c/(c + d)$ or, alternatively, $(ad - bc)/(a + b)(c + d)$. In table 1 the correlation between the symptom and the disease is close to zero.

Smedslund (1963) presented the information in table 1 to a group of Swedish nurses. He gave each nurse a pack of 100 cards supposedly representing excerpts from the files of 100 patients. Each card indicated the presence or absence of the symptom and the presence or absence of the disease in ratios corresponding to the information in the table. The nurses were asked to find out whether there was a relationship (connection) between the symptoms and the disease. They were told that they could study and organize the cards in any way they wished and that they could take notes. Of the 85% of the nurses who claimed that there was a relationship between the symptom and the disease, most justified their claim by pointing out that the number of cards in which the symptom and the disease were both present (37) was "the largest" or "large." Almost all the nurses found the task difficult. Smedslund concluded that "normal adults with no training in statistics" may lack a concept of correlation.

Smedslund's nurses tended to focus upon joint occurrences of the symptom and the disease. A somewhat different misuse of correlation-relevant information is reported by Ward and Jenkins (1965), who presented college students with contingency information concerning the relationship between cloud seeding and subsequent rainfall. One set of students focused upon confirming cases (the number of seed-rain days plus the number of no-seed–no-rain days) to estimate the success of cloud-seeding programs. They tended to ignore information about the number of no-seed–rain days and seed–no-rain days. Another set of students failed to compare conditional probabilities. They assessed the effectiveness of cloud seeding by the probability of rain for days when clouds were seeded without comparing it with the probability of rain for days when clouds were not seeded.

The claim that correlation is a nonintuitive concept requires some clarification. Specifically, one must distinguish between a *disinclination* to think correlationally and an *incapacity* to think correlationally. It is possible to get American college students to think correlationally, but the informational conditions one has to contrive to do so are quite unrepresentative of everyday life. For example, Ward and Jenkins (1965) discovered that when information was presented serially, namely, "on a trial by trial basis, as it usually occurs in the real world," only 17% of their students displayed correlational thinking in their estimates of the effectiveness of cloud seeding. Moreover, explicit summary information in the form of two-by-two contingency tables had "little influence on judgments when it was presented *after* they [had] followed a trial by trial presentation." The only informational condition in which a majority of students displayed correlational thinking was that in which they received summary information in the form of a two-by-two contingency table *without* prior trial-by-trial experience with the data. Most normal adults have the capacity to think correlationally, but

they do not apply the concept in their everyday judgments. If all concepts were intuitive, educational environments and explicit training would not be necessary.

Is correlational reasoning applied when normal adults estimate the extent to which meaningful events in their own experience go together? If not, what intuitive concepts take its place? As soon as events can be meaningfully linked to one another, magical thinking makes its appearance; normal adults substitute the intuitive concept of resemblance for the nonintuitive concept of correlation. Magical thinking is not *equivalent to* noncorrelational thinking; rather, it is *one type of* noncorrelational thinking. (Other types include the cognitive strategies just discussed, e.g., the focus on confirming cases.) In the light of the general disinclination of normal adults to manipulate information in a correlational way, magical thinking seems to occur when adults assess the degree of *empirical* relationship among objects or events that also *conceptually* affiliate or exclude one another in their minds. Thus what D'Andrade (1965) has called a "hazard of science" is an appropriate definition for magical thinking. Magical thinking can be defined as a confusion of "propositions of the world with propositions about language." Recent research by personality psychologists suggests that such thinking may characterize everyday judgments about personality. It has become increasingly apparent that the behavioral traits that play such an important role in our everyday descriptions of personality are not "out there" waiting to be discovered, but rather are the creations of the magical mind.

IN SEARCH OF THE MISSING TRAIT

For years, personality psychologists have labored to "discover" everyday personality traits (e.g., dependency, aggressiveness, character strength) in behavior (see, e.g., Newcomb 1929, Allport 1937, Cattell 1957, Norman 1963). Encouraged by the assumption that everyday trait terms encode information about "the relative frequencies of joint occurrences" of various "behavioral dispositions in other persons" (Passini and Norman 1966), they have searched in behavior for corresponding "unities" or "consistencies" in the way persons differ from one another over comparable situations. In pursuit of "dependency," they have hoped to find that the *same* child prods his mother for attention *and* "clings to her apron strings." In pursuit of "introversion," they have hoped to find that the *same* adult is shy with strangers *and* prefers to work alone. In pursuit of "character strength," they have hoped to find that self-reliance and responsibility go together. In general, they have failed (see, e.g., Raush, Dittmann, and Taylor 1959; Sears 1963; Endler and Hunt 1968; Mischel 1968, 1973; Moos 1969; Fiske 1974). What they have discovered is that (1) interobserver reliability is difficult to attain (see, e.g., Bourne 1976); for example, two observers of the *same* person agree very little in their answers to the question "What's he like?" and are not more likely to agree with each other than observers of *different* persons; (2) method variance is greater than person variance (see, e.g., Campbell and Fiske 1959, Burton 1970); distinguishable qualities (e.g., being disagreeable and promoting solidarity) measured using two methods (e.g., a personality inventory and a projective test) typically show higher correlations within each set of data than does the same quality across the two methods; and (3) hypothesized traits (e.g., friendliness, dominance) typically account for no more than 9–15% of the individual differences over naturally occurring situations (see Mischel 1968, 1973 for an overview; also see Shweder 1972*b*, 1973). The person who avoids strangers may or may not like parties; the person who is relatively assertive with his employer may or may not be relatively submissive with some other "authority figure" (e.g.,

his father); the assertive child at the breakfast table is not the assertive child in the playroom; etc. Different situations affect different persons differentially.

Everyday personality trait concepts have very meager predictive utility. Personality psychologists have discovered that they cannot consistently describe behavior in everyday trait-concept language. The qualifications required to describe accurately individual differences in behavior are complex and numerous and do not lend themselves to trait-theoretic reduction. The notion (and the hope) that there is a limited set of underlying qualities (traits, factors, dimensions, etc.) in terms of which individuals can be said to differ regularly seems to lack behavioral warrant. Individual differences in conduct are considerably less consistent, less unified, less integrated, less organized than previously supposed.

It is very difficult for us to accept the idea that everyday personality traits are not there to be discovered in behavior, just as it is difficult for the Azande to accept the notion that fowl excrement doesn't cure ringworm. What our resistance reveals is the pervasiveness of magical thinking and its power to influence our perceptions of reality. Everyday personality traits (e.g., dependent, aggressive, friendly) are not correlational patterns to be found in conduct; they are clusters of meaning evoked by conduct. Personality traits are symbols or interpretive categories that link together items of behavior and are linked to each other by conceptual relationships that have little to do with frequency and co-occurrence likelihoods. These conceptual affiliations (e.g., connections based on resemblance) dominate our understandings of what goes with what in the world of experience.

FROM BEHAVIOR TO MEANING

Personality traits are part of a language of interpersonal comprehension designed to help us "read" one another's conduct. They are the terms in which a person qua person acquires meaning. How does behavior give rise to meanings? To what kinds of meanings does it give rise? To answer these questions, we must consider the sense-making process in general.

Behind every attribution of a personality trait is a hidden inference, usually an induction. This may be seen in the following hypothetical conversation between two mothers:

A: "Your son is so self-confident." (inductive inference)
B: "Why do you say that?"
A: "I saw him organizing games in the playground." (minor inductive premise)
B: "Yes. I noticed that, but. . . ."
A: "Well, children who organize games are usually self-confident." (major inductive premise)

There are two points to be made about this piece of everyday talk about personality. The first point is that is inductive (not deductive) in form. While it is contradictory to assert "an oak, but not a tree," it is not contradictory to assert "organizes games, but not self-confident." The latter inference is not *entailed* by its premises (see, e.g., Black 1970:150–51); alternative inferences are possible without logical error. The second point is that this example is somewhat artificial; it is unduly labored, and it obscures our interpretive options.

The inductions of everyday life are rarely explicit. In fact, a notable feature of everyday talk about individual differences in conduct is its condensed and abbreviated form; everyday exchanges of words leave a lot unsaid. One or more of the premises (e.g., your son organizes games in the playground; children who organize games are usually self-confident) that support the inference (e.g., your son is self-confident) go unverbalized. Consequently, normal adults often talk as if they had observed "self-confidence" itself, not merely organizational initiative. Sometimes they even fool themselves. They should not be permitted to fool us. Self-confidence cannot be observed; it can only be inferred.

Abbreviations and condensations can occur in everyday talk primarily because normal adults can count on context and a shared body of knowledge, beliefs, and presuppositions in the listener (what Ziff [1972] calls a "special program" and anthropologists call "culture") to contribute whatever information is required to make their utterances comprehensible (see Bernstein [1975] on "restricted codes" for a discussion of the inverse relationship between explicitness and a common cultural background). In fact, it can be argued that the meaningfulness of everyday talk depends upon the successful negotiation of the context and culture linking speaker and listener. This becomes especially apparent when these two prerequisites for intelligibility (context and culture) are absent.

Bransford and McCarrell (1974:209) provide us with two instructive illustrations of the importance of context:

The trip was not delayed because the bottle shattered.
The haystack was important because the cloth ripped.

These utterances are bleached of context. They are difficult to comprehend until one is able to bring to mind the right kind of setting, occasion, or activity. Accompanied by a smile of recognition, the meaning of the sentences then becomes obvious: "Christening of a ship," of course! "Parachuting," aha!

Ziff (1972) sensitizes us to the covert contribution made by a common cultural background to mutual intelligibility. He alerts us (p. 130) to how much of what we intend to communicate, and succeed in communicating, is unstated and *can never be fully stated* (also see Cavell 1969:12; Garfinkle 1967: chap. 1; Mehan and Wood 1975:90–95 on the notion of "indexicality" and the inherent incompleteness of symbolic forms):

An officer to a private: "Shut the door!" "Sir," says the private and does nothing else. "Shut the door!" is not the same as "Shut the door now!" "Shut the door now!" said a wily officer whereupon a wilier private responded with "Sir" at once shutting and immediately reopening the door. "Shut the door now!" is not the same as "Shut the door now and leave it shut!" "Open the door!" said an officer but a private refused on the grounds that the preceding year the officer's superior had said to him, "Shut the door now and leave it shut!" and that order had not been rescinded.

The mother who says "Your son is so self-confident" can, under ideal conditions, comfortably protected by a shared context and a common culture, say this without fear of being misunderstood. She need not say the minor inductive premise, "Your son organizes games in the playground"; both mothers can see that. (How might she *say* what she saw? Perhaps: "Notice that, at random intervals during the last 25 minutes as recorded on my Timex watch, we have observed your son initiate eight games with three different groups of children. The age composition of those groups was as follows: etc. The sex composition of those groups was as follows: etc. The children in those groups were engaged in the following activities: etc. They were positioned as follows: etc." Fortunately for everyday discourse, only social scientists have to strive, *in vain*, to be *fully* explicit in their observations.) Moreover, even if the two mothers are sitting "out of context" (e.g., alone in a bar), and even if one of them has managed some form of the minor inductive premise ("We saw your son organize a lot of games yesterday"), the comprehensibility of the inference "Your son is so self-confident" does not require a public announcement that "in general, children who do that sort of thing are self-confident." "Your son is so self-confident" is the vocalized tip of an *inferential* iceberg. It is often all one has to say to make sense, but it is a sense "contributed to" behavior, not "discovered" in it.

Objects and events do not dictate their *unique* interpretation (see, e.g., Schachter and Singer 1962; Geertz 1973:chap. 1; Goffman 1974:chap. 12); as Weberian sociologists like to remind us, behavior is not equivalent to action. No amount of investigation of the womb of a pregnant woman will uncover a "right

to life." No amount of chemical analysis of the piece of paper we call a dollar bill will reveal the concept "value." No amount of physical measurement of the movements of children will distinguish an accidental "bump" from an intentional "provocation." No amount of detailed inspection of playground activity will uncover "self-confidence." There is a potential for ambiguity and alternative "readings" in all behavioral events. This is not to say that "anything goes," but only that "many things go." Mother to mother: "Your son is so self-confident." Reply: "Quite the contrary, he is shy and passive and is overcompensating for his deficiencies." Or, "No, you've got it wrong. I *ordered* him to organize the children in the playground." Or, "Ha! I guess he really is a good actor; he is practicing for a part in the school play." Or, "This playground certainly does strange things to him; he is never like that at home."

Within any particular culture or subculture, many potential "readings" of behavioral events never "get into court." Culture limits the number of interpretive options we are permitted seriously to consider. Mother to mother: "Your son is so self-confident." Reply: "Please excuse my son's misbehavior, but his father's ghost dwells in this playground and sometimes takes over his will." Mother to mother: "Oh, how interesting! What country do you come from? You are foreign, aren't you?" Or, perhaps, mother to mother: "Your son is so self-confident." Reply: "Not really. You see, he has been systematically conditioned since he was 18 months old automatically to emit the verbal response 'Let's build a sand castle' at the sight of visual cues similar to the pails of those children over there." Mother to mother: "Oh, how interesting! Are you really his mother? Or are you some kind of . . . uh . . . scientist?" Competent spokesmen for personified or mechanistic world views can often "handle" the same range of "facts," *if they dare express them* (see Pepper [1972] on the notion of "root metaphors" and the notion of alternative yet equally adequate "world hypotheses"). Serious talk of spirit possession and suggestions of robot-like programming violate the coherence of our culture's everyday discourse about behavior. Don't try it in an American playground!

Behavioral events are inherently ambiguous in part because all behavioral events simultaneously possess a number of features. They are the overt and physical movements of some actor. They are situationally located. They are temporally arranged with respect to other acts. They may be goal-oriented. They may be rule-governed (see, e.g., Burke 1969). To ask, "Was it Bill (the actor) or the chair (a feature of the situation) that caused him to fall down?" is like asking Donald Hebb's rhetorical question, "What contributes more to the area of a football field, its width or its length?" In the absence of *either* Bill *or* the chair, the specific event would not have occurred; *Bill* would not have tripped over *that* chair. The event is unique; it is a singular interaction effect, involving this particular actor, this particular situation, this particular goal, etc. Nonetheless, in everyday thought, behavioral events are *assimilated* to one kind of interpretive language or another all the time. "He tripped because he is clumsy." "I tripped because something got in my way" (see Jones and Nisbett 1972).

There are many possible interpretive points of view for ascribing meaning to conduct. We often explain why someone behaved as he did (e.g., "striking" another person) by reference to a trait of the actor (e.g., he is aggressive). We can also explain behavior, however, by reference to features of the situation (e.g., his apartment was too crowded), antecedent acts (e.g., he was struck first), goals (e.g., he was trying to establish his authority), rules (e.g., that's what's done in a boxing match), etc. Although the choice of an interpretive point of view is guided by some well-understood psychological principles (see, e.g., Kelley 1967, 1972), the selection of a particular interpretive perspective is never *logically* warranted by behavior itself. For example, one psychological principle might be formulated

as follows: equate propositions about *variations* in behavior with propositions about the *causes* of behavior. In everyday thought, the chair will be perceived as the cause of Bill's fall if everyone who enters the room (including Bill) trips over it. If, however, only Bill trips over it, he will be perceived as causally responsible for what is now interpreted as his own "clumsiness." To make attributions in this way is tantamount to what Campbell (1969) has called an "*a priori* preference for parsimony" and a "presumptive bias in favor of main effects." Bill and the chair are both causally relevant to the event, *and equally so*, no matter how many other people do or do not happen to fall over the same object. The "coincidence of two [or more] separate specific interaction effects" (e.g., ten people trip over the chair) is no logical warrant for the attribution of a general cause (e.g., it's the chair's fault).

Behavioral events do not speak for themselves; they evoke meanings that are not to be found in the behavior itself. The meaning may *seem* to be inextricably part of the event once an interpretation is made, but this phenomenological illusion should not mislead us into thinking that meanings are discovered. The temptation to confuse one's interpretive categories with the events they describe is the basis of magical thinking. As we shall see, one reason it is succumbed to by most normal adults is that resemblance and likeness are intuitive concepts while correlation and contingency are not. It is easy to confuse the conceptual connections among one's descriptive categories with observed connections among the objects and events described.

THE CREATIONS OF THE MAGICAL MIND

A series of cognitive experiments concerning everyday judgments about what goes with what across individual differences in conduct (Shweder 1972*b*, 1975*a*, 1976*a*, n.d. *a*) suggests the universality of magical thinking and its intimate relationship to the absence of the concept of correlation in normal adults.

RESEMBLANCE AND JUDGMENTS OF
CO-OCCURRENCE LIKELIHOOD

Resemblance is not a good predictor of co-occurrence likelihood. Pairs of objects or events that are conceptually linked (red and pink, ringworm and fowl's excrement, drafty rooms and runny noses, anal retentiveness and miserliness, self-esteem and leadership) do not typically co-occur in one's experience. This is not at all surprising. If resemblance could be substituted for co-occurrence likelihood, we would not have to observe our world. Conceptual relationships would automatically reveal empirical connections. Face (or content) validity would guarantee internal and external validity. Language and reality would merge. That would be surprising!

As far as we know, the conditions that affect true co-occurrence likelihoods have little to do with the existence or nonexistence of a conceptual relationship between interpretive categories, and variations in the degree of conceptual likeness among pairs of items are independent of variations in true co-occurrence likelihoods in experience. D'Andrade (1974) compares the structure (what goes with what) of experience with (1) the structure of observer reports about experience *and* (2) the semantic structure of the observers' categories. He shows that the correlational relationships among the behavioral referents of a set of categories used for coding small-group behavior (Bales's Interaction Process Analysis Scheme, e.g., jokes, suggests, disagrees) are relatively unstable and do not correspond to the semantic-similarity relationships among the category labels. He demonstrates that semantic-similarity relationships are decisive for observer estimates of what goes with what in their

experience. His study is the prototype for the analysis that follows.

The lack of fit between likeness and co-occurrence likelihood is fortunate for our research enterprise. It makes it possible for us to distinguish the two concepts. We can ask: Do normal adults collapse the distinction between likeness and co-occurrence likelihood when they report what goes with what in their experience? Which is the more important intellectual tool of the everyday mind, the concept of correlation or the concept of resemblance? To answer these questions, I reanalyzed Newcomb's 1929 study of extraverted and introverted social behavior among boys at a summer camp, focusing on his tables 22 and 23. Since this study consists of daily records and observer ratings on the same 26 items of behavior, it is possible to compare observers' *reports of what goes with what* in their experience with data (systematically collected by the observers themselves) on *what actually goes with what* in their experience.

In Newcomb's study, daily records were kept on each boy ($N = 24$) by a counselor, who noted the presence of an item of behavior as soon after its occurrence as possible. The 26 behaviors included such items as "speaks with confidence of his own abilities," "takes the initiative in organizing games," "spends more than an hour a day alone." An odd/even-day reliability check on behavior percentages produced a mean reliability coefficient of .78 over the 26 items. At the end of the camp session (24 days), six observers, including the counselor, rated the boys on a five-point scale for each of the 26 items. Their ratings were pooled in all analyses (see Shweder [1975a] for a more complete discussion). The data consist of 110 intercorrelations among pairs of the 26 behaviors, calculated separately for the daily records and for the observer ratings. The percentage of days an item appeared in a boy's behavior was used by Newcomb for calculating intercorrelations (what goes with what) in the daily records. The data make it possible to compare variations in the actual co-occurrence likelihoods among pairs of behaviors with variations in observers' reports about what goes with what in their experience.

Since my goal was to relate observer reports of what goes with what in experience to preexisting observer beliefs about resemblance or conceptual likeness, I asked ten University of Chicago students in one of my classes to make paired-comparison judgments of conceptual similarity among pairs of the 26 interpretive categories. A seven-point scale was used for making these judgments. Prior to making their judgments, the students were told that they would be presented with descriptions of the behavior of boys at a summer camp.

The reanalysis of Newcomb's data was guided by the following question: When the actual correlational relationship between a pair of behavioral categories *is unrelated to* observer beliefs about what is like what, what do observers of behavior report? Do they report what actually goes with what in their experience, or do they report what is like what in their preexisting conceptual system? For example, my students believe that "gives loud and spontaneous expressions of delight or disapproval" and "talks more than his share of the time at the table" resemble each other as concepts. In Newcomb's data the actual correlation between the two items in the boys' daily records is only .08, but in the observers' ratings of their experiences with the boys the correlation between the two items is .92. Despite experience to the contrary, in this case observers of behavior seem to rely on the concept of resemblance to estimate what goes with what in their experience. How general is this tendency?

To answer this question, I selected 33 item pairs in which either the degree of conceptual resemblance according to the student judges was *above* the mean for all pairs of items (4.1 on a 7-point scale) and the actual-behavior correlation (in the daily records for that pair) *below* the mean for all pairs (.10) or the converse. The reanalysis consisted of a comparison of the degree to which what goes with what in observers' reports

corresponds either (*a*) to what actually goes with what in their experience or (*b*) to what is like what in their preexisting beliefs. The degree of correspondence of what goes with what in daily records, observer reports, and preexisting beliefs was determined by correlating over variations in the correlations between equivalent pairs of behaviors from each pair of the three sources. The nonparametric statistic Spearman's rank-order correlation coefficient (r_s) was used in the analysis.

The results are as follows (fig. 1): When actual experience is unrelated to preexisting beliefs about what is like what ($r_s = -.36$), observer reports of what goes with what in experience replicate beliefs about resemblance ($r_s = .84$) and are unrelated to the observer's actual experience of what goes with what ($r_s = -.27$). Normal adults are magical thinkers; they confuse propositions about likeness with propositions about co-occurrence likelihood.

Do these results reflect a general disinclination of normal adults to draw correlational lessons from their experience? On the basis of these results alone, one cannot tell. It is possible the results reflect the tendency of human memory to "drift" in the direction of preexisting conceptual schemata (D'Andrade 1974). The next two experiments, however, lend credence to the hypothesis that the failure to report accurately upon correlational relationships in one's experience is an indication of the absence of a concept of correlation in normal adults.

NEGLECT OF CORRELATION-RELEVANT INFORMATION

Correlation-relevant information is available to observers of behavior, but tends not to be utilized or processed in a correlational manner. In the personality domain, normal adults have beliefs about the frequency with which various personality traits occur. Should they care to utilize their beliefs about frequency and organize them properly, they would have at their disposal all the information they need to compare conditional probabilities and arrive at an estimate of the correlational relationship among pairs of traits. For example, a characteristic pattern of frequency beliefs among American college students about the distribution of self-esteem and leadership "out of 100 people in the general population" is shown in table 2. Respondents thought that 20 out of 100 were likely to be leaders, 65 out of 100 were likely have self-esteem, 15 of the 20 leaders were likely to have self-esteem, etc. I will refer to such frequency beliefs as *correlation-relevant frequency information*. Whether these frequency estimates are accurate or not matters little in this context. The crucial question is, Do normal adults make use of their own correlation-relevant frequency information

Preexisting Beliefs about Resemblance

FIG. 1. Comparison (r_s) of what goes with what in actual experience, what goes with what in reports about experience, and what is like what in preexisting observer beliefs about the degree of resemblance between experienced events. N = 33 pairs of behavioral events in which judged-resemblance score for the pair is greater than 4.1 on a 7-point scale and actual-behavior correlation is less than .10, or the converse.

TABLE 2

Shweder: LIKENESS AND LIKELIHOOD IN EVERYDAY THOUGHT

CORRELATION-RELEVANT FREQUENCY INFORMATION IN BELIEFS OF AMERICAN COLLEGE STUDENTS ABOUT THE DISTRIBUTION OF SELF-ESTEEM AND LEADERSHIP OVER 100 PERSONS IN THE GENERAL POPULATION

		SELF-ESTEEM		
		Present (A₁)	Absent (A₂)	Total
LEADERSHIP	Present (B₁) ...	15 (*a*)	5 (*b*)	20
	Absent (B₂)	50 (*c*)	30 (*d*)	80
	Total	65	35	

(whether it is accurate or not)when they claim that two traits are associated in their experience?

In table 2 the conditional probability of being a leader (B_1) given that one has self-esteem (A_1) (15/65 or .23) is not much greater than the conditional probability of being a leader (B_1) given that one lacks self-esteem (A_2) (5/35 or .14). Thus, the contingent relationship between self-esteem and leadership is only .09. Do normal adults take this information into account when they judge the extent to which self-esteem and leadership are related in experience? In the case of self-esteem and leadership, most American college students claim that knowledge of whether or not someone has self-esteem does indeed enable them to predict whether or not that person is a leader. This claim is made despite their own correlation-relevant frequency information to the contrary. How typical is this failure to reason correlationally when formulating propositions about one's ability to infer one trait from another?

That normal adults have difficulty utilizing subjectively available frequency information to draw correlational inferences is illustrated by an experiment involving five trained observers of behavior, all University of Chicago students with professional experience in observing and coding behavioral interactions between mothers and infants. All five informants were women. I asked them for judgments about the contingent relationship within each of 20 pairs of trait terms, many of which appear in the personality psychology literature (see, e.g., Norman 1963, Smith 1967). The pairs differ in the degree to which judges believe that information about the presence or absence of the first trait enables them to draw an inference about the presence or absence of the second trait. For example, judges believe that knowledge of whether or not a person is "compulsive" enables them to predict whether or not he is "adaptable." They also believe that knowledge of whether or not a person is "cold" enables them to predict whether or not he is "friendly" and that knowledge of whether or not a person is "gentle" enables them to predict whether or not he is "good-natured." They do *not* believe that an inference can be drawn from "anxious" to "quiet" or from "cautious" to "talkative."

I gave the judges the following instructions:

Answer all the following questions in reference to a population of people with which you are familiar. We are interested in finding out a number of things about this population but lack direct information. Therefore we have to make some inferences. For example, we want to know *whether or not* a particular person is "friendly." But we are only able to acquire information about whether or not the person is "warm." To what extent will knowledge of whether or not that person is "warm" enable us to predict whether or not he is "friendly"?

Estimates were to be ranked on a scale ranging from 0 ("does not enable me to predict at all") to 100 ("enables me to predict perfectly") in 10-point intervals. After a brief pre-test for task comprehension, the judges were presented with the 20 trait pairs (e.g., compulsive-adaptable, cold-friendly, gentle–good-natured, has self-esteem–a leader, anxious-quiet, cautious-talkative). For each pair they were asked, "To what extent does knowledge of whether or not a person is [*first item of the pair*] enable us to predict whether or not he is [*second item of the pair*]?"

A second task was administered to elicit estimates concerning the marginal values of two-by-two contingency tables for each pair of traits. The judges were asked, "How many people out of 100 are [*each of the mentioned traits*]?" A third task required estimates of single conditional probabilities for each trait pair. The judges were asked, "What percentage of [*first trait item*] people are also [*second trait item*]?" For example, "Think of all the people who are gentle. What percentage of gentle people are also good-natured?" These frequency estimates provide the information needed to construct a two-by-two contingency table (e.g., see table 2) for each pair of traits for each judge. For each table the contingent relationship between the first and the second trait was calculated using the contingency measure discussed earlier. As a reliability check, all five judges were retested three to seven days after the initial test session. Thus for each of 20 pairs of traits, for each of five judges, for each of two test sessions, we have (1) a score (ranging from 0 to 100) reflecting the preexisting belief of the judge about the extent to which information about the first trait warrants an inference to the second trait and (2) a measure of the contingent relationship among the two traits in the judge's own correlation-relevant frequency information.

The data were analyzed to ascertain whether preexisting beliefs about what predicts what are based upon correlational reasoning. How stable are beliefs about the extent to which one trait warrants an inference to another trait? How stable is correlation-relevant frequency information? Do traits that are said to predict one another (e.g., cold-friendly, gentle–good-natured) also have more substantial contingency coefficients (regardless of sign) in the judge's own correlation-relevant frequency information? To answer these questions, I rank-ordered the 20 trait pairs in two different ways—by the degree to which the first trait was said to warrant an inference to the second trait and by the magnitude of the contingent relationship between the two traits in the correlation-relevant frequency information. The rankings were generated separately for each test session, producing four rankings of the 20 trait pairs for each judge. For each judge, all possible pairs of the four rankings were compared using the Spearman rank-order correlation coefficient (r_s).

The results of the rank-order analysis are noteworthy in two respects. The first is that a judge's preexisting beliefs about what predicts what are more stable than a judge's correlation-relevant frequency information. Over all five judges, nearly one-third (32%) of the correlation-relevant frequency information is *internally inconsistent* in one test session or the other or both. For example, consider the trait pair "tense-tolerant." One judge estimated that, out of 100 persons, 70 are tense and 75 are tolerant. This means, of course, that 30 are not tense, and 25 are not tolerant. The judge then estimated that *of those who are tense,* 10% (or 7) are tolerant. Given the earlier estimate of the judge that 75 persons out of 100 are tolerant, it follows that 68 (75 minus 7) must be both tolerant and *not tense.* This is a glaring contradiction. The judge first claims that only 30 persons out of 100 are *not tense.* Then she makes a conditional-probability estimate that requires that there actually be at least 68 (out of 100) who are *not tense.*[5]

It is also revealing to compare the test-retest reliabilities of the judges' preexisting beliefs about what predicts what with the test-retest reliabilities of the contingency coefficients in the judges' correlation-relevant frequency information. With internally inconsistent pairs dropped from the analysis, the test-

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[5] Because correlational reasoning is a strain on the human intellect, the reader may have difficulty recognizing this internal inconsistency. As an aid, I suggest that the reader construct a two-by-two contingency table (see, e.g., table 1) with marginal values of 70 (*a* + *b*) and 30 (*c* + *d*), on the one hand, and marginal values of 75 (*a* + *c*) and 25 (*b* + *d*), on the other, and set the value of cell *a* at 7; then he should try to fill in the other cell values without violating the constraints of the marginals.

retest reliabilities (r_s) for preexisting beliefs about what predicts what average .68 over the five judges. For the contingency measures generated from the correlation-relevant frequency information, the test-retest reliabilities average .34. The correlation-relevant frequency information of a judge seems less stable than the judge's inference about what predicts what in experience.

The second noteworthy feature of the rank-order analysis concerns the degree of correspondence between variations in preexisting beliefs about what predicts what and variations in the contingency coefficients derived from the correlation-relevant frequency information. By dropping internally inconsistent pairs from the analysis, we can make direct comparisons between the two sets of scores both within and across test and retest conditions. When this is done, the fit (r_s) between a judge's preexisting beliefs about what predicts what and the contingency coefficients in that judge's correlation-relevant frequency information averages .39. (See Shweder [1976a] for a related experiment and a more detailed discussion of this topic.)

These results suggest that frequency information is neither consistently utilized nor correlationally manipulated even when it is available.

THE DIFFICULTY OF THINKING CORRELATIONALLY

I have pointed out that the concept of correlation may be viewed as a comparison between two conditional probabilities (e.g., the likelihood that someone is a leader given that he has self-esteem in contrast to the likelihood that he is a leader given that he lacks self-esteem) and that adults possess all the information they need to make such comparisons. The question arises, How easily can normal adults assemble their own correlation-relevant frequency information and organize it into a format that makes it possible readily to compare conditional probabilities? Alternatively, one may ask, is all relevant information about conditional probabilities equally accessible for correlational manipulation when adults claim that two traits go together in their experience?

An experiment dealing with these questions (discussed more fully in Shweder 1976a) first elicited from 20 judges the correlation-relevant frequency information concerning self-esteem and leadership just presented as table 2 and then examined how that information was used. The table contains frequency information supporting the truth of each of four conditional assertions: (1) "In general, people who are leaders have self-esteem" (cells a, b); (2) "In general, people who are not leaders have self-esteem" (cells c, d); (3) "In general, people lacking self-esteem are not leaders" (cells b, d); (4) "In general, people with self-esteem are not leaders" (cells a, c). These four assertions and their denials (e.g., "In general, people who are leaders lack self-esteem") were placed on separate cards and presented in random order to University of Chicago students. The students were told that they would be shown statements about the relationship between self-esteem and leadership in the general population. They were asked to read each statement and then say whether it was true or false, responding as quickly and accurately as possible. Response times were recorded to a tenth of a second.

Table 3 displays the differential ease and accuracy with which the judges were able to think about each of the four conditional assertions (and their denials). The judges seemed to have the easiest time processing the assertion (and denial of the assertion) "In general, people who are leaders have self-esteem." Average response times were relatively short (3.98 seconds and 4.78 seconds, respectively), and the judgments of truth and falsity were invariably correct, *given the judges' own correlation-relevant frequency information* (see table 2, cells a, b). Judges were extraordinarily inaccurate (although relatively fast) in their response to the conditional assertion "In general, people

TABLE 3

EASE AND ACCURACY WITH WHICH 20 JUDGES THINK ABOUT THE TRUTH OR FALSITY OF FOUR CONDITIONAL ASSERTIONS AND THEIR DENIALS

ASSERTION AND DENIAL	TRUTH VALUE (table 2)	PER- CENTAGE CORRECT	RESPONSE TIME (seconds)
In general, people who are leaders have self-esteem....	True	100	3.98
In general, people who are leaders lack self-esteem....	False	100	4.78
In general, people who are not leaders have self-esteem...	True	55	9.33
In general, people who are not leaders lack self-esteem....	False	75	9.24
In general, people lacking self-esteem are not leaders.....	True	75	8.53
In general, people lacking self-esteem are leaders........	False	100	6.26
In general, people with self-esteem are not leaders.....	True	60	6.88
In general, people with self-esteem are leaders........	False	20	5.30

with self-esteem are leaders." They tended to say "true," contrary to their own frequency estimates (see table 2, cells a, c). Judges were slow and not very accurate in their responses to the conditional assertion "In general, people who are not leaders have self-esteem." This assertion is just relatively difficult to think about.

In general, the data in table 3 suggest that the requisite information for comparing conditional probabilities is *differentially* accessible. Some conditional assertions are hard to think about. Under these circumstances, it does not seem surprising that judges do not consistently refer to their own correlation-relevant frequency information when they claim that two traits go together (or exclude one another). The concept of correlation is not an intuitive concept of the everyday mind.

CONCEPTUAL SCHEMATIZATION

"Going togetherness" is the most general form of conceptual association among objects and events. It reflects the fact that everyday knowledge is a process of sorting objects and events into interrelated symbolic complexes (categories, groupings, taxa, kinds, etc.). By means of what criteria are symbolic complexes formed? How are objects and events conceptually interrelated? How do everyday thinkers schematize their universe?

I have suggested that the nonintuitive concept of correlation does not play a significant part in everyday efforts to sort objects and events into groupings. Everyday empirical claims about what does with what in experience typically turn out to be claims about *noncorrelational* relationships among interpretive categories. Up to this point, these noncorrelational relationships have been referred to as relationships of likeness or resemblance. Unfortunately, "likeness" and "resemblance" are somewhat misleading terms. They seem to suggest that interrelated items must "have something in common." "Having something in common" is of course one of the ways in which objects and events can be said to conceptually relate to one another, but it is not the only way. In fact, its importance for everyday thought has probably been exaggerated. Symbolic complexes are held together by many intuitive relationships other than (but sometimes including) attribute sharing. The terms "likeness" and "resemblance" *as used in this study* refer to this full set of intuitive ways in which objects and events

are conceptually associated. Attribute sharing is but one member of the set. Correlation is not a member at all.

How are objects and events organized into symbolic complexes in everyday thought? Why is it that spiders and flies, backgammon and solitaire, mother's brother and father's sister's husband, self-esteem and leadership go together in our thinking? One popular answer to this question (the so-called essentialist response) claims that to be an element in a symbolic complex (a member of a category, an instance of a kind) is to manifest the essence of that symbolic complex (category, kind, class, taxon, etc.). From an essentialist perspective, items go together because they have in common certain necessary and sufficient features (attributes, characteristics, etc.). Propositions that are true of the complex as a whole are held to be true of each of the elements of the complex.

There are two rather trivial versions of this essentialist perspective on the formation of symbolic complexes. Sometimes it is argued that what all the elements of a category have in common is simply *the fact of having been grouped together*, as evidenced by their name, e.g., "insects," "games," "uncles." Categorization is reduced to labeling. Sometimes it is also argued that what all the elements of a category have in common is the *disjunction* or list of the elements of the category. Categorization is reduced to enumeration. According to these two versions of essentialism, there are no better or worse groupings of elements, and new elements can be added to a category at whim. Neither account need be taken seriously.

A more formidable version of essentialism, however, has many adherents in anthropology, especially among "componential analysts" and "ethnoscientists." According to Lounsbury (1964), the motivation for a componential analysis is to arrive at "uniform criteria" for assigning various elements (e.g., kin types) to particular classes (e.g., kinship categories). As Lounsbury remarks, the goal of a componential analysis is to produce a Boolean class product or conjunctive definition of everyday terms. Moreover, most of the analytical concepts of ethnoscience, e.g., "domain," "contrast set," "hierarchy" (see, e.g., Frake 1962, Sturtevant 1964), can be traced back to "Aristotelian" taxonomic models that are essentialist in form (see, e.g., Cain 1958). The taxonomist and the ethnoscientist imagine the existence of discrete (well-bounded) collections of objects that lend themselves to partitioning into exhaustive, mutually exclusive groupings (e.g., species) by a process of logical division that presupposes that at any higher taxonomic level the objects have something in common (e.g., the genus, that which makes the species like other species). At each taxonomic level there is a search for an essence, that without which the objects in the grouping would be something else.

A difficulty with the essentialist's view of category formation *as applied to everyday thought* is that it confuses the (somewhat utopian) *ideals* of a scientific (and formal) language with the communicative requirements of everyday language. It mistakenly holds that one special language of man can provide a model for all his languages.

Scientific languages are special, perhaps unique, in that they aspire to *universal* intelligibility. One way they do this is to strip language of its communicative reliance on the context and culture linking speaker and listener. Essentialism, in fact, may be viewed as a recommendation for how to achieve precision and reduce communicative ambiguity free of context and culture (see Ziff 1972, Shweder 1976b). The recommendation: multiply the terms in one's vocabulary until all the objects or events mentioned by those terms have been exhaustively partitioned into mutually exclusive, homogeneous (i.e., conjunctively defined) subsets. Imagine that the everyday term "father" appears on a piece of paper before your eyes. What is being mentioned? A genitor? A pater? A priest? A god? One can't tell. Essentialism solves this "problem" of multiple senses by splitting up the one term into many. Father$_1$ will mean "male household head," Father$_2$ will mean "male sire," etc. According to the essentialist

ideal, scientific language terms should have determinate meanings regardless of who utters them or in what context they are uttered.

Scientific languages are not satisfying models of everyday languages partly because everyday languages do not attain their precision (and they can be very precise) by proliferating terms to label mutually exclusive senses. Rather, they rely on context and implicit understandings to clarify the *relevant* sense of a single term. (See Black [1963] for an example of the logical paradoxes that arise when context-dependent concepts are analyzed as if they were context-free.) When you and I are sitting together by a desk, "pass me the red book" can have a precise referent. Out of context, "the red book" might even be a magazine. Furthermore, everyday ambiguity sometimes has a point. Its elimination is not always the goal. On one's knees in a confessional, the ambiguity of "father" (god, confessor) is in the service of the institution. Moreover, everyday language users often welcome the opportunity to be "read" in more than one way. Ambiguity serves a social function; it allows one to transmit messages without having to assume responsibility for them.

Perhaps the central reason scientific languages are inadequate models of everyday languages is that the various senses of everyday terms *overlap*, sometimes in complex ways. They are not mutually exclusive; they do not lend themselves to nonarbitrary "splitting" (again, see Ziff 1972). Wittgenstein's (1968: paragraph 66) critique of essentialism alerts us to the possibility that everyday categories are more appropriately conceptualized as a *continuum of merging senses* than as a partitioned set of discrete meanings:

Consider for example the proceedings that we call "games." I mean board games, card games, ball games, Olympic games, and so on. What is common to them all?—Don't say: "There *must* be something common, or they would not be called 'games' "—but *look and see* whether there is anything common to all.—For if you look you will not see something that is common to *all*, but similarities, relationships, and a whole series of them at that. To repeat: don't think, but look!—Look for example at board games, with their multifarious relationships. Now pass to card games; here you find many correspondences with the first group, but many common features drop out, and others appear. When we pass next to ball games, much that is common is retained, but much is lost.—Are they all "amusing"? Compare chess with noughts and crosses. Or is there always winning and losing, or competition between players? Think of patience. In ball games there is winning and losing; but when a child throws his ball at the wall and catches it again, this feature has disappeared. Look at the parts played by skill and luck; and at the difference between skill in chess and skill in tennis. Think now of games like ring-a-ring-a-roses; here is the element of amusement, but how many other characteristic features have disappeared! And we can go through the many, many other groups of games in the same way; can see how similarities crop up and disappear.

The essentialist approach seems to do violence to everyday categories like "game." It presents us with the following choice: Either (1) we *arbitrarily* exclude, for example, "solitaire" from our conception of "games" because it lacks some purported essential attribute, e.g., "competition between players" (see Nerlove and Romney [1967] for an example of this exclusionary strategy in a componential analysis of "sibling" terminology), or (2) one by one we abandon all attributes as necessary and sufficient criteria for category membership, which of course brings an end to the *general* application of essentialism as an account of everyday category formation. "Having something in common" is not all there is to classification. Attribute sharing is not all there is to conceptual relatedness or "going togetherness."

Besides essentialism's image of partitioned subsets, there are at least two other major images of the way objects and events are organized into symbolic complexes in everyday thought.

The *polythetic* approach (see, e.g., Beckner 1959; Vygotsky 1962; Sokal and Sneath 1963, Wittgenstein [on "family resemblances"] 1968, Sokal 1974, Needham 1975) codifies the image of a merging continuum. The *prototype* approach (e.g., Berlin and Kay 1969, Putnam 1975a, Rosch 1975) disrupts the continuum somewhat. It introduces the image of nodal, salient, or exemplary members of a symbolic complex, around which other objects and events cluster or distribute.

A polythetic conceptualization of everyday categories has the following form (modified from Beckner 1959:22): To be an element in a grouping (category, class, etc.) is to possess *a large number* of the relevant attributes (features, properties) of the grouping, under conditions where the following is also true: (*a*) each member of the grouping possesses *a large number* of the relevant attributes of the grouping; (*b*) each relevant attribute of the grouping is possessed by *a large number* of the elements of the grouping; (*c*) no relevant attribute is possessed by *every* member of the grouping. Not only do not all members of a polythetic category have anything essential in common, but it is even possible (although perhaps unlikely) for some pairs of elements in the grouping to have *nothing* relevant in common. The question arises, What holds a polythetic grouping together?

The coherence of a polythetic grouping rests on the possibility that all members of the grouping are connected to each other by means of a chainlike series of overlapping intermediaries. A shares nothing with D but has a lot of attributes in common with B, which in turn has a lot of attributes in common with C (although *some* of the attributes B shares with C are different from the ones it shares with A), which has a lot of attributes in common with D. If D connects with E, which overlaps with F, which overlaps with A, we come full circle, and close to the view that everyday symbolic complexes approximate a merging continuum.

Polythetic accounts have certain advantages. They warrant comparative enquiry even in the absence of "universals." Solitaire and professional football are both "games," not by virtue of something they have in common, but by virtue of the distribution of overlapping attributes across the other "games" that link them. Those futile debates in anthropology over whether (e.g.) the "nuclear family" or "kinship" or "religion" is universal could probably have been avoided had theorists abandoned the essentialist presupposition that any exemplar of "a nuclear family" or "kinship" or "religion" must possess certain necessary and sufficient features. The polythetic approach can also be credited with raising a number of fruitful questions about the relevance (or irrelevance) of formal logic to everyday reasoning (see, e.g., Vygotsky 1962:71–72; Feldman and Toulmin 1975).

Nonetheless, the polythetic approach has certain shortcomings when applied to everyday judgments of conceptual relatedness. One difficulty is that it assigns equal status to all members of a category. Thus an important aspect of everyday beliefs about how objects and events sort into kinds is overlooked—that everyday language users think certain members of their symbolic complexes represent or stand for the category more faithfully than others. A sparrow is thought to be more "birdlike" than a turkey; an apple is thought to be more "fruitlike" than a watermelon (Rosch 1975). In the minds of everyday language users, symbolic complexes are differentially typified by their elements; everyday thinkers have well-developed and reliable perceptions of category *prototypes*.

The differential cognitive saliency of certain category elements seems to be related to the fact that normal adults have more things to say about some category elements than about others. For example, when normal adults are asked to give examples of the linguistic *use* of a category, the sentences they generate ("A bowl of *fruit* makes a nice centerpiece") are typically more appropriate when applied to independently judged category prototypes (e.g., apples, oranges, etc.) than to nonprototypical elements (e.g., watermelons, blueberries) (Rosch

1975). Category prototypes may be semantic myths from the point of view of *actual* usage (people *can* say "Fruit makes a nice dessert," in which case watermelons are as good exemplars as apples); nonetheless, these myths are our subject matter.

Everyday conceptual schemata seem to have nodal elements or conceptual "centers of gravity" around which other objects and events congregate. The question then arises, How might nonnodal elements be connected to nodal elements (and thus ultimately to each other)? Casagrande and Hale's (1967) study of folk definitions among the Papago suggests that objects and events are conceptually linked to one another in everyday thought in at least 13 different kinds of ways.

Casagrande and Hale's study is especially interesting because it lists "contingency" ("X is defined with respect to a usual or necessary antecedent or concomitant Y") as the second most frequently employed "relationship concept" in Papago folk definitions. In terms of frequency of usage it follows "attributive" definitions ("X is defined with respect to one or more distinctive or characteristic attributes Y") and precedes "functional" definitions ("X is defined as a means of effecting Y"). Since I have argued that normal adults lack a concept of correlation or contingency, genuine evidence of contingent thinking in folk definitions would be very damaging to my claims. An examination of the examples suggests, however, that the Papago are no more contingent in their thinking than Smedslund's Swedish nurses or Ward and Jenkins's college students. Papago folk "definitions" such as "Whenever we are *hungry* for something, we *eat* it" or "When there is *rain* there is *lightning*" do not involve a *comparison* of conditional probabilities. The Papago seem to be telling us that the number of times X (hunger) and Y (eating) *are both present* is large. Alternatively, they may be reporting on a *single* conditional probability, the likelihood of eating when hungry. They are intellectually processing only one part of the information needed to estimate a correlation. Genuine contingent reasoning does not appear in the Papago folk definitions reported.

Casagrande and Hale alert us to a set of concepts (e.g., function, synonymy, provenience, class inclusion, similarity, etc.) that are intuitively available for arranging objects and events into symbolic complexes. All peoples think with these relationship concepts, and any one of these intuitive concepts *might* substitute for the nonintuitive concepts of correlation (or contingency) when normal adults estimate what goes with what in their experience. Throughout this paper, in an omnibus way, I have referred to the collection of all these intuitive relationships as relationships of likeness or resemblance.

Nonetheless, in the personality domain one type of intuitive relationship concept seems to have a privileged position. Items of conduct are often said to go together because they jointly evoke an image of a symbolic behavioral type. For example, in 1969 the following items of conduct conceptually clustered together in the minds of American college students: "feels that others are generally too conforming to conventional social expectations," "demands pleasure and gratification for himself," "feels his independence is very important." The items clustered in this way because they worked together to evoke the image of a "hippie." The items could be interrelated as constituent parts of a symbolic whole, the image of the behavioral type. They were part of what it *meant* to be a hippie. Without the image, "demands pleasure and gratification" and "feels his independence is very important" might not have been linked at all. Intuitively available part-part and part-whole relationships of this type often replace the nonintuitive concept of correlation when normal adults judge that two items of conduct go together in behavior.

The question "To what extent do gentleness and good-naturedness go together in people?" evokes in American students such images as "pastoral males bathed in sunlight," "Santa Claus," "barmaids," and "old people." This process of organizing items of conduct by means of images of behavioral

types seems to be widely distributed across cultures. It certainly occurs in India as well as in the United States, as an experiment I conducted there (Shweder 1972b) shows.

Residents of the Old Temple Town of Bhubaneswar, Orissa, India were asked, of a friend, neighbor, or workmate, "Tell me in detail about his character, personality, and behavior *(tankɔrɔ cɔritrɔ, prɔkruti, o byɔbɔharɔ bisɔyɔre mɔte bhalɔbhabɔre kuhɔntu)."* One informant, for example, an untouchable, described a neighbor in some of the following terms: engages in debauchery *(randɔbaj kɔre)*, married the widow of his elder brother after his death *(nijɔ bɔdɔbhair mrutyu pɔre tarɔ bidhɔba maipɔku baha hela)*, smokes ganja (marijuana) *(gɔnjei khae)*, discusses other people's shortcomings *(pɔrɔrɔ chidrɔ alocɔna kɔre)*. Each of these items is connected to other items of conduct as a constituent of an image of a behavioral type. These images are used to estimate what goes with what in experience. As an illustration I focus upon the single item, "smokes ganja (marijuana)."

"Smokes ganja" is one of hundreds of items of conduct elicited by the interview question. Two Indian informants helped reduce the corpus to 99 items by sorting items into groupings, each of which contained relatively synonymous elements, and selecting an exemplary item or prototype to represent each grouping. "Smokes ganja" was replaced by "takes all kinds of intoxicants like alcohol, ganja, etc." in this grouping process. The 99 items were then placed on cards and presented to 25 Old Town residents. The informants were asked to place items together that went together in people and to label the groupings. For example, one informant, a 26-year-old Brahman, closely associated the item "takes all kinds of intoxicants like alcohol, ganja, etc." with "goes after women, makes love to them" and "teases friends behind their back." The items were placed together because they evoked in the informant the image of a "shameless" person (the informant labeled the grouping *behia*, "shameless"). On subsequent groupings, the informant also associated the original item ("takes all kinds of intoxicants . . .") with items such as "bluffs when he does not know something," "beats his wife and children," and "cannot stand the prosperity of others." All these items were said to go together in people because they evoked the image of an uncivilized, rude person *(ɔbhɔdrɔ lokɔ)*.

Hundreds of images of behavioral types were evoked by this grouping and labeling task. This enlarged corpus was eventually reduced to an exemplary or prototypical corpus of 81 images by means of the procedure discussed above. One item of this reduced corpus was *ɔbhɔdrɔ* ("uncivilized, rude"). Using this corpus, the sorting and labeling task was repeated with 25 Old Town Oriya Brahmans. One informant, for example, grouped *ɔbhɔdrɔ* along with items such as "uncontrollable and threatening" *(durdantɔ)* and "mischievous" *(dustɔ)*. They were placed together because they evoked the image of a "demon" *(Rakhyɔsɔ)*. On subsequent groupings the informant also associated *ɔbhɔdrɔ* with items such as "jealous" *(pɔrɔsrikatɔrɔ)* and "selfish" *(swarthɔpɔrɔ)*. All these items were placed together because they evoked the well-known master symbol of Ravana *(RawɔNɔ)*, the wicked adversary of Rama, as portrayed in the Indian epic the *Ramayana*.

The conceptual excursion from "smokes ganja" to "Ravana" seems to have little reference to co-occurrence likelihoods in actual behavior. Informants apparently estimate the degree to which items go together in behavior by the ease with which they can bring to mind a higher-order symbolic behavioral type in which the items compatibly fit as parts of the imagined whole (see Tversky and Kahneman [1973, 1974] on the everyday use of "availability heuristics" for estimating frequency under conditions of uncertainty). They seem to substitute one type of relationship concept (e.g., part-whole) for another (e.g., correlation or contingency). In this way, likeness and co-occurrence likelihood become merged in everyday thought.

DISCUSSION

The main theme of this study is that magical thinking (the confusion of propositions about the world with propositions about language) is an expression of a universal disinclination of normal adults to draw correlational lessons from their experiences. Correlation and contingency are concepts that are relatively difficult to think with. They are not intuitively available to normal adults, and they are absent from most ordinary thinking. Moreover, in everyday life the concept of correlation is often replaced by intuitive notions such as resemblance and likeness when estimates are made about what goes with what in experience. This typically occurs when objects and events can be linked to one another by conceptual relationships such as similarity and contrast, part-whole, etc. Normal adults substitute propositions about conceptual relatedness (e.g. part-whole, similarity, etc.) for propositions about co-occurrence likelihood. In everyday thought, resemblance is not carefully distinguished from contingency.

To comprehend the magical mind, it has been necessary to address some of the seminal questions in the anthropology of thought (also see Shweder 1972a, 1975b, 1977), among them What does the organization of symbolic systems reveal about the predilections and disinclinations of the human mind? In what ways do symbolic systems influence the intellectual processes of those who use them? How are symbolic systems to be described? How do objects and events give rise to meanings? The goal of this final section is to anticipate some possible misunderstandings that may arise over what has been claimed.

It should be kept in mind that this paper has been concerned primarily with the kinds of concepts men employ to *organize and manipulate* information, regardless of the interpretive content of the information processed. Evidence of a universal disinclination of the everyday mind to employ a concept like correlation is perfectly compatible with documented cross-cultural differences in the content of the categories into which objects and events are sorted. Fowl excrement and ringworm go together for the Azande, not Americans.

Evidence of a disinclination of normal adults to employ the concept of correlation has implications for our understanding of Piaget's theory of intellectual development. Although *under special circumstances* it is possible to elicit correlational reasoning from normal adults, the very need to contrive such circumstances suggests that the transition to formal operational thinking is not a spontaneous occurrence and that when it does occur it is to be explained in different theoretical terms than the transition to concrete operational thinking (which seems to occur universally and perhaps earlier than many suppose) (see, e.g., Mehler and Bever 1967).

It would not be surprising if this study reminded some readers of late 19th-century accounts of the primitive mind. Frazer, for example, sorted magical thinking into two classes, homeopathic and contagious. The two classes were defined by the confusion of concepts such as similarity and contiguity with the concept of causation. In effect, this study provides a cognitive-processing basis for Frazer's insight and argues for the universality of the magical mode of thought.

There is a more profound sense in which the study is neo-Frazerian or neo-Tylorian (see Horton 1967, 1968). It shares the following two assumptions (and only the following two assumptions) with these 19th-century evolutionists: (*a*) Normal adults in all cultures strive to understand, explain, and arrive at generalizations about the empirical relationships among objects and events in their experience. This is true regardless of the presence or absence of explicit, self-conscious scientific canons of objectivity and verification in their society (contrast this assumption with Tambiah's [1973]). (*b*) Some of the concepts developed by normal adults to arrive at these generaliza-

tions are less adequate to the task than some of the concepts of Western scientists (Lévi-Strauss [1966:1–22] seems to agree but hedges). Resemblance is one of those inadequate concepts. It is a relatively poor index of correlational relationships in experience (in spite of Lévi-Strauss's opinion to the contrary); yet it is relied upon when normal adults judge what goes with what in their lives. As a neo-Tylorian might put it, magical thinking is inductive in its intent but mistaken in its conclusions. Unfortunately, these two neo-Tylorian or neo-Frazerian assumptions are easily misconstrued. I would clarify them in the following ways.

Firstly, to claim that all men are applied scientists (and rather poor ones at that) is not the same as asserting that men are only applied scientists. I have no quarrel with the caveat that investigators of thought should not confuse poetry with science, persuasion with induction, matters of value with matters of fact, or performative utterances with propositional utterances. I agree that it can only lead to misunderstanding to compare all these cognitive activities using the same yardstick. Truth and falsity, error and ignorance are not the only criteria available for assessing cognitive acts. For example, if it is the *goal* (the point, the purpose) of mythological thought to use propositions about the world to make propositions about language (as Lévi-Strauss [1963a] seems to suggest), it would be unjustified to interpret mythological formulations as "confusions." Similarly, it would be misleading to interpret true instances of erroneous inference (e.g., those discussed in this paper) as *intentionally* "symbolic."

Secondly, cognitive acts cannot always be assessed using some *single* yardstick. It would be overly simplistic, and perhaps ethnocentric, to suggest that they can. Gellner (1973:174) makes the point that a feature of the "savage" (everyday?) mind is "the conflation of the descriptive, evaluative, identificatory, status-conferring, etc., roles of language." To treat magical thinking as a form of thought in which propositions about language are confused with propositions about the world (i.e., to treat it as erroneous science) is not to deny the possibility that it has other functions.

Finally, it is important to return to the distinction between intuitive and nonintuitive understandings, a distinction that is rarely respected in anthropological studies. The explicit canons of science in the West insist upon a sharp division between likeness and co-occurrence likelihood. Some might even argue that this formal recognition of the difference between face validity and all other types of validity is distinctive of Western scientific ideals. Nonetheless, it should be kept in mind that intuitive understandings are usually unaffected by nonintuitive reflection. (For example, Hume's epistemological dualism has had no influence on everyday intuitions that the world can be *directly* perceived.) Despite our own scientific ideals, in everyday practice we are as magical as anyone.

Inversely, the absence of an explicit cultural recognition of a concept, e.g., in theory, ideology, or ethics, is *not* evidence of the nonexistence of the concept at an intuitive level. Investigations of thought can often be distinguished by their adherence or nonadherence to this principle. This study adheres to it; Sahlins (1976a, b) and Tambiah (1973) seem to disregard it. For example, Sahlins has described the utilitarian thought of a number of Western social scientists as a "self-conception of capitalism parading as a theory of society." It may well be that the "economic approach to human behavior" and its constitutive concepts, e.g., utility, the rational pursuit of self-interest (i.e., the selection of means to accomplish ends in the most efficient way possible), market equilibrium, etc., is a very poor *general* theory of society (see Becker [1976] for an attempt to advance such a general theory). My own view is that many of the objections raised to utilitarian social theories by Durkheim (1965 [1915]) and Sahlins (1976a, b) are compelling. Utilitarian theories have their place, but they must be kept in their place. (Similarly, symbolic theories of society have their place, but

must be kept in it. The realm of the profane and the realm of the sacred cannot be reduced to one another.) Nonetheless, it does not follow that the utilitarian mode of thought is (merely?) a "self-conception of capitalism," at least not if the term "capitalism" is being used to describe a social institution limited in historical time and space.

Parsons (1968) suggests what I view as a reasonable alternative to this "folk-concept" (and hence culturally relativistic) approach to utilitarian thought. He notes (p. 19) that "the central fact—a fact beyond all question—is that in certain aspects and to certain degrees, under certain conditions, human action is rational." He goes on to remark (p. 51) that the mode of thinking that gives rise to utilitarianism "is deeply rooted in the common-sense experience of everyday life, and it is of a range of such experience that it may be regarded as universal to all human beings." It may well be that some theorists in some societies explicate and formalize the utilitarian mentality. Some of these theorists may even advance utilitarianism as a general theory of society, and some of these societies may even enshrine utilitarian concepts as ideology or ethics (see, e.g., Weber 1958). In all such cases, however, ideology builds upon and codifies intuitive concepts (e.g., means, end, relative efficiency, self-interest, etc.) that are universally available to the human mind. The process by which the utilitarian mode of thought becomes *enshrined* is a cultural process, and Sahlins (1976a) has much of importance to tell us about enshrinement. Nonetheless, normal adults in all cultures are utilitarians in *some* aspect of their lives, or, as Parsons puts it, "in certain aspects and to certain degrees, under certain conditions."

Similarly, I have argued that normal adults in all cultures seek to acquire empirical knowledge about what causes what, what influences what, what goes with what in their environment. Neither the prior explication of the canons of inductive logic nor the cultural deification of man as scientist is a prerequisite for such everyday sciencing. All men are applied scientists. Very few are philosophers or ideologists of science.

Comments

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I find this an intriguing and provocative, if not totally convincing, article. That early much maligned student of the anthropology of thought, Lucien Lévy-Bruhl—who might have been accorded at least a passing nod of recognition—would have approved.

Shweder himself seems to indulge in a bit of magical thinking. On the basis of likenesses in the data derived from a series of essentially experimental, and thus contrived, studies he infers the likelihood of a "universal disinclination of normal adults to draw correlational lessons from their experiences." That strikes me as a giant leap over much uncharted terrain. Moreover, Shweder's argument relies heavily on evidence drawn from but one experiential domain—everyday judgment about personality traits and the likelihood of their frequency and co-occurrence. As Shweder himself suggests, personality is a rather special domain, heavily freighted with stereotyped images of "symbolic behavioral types" and, one might add, fraught with normative expectations and evaluations of traits that *ought* to go together, e.g., to make a "good" person. If normal adults are so inept in perceiving the probability that certain personality traits co-occur, how is it that we find the behavior in ordinary social interaction of persons we know well to be quite predictable? Are we merely deluded?

Shweder bases his conclusions exclusively on verbal reports and judgments of what people *believe* to be the case rather than

on what people do. One might grant that we humans are cognitive hostages to the semantics of the languages we speak, but is thinking manifest only in verbal behavior? Perhaps Shweder wishes to restrict the notion of cognition to those mental operations that can be brought fully into awareness and accurately represented in verbal reports. If not, what kind of sense are we to make, say, of the "high order of intellectual functioning" (and presumably correlational thinking) displayed by the Trukese navigator in making his way from one tiny island to another (Gladwin 1964:175)? The Trukese navigator is at a loss to provide a verbal account of what he does, but surely he draws correlational lessons from his experience. If people could report fully and accurately on what they feel, think, and do, there would be little scope for ethnography.

Shweder quite accurately states that Papago "folk definitions" (Casagrande and Hale 1967), and particularly the type of semantic relationship in such definitions that we have labeled "contingent," do not involve a comparison of conditional probabilities. But in constructing a "folk definition," why should one be motivated to employ a comparison of conditional probabilities? The absence of such a "second-order" semantic relationship in these definitions gives no warrant for the conclusion that the Papago are not contingent in their thinking in other contexts. Again, this is an overgeneralization.

By the time this comment reaches Shweder, he may well have seen a paper by Kirk and Burton (n.d.), "Meaning and Context: A Study of Contextual Shifts in Meaning of Maasai Personality Descriptors," which won the 1976 Stirling Award in Culture and Personality. Using triads tests and multidimensional scaling, Kirk and Burton show how Maasai personality trait descriptors vary systematically across social identities differentiated by age and sex. This admirable paper strikes me as highly relevant to Shweder's own study and provides an excellent base for further research using some of his procedures.

Imbued as it is with magical language, and subject as well to deep and subtle logocentric biases, comparative research on human cognition is clearly one of the most refractory of anthropological problems. Very little is yet known about how we natives think, and one can only applaud Shweder's bold efforts to think about how we may think.

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Shweder has made a valuable contribution in pointing out some of the ways in which we do not think as we think we think. Especially in our cognitive activity about people and behavior, we are not as logical or even as systematic as we believe we are. Part of the scientific approach is a distrust of everyday perceiving, thinking, and construing, particularly when these are done by a person with a stake in the outcome. The scientist has to learn to think in terms of correlation and contingency. More precisely, he learns to check his own free thinking—which is often quite intuitive or magical—by standardized applications of agreed-upon tools for analyzing systematic observations. Most scientists who study people and behavior have not developed that distrust of everyday cognizing and have not objectively tested their magical thinking.

If magical thinking reflects a limitation of general human functioning, how does it happen that people adapt moderately well and manage to get along in their everyday lives? A possible answer is that likeness is a basic starting point. People adapt and cope by seeing essential resemblances between the situation immediately confronting them and previous situations and by reacting in terms of what worked or did not work in those earlier situations. Insofar as they are correct in perceiving critical components of the present situation as like those in previous ones, their coping is adequate. If they perceive a re-

semblance which is irrelevant, their reaction may not be adaptive.

Much thinking and explaining seems almost epiphenomenal. It pertains to a type of adapting that appears to parallel adaptive acting and reacting. In addition to getting along in the realm of behavior, people have to cope with their need for explanation and understanding. People are forever asking themselves "Why did this happen?" or "Why did that person do that?" Almost any explanation will suffice. We often feel we understand other people when we have labelled them, when we have attributed some quality to them. Our attributions satisfy our need to understand. And once we have made a plausible attribution, we rarely give it up, because hardly anything can happen that clearly disproves it. Even in formal theorizing, we can account for perceived behavior which is the direct opposite of an attribution by inventing a disposition to act in a way that is contrary to one's "real" nature.

Making attributions about other people, labelling them, is epiphenomenal in the sense that it is a secondary kind of adapting, less essential than our moment-to-moment behavioral activity. Our labelling of those with whom we are interacting does not determine our action at a given moment. Instead, it helps in expressing our feelings, in communicating with other people, and in trying to reach some understanding of them.

I used to believe in traits as scientific entities. The fact that traits are, as Shweder says, constructs of the human mind did not bother me, because I see all scientific concepts as such constructs. I have gradually become convinced that trait concepts are of limited value, especially as scientific constructs. They are, however, important and interesting objects of study in their own right: How do people label others, and how do these labels come to be chosen? In addition, we cannot ignore the fact that these labels, especially when they indicate evaluative judgments, play crucial roles in human decisions about selecting people for training programs or jobs and about helping people who are in distress or who are causing distress to others. In our scientific work, we must do what we can to make sure that this significant labelling is done on as sound a basis as possible, with a minimum of magical thinking. We must help those labellers to think in terms of likelihood, not superficial likeness.

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The merits of this intelligent and often brilliant paper seem so obvious to me that it may be more useful to concentrate on the limits of its application—limits which Shweder himself does not specify very clearly. In doing so, I shall also draw heavily on a Wittgensteinian (1968) perspective.

Among other things, Shweder warns us against assuming that an observed personality *trait* can, by itself, be treated as having *causal force* in shaping behavior, i.e., that such traits are (in Wittgenstein's terms) internal mechanisms that produce external behaviors. To believe in such illicit causality, e.g., that self-confidence *causes* a child to organize games, is indeed as magical as to believe that fowl excrement cures ringworm. But Wittgenstein also warns us not treat concepts simplistically. "Magic" references many activities, some of which draw on deeper insights into the social-psychological origin of disease than those common in Western medicine (Horton 1967). Similarly, organizing games can be taken not as a causal *consequence* (what Wittgenstein would call a "symptom") of self-confidence, but as one of the *criteria* we have for attributing it to particular persons. In that case, the claim that the game-organizing child is self-confident is by no means simply magical thinking. In laying it down that "no amount of detailed inspection of playground activity will uncover 'self-confidence,'" Shweder too

quickly dismisses this distinction between symptoms and criteria.

There is a deeper problem with Shweder's all too unqualified finding that correlational thinking is not present in everyday life. Smedslund's (1963) nurses were presented with cards in an experimental setting rather than with patients in the familiar context of actual hospital practice. This seems to be rather artificial evidence at best on which to answer the global question: "Which is the more important intellectual tool of the everyday mind, the concept of correlation or the concept of resemblance?" Shweder's answer, that "correlation is not an intuitive concept of the everyday mind," seems much too sweeping. We may stipulate that more often than not likeness is preferred by the everyday mind to likelihood, although what universe of cases we are to use in making this comparison is by no means clear to me. In fact, plumbers, farmers, artisans of every sort seek to maximize the probability of desired outcomes in choosing between alternative courses of conduct. They acquire the practical knowledge that course A produces outcome X *more frequently* than does course B. In other words, their behavior indicates that they know the relevant conditional probabilities—and all without Statistics 101. Particularly given feedback from experience—which Smedslund's nurses cannot obtain from a deck of cards—such correlational thinking is by no means uncommon.

The point here is central to Wittgenstein's enterprise. Human thought flourishes where our concepts are used in appropriate contexts. Yet it remains the case that, when concepts are taken out of such contexts, our minds are indeed bewitched. In such cases, as Shweder so persuasively argues, our thinking is as magical as the Azande's.

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Shweder argues from the indisputable fact that people do not generally think in terms of two-by-two contingency tables and correlation-relevant frequency information to the much more controversial point that people think in the same way when they engage in judgements of personality and in what is generally called primitive magic. This argument is couched in terms of the basic claim that magic involves "a confusion of propositions about the world with propositions about language." In other words, Shweder is asserting that since people do not generally think in terms of strict correlations, they must be making recourse to relations of resemblance. Since by definition such relations cannot be established by the correct use of the technique of correlation, these connexions cannot easily accord with scientifically valid relations. Therefore for us, as for the Azande, conceptual relations (being a leader entails having self-esteem) take over from or are "confused with" what is in fact the case (that not all leaders have self-esteem).

Many anthropologists would agree with Shweder's basic claim as it applies to magic as traditionally circumscribed. Thus Beattie has argued that magical premises "are founded in metaphor and drama rather than in the controlled hypothesis-testing of science" (1970:259). Not so many would agree with the scope which Shweder accords to magic ("in everyday practice we are as magical as anyone"). It is one thing to argue that because we do not think in terms of strict correlatory techniques we "confuse" our interpretative categories with the events they describe; it is another to make the stronger, neo-Frazerian claim that "normal adults *substitute* the intuitive concept of resemblance for the nonintuitive concept of correlation" (my emphasis). As these two quotations suggest, Shweder is not altogether clear about the extent to which conceptual relations take over from relations established via correlation. Thus we are variously told that the distinction between the two is "collapsed," "confused" (which implies that it is somehow still kept distinct), or "ignored," that the former "often replaces" the latter, that a degree of correlatory thinking enters into folk conceptualisations of the world (as with the Papago), and so forth. By not sufficiently recognising the differences between connexions involving an inadequate grasp of the technique of correlation (as with the nurses who do not properly handle the relationship between symptom and disease), connexions which are partly based on empirical correlations but also on symbolic configurations (as with those Indians who make some appeal to Ravana), and connexions which make minimal appeal to scientifically correlatable evidence (as with Zande magic), Shweder makes social life more magical than it really is.

The reason for this must surely be that the strict criterion of correlation he employs results in his classing all the cases just mentioned as instances of some degree of confusion between semantic, symbolic connexions and what is actually the case from a scientific viewpoint. The fact remains that some of them make more use of empirical correlations than do others. There are degrees of magicality, and unless this is recognised we are drawn back to all the criticisms which have been directed to Frazer and Lévy-Bruhl. If connexions established by resemblance took over from or were "confused with" connexions which exist by virtue of scientific trial-and-error, inductive and correlatory evidence to the extent that Shweder in the main suggests, then we would be left with a social world which could not work. Personality judgements, even assessments of the type "fire is hot" or "maize grows well," might not be based on the application of sophisticated statistical techniques, but this is not to say that they are not based on some degree of empirical evidence and probabilistic thinking (see Shweder himself and also D'Andrade 1974:161 and Weaver 1977). Magic, as traditionally circumscribed, cannot provide the basis for many everyday activities. Therefore analytic schemes should leave room for degrees of magic—for types of connexions existing between the strictly scientific and the magical.

If Shweder maintains his equivalence between personality judgements and Zande magic, he makes personality judgements too magical to work: imagine a society whose judgements were so far removed from a relatively realistic assessment of intrapersonal relationships as to say that leaders lack self-esteem and do not have the capacity to influence others. But if, as surely he must, he allows the equivalence to relax, the thesis lapses into something akin to that maintained by Beattie and others. However, this is not to say that Shweder has not made a valuable contribution in showing, in interdisciplinary terms, that semantic relations of resemblance inform connexions more than anthropologists have often assumed. This insight can now profitably be combined with the fact that there are varieties of interplay between degrees of correlatory expertise and trial-and-error reasoning (for surely the statement "fire is hot" is best examined in noncorrelatory terms) and types of resemblances. The drawback of Schweder's paper is that by returning to Frazer he does not adequately analyse the difference between magic as traditionally isolated (where almost "anything goes") and connexions operative in everyday life (where empirical necessities mean that not even "many things can go").

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One of the persisting problems which bedevils discussions of cultural differences and similarities in thought is the lack of an agreed-upon definition of thought within or between various social science disciplines. The differing empirical referents and

theoretical implications of terms having to do with intellectual activity are a recurring cause of confusion. In selecting a topic for comment among the many interesting issues raised by Shweder's provocative article, we have chosen to concentrate upon his use(s) of words referring to thinking in the hope that a few distinctions important in our work will prove helpful to others.

If we understand Shweder correctly, one of his key suggestions is that anthropologists (and probably other social scientists as well) "have mistaken a difference in the content of thought for a difference in mode of thought." The common mode of thought which Shweder hypothesizes is a reliance on similarity to make judgments that are presumably causal, combined with a failure to draw inferences from data on the relative probabilities with which different events co-occur.

Before considering the implications of Shweder's terminology, we should make it clear that we fully accept the experimentally demonstrated phenomena to which he refers and suspect that such phenomena may be operating in some of the situations to which anthropologists apply the term "magical thinking." What we question is the usefulness of such terms as "correlational thinking" and "intuitive" and "nonintuitive" concepts and of Shweder's references to Piaget's theory of stages of mental development. Perhaps our comments could be reduced to the suggestion that Shweder's references to thought be interpreted as statements about beliefs, as cultural phenomena, but not as statements about the psychological processes which give rise to the phenomena he points at.

To illustrate the distinction we are trying to make between outcomes and underlying processes, we will consider an extensive series of studies carried out by Rescorla and his colleagues (1967) on the problem of co-occurrence and contingency. This work demonstrates correlational thinking of the sort Shweder generally claims to be a rare achievement of the human mind and can serve to illustrate the problems we see in using such terms as "thought process" and "magical thinking."

The experimental procedure involves the presentation of two events (a tone and a shock) while the subject is emitting a frequent and regular response that itself has no prior association with either event. Responding is expected to be suppressed if the tone regularly predicts the shock.

Rescorla was concerned about the amount of suppression of the ongoing response in the presence of the tone as a function of the subject's history of tone-shock pairings, especially the contingencies that exist between the two events. From data reported by Rescorla, we have constructed a table reflecting tone-shock co-occurrences and response suppression. Table 1 represents the amount of response suppression as a function of the co-occurrence of tone and shock events; p (shock/tone) represents the likelihood that tones could predict shock onset. The index of response suppression is zero when suppression is complete and .5 when there is no change in the rate of ongoing behavior. It is easy to see that co-occurrence and p (shock/tone) are poor predictors of response suppression. The first four entries show that the response rate ranges from completely suppressed to entirely unaffected with the same co-occurrence frequency.

Table 2, on the other hand, shows the reordering of these ten conditions in terms of the *correlation* of tone and shock as determined by the contingency coefficient (C). This ordering quite clearly shows the influence of the correlation between tone and shock events on response suppression. In fact, the contingency coefficient accounts for 92% of the suppression variance.

Here, then, we have an example of behavior that reflects the correlation of events in situations logically identical to those referred to by Shweder where such correlational behavior was not observed. Some account of the way the organism is processing and retaining information other than a label of correlational or noncorrelational is in order. We might also want to take into account the fact that the subjects in all of Rescorla's work were white rats. Whatever one wants to conclude about the intellectual capacities of these antique objects of study, it seems unlikely that one would want to conclude that most adults in all cultures are not formal operational thinkers, but most rats in Yale psychology laboratories are.

A starting point for a different framework within which to consider problems of specifying thought processes is to make a distinction between the task environments (problems) which are the occasions for thinking and the processes which are assembled in these task environments and which get labelled as thinking. When Piaget began his monumental studies into the development of thinking in children, he began, as does Shweder, by labelling thought processes with the names of various content areas (e.g., the child's concept of space, of number, of causality, etc.). From the very beginning, however, Piaget sought to discern the processes at work within and between the problem situations with which he presented children, arriving at such notions as reversibility, compensation, etc., which are operative across problems and which together serve to determine the "kind of thinking" needed to deal with specific classes of problems. It is in this spirit that "correlational" thinking comes to be subsumed under the category "formal operational thinking." "Correlational" refers to the joint presence of an environment which places certain information at an organism's disposal and a combination of behaviors (thought processes) which may occur in such environments to produce particular outcomes (e.g., correlational inference).

The processes Shweder refers to as correlational are currently the subject of rather intense research within cognitive psychology (cf. Estes 1976). The burden of current evidence suggests that the way in which component intellectual processes get assembled to produce (or not produce) "correlational thinking" as an outcome depends critically on the amount of information that the organism has to process and the extent to which negative instances are made available to the subject for analysis. Within this framework it is not necessary to hypothesize, as does Shweder, that the transition to formal operational thinking is a nonspontaneous occurrence to be explained in terms different from the spontaneous transition to concrete operational thinking. "Spontaneous" as a concept is anathema to scientific explanation.

What is remarkable about the contexts which are used to illustrate operational thinking (assuming that the processes in-

TABLE 1

RESPONSE SUPPRESSION AS A FUNCTION OF TONE-SHOCK CO-OCCURRENCE

	PAIRING NUMBER									
	1	3	5	8	2	6	7	4	9	10
Co-occurrence..	24	24	24	24	12	12	12	6	6	0
p(shock/tone)..	.40	.40	.40	.40	.20	.20	.20	.10	.10	0
Suppression....	0	.23	.35	.49	.10	.30	.44	.21	.51	.49

SOURCE: Rescorla (1967).

TABLE 2

RESPONSE SUPPRESSION AS A FUNCTION OF THE CORRELATION OF TONE AND SHOCK

	PAIRING NUMBER									
	1	2	3	4	5	6	7	8	9	10
C.............	.51	.38	.31	.28	.18	.12	0	0	0	0
Suppression....	0	.10	.23	.22	.35	.30	.44	.49	.51	.49

SOURCE: Rescorla (1967).

volved in the correlational tasks to which Shweder refers are examples of formal operational tasks) is the very circumscribed set of environmental events they represent. Shweder correctly points this out in support of his speculation that the acquisition of formal operations requires some new theoretical apparatus. But if we are correct in our analysis, the primary difference between concrete and formal operational tasks (or between intuitive and nonintuitive tasks) is the likelihood that individuals will encounter environments which make corresponding adaptive intellectual activities possible.

Such a perspective seems in no way to undermine Shweder's emphasis on the common predicament of all people as they struggle with the "hazards of everyday thought." It has the virtue of refocusing our attention on the question we consider central to the understanding of culture and thought: how do different cultures organize the social and physical environments within which members do their thinking? It is in the interaction between individuals and task environments, not the intrapsychic capacities of individuals, that cultural differences in thought reside.

by David F. Lancy
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Shweder has done a very good job in arguing for the universality of concept formation through object or event resemblance rather than through contingency. A substantial body of research in cross-cultural psychology has shown that, when familiar materials and problems are framed as cognitive tests, African villagers exhibit Western patterns of thought (cf. Cole and Scribner 1974). Shweder takes the opposite tack by showing that, in everyday situations, well-educated Westerners show patterns of thought which have previously been labelled primitive or magical. His statement that "anthropologists interested in thought may have mistaken a difference in the content of thought for a difference in mode of thought" therefore bears repeating.

While I subscribe wholeheartedly to Shweder's thesis, I think two qualifications might be made. The first is that, while there may be a universal disinclination to use contingency information in making judgements, Westerners, as a result of formal education and circumstances related to involvement in various professional activities, probably reason correlationally more often than non-Westerners. And, in general, societies will vary in the opportunities and encouragement they provide to think correlationally.

Second, beliefs about relationships may be held with varying degrees of conviction. Westerners may have more flexible belief structures and be able to forego magical thought patterns more easily than non-Westerners. In particular, Swedish nurses would probably learn to make accurate predictions of symptom/disease relationships more easily than Azande medicos would learn to make accurate treatment/cure predictions.

Reply

by Richard A. Shweder
Chicago, Ill., U.S.A. 12 vii 77

I don't think Lévy-Bruhl would have approved of my study (see Casagrande's comment). In my account of the shortcomings of the everyday mind he would have recognized faint traces of an approach he abhorred.

In 1910 Lévy-Bruhl contrasted the modern mentality and the primitive mentality in more or less the following terms: (*a*) Primitives think in ways any logician would consider contra-

dictory; for example, assertions such as "We are red parrots" and "Sorcerers are bush cats" violate the law of the excluded middle, which states that an object cannot be both itself and not itself. (*b*) Primitives are "uncultivated in following a chain of reasoning which is in the slightest degree abstract." (*c*) Primitives display a predilection for explaining occurrences, even minor, accidental ones, by reference to occult invisible forces. (*d*) Primitives are impervious to the evidence of experience.

Nothing in this descriptive account of the primitive mind distinguishes Lévy-Bruhl's position from that of his predecessor and antagonist Tylor. What did distinguish Lévy-Bruhl from 19th-century scholars such as Tylor and Frazer was his claim that primitive thinkers are neither stupid nor confused, but rather accept the authority of methods of thought so different from ours that our models of rationality are irrelevant for their evaluation and inadequate for their description. It was Tylor and Frazer who saw in the primitive mind, and especially in magical thinking, a rationality similar to our own but less developed.

Lévy-Bruhl held no such denigrating view of the primitive mentality. Quite the contrary. His position was explicit: (*a*) Primitives are perfectly capable of logical and scientific thinking; they have the requisite cerebral endowment. Obviously, I agree; I state, "Most normal adults have the capacity to think correlationally. . . ." (*b*) The intellectual procedures of the primitive mentality are *not* deficient applications of the canons of logic and science; they are *a*logical, not *il*logical. (Lévy-Bruhl used the term "*pre*logical" to describe the primitive mentality, and the term has plagued him; it is clear that he meant *a*logical.) Obviously, I disagree with him on this point; I state that "magical thinking is inductive in its intent but mistaken in its conclusions."

Lévy-Bruhl's notoriety comes from the claim that the canons that govern the primitive mentality are neither deductive, inductive, nor causal. Thus he held that the primitive mentality is so fundamentally different from the modern mentality that comparisons are meaningless. He would not have approved of my use of scientific concepts like correlation to assess everyday thought or of my portrait of the deficient rationality of the everyday mind.

There are severe difficulties with Lévy-Bruhl's position. One of these has to do with the consequences of prohibition of the application of the law of the excluded middle. The prohibition is tantamount to denying the fact-stating function of language (see Black 1963). As Black notes, it would be impossible to convey information if one could not draw inferences from empirical premises, e.g., if one could not validly draw an inference from the assertion "The book is on the table" to the conclusion "The book is not under the table." No society could exist under such whimsical communicative conditions.

Most post–Lévy-Bruhlian scholars have tried to preserve the relevance of logic for an appraisal of primitive thought. Thus they have tended to treat examples like "We are red parrots" and "Sorcerers are bush cats" as either metaphors (see Lévi-Strauss 1963*b*), parallel descriptive systems, one commonsensical and the other theoretical, of the "Table salt is sodium chloride" variety (see Horton 1967), or bad translations of ambiguous or polysemantic terms. Vygotsky (1962:71–72), for example, imagines that the Bororo have a polythetic category which can be used to refer either to themselves or to parrots (see my discussion of polythetic categories).

A second difficulty with Lévy-Bruhl's position is also to be found in the work of Tylor and Frazer. All three scholars were prone to compare the intellectual canons or ideals of the logician and scientist with the everyday intellectual performances of non-Western peoples. *At the performance level*, however, there is good reason to believe that even the Harvard undergraduate is "uncultivated" in abstract reasoning (see Wason and Johnson-Laird 1972) and impervious to the evidence of experience (see,

e.g., Chapman and Chapman 1967, D'Andrade 1974, Tversky and Kahneman 1974).

The theoretical consequence of the discovery of massive deficiencies in the intellectual performances of normal adults in our own culture has been a fascinating one. Not only have certain 19th-century evolutionary ideas that many had thought extinct reemerged and flourished among those who study the human mind, especially cognitive and social psychologists (e.g., Wason and Johnson-Laird 1972, Tversky and Kahneman 1974, Nisbett et al. 1976, Ross 1977), but the primitive mind studied a century ago by Tylor and Frazer has been theoretically transformed into the "everyday" or "intuitive" mind of normal adults in all cultures. What was once an arrogant and ethnocentric portrayal of the shortcomings of the primitive mind has today become universalized into a more humble and *self*-denigrating portrait of the limited rationality of normal adults in all cultures.

The following modern portrait of the "intuitive" (formerly "primitive") mind emerges: The intuitive mind is intentionally rational. One of its goals is to infer causes for the events it witnesses, to construct explanations, to understand what goes with what in experience. Yet despite its intentions the intuitive mind practices a deficient science (again, Lévy-Bruhl would definitely not approve). Its ways of acquiring, analyzing, and interpreting evidence are faulty. It would be fruitless to enumerate all the shortcomings of the intuitive mind. However, it might be helpful to consider a few examples:

1. Deficient data-gathering strategies: Wason and Johnson-Laird (1972) report a study in which college students sought to discover the experimenter's rule for generating the number series 2-4-6. Subjects were permitted to ask the experimenter questions; in particular, they could present him with any set of three numbers and he would tell them whether their number sequence could be generated by his rule. Most subjects quickly arrived at a private hypothesis concerning the experimenter's rule (e.g., integers increasing by two) and adopted a highly restrictive (and redundant) information-gathering strategy by which they sought to verify (not disconfirm or vary) their hypothesis. They did this by presenting the experimenter only with number trios compatible with their hypothesis (e.g., 1-3-5; 6-8-10), after some time confidently arriving at an inference about the rule (integers increasing by two). The hypothesis appears plausible given their own self-restricted sample of exemplars, but it was wrong. The rule was simply that the numbers get larger (e.g., .73-40-321 could be generated by the rule). Most subjects had difficulty discovering this simple rule, and these were highly educated college students (see Lancy's comment). How to go about asking informative questions of reality is not transparent.

2. Limited deductive skills: Wason and Johnson-Laird (1972) presented subjects with a series of cards, each of which had a number on one side and a letter on the other side, and asked them to name those cards that would have to be turned over to make sure that the rule "if the card has a vowel on one side, then it has an even number on the other side" had not been violated. Given the four cards K, E, 7, and 4, which of them must be turned over to make sure the rule is violated? The authors report that fewer than 5% of formally schooled subjects drew the proper deduction—"if a vowel then an even number (if p then q)" implies "if not an even number then not a vowel (if not q then not p)"; hence inspect E and 7. Most subjects inspected only E or E and 4 (although "if p then q" does not imply "if q then p"). Further, they had difficulty recognizing the source of their error. (An E card with an odd number on the other side is a rule violation, but so is an odd-number card with an E on the other side; hence also inspect the 7 card.) Wason and Johnson-Laird suspect (p. 193) that "formal operational thought is less general than Piaget supposed."

3. Faulty inductive inference procedures: I have presented judges with the following two abbreviated inductive arguments —"M.G. has self-esteem. Therefore he probably is not a leader."

/"M.G. has self-esteem. Therefore he probably is a leader."— and asked them which of these two arguments was the sounder. In order to assess the soundness of these arguments, a judge must implicitly contribute the missing premise, "Most people with self-esteem are not leaders" (see my table 2). Yet 85% of those subjects, who in fact believed that "most people with self-esteem are not leaders," erroneously selected the second argument.

This example is an interesting one because it highlights some of the difficulties we have estimating likelihoods and drawing inductive inferences. On the one hand, judges ignored relevant base-rate information on the frequency of self-esteem and leadership in the general population. This unwillingness to utilize base-rate information when making predictions and drawing inferences is quite widespread (see, e.g., Nisbett and Borgida 1975, Nisbett et al. 1976). For example, Tversky and Kahneman asked subjects to estimate the likelihood that the following person was either a lawyer or an engineer: "Jack is a 45-year-old man. He is married and has four children. He is generally conservative, careful and ambitious. He shows no interest in political and social issues and spends most of his free time on his many hobbies which include home carpentry, sailing and mathematical puzzles." Subjects were told that Jack came from a population containing 70 lawyers and 30 engineers or, alternatively, 30 lawyers and 70 engineers. The base-rate information had no effect on their judgments that Jack was an engineer.

On the other hand, judges seemed to rely on "availability heuristics" (see Tversky and Kahneman 1974) when drawing the erroneous inference "He probably is a leader" from the premise "M.G. has self-esteem." The "availability heuristic" refers to the fact that people often assess the "frequency of a class or the probability of an event by the ease with which instances or occurrences can be brought to mind." This heuristic device misfires badly whenever the conditions that influence the ease with which an instance can be brought to mind are not correlated with frequency information, which seems to be rather often. In the case of self-esteem and leadership, the introspections of judges on their own thought processes are revealing: "I can't picture self-esteem, but I can picture leaders, e.g., Roosevelt, Hitler, Napoleon. I can't think of any people in general noted for self-esteem. Everyone that is a leader has self-esteem." Almost all judges reported that they focused on "people who are leaders" and not "people who have self-esteem" when judging that self-esteem and leadership went together. "Presidents," "political leaders," "public leaders," "Kennedy," "Nixon," "Ford," and "some of my better teachers" were all mentioned. One judge noted, "Strong in my mind are social leaders, community leaders, people who can direct their affairs and those of others. Also moral leaders, e.g., priests and doctors, who act as cornerstones in helping others make personal decisions. Finally I think of my dad who is a medical director of a cancer prevention center and a leader of other organizations, president, chairman, etc." The concept "leader" seems to be more elaborated in American culture than the concept "person with self-esteem." There is more to say about leaders than people with self-esteem. Consequently, leaders are easier to think about; instances of leadership are easier to bring to mind.

A major point of all these so-called neo-Tylorian examples is that normal adults in all cultures lack many of the concepts required to arrive at a veridical understanding of what goes with what and what influences what in their environments. Normal adults in all cultures are inclined to organize information, draw inferences, and estimate likelihoods by means of self-defeating intellectual procedures, procedures which if applied would reduce their chances of answering such practical questions as "Did my fertilizing program increase crop yield?" "Will physi-

cal punishment curb my child's misconduct?" and "How effective is this therapy at controlling my aches and pains?"

A major conclusion of neo-Tylorian research is that man's potential for rational adaptive behavior, to the extent there is such a potential, cannot be derived from what we know about the deductive and inductive powers of the *individual conscious* mind. This conclusion prepares the way for the major question confronting neo-Tylorian researchers in the future. It is the question posed by Fiske (and asked in one way or another by most commentators on my paper): ". . . how does it happen that people adapt moderately well and manage to get along in their everyday lives?" It seems to me there are at least five possible answers. Each has merit in some instance and lacks merit in some other (see Shweder n.d.c). One or more of these answers is advanced by most of the commentators.

1. The notion that organisms become better and better fitted to the contingencies of their environments may have only limited relevance to human learning. Normal adults certainly adapt, but only with respect to their conscious understandings of the contingencies of their environment, and these understandings can be quite mistaken. Hence, human behavior is "not even approximately optimal with respect to the real world" (Simon 1957:199); that is, we don't adapt very well at all.

2. Adaptive processes don't require complex intellectual manipulations or the kinds of "higher" mental processing we often associate with "thinking" (and theorizing) as opposed to "perceiving." For example, one can literally see that an axe works to split logs. All that is required is the perception of occurrences that are contiguous in time and space. Formal operational and hypothetical thinking is not required. Imitation and the intelligent use of one's senses may be all that one needs to acquire valid knowledge of one's world.

3. It may be a mistake to attempt to derive mankind's potential for rational adaptive behavior from the deductive and inductive abilities of the *individual* mind. Intelligence may be more a property of organizations, systems, traditions, contexts, or task environments than a property of individuals. As Dawes (1976:10) remarks, and as Henry Ford apparently knew, we may be inclined "to confuse the cumulative technological advances of our society with the power of the single human mind. The fact that a lot of us with the aid of a printing press, telephone, and verbal communication can create a H-bomb does not mean that any of us singly can think very straight." (Also see Gellner 1973.)

4. It may be a mistake to attempt to derive mankind's potential for rational adaptive behavior from the deductive and inductive abilities of the *conscious* mind. Our intellectual shortcomings may be only a feature of our self-reflective and deliberate efforts at ratiocination. Unconsciously we may draw inferences and arrive at conclusions by means of relatively sophisticated intellectual procedures. It follows that the information-processing system that controls adaptive behavior is independent of the information-processing system that controls conscious explanatory behavior (see, e.g., Nisbett and Wilson 1977).

5. Everyday explanatory behavior may have very little to do with adaptation, prediction, and control. The activities that induce explanatory behavior in everyday life, the activities that produce "why questions," may only require that the uncanny be made to seem intelligible (see Geertz 1973:101) or that praise and blame be somehow allocated. Predictive accuracy and manipulatory control may not be relevant goals.

Let me now respond in detail to specific points raised in the very thoughtful commentaries on my paper:

Casagrande skeptically asks if we are "merely deluded" in our belief that "the behavior in ordinary social interaction of people we know well" is "predictable." If he is referring (as he seems to be) to our notions of the "probability that certain personality traits co-occur," the answer is yes. However, I suspect that there is something else in the back of Casagrande's mind

that gives him the feeling that he can make relatively decent predictions about behavior.

Personality theorists are concerned with predicting individual differences in one context from knowledge of individual differences in another context. There is no parsimonious way to do this. The world of behavior is not organized in the way envisioned by global trait theories of individual differences. The Whitings' (1975:163) recent cross-cultural evidence on children's social behavior is typical. In a comparison of the behavior of 134 children from six cultures, the highest level of cross-situational consistency for a system of behavior is .29 (Pearson r). For nurturant behavior, the consistency coefficient is .05. Knowing that a child is relatively more nurturant than others to his parents tells you nothing about whether he will be more nurturant than others to his peers. Because of the enormous discrepancy between the unparsimonious organization of individual differences, on the one hand, and our neat traitlike perceptions of individual differences, on the other hand, many scholars are investigating the ways the human mind "creates and maintains the [illusory] perception of continuity even in the face of perpetual observed changes in actual behavior" (Hayden and Mischel 1976:110; also see Jones and Nisbett 1972, Ross 1977).

What does Casagrande have in the back of his mind? What gives him the illusion that he can predict behavior? I can only guess. We are prone to draw inferences about a person from behavior observed over an inadequate sample of situations. Casagrande can probably predict with modest accuracy which of his colleagues will talk more at a faculty meeting, but his predictive success comes from having observed their behavior in *that* situation before. From knowledge of talking patterns at faculty meetings, I would not advise him to predict which of those colleagues will talk more with their wives at the dinner table. As many have noted, the best prediction one can make about a person's behavior in any situation is that he/she will do exactly what he/she did before in that situation. We also tend to overlook the influence of our own behavior (and expectations) towards another on that person's selection of a behavior to exhibit. Here, personality is confused with the consistent complementary shaping of behavior by means of antecedent acts (see, e.g., Kelley and Stahelski 1970). A good way to predict what someone will do next is to know what act has just occurred (see Raush 1965). Finally, Casagrande probably has a good deal of valid knowledge about social interactions. He knows, for example, that people he knows well talk more at cocktail parties than in libraries and touch more in bedrooms than in kitchens. That kind of knowledge, however, is not what personality theory is about. I hope sometime Casagrande will have a chance to take a close look at some of the evidence reviewed by Mischel (1968, 1973).

Casagrande's observation on the conduct of Trukese navigators is of the fourth variety of answer listed above. A combination of the second and third seems more likely to me. At the very least, it should be clear that one need not infer a "high order of intellectual functioning" from the mere existence of a culturally transmitted and inherited skill.

Finally, as far as I can tell I did not overgeneralize in my discussion of Papago folk definitions. That discussion (if not others) was a rather restricted one. I simply argued that "genuine contingent reasoning does not appear in the Papago folk definitions reported." This was done because at first glance the evidence might have been interpreted otherwise (one type of definition was labeled "contingent" by Casagrande and Hale). I never argued that the nonexistence of contingent reasoning in Papago folk definitions could itself be taken as evidence that the Papago are disinclined to think correlationally, although I have no reason to believe that the Papago are different from Smedslund's (1963) Swedish nurses or Ward and Jenkins's (1965) Columbia University students.

Fiske reveals the numerous theoretical options one has in

making sense of our illusory constructions of personality. When he describes thinking and explaining as "epiphenomenal," he opts for Answer 5 above. When he discusses what is essential for our "moment-to-moment" behavioral activity, he opts for Answer 2. When he discusses the role of labels in our evaluative judgments and personnel selections, he opts for Answer 1. His remarks suggest important areas for future research. I would simply note that although, as outside observers, we can distinguish between "plausible attributions" that make events seem intelligible and adequate explanations that have predictive validity, the distinction is not one that is readily admitted in everyday life. The everyday mind does not say "Oh, what does it matter if my explanation is false, at least it is plausible!" Rather, the everyday mind symbolizes by means of the rhetoric of applied science.

Greenstone asks to what "universe of cases" my analysis applies (also see comments by Casagrande and Heelas). The question is a crucial one, and it is a difficult one to answer. The evidence cited in my study is of two varieties, observational and experimental. The observational evidence (e.g., the reanalysis of Newcomb's data; also D'Andrade's 1974 study) concerns informant perceptions of what goes with what in behavior in the "real world" of individual differences. The experimental evidence concerns subject perceptions of the contingent or correlational relationship between such varied things as symptoms and diseases (Smedslund), cloud seeding and rainfall (Ward and Jenkins), and body parts and personality attributes (Chapman and Chapman). Some of the commentators complain that, on the one hand, the domain of personality is rather special (Casagrande) and, on the other hand, experimental evidence is contrived and artificial (Casagrande, Greenstone).

It seems to me that the burden of proof is upon the shoulders of these critics. Well-designed experiments mirror relevant aspects of reality while trying to reduce the confounding influence of irrelevant aspects of the experimental situation (which is admittedly hard to do). Ward and Jenkins discover that correlational thinking is more difficult to elicit when evidence is experienced on a trial-by-trial basis (i.e., as it approaches "real-world" conditions). Smedslund's study, however, clearly does not model reality. Most of the world is illiterate. Most of the world does not have the opportunity to take notes and keep written records on experience. But does not this make Smedslund's findings even more compelling?

What is it that is missed in the experimental situation, if anything? It is important that future research examine this question. Greenstone says "feedback," but what would he make of the Dorze farmer in Ethiopia who asserts that "if the head of the family does not himself sow the first seeds, the harvest will be bad" (Sperber 1974:3)? Doesn't the farmer have feedback, and isn't his experience in a domain other than personality? Again, what about that ringworm and fowl excrement? We know a good deal about the resiliency of paradigms in the face of contrary evidence (see, e.g., Evans-Pritchard 1937, Kuhn 1970, Barnes 1973, Ross 1977). How one processes (or fails to process) the feedback one gets is not dictated by the feedback itself.

There is another reason it is difficult to answer Greenstone's question concerning the "universe of cases" to which my analysis applies. A study by de Groot (discussed by Simon 1969) exemplifies the point (see Shweder 1977). Chess grand masters and neophytes were permitted a five-second viewing of 24 chess pieces and then asked to reconstruct the board. When the pieces were randomly placed, grand masters and neophytes recalled at comparable levels, levels consistent with the rather considerable evidence on the unimpressive short-term memory capacity of our species (i.e., between five and nine "units" of information). However, when the pieces were "positioned" as they might be in a chess game, the recall of grand masters increased to near perfection; neophyte memory remained unchanged. Thus the rules of chess transformed the forgettable placement of a piece

of wood on a checkered board into a memorable "move" in a meaningful activity. Is the performance of the grand masters to be interpreted as evidence that our short-term memory capacities are really not limited, or rather as evidence that the true limitations of the human mind can be augmented by a cultural inheritance like the rules of chess (see Answer 3 above)?

Central to Greenstone's comment is the notion of "bewitchment" (or false consciousness?). It seems to me that "bewitchment" is the standard state of affairs and that the experimental situation may be more like reality than many suppose. For example, nothing in our use of language requires that we develop everyday semantic theories about our own language. Nonetheless, all of us develop quite erroneous intuitions about the meanings of terms (e.g., "game"; see my quote from Wittgenstein), and all of us are quite prepared to report, to anyone who might ask, the semantic myth that (e.g.,) a "male sibling" is the prototypical instance of a "brother," despite the fact that in many instances of linguistic usage (e.g., "Has he been a brother to you?"; Ziff 1972:73) "blood" is simply irrelevant. Our everyday theorizing is typically alienated.

Finally, it does not seem to me that the relationship between "self-confidence" and "organizes games" is analytic. It is intelligible to assert "organizes games but *really* feels insecure around other people" (see Fiske's comment). What would Freud have done without such a possibility?

I hope Heelas will forgive me if I am not fully responsive to his comment, but much of it is premised on a misreading of my position. I did not assert, as Heelas claims, "that since people do not generally think in terms of strict correlations, they must be making recourse to relations of resemblance," nor did I ever say of the diagnostic inferences of those nurses or the folk definitions of the Papago that they evidenced "some degree of confusion between semantic, symbolic connexions and what is actually the case from a scientific viewpoint." What I did say was that the concept of correlation was a relatively complex second-order one that was not readily applied by the human mind, and that *under certain circumstances* (e.g., "as soon as events can be meaningfully linked to one another") it was confused with the concept of resemblance. (Obviously, this remark can only be made from the point of view of an outside observer who does draw the distinction.) I tried to be explicit. I stated that "magical thinking is not *equivalent to* noncorrelational thinking; rather it is *one type of* noncorrelational thinking. (Other types include the cognitive strategies just discussed, e.g., the focus on confirming cases.)" What I had just discussed was a series of experiments in which subjects manipulated correlation-relevant information in a noncorrelational *and* nonmagical way. There is nothing magical about failing to compare conditional probabilities or focusing only upon confirming cases when drawing a correlational inference, nor is there any relative virtue in the fact that these misuses of correlation-relevant information are based, as Heelas states, on "some degree of empirical evidence." A world seen through the lense of confirming cases can be just as distorted as a world perceived by means of resemblance. However, Answer 2 (above) certainly does make sense in many instances.

The comment from the Laboratory of Comparative Human Cognition (Cole et al.) is the result of a cooperative effort, perhaps one involving some division of labor. This may explain the discrepancy between what is said concerning those Yale rats, viz., "It is easy to see that co-occurrence and p (shock/tone) are poor predictors of response suppression," and the pattern of results displayed in tables 1 and 2. I chuckled when Cole et al. revealed that their evidence on the presence of correlational thinking came from a rat study. Then I took a closer look at their tables. My rank-order analysis of the data in their tables 1 and 2 (in my analysis, tied ranks were randomly broken by following the order of identification numbers in the tables) indi-

cates that the association between simple frequency of positive co-occurrence and response suppression is quite substantial ($r_s = .61$). Moreover, the frequency of positive co-occurrence is confounded with the degree of correlation (C) between shock and tone ($r_s = .72$). Cole et al. have presented evidence in which the strategy of focusing on the frequency of positive co-occurrence of two events happens to be highly predictive of the degree of correlational relationship between them. Although the data in tables 1 and 2 do not suggest that rats think correlationally on the Yale campus, it would certainly be interesting to find out how far one can get in the world focusing only on positive co-occurrences or confirming cases.

While we are on matters "of mice and men", I think a more relevant study is the one by Garcia and Koelling (1966; also see Seligman and Hager 1972). They tried to train laboratory rats (in this instance, 90-day-old Sprague-Dawley males) to avoid drinking water. Two kinds of drinking water ("tasty water" and "bright and noisy water") were crossed with two kinds of aversive consequences of water drinking (shock and nausea). The experimenters discovered that the rats could not learn to associate shock with the "tasty water" or to associate nausea with the "bright and noisy water." The rats were unable to link the occurrence of shock with their own efforts to drink the "tasty water" even when the act and outcome occurred together; they suffered shock and came back for more of that water. Neither frequency nor correlational information had much influence on the rats' aversive behavior. Certain prepotent connections (e.g., the link between the olfactory properties of the "tasty water" and nausea) were learned very easily, that is, with very few learning trials and despite long delays between the drinking episode and the subsequent nausea. Other connections (e.g., shock and "tasty water") were difficult to learn even with frequent contiguous pairings. What is prepotent for rats is typically established in the evolutionary history of their species and is genetically based. What is prepotent for humans is often established in the cultural history of one's group and is symbolically based. Both genes and symbols may cause an "organism" to fail to adapt rationally to the contingencies of a local environment.

There are some aspects of the comment by Cole et al. that I find puzzling. My references to thought are said to be "statements about beliefs, as cultural phenomena" rather than statements about "psychological processes." It is said that they do not give an account of the way information is processed and retained. I assume that Cole et al. distinguish, as I do, between psychological processes (i.e., *procedures* for gathering, organizing, and manipulating information to accomplish intellectual purposes such as remembering, drawing an inference, or making a prediction) and intellectual capacities. In my study I credit our species with both the capacity to think correlationally and a disinclination to do so. It should be clear that "correlation" as used in my study is not merely a label, but a processing concept. It refers to a particular way of organizing and manipulating information. In processing terms, "correlation" is sufficiently described as comparing the conditional probabilities derived from information about the joint occurrences and nonoccurrences of events. I am unable to make sense of the remark that statements such as "People rely on the perceived similarity among events to estimate their co-occurrence likelihood" or "People estimate the likelihood of an event by the ease with which instances can be brought to mind" are cultural beliefs, unless Cole et al. mean that they are the historically conditioned beliefs of Western psychologists and anthropologists. But the history of ideas does not seem to be what their comment is about.

Cole et al. seem to feel that the distinction between what one thinks about (task environments) and how one thinks is controversial. The distinction is not controversial; I would certainly grant that what one thinks about can be decisive for how one thinks (see Wason and Johnson-Laird 1972 for an example of this in the card experiment discussed earlier). But the distinc-

tion is incomplete. To understand thinking one must also study, among other things, who does the thinking. The "who" of my study is humankind. I am concerned, not with individual or cultural differences, but with a universal disinclination or *limit*. In discussing that disinclination I explicitly noted that "It is *possible* to get American college students to think correlationally, but the informational conditions [the "task environment" of Cole et al.?] one has to contrive to do so are quite unrepresentative of everyday life." It seems to me this is a crucial point. The relevant question for my research is not whether there is some task environment one can contrive to elicit correlational thinking (of course there is). The relevant questions are (a) What are we to make of the fact that such environments are difficult to arrange for and unrepresentative of everyday life? and (b) What are we to make of the fact that (e.g.) correlational thinking is not forthcoming in many environments in which it would be the adaptive response? One thing these questions suggest is that we must distinguish between intuitive and nonintuitive (spontaneous and nonspontaneous) concepts.

One can understand why certain universal concepts like object permanence, time, space, and all the other Kantian synthetic a priori domains have been referred to as "spontaneous." From the point of view of the learning history of an individual, these concepts seem to be acquired without any instruction. In some cases they seem to be merely "released" by experience, and by rather minimal amounts of experience at that. To label a concept "spontaneous" is not to abandon scientific investigation. It is rather to take seriously the fact that concepts vary both in the relative ease with which they can be attained and applied and in the kinds of learning inputs (e.g., explication, incentives) and environmental orchestration (e.g., the prominent display of negative instances) that are required for acquisition and application to occur. (The universality of a concept I take to be an indication of its relative acquisitional ease.) Adult mastery of calculus (a rather narrowly distributed "nonspontaneous" phenomenon) is not to be explained in the same terms as infant mastery of the concept of object constancy (a universal "spontaneous" phenomenon). The theoretical position advocated by Cole et al. has no place in it for the notion of a "disinclination" to think in a certain way. This is a major weakness.

Finally, I have little faith in the espoused "situationalism" or "environmentalism." Task environments do not typically have a consistent influence on the thought processes of those who "encounter" them, and they are notoriously difficult to define without reference to who is doing the thinking. For example, Newell and Simon (1972:54) discuss the problem of defining a task environment in problem-solving research as follows:

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 refused 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.

Whose "intellectual activities" are undergoing "adaptation in this example," the subject's or the researcher's? Task environments don't insist!

Not all features of thought are widely distributed across cultural communities. Lancy's remarks happily alert us to this fact. Horton (1967), for example, has tried to explain traditional African thought systems by reference to a rather special (and idealized) "intellectual predicament," the nonexistence of competing alternative perspectives (or theories). One feature associated with this "closed" predicament is the kind of inflexibility of belief mentioned by Lancy. Another is the absence of

self-conscious reflection about the rules of thought and explanation (i.e., the absence of formal operational thinking). Explicit rules, for example, those of logic, are necessary, Horton argues, only when choices must be made between alternative arguments, etc. (also see Levy 1973:268–69). Horton believes this absence of "reflective norms of thought" leads to a confusion of the explanatory and predictive functions of thought with emotional concerns. (Horton does not mention Piaget in his work, but it seems apparent that he is, somewhat surprisingly, trying to derive a number of features of formal operational thinking, e.g., self-reflection and the manipulation of abstract representations, from a distinctive condition of concrete operational thought, i.e., awareness of multiple perspectives.) Horton speculates that the "closed" predicament wanes with the advent of urban centers, literacy, and external trade. There are many who would probably argue (as Lancy does) for a formal-schooling effect. In most research in this area I find the variable "formal schooling" unspecified and therefore unilluminating, and I rather suspect that the various dimensions of thought that have been *conceptually* packaged by Horton do not *empirically* hang together. Nevertheless, Horton and Lancy have identified some potentially important parameters for investigating cultural differences in thought.

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