CHAPTER 3

UNITS OF ANALYSIS FOR THE DESCRIPTION AND EXPLANATION OF PERSONALITY

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I. THE NEED FOR UNITS OF ANALYSIS

Every science has a nomenclature that describes and defines its domain of study. Nuclear physicists talk about subatomic particles; chemists analyze molecules and compounds; and evolutionary biologists ponder genes, populations, and species. In what sort of language do psychologists describe and explain personality?

The list of the units of analysis used by personality psychologists is wildly diverse: cognitive styles, complexes, current concerns, dispositions, folk concepts, goals, instincts, interests, motives, needs, personal projects, plans, personal constructs, strivings, sentiments, themas, types, and values are a few (cf. D. M. Buss & Cantor, 1989; Emmons, 1989, this volume, chap. 20). The length and complexity of this list suggest that our problem concerns organizing and simplifying our potpourri of conceptual units rather than choosing among them.

This chapter suggests that the apparent diversity of the proposed units of analysis masks a unity underlying them and that this unity is captured by the term trait (Allport, 1937). I begin by examining the meaning of traits and suggest that the trait notion is virtually required for a systematic understanding of personality. I then review criticisms of the trait concept and suggest that these criticisms are not always well founded.
Next I distinguish between phenotypic and genotypic traits, and discuss the implication of this distinction for the twin tasks of description versus explanation and for the validity of self-assessments versus observer assessments. This leads to the view that, in the process of assessment, the genotypic/phenotypic distinction disappears. Finally, I consider whether a special unit of analysis is necessary to describe the uniqueness of individuals, and whether type language might be as useful as trait language.

II. TRAITS AS UNITS OF ANALYSIS

A. The Meaning of the Trait Concept

Traits are consistent patterns of thoughts, feelings, or actions that distinguish people from one another. The reader should note three features of the foregoing definition. First, traits can refer to thoughts, feelings, or behavior. This point is often overlooked by psychologists who define personality only in terms of consistent behaviors. Second, trait ascription invariably involves comparisons between people. If I say that Jones is obsessive-compulsive, I mean that he seems to have more intrusive thoughts and guilt feelings and demonstrates more ritualistic behavior than people in general. Third, for traits to distinguish people from one another, they must display some distinctive consistency. If Jones's obsessive experiences and compulsive activities diminish to the point that they are no more frequent than those of the general population, then they would no longer distinguish Jones from people in general.

Traits seem to be required for science of personality, because any science involves detecting and explaining consistent patterns (Hanson, 1958). Imagine trying to construct a science of chemistry if elements and compounds did not possess stable properties—if sodium chloride were only sometimes water soluble. If people had no stable properties (i.e., traits), they could not be studied scientifically.

B. The Situationist Challenge to the Trait Concept

Although a science of personality seems to depend on studying consistencies, the existence of traits has been questioned from World War I to the present (see Kenrick & Funder, 1988, for a review). It is primarily writers with a behaviorist orientation who doubt the existence of traits. They claim that behavior depends on social contexts and that consistencies are an artifact of a person being in similar situations (Ross, 1977). Stimulus–response behaviorism (Hendrick, 1977) seeks to identify stable S → R laws rather than stable properties of persons.

The claim that behavior depends on situational cues seems congruent with common sense. We behave differently at wedding receptions and funerals. We behave differently in the different roles we play, for example, as employee, spouse, or parent. These examples from everyday experience are consistent with the view that social situations determine our behavior. Behaviorists have confirmed this
common sense observation countless times in their laboratories, where they show that people's behavior responds to experimental treatments or manipulations (A. H. Buss, 1989). As further evidence for the power of situations, trait critics cite Mischel's (1968) claim that personality test scores (which presumably reflect traits) seldom correlate higher than .30 with behavioral criteria.

C. Responses to the Situationist Challenge

Despite the intuitive appeal of the behaviorist argument, their demonstrations fail to disprove the existence of traits for five reasons.

1. Reliable Situational Influence Requires an Enduring Capacity to Be Influenced

First, if situations reliably control behavior, then people must have a capacity to respond to situational cues, that is, the trait of being responsive to situations. This point has been recognized even by the most vocal critics of traits (e.g., Ross, 1977, p. 176): “For instance, in accounting for Jack's purchase of a house the 'situational' explanation (i.e., ‘because it was so secluded’) implies a disposition on the part of this particular actor to favor seclusion.”

2. Individuals Respond Differently to the Same Situation

Second, even in the most (allegedly) powerful situations, such as the Asch perceptual conformity paradigm, people's responses to the situation will differ as a function of personality (Barron, 1953). This point has also been acknowledged by trait critics (Ross & Nisbett, 1992).

3. Having a Trait Means Reacting Consistently to the Same Situation, Not Different Situations

Third, the fact that a person is inconsistent across different situations is completely irrelevant to the validity of the trait concept. This point is often overlooked even by trait defenders (e.g., Kenrick & Funder, 1988), who state that traits imply "cross-situational consistency." I regard the issue of cross-situational consistency as a red herring. Salt need not dissolve in benzene before we describe it as water soluble; likewise, persons need not exhibit identical behaviors in different environments before we can say they have traits. The trait concept implies consistent reactions to similar situations over time, not consistent reactions across different situations. Being characterized by a trait automatically implies the relevant type of situation; for example, cooperative means consistently complying with reasonable requests (Alston, 1975), not indiscriminately complying with others' wishes on every occasion.

4. Having a Trait Does Not Mean Your Reactions Are Absolutely Consistent

Fourth, for behavior to be consistent across time (temporal continuity) it need not be identical in every relevant situation. For example, a lascivious person takes
advantage of opportunities for sex more often than the average person, but does not attempt to copulate with every person he or she meets (Johnson, 1993). This point is missed by those who criticize trait measures for not predicting behavior in a single experimental situation. Proponents of traits never claimed that their measures could do this. Rather, trait measures predict trends in behavior over time (Epstein, 1983). A trait score is like a batting average. Knowing that a baseball player hits .300 does not allow you to predict what he or she will do in a particular at-bat, but does predict his or her performance over the course of a season.

5. Behavioral Inconsistency Does Not Rule Out Inner Consistency

Fifth, the lack of consistency in behavior over time does not rule out the existence of emotional or cognitive traits. An emotional trait, for example, may be rarely expressed because of conflicts with other emotional traits. A person might consistently desire to attend parties, but might do so infrequently due to a stronger consistent desire to work (Alston, 1975). Lack of opportunity can also prevent the expression of emotional and cognitive traits. A courageous person—that is, someone who could certainly overcome fear and act decisively in an emergency—may rarely be faced with emergencies in which he or she could actually be heroic. Cognitive personality traits (e.g., persistently attending to details) likewise might not be detected unless a person is given the opportunity to demonstrate the trait. Finally, Alston (1975) also points out that needs may be (like hunger) cyclical.

D. Phenotypic versus Genotypic Traits

Two forms of traits have been traditionally distinguished (Allport, 1937; MacKinnon, 1941): outer traits that can be directly observed (behavioral traits) and inner traits (emotional and cognitive traits) that must be inferred. Meehl (1956) borrowed from biology the terms phenotypic and genotypic to distinguish the two types of traits. Others (Weimer, 1974; Wiggins, this volume, chap. 4) use a linguistic metaphor. Behavioral traits are analogous to the surface structure of a sentence whose meaning is indeterminate, whereas inner traits represent the intention or deep structure that disambiguates (explains) the meaning of behavior.

Two common assumptions about the two types of traits are worth noting. The first concerns the view that outer traits are descriptions of behavior that need explanation, whereas inner traits are the causes or reasons that explain the outer traits (Alston, 1975; Johnson, 1990a; Wiggins & Trapnell, this volume, chap. 28). Second, observers are assumed to have privileged access to outer traits, and actors to inner traits. This access, in turn, is assumed to affect the validity of observer

1 Trait measures can also fail to predict behavior because they lack reliability or validity (Block, 1977). When reliable, valid measures are used, Mischel’s alleged .30 ceiling is easily broken (Hogan, DeSoto, & Solano, 1977).
ratings versus self-ratings of the two types of traits. Although both assumptions have merit, they are only partially correct, as I argue below.

E. Traits as Descriptions and as Explanations

To explain behavior in terms of traits—e.g., Joe hit Fred because Joe is aggressive—is sometimes criticized as description rather than a genuine explanation (Weimer, 1984). Trait critics often note the apparent circularity in statements such as “He acted aggressively because he is [behaviorally] aggressive” (Rholes, Newman, & Ruble, 1990, p. 371). However, to explain a single behavioral act as an exemplification of a behavioral trait is a valid account of an act (Wiggins, this volume, chap. 4). When I say that Joe hit Fred because Joe is an aggressive person, this implies that Joe’s behavior is not unusual for him and therefore requires no further explanation. Many nonscientists are satisfied with explanations such as “that's typical” or “that's his or her nature” (Young, 1975). People generally seek deeper explanations only for behaviors that are out of character or unusual.

Psychologists, on the other hand, also want to explain normal behavioral traits. One approach to this is to hypothesize inner emotional and cognitive traits that generate the behavioral traits. In many accounts, these inner traits form part of the basic level of theoretical explanation, as illustrated in the following (see Weimer, 1984):

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behavioral act, “Joe hit Fred”     fact             explained nonexplainer
         ↑                             ↑
behavioral trait, aggressiveness  law              explained explainer
         ↑                             ↑
inner trait, aggressive feelings   theory           nonexplained explainer
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In the Weimer (1984) model, “explained nonexplainers” are single events that require an explanation but themselves explain nothing. Isolated behavioral acts fit this category. “Explained explainers” are empirical regularities or laws used to explain single events, but require an explanation themselves. Common behavioral traits fall into this category. “Nonexplained explainers” are metatheoretical assumptions about nonverifiable, structural entities that explain empirical laws but are themselves beyond explanation. Unobservable, psychic structures represent a type of metatheoretical primitive.

The notion that emotional and cognitive traits underlie and explain behavioral traits is actually common sensical and “familiar to all of us since childhood” (Alston,

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2 Behavioral traits can also be invoked to help explain the reactions of an audience to the actor, that is, as part of a social-psychological explanation. For example, the poor morale and unproductivity of a team might be attributed to the ineffective behavioral traits of the leader.

3 Alternatively, what I might really mean when I say, “Joe hit Fred because Joe is an aggressive person,” is that Joe has hostile, aggressive urges or feelings. If this is the case, I am invoking “aggressive” as a motivational concept rather than a behavioral trait.
An example from Johnson (1990a) illustrates this. Suppose that Mary, a therapist, consistently treats her clients in an empathic manner. She uses reflective listening to promote accurate understanding, appears attentive and interested in her clients' problems, and often reassures and supports them. The trait empathic aptly describes her counseling style. Why is Mary consistently empathic?

A common sense explanation would refer to Mary's desires (emotional traits) and beliefs (cognitive traits). Presumably, Mary wants to promote her clients' psychological well-being. There are many ways to say this: Mary desires their well-being; she values their well-being; she has established their well-being as a goal. The precise term is unimportant—what counts is that Mary is motivated to promote her clients' well-being. Being motivated to achieve goals involves emotional processes (see Averill, this volume, chap. 21). Promoting the well-being of her clients makes Mary feel good whereas failing to achieve this goal would make her feel bad.

Mary's motives, desires, or goals provide only half of the explanation, however. We also need to know about her beliefs concerning how she can achieve her goals. Mary believes that treating her clients empathically will have positive outcomes for them. In short, Mary behaves empathically because (a) she wants to promote the well-being of her clients, and (b) believes that behaving empathically will help her enhance their well-being.

Should this seem too simple, I might note that behavioral traits often serve several goals simultaneously. Mary's empathic behavior may satisfy not only her need to promote her clients' well-being, but also her need to be liked and admired by her clients. She might also believe that her empathic style will lead to a good performance evaluation and a larger paycheck.

Furthermore, some behavioral traits may be habits that once promoted goals but now are no longer useful. Or, if the depth psychologists are correct, behavioral traits may express unconscious desires. Depth psychologists also suggest that conflicting motives can become fused and lead to a compromise activity not clearly linked to any one goal. Ethologists talk about displacement activities, (e.g., eating, self-grooming) that serve to relieve tension when conflicting instincts arise. Thus, there is not always a simple one-to-one correspondence between behavioral traits and underlying motivational or cognitive traits.

Even if one could identify all of the relevant motives and beliefs underlying a behavioral trait, some would regard this explanation as incomplete. One might further inquire into the origin of the motives and beliefs. Why does Mary want to be a therapist rather than a truck driver? How did she conclude that Rogerian therapy is more effective than Freudian therapy? The answers to these questions can be found in personality development (e.g., Eder & Mangelsdorf, this volume, chap. 9) and in evolutionary psychology (e.g., D. Buss, this volume, chap. 13; A. Buss, this volume, chap. 14). Developmental and evolutionary explanations complement intentional explanations (Wakefield, 1989).

Some psychologists feel that it is insufficient to explain behavioral traits in terms of goals and beliefs for yet another reason: "Scientific" explanations should transcend common sense (McCrae, Costa, & Piedmont, 1993). Theoretical physics
is often presented as a science that contradicts our common sense assumptions about the solidity of objects and the absolute nature of time and space. From this perspective, the most important concepts in personality psychology are not well represented in ordinary language. McCrae (1990) proposes openness to experience as an example of such a concept.

Hofstee, de Raad, and Goldberg (1992) warn us, however, about the dangers of stepping out of the area of common language: “There is nothing against this advice, as long as the pertinent outcomes do not have to be communicated in words” (p. 162). Cattell transcended ordinary language with terms such as Harria, Presemsia, Alaxia, and Protension; ultimately these terms had to be translated into common trait language to be useful to practitioners (IPAT Staff, 1979).

In summary, most psychologists regard “outer” (behavioral) traits as descriptions that need explanation, and they assume that “inner” (emotional and cognitive) traits generate and therefore explain outer traits. Behavioral traits or consistencies may be determined by the interaction of several emotional and cognitive traits. Some personality psychologists, in order to provide deeper explanations of inner traits, inquire into their genetic and physiological bases, developmental histories, and roots in the evolution of the human species. Whether a scientific conception of traits needs to go beyond ordinary trait language is a matter of current debate.

### III. Issues in the Assessment of Traits

#### A. Genotypic and Phenotypic Traits from the Perspective of the Self and Others

Self-ratings of traits correlate substantially (rs in the .4–.6 range—see Johnson, 1994) with ratings of the same trait made by others. Nonetheless, in individual cases, self-ratings sometimes disagree with ratings by other persons. This raises an interesting issue: When self-ratings disagree with ratings by others, whom are we to believe? The chapters in this handbook by Funder and Colvin (chap. 24) and Robins and John (chap. 25) discuss this issue in some detail. This section addresses the accuracy of ratings of genotypic versus phenotypic traits made by the self and others.

1. **Actors Can Directly Experience Their Own Inner Traits, but Observers Must Infer Others’ Inner Traits**

The outer/inner trait distinction is often assumed to affect the validity of personality assessment by self-ratings versus ratings by other persons. Self-assessment of genotypic traits is potentially more valid than other-assessment of these traits because people may directly experience their own inner traits whereas observers must infer them from verbal reports and nonverbal behavior. For example, anxious individuals should be aware of their anxiety level, whereas observers must infer their anxiety from signs such as a furrowed brow, sweaty palms, tremors, and hesitant speech style. Kenrick and Stringfield (1980) report that personality scores are more valid—
i.e., self-ratings correlate more highly with other ratings—for individuals who openly express their traits (see also Funder & Colvin, this volume, chap. 24). Funder and Dobroth (1987) found interjudge agreement to be higher for traits related to be highly visible.

2. **Observers Can Directly Experience Others' Outer Traits, but Actors Must Infer Their Own Outer Traits**

Individuals may have direct access to their inner traits, but they cannot directly observe their own behavior. This implies that external observers may provide a more valid assessment of a person's phenotypic traits. For example, people are typically poor judges of how charming they are, but this outer trait is obvious to an observer. Cooley's (1902) concept of the "looking glass self" suggests that our understanding of our own personality is determined by the feedback reflected from others around us, at least until we are able to take the perspective of others to imagine how we appear to them (Mead, 1934). Whether we listen to a real or an imagined audience, our knowledge of our outer traits is indirect and inferential.

3. **Defensiveness Hinders Accurate Self-Assessment**

Although the relationship between outer/inner traits and the validity of self- and other-assessment may seem intuitively compelling, it is incomplete. Individuals may be unaware, mistaken, or self-deceived about their inner traits (Averill, this volume, chap. 21; Paulhus & Reid, 1991; Paulhus, Fridhandler, & Hayes, this volume, chap. 22; Robins & John, this volume, chap. 25). Aspects of personality that are unknown to the individual but known to others are found in the "blind area" of the Johari window (named after Joe Luft and Harry Ingram; see Luft, 1966). The Johari window is illustrated in Figure 1. In the case of blind spots, observers provide more accurate assessments of inner personality than the person observed.

Individuals may also be reluctant to describe all their inner traits as they actually perceive them, preferring instead to describe traits that they would like others to believe they have. Paulhus and Reid (1991) call this process impression management. Inner traits that are not described to others are the "secret area" of the Johari window.

![Known to Self Unknown to Self](table)

<table>
<thead>
<tr>
<th>Known to Others</th>
<th>Known to Self</th>
<th>Unknown to Self</th>
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<tr>
<td>Public Area</td>
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<td>Blind Area</td>
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<td>Unconscious Area</td>
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*Figure 1* The Johari window.
The Johari window and Paulhus' research on social desirability responding suggest that self-reports of inner traits may be more accurate than observer assessments, but in the case of self-deception the converse is true. We may also have "secret areas" that we choose not to reveal.

4. Self-Observer Agreement Requires a Common Understanding of Language

Accurate knowledge of one's inner traits and a willingness to share this with others are still insufficient to ensure agreement between self-description and description of the self provided by others. Full agreement also depends on the actor and observer using the same semantic and pragmatic rules for communication. For example, a person rating himself or herself for thoughtfulness will provide an inappropriate rating if he or she believes thoughtful to mean considerate but the rater believes the term means contemplative. Misunderstandings of the meaning of trait terms occur more often than researchers may realize (Goldberg & Kilkowski, 1985).

When we assess personality with questionnaires rather than rating scales, we encounter pragmatic as well as semantic misunderstandings. Pragmatic rules are implicit social conventions about meaning that can vary across subcultures who share the same language. The impact of pragmatics on measurement validity is virtually unexplored, but I can cite two illustrations here.

First, Johnson (1997) notes that item 77 on the California Psychological Inventory (CPI; Gough, 1987), “When I get bored I like to stir up some excitement,” is a phrase used by delinquents who relieve their boredom by doing something illegal. For this reason, Johnson (1997) interprets the item as reflecting (lack of) conscientiousness. McCrae et al. (1993) apparently preferred a more literal interpretation and they judged the item to reflect extraversion. We do not know how a typical respondent interprets this item.

We do know that people who focus on the narrow, literal meaning of words often miss the broader social implications of personality items and therefore respond inappropriately (Johnson, 1993). A punctual and conscientious person who answers “false” to the item, “I am never late to appointments,” because he or she thinks never means literally never has missed the point of the item. Pragmatic rules—i.e., our social conventions of language use—allow punctual people to say, “I am never late,” because we know this really means, “I am a conscientious person who is rarely late.” Similarly, pragmatic rules suggest that an intellectual person should answer “true” to CPI item 152, “I read at least ten books a year,” even if he or she reads only three books a year.

The point of these examples of pragmatic ambiguity is as follows. People might be perfectly aware of their actual dispositions to be delinquent, extraverted, conscientious, intellectual, and so forth, and also be quite willing to acknowledge these dispositions through the items discussed above. Nonetheless, their responses to these items will convey valid information only when the test taker and test constructor interpret the item response in the same way.
5. Observer Ratings of Outer Traits Are Valid Almost by Definition

Whereas observer ratings of another person's inner traits are only rarely more valid than self-ratings, observer ratings of outer traits are usually more accurate than self-ratings of outer traits. An exception might be a case where an observer is prejudiced against the person he or she is rating. But on the whole, outer traits are—almost by definition—whatever impressions an actor makes on observers. If an individual is perceived by others as a loudmouth, then by definition that person is a loudmouth. This implies that observer ratings constitute an "ultimate criterion" of sorts for validating self-reports of outer traits (Hofstee, 1994).

Outer traits are social constructions of reality (Berger & Luckmann, 1966). From a social constructivist perspective, the "actual" traits that are assigned to a person are whatever the majority of observers believe should be assigned. Hogan and Briggs (1986) refer to the social consensus as a person's reputation. A person's view of his/her own reputation may be as correct but not more correct than his/her reputation as constructed by the social group.

To summarize, self-ratings of one's inner traits tend to be more valid than observer ratings of those traits except in cases of blind areas (self-deception) or secret areas (impression management). Conversely, observer ratings of one's outer traits (reputation) are almost always more valid than self-ratings of outer traits. People must have highly developed perspective-taking skills to describe accurately the way they appear to others (Mills & Hogan, 1978).

The fact that individuals may not provide valid self-ratings in the blind and secret areas presents problems for those of us who wish to assess personality with questionnaires and self-rating scales. How does one address this problem?

One possible solution is to identify subtle items, that is, items with less-than-obvious psychological significance or implications. Different responses to subtle items covary empirically with individual differences along a trait dimension for reasons unknown to the test taker and sometimes even the test constructor. Subtle items can be found through brute, dust-bowl empiricism. Unfortunately, research has demonstrated that subtle items are almost invariably not valid (Johnson, 1993). Valid self-assessment of blind and secret areas remains a challenge for personality researchers who are attempting to build better mousetraps.

6. Controversy Surrounds the Assessment of Unconscious Traits

The Johari window contains one more pane we have not discussed: the unconscious area. Unconscious traits are the foundation of psychoanalytic theories, but are often ignored by mainstream personality psychologists working within the cognitive Zeitgeist (Hogan, 1979; Weinberger & McClelland, 1990). Some might argue that unconscious traits, because they are unseen by the self or others, are not amenable to scientific study. Others would counter that unconscious traits are like nuclear particles. We cannot see these particles, but they leave traces in cloud chambers.
and certainly have a palpable impact on us. Unconscious traits similarly cannot be
directly observed, but leave traces of their activity and have an impact on us. The
question is whether we can devise the equivalent of a cloud chamber or Geiger
counter to assess unconscious traits.

Although clinicians often rely on their own intuition to access unconscious
traits (Reik, 1948), some modern researchers claim that projective tests constitute
a cloud chamber for the unconscious. Projective tests are simply stimuli (inkblots,
photographs, sentence fragments) with open-ended response options. Rather than
responding true or false, respondents can say or write as little or as much as they
like. Their responses are then scored according to a set of rules to yield an evaluation
of the respondent’s level on various unconscious needs or motives.

An influential review paper by Entwistle (1972) cast serious doubts on the
reliability and validity of projective tests. Undaunted, McClelland and his colleagues
(McClelland, 1980; Weinberger & McClelland, 1990) marshalled further evidence
for the reliability, validity, and utility of projective measures. McClelland also
replaced the usual psychoanalytic framework for projective testing with an ethologi-
cal framework. Summarizing research on projective measures in applied settings,
Hogan (1991) concluded that these tests are about as valid as objective measures.

Scores on projective tests tend not to correlate with scores on objective tests
measuring the same construct (Weinberger & McClelland, 1990). This finding led
McClelland (1980) to assert that projective and objective tests measure two different
kinds of traits. Specifically, he suggested that projective tests tap a more primitive,
biologically based, affect-laden type of trait, whereas questionnaires assess a more
cognitive, symbolic type of trait. The Weinberger and McClelland (1990) chapter
reviews studies indicating that projective and objective measures predict different
types of activities.

Is McClelland correct to argue that qualitatively different kinds of traits exist
and that we need different kinds of measures to assess these types of traits? That
is the question addressed next.

B. Trait Measurement through Questionnaires

1. Do Different Questionnaires Measure Different Kinds
   of Traits?

The Edwards Personal Preference Schedule (EPPS; Edwards, 1959) purports to
measure Murray’s needs, Gough’s (1987) CPI allegedly assesses “folk concepts,” the
Guilford–Zimmerman Temperament Inventory (GZTS; Guilford, Zimmerman, &
Guilford, 1976) obviously aims to measure temperaments, and the Myers–Briggs
Type Indicator (MBTI; Myers & McCaulley, 1985) is supposed to capture the
cognitive functions in Jung’s psychological types. Do these four tests actually mea-
sure four different kinds of traits?

According to McCrae and Costa (1989; McCrae, 1989; McCrae et al., 1993;
Piedmont, McCrae, & Costa, 1992) the answer to this question is clearly no. McCrae
and Costa have demonstrated that the scales on these inventories assess the same five traits measured by their own NEO-PI (Costa & McCrae, 1992): Neuroticism, Extraversion, Openness to Experience, Agreeableness, and Conscientiousness. In fact, McCrae and Costa have ingeniously and systematically demonstrated that virtually every major inventory assesses some or all of the “Big Five” or Five-Factor Model (FFM) traits.

A look at actual items on these inventories also suggests that they are not measuring different kinds of traits. Consider the following items: “I like to plan and organize the details of any work that I have to undertake.” “I always see to it that my work is carefully planned and organized.” “You like work that requires considerable attention to details.” “When you start a big project that is due in a week, do you (a) take time to list the separate things to be done and the order of doing them, or (b) plunge in?” “I like to follow a strict routine in my work.” Can you tell which item measures a need, which measures a folk concept, and which measures a cognitive style?

All the items indicate a planful and serious-minded approach to work. But anyone unfamiliar with these inventories would likely be unable to tell that they came from the EPPS, CPI, GZTS, MBTI, and NEO-PI, respectively. The empirical and semantic overlap in the items across these instruments does not imply that needs, folk concepts, temperaments, and cognitive styles are identical concepts. Nonetheless, the questionnaires seem to be measuring similar, if not identical, constructs. It the items reflect patterns of thoughts, feelings, or actions, one might as well use the generic term trait to describe what they measure.

I think it is pointless to worry about conceptual distinctions between trait constructs if they are measured in identical ways or if scores from different measures behave in similar ways (e.g., predict the same criteria equally well). Kilkowski (1975), for example, provides an interesting six-page analysis of the conceptual differences between Allport’s traits and Murrays’ needs. But he does not describe different methods for measuring traits and needs.

2. Do Questionnaires Measure Phenotypic or Genotypic Traits?

A closer look at the five items listed above shows that two refer to actual planful behavior and three refer to a liking for organization. Might it be important to distinguish phenotypic (outer behavioral) items from genotypic (inner cognitive or emotional) items? Angleitner, John, and Löhr (1986) and Werner and Pervin (1986) report that different inventories contain different proportions of phenotypic and genotypic items. They then assert that item characteristics may affect test validity,

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4 Emmons (this volume, chap. 20) also has hinted at the futility of trying to distinguish allegedly different goal constructs from each other. As he points out, the statements in Table 2 of his chapter representing four purportedly different goal constructs—current concerns, personal projects, life tasks, and personal strivings—are very similar.
but neither research team examined whether phenotypic or genotypic items were superior for predicting nontest criteria.

Johnson (1993a) examined the ability of phenotypic and genotypic items on the CPI to predict acquaintance ratings. He found that phenotypic items predicted extraversion ratings better than genotypic items. In domains other than extraversion, however, reference to outer or inner traits was not related to validity. I think genotypic versus phenotypic wording is unrelated to validity because genotypic tendencies normally find phenotypic expression. For example, people who want to get ahead eventually act in ambitious ways; conversely, people who act ambitiously normally have ambitious motives. Thus, to endorse the genotypic item, “I have a strong desire to be a success in the world,” is tantamount to endorsing the phenotypic item, “I do whatever I can to get ahead” (and vice versa).

Because personality questionnaires simultaneously assess phenotypic and genotypic traits, I have found it useful to think about personality in terms of a trait construct that incorporates both levels of personality: **self-presentational style.** Self-presentations are any behaviors (including responses to questionnaire items) guided by inner traits that create impressions in others. I believe that all noncognitive questionnaires assess self-presentational styles (Johnson, 1981; Mills & Hogan, 1978). It does not matter whether the test is intended to measure moral reasoning (Johnson & Hogan, 1981a), vocational interests (Johnson & Hogan, 1981b), attitudes (Johnson, Hogan, Zonderman, Callens, & Rogolsky, 1981), or philosophical world views (Johnson, Germer, Efran, & Overton, 1988); responses to these various inventories create a distinctive impression on those who see the responses.

Self-presentation of traits that are already well known to everyone (the public area of the Johari window) is direct and straightforward (Wolfe, 1993). In the public area of personality one can take item responses at face value. If someone endorses the item, “I am rarely late for appointments,” we can accept that this person is punctual. I agree with Wolfe (1993) that personality assessment via questionnaires proceeds in a straightforward fashion in most cases, even in contexts such as personnel selection (Hogan, 1991).

Nonetheless, self-presentation on questionnaires—like social behavior in everyday life—contains both conscious, intentional and unconscious, unintentional elements. This implies that we cannot always take item responses at their face value; ultimately we must determine, in an empirical fashion, what an item response means (Meehl, 1945). In particular, we cannot trust item content when blind, unconscious, or secret aspects of personality are being assessed.

Clearly, persons cannot disclose blind or unconscious traits by endorsing items whose content describes the trait. An overly critical person who is unaware that he or she is overly critical cannot validly respond to an item such as “I am overly critical.” What is needed is an item that allows an observer to infer the trait. A more oblique item, such as “Spare the rod and spoil the child,” might be endorsed by overly critical but unaware individuals. In everyday interactions, perceptive observers can make inferences about blind or unconscious areas from another person’s slips of the tongue or body language; in questionnaires we depend upon
nonobvious empirical correlates of personality items to reveal information beyond
the manifest content of the item.5

During normal interaction we know that people do not always tell the truth.
We may, therefore, watch for signs of dissembling such as laughing nervously,
averting the eyes, and touching one's face. Questionnaire items do not give us the
nonverbal cues to detect dissembling, but over the years researchers have developed
various techniques for detecting intentional misrepresentation. Items on dissembling
keys often contain "unlikely virtues" (see Gough, 1987; Tellegen, in press)—they
describe behaviors that are socially desirable but unlikely to be literally true (e.g.,
"I have never told a lie"). Interpreting these dissembling scales is problematic,
however, because people exaggerate their virtues in everyday life as well as on
questionnaires, and unlikely virtue scales predict nontest behavior (Johnson, 1990b).

This section has argued that all personality questionnaires measure a trait
I call self-presentational style. Do other assessment modes such as cognitive
tests (Emmons, this volume, chap. 20) and projective tests also measure self-
presentational style? I believe so. Although the format of cognitive tests differs
from questionnaires, I believe that persons who endorse statements such as those
found in Table 2 of Emmons's (this volume, chap. 20) chapter will create a distinctive
impression on others. Whether scores from the measures of cognitive style and
needs are empirically distinguishable from each other and from ordinary personality
questionnaire scores remains to be seen.

I am less certain about projective tests. Gough (1948) long ago argued that
responding to projective tests involves self-presentation. More recently, however,
Weinberger and McClelland (1990) have argued that scores on projective tests are
uncorrelated with scores on objective tests because responses to the two types of
tests are generated from two different parts of the brain. I think it is important to
link units of personality analysis to neurophysiology, but I also think much more
data will be required to forge this link. Until then I will stand by my view that
responses to all forms of personality assessment involve self-presentation.

IV. ALTERNATIVES TO TRAITS

A. Units for Capturing Uniqueness

Because each person is obviously unique, psychologists occasionally suggest that
we should use special units of analysis designed to capture the uniqueness of
individual personality. This position, called the idiographic approach, contrasts with
the nomothetic view that we should compare individuals with a common set of units.

I believe that the idiographic–nomothetic issue concerns how detailed our
descriptions are rather than what kind of units we use to describe personality.

5 Sadly, however, the track record for subtle items is very poor (Johnson, 1993b). We simply
have not been very successful at designing subtle but valid items.
Ordinary traits are perfectly capable of describing what is unique about us. Consider the definition of traits as patterns of consistent thoughts, feelings, or actions that distinguish persons from one another. Traits, by definition, describe how we differ from one another, and the sum of these differences defines our uniqueness.

I think the real objection of idiographic psychologists to nomothetic trait description is that a limited number of trait dimensions (e.g., the Big Five; see Section III.B.1) fail to capture the richness and complexity of a unique person. Indeed, the Big Five, even broken down into six facets each (Costa & McCrae, 1992), cannot describe everything about someone's personality. But to think that the Five-Factor Model or any other model of personality can completely describe a person is to misunderstand models in science (Holt 1962; Rosenblueth & Wiener, 1945). A useful model is, by definition, a simplification: it retains only the important features of the infinitely complex domain it represents (Eckhardt, 1979). The precise number of important traits is still a matter of debate, but we cannot expect any of our limited models to capture every detail about personality.

B. Types versus Traits

One final possible unit of analysis in the study of personality is the type construct. The notion of personality types is nearly as complex as personality traits, and I will not examine all of these complexities. Grant Dahlstrom (1972) has written a definitive monograph on the meaning of type. I also recommend articles by Gangestad and Snyder (1985, 1991) and by Paul Meehl (1992). Rather than reviewing these works, I will discuss two properties of types that are most often cited as distinguishing types from traits: their holistic character and their discrete character. To anticipate my conclusion, I believe that, in practice, the trait and type concepts are actually almost indistinguishable.

1. Are Types Holistic?

In previous writings, I have stressed the holistic nature of the type concept (Hogan & Johnson, 1981; Johnson & Ostendorf, 1993). I conceptualized types as constellations or patterns of traits that naturally co-occurred in persons. My metaphor for a type was a chemical compound composed of simpler elements. Types, like compounds, possess emergent properties, that is, properties not found in the traits (elements) taken by themselves—e.g., hydrogen and oxygen do not resemble water. If types have emergent properties, then the holistic adage, “the whole is more than the sum of its parts,” applies.

In an important paper, Mendelsohn, Weiss, and Feimer (1982; see also Weiss, Mendelsohn, & Feimer, 1982) provide a persuasive empirical and conceptual argument against the holistic conception of types. If their argument is correct, and it seems to be, properties of types can be derived from an understanding of the properties of the traits that make up the type, not from an emergent configuration of trait properties. For example, the type notion “extravert” can be broken down into the traits of gregariousness, impulsivity, and excitement-seeking, and all that
is predictable about extraverts can be traced to these traits. And so on for all the other type concepts.

If type concepts are not holistic and we reconceptualize them as collections (rather than configurations) of traits, types become nearly synonymous with broad traits. Consider the “Big Five” traits assessed by Costa and McCrae’s (1992) NEO-PI; each trait is decomposed into narrower facets. These facets can be further decomposed into more specific thoughts, feelings, and behaviors described by individual items. So perhaps my metaphor that contrasts types as compounds with traits as elements is misleading. It may be more accurate to say that types and traits vary on a continuum of breadth.

2. Are Types Discrete?

The second alleged difference between types and traits is that types are discrete or discontinuous, whereas traits exist along a continuum of values. This may be true at a conceptual level, but at the level of assessment and application, this distinction vanishes (Hofstee & de Raad, 1992). To apply typologies in a continuous manner, one need only describe a person’s degree of resemblance (on as finely a graded a scale as one desires) to as many type constructs as one desires.

3. But Are Traits Continuous?

At a conceptual level, traits exist along a continuum of values. However, at the level of measurement, a person’s score on a trait questionnaire does not necessarily indicate the degree or amount of the trait possessed. In the words of Meehl and Hathaway (1946), “simply counting how many responses . . . have been made seems to be very crude; . . . [our mathematical scaling procedures] should not mislead us into supposing that we are doing anything very close to what the physicist does when he cumulates centimeters” (p. 557).

In actuality, the number of keyed responses endorsed by a person yields a probability statement about whether the positive or negative pole of the trait concept applies to him or her. This point is most clearly seen in the case of scales constructed empirically by contrasting the responses of two criterion groups (e.g., schizophrenics versus normals). If one has a very high score, it becomes more probable that we should apply the (type!) label “schizophrenic.” But a score at the midpoint does not necessarily mean that the person is moderately schizophrenic. This may be true, but technically an average score means that the probability of correctly labeling the person (as normal or schizophrenic) approaches zero. Average scores on scales constructed by rational means or internal consistency are also ambiguous. An average score on a rationally constructed scale of, say, sociability may indicate either (a) a moderate amount of sociability or (b) uncertainty about the applicability of the labels “unsociable” or “sociable” (see Baumeister & Tice, 1988).

In practice, personality test users often convert trait scores into type categories. And, contrary to popular belief, one can do this without losing much information.
Hofstee and de Raad (1992) explain:

An optimistic estimate of the proportion of true trait variance of a personality scale, after subtraction of both unreliable and method variance, is 0.5, giving a standard error of measurement of 0.7. So, a standard score would have to be below -1.4 or over +1.4 to be significantly \( p = .05 \) different from 0. To trichotomize a population into extraverts, introverts, and neither accordingly would be quite realistic in view of the large error of measurement. (p. 62)

Real-world decisions about persons are almost always binary (yes–no) or categorical (friend–foe). This means that trait scores are usually transformed into categorical terms. Consider a programmer’s task of deciding whether a particular personality description should be triggered in a computer-generated personality report (Johnson, 1996). Either the score is high (or low) enough to print the narrative paragraph or it is not. Consider an employer hiring people on the basis of personality test scores. Either their scores are sufficiently auspicious to hire the person or they are not. The same situation occurs when counselors decide what careers to recommend or which therapies to administer to clients.

I conclude, then, that the trait concept, interpreted as a facet of a person’s self-presentational style, serves as the best unit of analysis for personality research. In applied settings, however, the real world forces trait continua to be treated as discrete types.

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**REFERENCES**


In a recent review of the field generally designated "personality assessment," I rendered the optimistic albeit highly qualified opinion that visible signs of progress could be discerned in this still youthful science (Wiggins, 1973). Such a conclusion is considerably more sanguine than that reached a decade earlier by Vernon (1964), or more recently by Mischel (1968). My disagreement with Vernon involves little more than a tendency on my part to view glasses as half-full, rather than half-empty. My differences with Mischel are more profound. In a classic Rashomon-type example of individual differences in perception, Mischel and I observed the same events, but provided quite different narratives. This disagreement is all the more striking when it is realized that the two observers appear to share many of the same conceptual biases (e.g., the importance of generalizability, utility analysis, and explicit theoretical bases for assessment).

Different perspectives on fields as broad as personality assessment frequently arise from stylistic differences in taxonomic behaviors. Categories may be broad or narrow, concrete or overinclusive. In this respect, Vernon (1964) exhibited a preference for categories of broad width when he lumped together decision making, psychoanalytic theory, psychotherapy, descriptive psychiatry, and all things "clinical" within a single and particularly unwholesome bin (Wiggins, 1964). Mischel (1968) erred even more in the direction of overinclusion when he categorized such...
diverse offenses against human nature as psychodynamic theory, factor analysis, and the medical model as all belonging to a monolithic "trait" conspiracy.

Quibbling over taxonomic niceties should generally be left to textbook writers and others who continue the scholastic tradition. But there is more at stake in the present instance. Conclusions stemming from Mischel's broadband view of the "trait construct" have had an extraordinary impact on our field. Behaviorally inclined clinicians appear to be celebrating a decisive victory, a blitzkrieg, so to speak, that defined and destroyed the enemy almost simultaneously. Psychodynamically oriented clinicians, long used to being "sold up the river," to use Holt's (1958) phrase, must now bear the added humiliation of having multivariate-trait psychologists as traveling companions.

Mischel's (1968) textbook, and his subsequent writings along similar lines (Mischel, 1969, 1971, 1973a, 1973b), have had a considerable impact on the field of personality in general, and personality assessment in particular. His views have not gone unchallenged, and there is a still-growing literature of criticisms directed at one or another facet of his arguments (Alker, 1972; Averill, 1973; Bowers, 1973; Craik, 1969; Wachtel, 1973a, 1973b; Wallach & Leggett, 1972). Yet Mischel's writings have tended to polarize his readership into a relatively homogeneous group of satisfied social behaviorists and social psychologists on the one hand, and a highly heterogeneous and most dissatisfied group of clinicians, psychometricians, and personality theorists on the other.

The reason that Mischel's writings have had such a diffuse effect is that his arguments themselves are diffuse and multipronged. At one level, he is challenging the field of personality as traditionally defined and the field of personality assessment as it has traditionally been implemented. At another level, he is extolling the virtues of social-behavioral conceptions, as opposed to psychodynamic and other nonbehavioral views. And at still another level, he is arguing for the utility of certain methods of behavior modification and control for both practical and theoretical purposes. When considered one at a time, these are each complex issues worthy of debate. However, when considered in toto, they appear as a shifting myriad of targets against which it is difficult, if not impossible, to take aim. For example, if one must assume that the recent triumphs of some clinicians in reducing or eliminating fears of snakes in their clients reflect unfavorably on the psychometric adequacy of multivariate personality inventories, then one is doomed to argue from a position of weakness and bewilderment.

To bring the issues into clearer focus, I would like to consider the trait concept: (1) as it is used, or could be used, in personality measurement and assessment, and (2) as it is used, or should be used, in a theory of personality. The two sets of considerations are not unrelated. The most modest attempts to quantify personal characteristics should be guided by explicit theoretical considerations; the grandest theories must eventually be assessed with reference to concrete measurement procedures. With this in mind, I will first attempt to discredit the growing philosophical skepticism that has been expressed regarding the existence of traits, and then suggest the place that trait measures may have within a theory of personality.
I. THEORIES AND VIEWPOINTS

For a number of reasons, it is convenient to consider (the field of) “personality” as the general psychology of individual differences (Wiggins, Renner, Clore, & Rose, 1971). In attempting to account for (the fact of) individual differences, a variety of viewpoints must be brought to bear on a common subject matter. At the least, it would seem that the methods and concepts of biological, experimental, social, and psychometric-trait approaches are necessary for providing a complete account of human differences. Pitting one approach against another can result in such fluid controversies as the ancient “heredity versus environment” issue or the recently revived “trait versus situation” debate.

When one approach to knowledge is compared with another, the game is not zero sum. Whether from genuine conviction or from the observance of good form, it is common to concede that other approaches have virtues as well as limitations, triumphs as well as failures. Nevertheless, while the biological, experimental, and social approaches have all recently had their days in the sun, the psychometric-trait approach has fallen upon extremely bad times. The possibility exists that the methods and distinctions of the trait approach have simply outlived their usefulness in comparison with recent advances in other viewpoints. I personally do not believe this to be the case. Instead, I believe that a widespread discontent with certain theories of personality has resulted in an attempted purge of concepts essential to the psychometric-trait viewpoint in personality study. The distinction between viewpoint and theory is critical here.

A viewpoint is an approach to the empirical study of personality that is based on assumptions concerning the importance of certain kinds of constructs and that advocates the use of certain methods of observation and measurement (Wiggins et al., 1971). The term “viewpoint” is used in place of “method” to emphasize that methods involve constructs and that they impose constraints upon observations. A theory is an extended construct system of broad range and scope that typically attempts an integration of constructs from several viewpoints.2

I believe it fair to say that the viewpoints of personality study are reasonably “established” in their own right, because they represent traditional and respectable areas of psychological investigation (biological psychology, experimental psychology, social psychology, and psychometrics). Theories of personality are, of course, another story. And it is important to note at the outset that although a theory of personality may achieve a certain prestige by emphasizing a particular viewpoint, the methods of the viewpoint cannot be substituted for the propositions of the theory. Bowers (1973) has made this point in reference to the misidentification of S-R theory with the experimental method:

2 This distinction between viewpoint and theory is similar to Fiske’s (1971) distinction between mode and perspective.
As it happens, the experimental method as generally employed is differentially sensitive to the impact of situational variables, and correspondingly insensitive to organismic variables. . . . However, the experimental method does not, so to speak, comment on this differential sensitivity; it is simply a procedure for acquiring a controlled observation. Thus, independent–dependent variable relationships are metaphysically neutral. This is not the case for their S-R counterparts, which do carry a great load of metaphysical freight. (p. 309)

The converse is also true. Just as a method cannot be used to justify a theory, a theory can be discredited without discrediting the method that it espouses. A discreditation of S-R theory is no reflection on the experimental method. However, I believe it to be the case that the psychometric-trait viewpoint has recently been judged guilty in virtue of its association with certain personality theories. The fact that trait-like or dispositional concepts are so ubiquitous in personality theory should not make one particular interpretation of traits subject to the criticisms of other interpretations. Traits are many things to many theorists, and it is precisely this conceptual plurality that has provided a composite straw man for those who have criticized trait measurement.

One of the chief concerns of the psychometric approach to personality study is the development of quantitative procedures for the measurement of human tendencies (pronenesses, proclivities, propensities, dispositions, inclinations) to act or not to act in certain ways on certain occasions. These tendencies are not "postulated," they are accepted from common sense as expressed in ordinary language usage. If persons are not more or less prone to behave in certain ways on certain occasions, then the psychometric approach is out of business at the outset, as are all approaches to personality study.

Because of its concern with human tendencies as expressed in ordinary language, a case could be made for the psychometric-trait viewpoint being propaedeutic to other approaches to personality study. However, as I hope to make clear later, the closeness of the psychometric-trait approach to the obvious subject matter of personality study need not imply, and in fact should not imply, that trait concepts will figure prominently in our eventual systematic accounts or explanations of that subject matter. It seems more likely that theoretical explanations of human tendencies will emerge from the social, experimental, and biological viewpoints.

In the material that follows, I hope to illuminate the nature of the trait concept by considering the ways in which trait terms are employed in everyday discourse. But to show that a consistent and meaningful account of traits is provided by ordinary language usage is not to show that a scientific account of traits is easily achieved. A number of additional steps are required, the first of which has to do with the specification of measurement procedures. However, although the current state of psychometrics may be primitive in comparison with measurement in the physical sciences, it is clear from reading Fiske's (1971) recent book that the primary obstacle to measuring the concepts of personality has been conceptual rather than mensurational. Within the field of personality, there appears to be greater agreement concerning how concepts should be measured (e.g., Fiske, 1971) than on what
concepts should be measured (e.g., Levy, 1970). Thus, achievement of a working consensus on the nature of the trait concept would be a large first step toward a psychology of personality.

Most previous discussions of the trait concept in psychology have focused on traits as attributes of persons. Thus, the first task of psychometrics has been considered to be the development of scales and inventories to measure the “tendencies” of persons to act in certain ways on certain occasions (e.g., Edwards, 1970). But this logically presupposes a clear conception of which particular actions are to be accounted for on what particular occasions. To say that a person is “aggressive” is to say that the person has behaved or is likely to behave “aggressively” on certain occasions. But what is an aggressive action? And how would we quantify the “aggressiveness” of an action?

I intend to distinguish among statements expressing: qualities of actions (“John pushed the boy aggressively”), (2) properties of persons (“John is aggressive”), and (3) aspects of future occurrences (“If frustrated, John is likely to behave aggressively”). I will argue that the first statement conveys an institutional fact; the second, a categorical summary of the general trend of a person’s conduct to date; and the third, a hypothetical proposition that is inferred, but not deduced, from statements of the second type. In the specialized terminology of the psychometric-trait approach to personality, these distinctions correspond roughly to those sometimes made among “observation,” “assessment,” and “prediction.”

II. TRAITS AS ATTRIBUTES OF BEHAVIOR

“John pushed the boy” describes an action or sequence of behavior. “John pushed the boy hard, repeatedly, and for a long time” qualifies the description in terms of qualities which may be thought of as attributes of the action, rather than of John, or of the observer. The observer is making judgments here (How hard is “hard”? How often is “repeatedly”? How long is “long”?), but the normative basis for such judgments can easily be made explicit. The important point is that the descriptive qualities tell how John pushed the boy, not why John pushed, nor why the observer described John’s actions in these terms.

“John pushed the boy aggressively” qualifies the description of the action, but in a different way. Its meaning is not synonymous with the description yielded by the use of primary attributes (amplitude, frequency, duration). Nor can the quality of aggression be recorded by mechanical devices in the absence of a human observer. Nevertheless, the qualifier (“aggressively”) should not automatically be relegated to the domain of emotive responses (evaluations) simply because an observer is involved. One can describe John pushing aggressively, affectionately, or playfully in a thoroughly dispassionate manner. That, of course, is what is meant by an “objective” observer.

The description of John’s pushing as “aggressive” does not, in ordinary usage, refer to John’s intentions. As Anscombe (1963) put it, “We do not add anything
attaching to the action at the time it is done by describing it as intentional” (p. 28).
The description “aggressively” tells how John pushed the boy, not why John pushed,
nor whether the action was “voluntary” or “involuntary.” The latter distinction
typically arises in connection with actions that result in improprietous outcomes
(Ryle, 1949, pp. 69–74). We may determine whether a person meant or intended
to do something (wrong) by inquiring whether he was competent to do it right (and
failed to do so) or by establishing whether or not external factors prevented him
from performing properly. But it is the description of the present action as “aggres­
sive” that establishes its character, and subsequent attempts to excuse or condemn
John for taking that kind of action represent a distinctly different line of inquiry.

The characterization of John’s pushing as “aggressive” does not refer to John’s
disposition to perform aggressive acts in this or other circumstances. The word
“aggressively” modifies the verb (pushed) and not the subject (John). The sentence
“Quite uncharacteristically John gave the boy an aggressive push” makes good
sense and conveys both that the act was aggressive and that John is not. True, if John
continues to shove people around, we might wish to revise our original appraisal of
his aggressiveness. But the direction of inferences is from act to disposition and
not the converse.

Could it be that the action under consideration is described as “aggressive”
because of its relation to certain antecedent conditions? It is true, for example, that
aggression may follow frustration and in that sense be “provoked.” Thus, it might
be the case that “aggressive” refers to a class of actions which are likely to follow
actions or circumstances that are harmful, insulting, or frustrating to the actor. But
the lawfulness of the implied R-R relation does not seem strong enough to enable
us to avoid frequent miscategorizations. Provocations may be (and often are) met
with a smile, ignored, or submitted to. More damaging to the “antecedent” account
of aggression, however, is the fact that the term “aggressive” is applied to topographi­
cally dissimilar actions that follow submissive, generous, dominant, affectionate,
or almost any conceivable kind of action. It is, of course, possible to distinguish
“provoked” and “unprovoked” aggression. But such an inquiry into the reasons
for an action does not illuminate the qualities that made that action “aggressive”
in the first place.

If an action is not classified as “aggressive” on the basis of antecedent events,
perhaps it is classified on the basis of consequent events or outcomes. Let us try:
An act is described as “aggressive” if and only if it results in (is followed by) the
harm, injury, discomfort, or ridicule of another. This basis for discriminating the
attribute of aggressiveness has one clear advantage over the antecedent event
account: topographically dissimilar actions (pushing, hitting, swearing) are encom­
passed by a single term, “aggressive,” which connotes a common property. But the
requirement that a specific outcome must occur is too strong. Not all aggressive
actions result in harm or injury. If John takes a swing at the boy with a meat axe
and misses, the action is still unambiguously “aggressive.”

Thus far, I have argued that the sense of the attribute of “aggressiveness” is
not to be found in: (1) primary qualities of the act, (2) evaluative responses of the
observer, (3) intentions of the actor, (4) tendencies of the actor, (5) the conditions antecedent to the act, or (6) the immediate consequences of the act. What is left? Clearly, we have not exhausted all possibilities; but rather than pursue additional false leads, it seems appropriate to state what I believe is meant when a trait quality is attributed to an action: the action belongs to a class of actions that are likely to lead to a particular outcome.

The "outcomes" at issue here are social in nature. They may be characterized by such phrases as "being harmed, injured, discomforted, or ridiculed," "being praised, admired, revered, or lauded," and "being influenced, directed, persuaded, or restrained," considering the likely effects of aggressive, deferential, and dominant actions, respectively. But how do we know these things, and what is it that we know?

Searle (1969, pp. 50-53) has proposed a useful epistemological distinction between "brute facts" and "institutional facts." Brute facts are, roughly, those objects, relations, and primary qualities that lend themselves to direct observation and with which the natural sciences are concerned. In contrast, institutional facts do not stand on their own, but presuppose the existence of certain human institutions. These institutions are systems of constitutive rules of the form "X counts as Y in context C".

Consider the following institutional facts reported in a newspaper: "In the closing minutes of the game, pass interference was called in the Redskin's end zone and the Dolphins won by a score of 6 to 0." Clearly, the existence of the facts recorded in this statement presupposes the existence of the institution of football, an institution that furnishes the rules that impart a special meaning to the various brute facts of the game. One rule is of the following form:

"An action (pushing) that is likely to prevent a receiver from catching the ball (X) counts as interference (Y) in the context of the rules of football (C)."

Similarly, the statement "John pushed the boy aggressively" contains an institutional fact defined by the following:

"An action (pushing) that is likely to harm or injure another (X) counts as aggressive (Y) in the context of the rules for classifying the consequences of social actions (C)."

To designate trait qualities as institutional is not to imply that they are (merely) conventions instead of (actual) facts. Trait attributions are conventions about reporting facts.

As psychologists, we are perhaps less interested in the ontological status of institutional facts than we are in the origins and functions of the rules which, in ordinary usage, provide definitions of those facts. It seems likely that trait terms were coined to express the law-like relations that have been observed between certain kinds of human actions and particular classes of social outcomes. Given the variety and complexity of actions that may result in the same social outcome, it is not possible for single individual to learn, by direct experience, even a fragment of these regularities. Trait attributions convey the shared folk wisdom concerning actions and outcomes in an extraordinarily efficient manner. Because the truth of a trait attribution is not defined by a particular immediate outcome, the sense of
the law-like relation conveyed is "probabilistic" rather than deterministic. Indeed, it would be a surprise—if not an embarrassment—if the laws of common sense psychology were stronger than those of scientific psychology.

One final point regarding traits as attributes of behavior: most psychologists like to think of actions that share a common attribute as constituting a "response class." On an observational level, we have defined trait attributes as response classes that have common environmental effects. Hence, our definition should be acceptable to groups as divergent as ordinary people, Skinnerians, and trait theorists. As we move from behavior observation to conceptualization, however, we find that these groups diverge in their views. Both Skinnerians and trait theorists impose additional requirements beyond the level of a single observation. In addition to requiring that members of a response class share the attribute of a common environmental effect, Skinnerians require that the actions enter into the same functional relationships with "controlling" stimulus conditions. The additional requirement imposed by trait theorists is that members of a response class exhibit significant covariation within a group of individuals. Ordinary people could be talked into either conceptualization since the level of discourse is that of psychological theory and not that of common sense. But agreement as to what trait attributes are on an observational level is not to be treated lightly. It suggests that we all know what trait attributes are, and that they "really" exist.

III. TRAITS AS ATTRIBUTES OF PERSONS

Having identified the conditions under which trait qualities are ascribed to behavior, the manner in which trait terms are used to describe persons should be fairly evident. To say, "John is aggressive," is to say that in certain circumstances, John has behaved, or is likely to behave, in a manner likely to result in harm, injury, or discomfort to others.

But which is it: "has behaved" or "is likely to behave"? The "is likely to behave" account is the classical argument of Gilbert Ryle, who maintained that traits are dispositions which function as law-like inference tickets. The "has behaved" account is a refutation of the classical argument which maintains that trait attributions are summarizing statements that do not commit the speaker to conditional predictions (Hampshire, 1953). The two views are sufficiently disparate to warrant separate consideration.

A. Traits as Causal Dispositions

Ryle (1949) maintained that trait attributions to persons function in the same manner as dispositional statements in physics. Thus, to say "John is aggressive" is akin to saying "The glass is brittle" or "The sugar is soluble." In this sense, all trait statements are hypothetical propositions that convey law-like relationships. The status of dispositional concepts, as applied to objects, is reasonably clear: "To
be brittle is just to be bound or likely to fly into fragments in such and such conditions" (Ryle, 1949, p. 43). Thus, the statement "The glass is brittle" can be verified with reference to a bilateral reduction sentence (Carnap, 1936) of the following form:

If a case $x$ (the glass) satisfies the test condition $S$ (being struck with a stone), then $x$ is an instance of $C$ (brittle) if and only if $x$ shows the response $R$ (shatters).

According to the dispositional view, when we say that John is aggressive, we are asserting that *it is a good bet that in certain circumstances, John will behave aggressively*. But the form of the subjunctive conditional and the conditions for its verification are different, in several respects, from those involved in the meaning and verification of dispositional statements applied to physical objects:

1. The "good bet" (Ryle's words) indicates that the subjunctive conditional is probabilistic in form, rather than "if and only if." This is not damaging to the position since it could be argued that all predictions are probabilistic, or certainly all predictions of human behavior. There is, of course, the question of how good the bet has to be before we will make it, but that question is problematic for any account of the prediction of behavior.

2. The "in certain circumstances" clause embraces a much larger set of conditions than is the case with physical objects. Whereas brittleness is a single-tract disposition that can be defined in terms of a reduction sentence involving a single, sufficient occurrence (shattering), traits convey an indefinite series of hypothetical propositions. It is not clear which of the many possible circumstances should serve as the test condition for John's aggressiveness, nor is it clear whether disconfirmation of the hypothetical proposition in a specific instance could be discounted on the grounds that the wrong circumstance had been selected.

3. The criteria by which we could decide that John had "behaved aggressively" are not as evident as shattering in the case of brittleness. Ryle insists that all trait words are dispositional words, and thus makes no provision for actions which may be described as "aggressive actions" (Powell, 1959). In his view, actions may be described as exercises of John's aggressive disposition in the same way that speech acts may be described as exercises of John's knowledge of French. But just as there are no brittle occurrences, there are no aggressive actions. Although the concept of a physical disposition avoids circularity by specifying a manageable number of criterion responses, this is not so easily achieved with reference to the concept of a trait disposition.

**B. Traits as Categorical Summaries**

Hampshire (1953) contends that trait attributions do not involve hypothetical or quasi-hypothetical statements. Instead, trait attributions are summarizing statements that describe the general trend of a person's conduct to date. The claim of the dispositional statement is of the form, "So far, the word aggressive is the right word to summarize the general trend of John's conduct."
Hampshire’s arguments rely on a distinction between statements expressing causal properties and categorical statements that merely summarize. For Ryle, statements about human dispositions are statements expressing causal properties that can be restated in terms of reduction sentences of the “if . . . then” form. For Hampshire, statements about human dispositions are summarizing statements that do not comment on causality. Such statements may also be applied to material objects:

... one may often choose, or may be compelled by ignorance, to summarize the general character of some physical things, rather than to describe their behavior in terms of their physical constitution and of the laws which govern the behavior of objects so constituted. (Hampshire, 1952, p. 7)

Examples of such statements would be “It tends to rain in Vancouver” and “This river tends to overflow its banks.”

Hampshire emphasizes three bases for distinguishing summarizing statements from causal statements: (1) Summarizing statements imply that manifestations of the disposition have occurred in the past; causal statements do not: sugar may be soluble without ever having dissolved. (2) Summarizing statements imply that a disposition manifests itself more or less continuously over some period of time; causal statements do not: “being electrically charged is a property which may be switched on and off” (p. 8). (3) The manifestations of dispositions described by summarizing statements are various and indeterminate (John’s aggressiveness may be manifested in a multitude of behaviors). Manifestations of causal dispositions are specific and determinate (shattering and dissolving).

If, in ordinary usage, the statement “John is aggressive” conveys that John has been observed to engage in topographically dissimilar aggressive actions over a period of time, then the advantages of Hampshire’s summarizing statement account over Ryle’s causal statement account are evident. Therefore, it seems worthwhile to consider at least the more obvious objections that might be raised to the implications just stated.

That the manifestations of a trait are heterogeneous and indeterminate is already conceded in Ryle’s account. But could we, or would we, describe John as aggressive on the basis of his repeatedly performing a single act in a given situation? The disposition to perform a single act in a single situation is ordinarily referred to as a habit rather than as a trait. In the case of aggressive behavior, such an action might even be referred to as a tic or mannerism.

Would it make sense to assert that John is aggressive if John had never been observed to perform an aggressive act? Brandt (1970) does not see such an assertion as a contradiction. He argues: If we knew a person had lived a sheltered life and had never been required to act courageously, we would not infer that the person cannot be courageous. Further, “there are conceivable psychological tests such that, given a certain result on these tests, we would say that the person is probably a courageous person” (p. 26). The fact that such an assertion is probabilistic “shows something, not about the meaning of courageous (or other trait-names of interest
to us), but about our convictions on what is adequate evidence for trait-ascriptions” (p. 26).

It is readily conceded that if we never observed a person in a situation in which a manifestation of a trait would have been expected or likely, we cannot conclude that the person does not have the trait. Nor, of course, can we conclude that the person has the trait. We simply do not know. The “conceivable psychological tests” argument is more likely to appeal to philosophers than to those of us familiar with the grim realities of psychological testing. But to show that such probabilistic inferences might be made, in principle, is not to give an account of the ordinary usage of trait terms.

The major argument that Brandt (1970) puts forth to show that the summary view is “simply wrong” is based on the fact that trait inferences may be made on the basis of a single act:

But how could we draw such an inference, with high confidence, from any amount of information about a single situation if trait-affirmations were assertions about the frequency of behavior in the past? (The present behavior is, of course, one case; but to say that a person is courageous is surely not to say merely that he has acted courageously once). (p. 26)

There are circumstances in which a trait inference may be drawn on the basis of a single action. At issue are the relations that may exist between the dispositional assertion (D), the single action just observed (A₁), and past actions that are heterogeneous manifestations of the trait (Aₙ). Table I outlines the assertions and negations that may be made under different sets of conditions. For simplicity, the single action just observed (A₁) is not considered a subset of past actions (Aₙ), although both are members of the larger set of all possible trait manifestations (Aₙ).

The first statement in Table I asserts that John is aggressive (D), that John pushed the boy aggressively on this occasion (A₁), and that other instances of John’s

| Table I |

<table>
<thead>
<tr>
<th>Possible Assertions about a Disposition (D), an Action Just Observed (A₁), and Past Actions That Are Manifestations of the Disposition (Aₙ)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. Typical assertion</td>
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<tr>
<td>2. Typical negation</td>
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<tr>
<td>3. Implausible</td>
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<tr>
<td>4. Contrary disposition</td>
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<tr>
<td>5. Special circumstances</td>
</tr>
<tr>
<td>6. Out of character</td>
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<tr>
<td>7. Change of character</td>
</tr>
<tr>
<td>8. Single occurrence</td>
</tr>
</tbody>
</table>

*Overbar denotes the opposite connotation; for example, D lacks the disposition under discussion.*
aggressiveness have been observed in the past ($A_n$). The second statement asserts that none of these is true. The normalcy of both these statements is recognized by Brandt (1970, pp. 25-26). The third statement is considered by Brandt to be "not contradictory," but, on the evidence he offers, is labeled implausible. At best, one could argue that John is "potentially" aggressive, with all the attendant difficulties in disproving such an assertion.

In the fourth statement John is said not to be aggressive, even though he just pushed the boy, and even though he has acted aggressively in the past. Such a statement can be true when the speaker is able to cite a variety of incidents which serve as evidence of a contrary disposition: "the final and conclusive argument must be a balancing of one set of actual incidents against another set of actual incidents" (Hampshire, 1953, p. 6). In this example, it is assumed that accounts of John's loving, cooperative, and pacifistic behaviors in the past are weighed more heavily than accounts of his aggressive actions.

In the fifth statement John is said to be aggressive, in light of his past history of aggressive actions, even though he did not push the boy on this occasion. Such an apparent exception to trait attribution rules does not pose a problem for either the causal-dispositional or summary view. Ryle (1949) distinguishes tendencies from capacities: "tends to" implies "can" but is not implied by it (p. 131). Hampshire (1953) considers the possibility of an exception "part of the force of calling statements of disposition summarizing statements" (p. 7). But neither position provides an explanation of this apparent exception. If there is good reason to believe that John is aggressive (e.g., his past actions) and he does not push the boy aggressively, we could appeal to some special circumstances such that John "would have" pushed the boy aggressively were it not for those circumstances. However, in the case of desirable or socially sanctioned actions (not pushing), we are unlikely to seek "excuses," "justifications," "extenuations," and the like (Austin, 1957).

In Statement 6, John is said to be not aggressive, in light of the lack of aggressive incidents in his past, even though he pushed the boy on this occasion. Since the action in this case may be considered reprehensible (pushing), excuses or justifications are likely to be sought. Although special circumstances could be cited (e.g., extreme provocation), the best explanation (defense) would seem to be John's record. In light of the lack of aggressive actions in his past, it could be argued that John's pushing was an action out of character. This is not to infer, as have some moral philosophers, that character "causes" actions (Pitcher, 1961). Rather, it is to recognize the possibility of uncharacteristic actions in virtue of the argument that although D is dependent on $A_n$, $A_1$ is independent of D (Powell, 1959). That is, although our characterization of John as "aggressive" requires past incidents of aggressive actions, we may classify John's pushing the boy as "aggressive" independently of John's disposition to be or not to be aggressive. It is not clear how an aggressive action could be performed by a nonaggressive person within Ryle's account (Powell, 1959).

In Statement 7, John does not push the boy nor is he said to be aggressive, even though there are incidents of aggression in his past. When an aggressive person
fails to perform an aggressive action, special circumstances may be invoked. But, as in the present case, when a person with a history of aggressive actions fails to behave aggressively and is called “not aggressive,” the possibility of a change of character may be entertained. Although the “Contrary disposition” account would be more compelling, people do change, or at least we revise our opinions about them. However, this explanation cannot be invoked too often: “Character may change suddenly; but it must not change suddenly too often, or it ceases to be character” (Hampshire, 1953, p. 6).

The eighth statement is the one that Brandt considers damaging to the summary view. On the basis of a single action (pushing), and in the absence of previous aggressive actions, the disposition to be aggressive is attributed to John. Although such inferences may not be drawn “often,” as Brandt puts it, they may, on some occasions, be drawn. The issue is whether or not the inference is consistent with the claim of a summarizing statement: “So far, the word aggressive is the right word to summarize the general trend of John’s conduct.”

Clearly, a dispositional inference could be in the form of a summarizing statement if the act in question (e.g., pushing) was the first and only action ever observed. But Brandt is concerned with the situation in which previous (and presumably extensive) observations were not occasions of trait manifestations. He appears to be arguing that John was (latent?) aggressive all this time, but that the disposition had not been previously brought into play. This might be analogous to a glass that has always been brittle, though never struck, and therefore never shattered.

If we were to unpack all, or some, of the hypothetical propositions implied in the statement that John is aggressive, we would have to conclude that the circumstances of the subjunctive conditionals (e.g., “If John is frustrated . . . ”) had never before been satisfied. That one condition was satisfied on this recent occasion, and that John did perform an aggressive act (however that might be determined within the causal-dispositional framework), is apparently sufficient to justify a trait attribution, in Brandt’s view. And such an attribution is indeed inconsistent with a summary view of dispositions since no hypothetical propositions are implied by that view, and since the attribution would not provide an accurate summary of conduct to date.

Uncontrived examples of the borderline case considered by Brandt do not immediately come to mind. The research of Megargee and his associates (Megargee, 1966; Megargee & Mendelsohn, 1962; Megargee & Menzies, 1971) may provide one such example. Briefly, Megargee has studied the histories and personality characteristics of prisoners who committed extremely assaultive crimes (e.g., murder), moderately assaultive crimes (e.g., battery), or nonassaultive crimes (e.g., robbery). Both case histories and psychological test data suggested that a substantial proportion of prisoners who committed extremely assaultive crimes were less aggressive, more controlled, and less likely to have committed previous offenses than other prisoners. Newspaper accounts of extremely assaultive crimes also tended to corroborate these findings: “In case after case the extremely assaultive offender proves to be a rather passive person with no previous history of aggression” (Megargee 1966, p. 2).
Both Megargee’s research and newspaper accounts of such “puzzling,” “sense­less,” and “shocking” crimes are fascinating, or at least, extremely interesting. It is informative to ask why this is the case. Would an adherent of the causal-dispositional view answer, “It is amazing that these aggressive persons should have been so sheltered from circumstances that would provide occasions for the expression of their violence”? Perhaps. But newspaper readers, the psychologists who conducted this research, and adherents of the summary view would answer, “It is amazing that such heinous crimes could be committed by such nonaggressive persons.”

It is important to note that a “change of character” does not seem to have been involved since psychological testing and behavior observations made after incarceration still revealed a picture of a nonaggressive personality. Murder is, of course, an aggressive action. But murder can be committed by nonaggressive persons, at least according to the summary view.

To opt for the categorical-summary view over the causal-dispositional account is not to deny the importance of Ryle’s original distinction between dispositions and tendencies on the one hand and episodes and occurrences on the other. When we say that John is aggressive, we are asserting that the general trend or disposition of his conduct, to date, has been to engage in a variety of aggressive actions over a period of time. John’s “aggressiveness” is not something that occurs over and above, or under and below, his aggressive actions. It is not something that occurs for short or long periods of time, in a real world or in a transpatial world. Nor, and here is where we depart from Ryle, is John’s aggressiveness the cause of, or reason for, his aggressive actions. In addition to appearing closer to ordinary usage, the categorical-summary account carries less metaphysical freight.

IV. TRAITS AS PREDICTORS OF BEHAVIOR

A. Prediction in Everyday Life

We can, and on occasion do, use our knowledge of persons’ past actions as a basis for predicting their future behavior. But the extent to which such predictions are made in everyday social transactions has probably been exaggerated. According to George Kelly’s (1955) model of The Human as Scientist, persons are almost continuously engaged in gathering data, erecting hypotheses, and subjecting hypotheses to test by prediction. Although this model has provided a heuristic metaphor for psychological research, it should not be interpreted literally. As Little (1972) has noted, to say that all persons are scientists (or predictors) smacks of academico-mimesis: “Everyone is just like me.” Some individuals may spend significant portions of their lives predicting the future behavior of others, but others may be mainly concerned with reflecting on the past, or pondering the present.

The philosophical view of persons as in a constant state of readiness to unpack the conditional predictions entailed by their trait attributions seems equally academ-
icomimetic. If you were to assert that John is aggressive, and I were to ask you what you meant by that, you would probably cite corroborative incidents of aggressive actions from John's past. It seems unlikely you would reply that you intended to convey the proposition that if John were in such and such a situation, then John would act in such and such a way. The equating of the meaning of a statement with the method of its verification is a philosophical language game, not an ordinary discourse game. If you and I were psychologists, we might conspire to contrive a situational test of John's aggressiveness, but ordinary people seldom engage in such practices. Nor would ordinary people be bound to agree that the original attribution was false if the experiment yielded negative findings.

B. Prediction in Personality Assessment

There may be unresolved questions concerning the extent to which persons engage in predictive behaviors in ordinary life, but there is little question that prediction is the major professional activity of the applied personality assessment psychologist (Wiggins, 1973). Since the principal charge leveled against the trait construct in recent times has been that of a lack of predictive utility (Mischel, 1968; Peterson, 1968), it is instructive to consider what kinds of behaviors, and in what circumstances, trait measures would be reasonably expected to predict.

Although there is still room for refinement of current psychometric trait measures, the relatively poor showing of such measures in predicting behavioral criteria may very well reflect inappropriateness of the criteria rather than shortcomings of the predictors. This line of reasoning has recently been pursued by Martin Fishbein and his associates (Fishbein, 1973; Fishbein & Ajzen, 1972, 1974) in the context of attitude–behavior relationships. Fishbein and Ajzen (1974) have demonstrated that whereas dispositional measures are relatively poor predictors of single acts, they are substantially related to criteria based on multiple acts in varied circumstances.

Jaccard (1974) has recently extended this line of reasoning to the evaluation of traditional trait measures. He assembled a set of multiple-act criterion measures of dominance that included behaviors likely to be performed by dominant persons in a variety of circumstances. His subjects were also administered Gough’s (1957) CPI dominance scale, Jackson’s (1967) PRF dominance scale, and a single, self-rated scale of dominant tendencies. The average correlations of the trait scales with individual dominant behaviors were barely .20 and were not statistically significant. In contrast, the correlations of the trait measures with the sum of the multiple acts were close to .60 and were highly significant. If we choose, as did Mischel (1968), to express the relation between trait and behaviors as a “personality coefficient” based on the average relation between a trait and single behaviors, then we are likely to obtain coefficients of disappointing size. But if we choose to express the relation as the correlation between a trait measure and a criterion of multiple acts in multiple situations, there is good reason to believe that the magnitude of validity
coefficients will be similar to those found by Jaccard (1974). And such a choice would be consistent with a summary view of traits.

A psychometric measure of a trait should reflect the general trend of a person's conduct to date. In assessing a single individual, the trait measure should be based on items that provide a broad and representative sampling of relevant acts in multiple situations. Most prediction in personality assessment is nomothetic, however, and thus we must devise measures that also reflect the relative tendencies of persons to behave in certain ways on certain occasions. Fishbein's work in attitude measurement is also relevant here, since he has demonstrated the importance of several previously neglected item properties that should increase differentiation among persons. These include: (1) the probability of a trait given an act, $p(T \mid A)$, (2) the probability of a trait given the act is not performed, $p(T \mid \bar{A})$, and the base rate of the act in the situation $p(A)$. The difference between the first two probabilities is a powerful index of item validity (Fishbein & Ajzen, 1974).

The predictive utility of a trait measure is a direct function of the ability of the measure to postdict the general trend of an individual's past behavior. Once this is realized, it becomes clear that attempts to assess underlying motives, latent tendencies, and the like are quite beside the point. Prediction from trait measures is based on the logic of the old adage that the best indication of what a person will do in the future is what that person has done in the past. When this is not the case, other measures may be called for, but they are not properly called trait measures.

V. Traits as Explanations of Behavior

The summary view of traits that I have advanced thus far does not comment on the causal properties of traits or on the use of traits as explanations of behavior. It is true, of course, that a summary of a person's conduct to date does not provide an account or explanation of his or her conduct. At issue is whether ordinary trait attributions are intended to be explanations. Attribution theorists think they are so intended and they view trait attributions as "naive causal inferences" based for the most part on insufficient data (Heider, 1958; Jones & Nisbett, 1971; Kelley, 1967). But the subjects of attribution studies are required to provide "causal" explanations on the basis of insufficient data, leaving open the possibility that naiveté may be attributed more justly to those who design such experiments.

According to the summary view, trait attributions are made in just those circumstances in which the speaker is ignorant of the true causes of the behavior pattern at issue. It may be that such attributions are meant to "stand in" for explicit explanations (Harré & Secord, 1972, p. 270), but it seems farfetched to regard attributions of traits as proffered explanations of either a scientific or a prescientific sort. When I say that it tends to rain in Vancouver (or that Vancouver is rainy), I do not presume to be offering an explanation of that tendency, nor to have intimate knowledge of the nature of whatever meteorological forces may be operative. Were you to ask me why it rains in Vancouver, which is a quite different issue, you would
expose my "naive inferences" based on my lack of knowledge of meteorology. When I say that John is aggressive (or tends to be aggressive), I would also not be so presumptuous as to think that I was providing anything resembling an explanation of John's acts in terms of a well-substantiated theory of human nature.

There are circumstances in which the ascription of a trait to a person serves as a partial explanation of that person's behavior. If you are not acquainted with John and if you ask me why John pushed the boy on a certain occasion, I might reply that John is aggressive. In effect, I am saying that such behavior is not unusual or unexpected for John, and such an "explanation" might serve as an answer to your question. However, if you and I both know John well, my telling you that John is aggressive does not answer your question. Were I to inform you that the boy had pushed John yesterday, you might very well feel that I had provided a satisfactory account of the incident (see Averill, 1973, p. 280). These two different "causal attributions" correspond, roughly, to the person versus situation dichotomy discussed by attribution theorists (e.g., Jones & Nisbett, 1971). But the first is simply a statement to the effect that the behavior is not unusual and the second is primarily a justification of that behavior. Neither statement specifies the efficient cause of the behavior in question.

Although laymen use trait terms to "stand in" for explanations, psychologists have used them as explanatory constructs. In moving from the level of ordinary language description to the level of theoretical explanation, virtually all "trait theorists" (Allport, 1937; Cattell, 1957; Eysenck, 1953; Guilford, 1959; Murray, 1938) consider traits to be causal entities rather than categorical summaries. Patterns of past conduct are energized and granted explanatory status as efficient causes of future behavior. To avoid charges of circularity of reasoning, trait theorists speak of traits as "hypothetical constructs," inferred from patterns of past conduct and used to predict future behavior. But hypothetical trait constructs often suffer from an intrinsic conceptual fuzziness that blurs distinctions between reasons and causes in the explanation of social behavior. Persons are seen as having certain dispositions in virtue of having certain hypothetical constructs, but these constructs are an ambiguous blend of institutional facts and efficient natural causes.

The tradition of using traits as causal explanations of behavior has a long history that stems from the early faculty psychologists to the present day. Behavior is explained by reference to generative mechanisms (traits, dispositions, needs, instincts, motives, etc.) which are structurally isomorphic with the behavior pattern requiring explanation. Thus a person behaves aggressively because he or she "has" an aggressive trait, need, or whatever that causes him or her to so behave. The behavior is described as phenotypic, manifest, or surface, while the trait is genotypic, latent, or source. The general pattern of the person's social behavior is mirrored in an underlying "structure" of personality traits.

This type of theorizing seems, to me, implausible. Recall, first of all, that the criteria for classifying behaviors as instances of a trait are institutional (social) in nature. These criteria would be expected to vary, not only cross culturally, but within cultural subgroups (what is "aggressive" behavior for one socioeconomic
group may not be for another). Similarly, the situations or "occasions" on which these behaviors occur will be defined quite differently in different cultural settings. Hence, the subject of an efficient cause or of a physical basis for a set of institutional conventions must be approached with great caution. Institutional rules impart meaning to topographically dissimilar actions in varied circumstances. The existence of generative mechanisms which are structurally isomorphic with institutional rules seems most unlikely. It conjures up the image of a cultural homunculus within each of us.

The organization or "structure" of traits may well reside within a pattern of interrelated institutional rules, rather than within individuals. This strong form of the "biosocial" position is implicit in the writings of ethnomethodologists (e.g., Garfinkel, 1967; Goffman, 1959), and is quite explicit in the formulations of ethogenic personality theorists (e.g., Harré & Secord, 1972) who would locate "personas" in the shared perceptions of others and who reject substantive interpretations of traits.

Within the realm of personality assessment, one of the most highly "structured," replicable, and theoretically meaningful systems of behavioral classification has been found in the domain of interpersonal behavior (Wiggins, 1968, 1973). Here I refer to the work of Leary (1957) and his associates and its subsequent development and refinement by Lorr and McNair (1963), Stern (1970), and others. From both observational and self-report data, it is clear that the relation among the major categories of interpersonal behavior may be represented structurally as a two-dimensional circumplex of trait vectors. But what is reflected in that structure?

The traditional view of the interpersonal circumplex is that it reflects the organization of needs or traits within individuals. Descriptively this organization refers to summaries of past conduct; but theoretically, the organization is held to mirror the arrangement of internal needs, dynamisms, or whatever. I think it more plausible that the structure of interpersonal behavior mirrors a set of interrelated social rules for classifying behavior in terms of its likely interpersonal consequences. The remarkable structural convergences that have been found among diverse theoretical systems of interpersonal behavior (Foa, 1961) do not stem from the similarity of generative mechanisms postulated (needs, traits, dynamisms, etc.). Instead, the convergences reflect the common system of institutional rules that classifies interpersonal behavior measured by different techniques. Further, the promising typologies developed by Leary (1957) and Lorr, Bishop, and McNair (1965) for identifying central interpersonal dispositions (e.g., managerial-autocratic) are more properly viewed as institutionally defined roles than as internal generative mechanisms. Finally, the sequential patterns of interpersonal transactions that have been described by Leary and others (e.g., power provokes obedience) seem to represent normative rules of conduct rather than mechanistic chain reactions.

Once we have clearly separated what is to be explained (patterns of past conduct) from plausible explanatory constructs (generative mechanisms that cause persons to be rule-following agents), the enormity of the task of personality theory becomes evident. In our present state of ignorance of the nature of human nature,
we have lapsed into the layman's tendency to allow trait terms to "stand in" for genuine explanations. As a consequence, we have neglected the promising leads of those trait theorists who have recognized, at least implicitly, the importance of a distinction between traits and generative mechanisms.

Cattell (1946) has, for many years, distinguished surface traits (ordinary language descriptions of person attributes) from source traits (underlying generative mechanisms responsible for behavior classified by ordinary language). Perhaps the conceptual significance of this distinction has been overlooked because: (1) despite the use of neologisms, "source" traits appear to represent familiar "surface" themes, (2) it does not seem intuitively obvious that source mechanisms can be detected by the multivariate analysis of ordinary language attributions, and (3) identified "source" patterns have been interpreted, rather casually, by reference to psychoanalytic mechanisms. But the conceptual distinction between surface traits and source mechanisms resembles, or is at least compatible with, Chomsky's (1965) distinction between the surface and deep structures of language, a model that may be especially useful in stimulating thought about the plausible origins of personality traits (Harré & Secord, 1972; Stagner, 1973).

ACKNOWLEDGMENTS

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REFERENCES

Sechrest once observed that

to too great an extent the field of personality has been dominated . . . by psychologists pursuing the ubiquitous but elusive, and maybe even chimerical, differences between persons. Paradoxically, these psychologists have operated more often than not from a theory that posits not differences but universals. Freud's developmental states, Adler's striving for superiority, Jung's animus-anima, Maslow's need hierarchy, and many other concepts were meant to apply to everyone everywhere, but psychologists became bogged down in studying differences in a way that has never been very productive.

(1976, p. 4, emphasis added)

My own position on personality psychology's current state takes these remarks by Sechrest very seriously. Contra the view that has dominated the thinking of mainstream personality investigators for much of the greater part of the present century, I have been arguing that the assessment and study of individual differences is fundamentally and irremediably ill-suited to the task of advancing personality theory. As an alternative to what many have, for years, been pleased to believe qualifies as nomothetic inquiry, I have further argued that the overarching theoretical objectives of personality psychology would be better served by inquiry that proceeds “idiothetically” (Lamiell, 1981). However infelicitous that neologism may have been, what I have meant to suggest by it is an approach to the investigation of psychological phenomena which respects the individuality of those phenomena—the fact that every perception, emotion, cognition, and action is someone's—without
compromising the search for truly general or nomothetic principles, that is, principles in terms of which one might understand the perceptions, emotions, cognition, actions, and so forth of everyone (Lamiell, 1987, 1990a, 1990b). "Idiothetic" inquiry accommodates individual differences without making those differences themselves the focus of inquiry.

Consistent with the editors' vision of the present volume as a handbook, my objective in this chapter is to trace the major lines of my argument as it has been developed to date. The reader interested in a more detailed exposition is referred to my Psychology of Personality: An Epistemological Inquiry (1987; see also Lamiell, 1990a).

I. Why Individual Differences Research Cannot Advance Personality Theory

Actually, the case against individual differences research as a framework for the advancement of personality psychology's theoretical concerns is logically quite straightforward, and can be stated concisely as follows:

Thesis 1: Any theory of personality is a conceptual framework designed to provide explanations for and hence an understanding of individual behavior/psychological functioning.¹

Thesis 2: Except under hypothetical conditions never realized in practice, the reliability and validity coefficients and other statistical indices generated by studies of individual differences variables (alone or in combination with "situational" or "treatment" variables; see Cronbach, 1957) bear no legitimate interpretation of any kind whatsoever at the level of the individual.

Conclusion: Knowledge of the sort yielded by individual differences research is fundamentally and irremediably ill-suited to the task of advancing theories of individual behavior/psychological functioning—however useful that same knowledge might be for other purposes—and the discipline of personality psychology is therefore in need of a viable alternative research paradigm.

To the best of my knowledge, no one has ever seriously questioned Murray's (1938) contention that, in the psychology of personality, "the objects of study are individual organisms and not aggregates of organisms" (p. 127, emphasis added). Thus, we may take Thesis 1 as one for which there is and has always been general agreement. But if this is so, and if Thesis 2 is also true, then the conclusion follows by force of logic, and the debate over whether investigators

¹ This is not to deny legitimate theoretical divergences concerning such matters as the sources of personality and the dynamics of its development. It is merely to point out that individual persons are the loci of personality functioning, whatever its presumed sources and dynamics. This is true, for example, even for theorists who would emphasize various facets of socialization in their theoretical conception of personality and its development (see, e.g., Harré, 1984; Hurrelmann, 1988).
should abandon traditional "nomothetic" inquiry in favor of a more apposite framework is over. This is only how things ought to be, since, as a matter of logical fact, Thesis 2 above is true. Alas, it is just this point with which apologists for conventional "nomothetic" inquiry cannot or will not reconcile themselves (see, e.g., Dar & Serlin, 1990; Ozer, 1990; cf. Lamiell, 1990b), and it is at least partly for this reason that the debate continues.

A. Knowledge about Individual Differences Variables Is Not Knowledge about Individuals

The difficulty here is vividly illustrated by the following example, which, though anecdotal, is not apocryphal: Several years ago I attended a lecture given by a senior and still very prominent personality investigator who, in the course of his comments, chided a peer for making generalizations about individuals on the basis of experimental treatment group means. The lecture then proceeded with a very enthusiastic report of numerous validity coefficients he had obtained in a longitudinal study of selected individual difference variables. I later asked the speaker if he was in any way troubled by the fact that the Pearson product—moment correlation coefficients by which he was placing such store as grounds for his generalizations about individuals were themselves group means. He replied, "Well, there are group means and then there are group means."

And so there are. Specifically, and with regard to the legitimacy of using group means as empirical grounds for generalizations about individuals, there are those around which the variances are zero, and then there are those around which the variances are not zero. The former will logically support at least some generalizations about individuals, and the latter will not. So too with the Pearson product—moment correlation coefficients that, in the (all too) appropriate words of Bem and Allen (1974), are "the sacred coin of the realm" in mainstream personality research: there are correlations which are perfect, and then there are correlations which are not perfect. The former will bear at least some interpretation at the level of the individual precisely because they are, in effect, group means around which the variances are zero. The latter, that is, correlations which are not perfect, will not bear any interpretation at the level of the individual precisely because they are, in effect, group means around which the variances are not zero.

The problem here is not that a statement about an individual based on a group mean around which the variance is nonzero, or on a Pearson $r$ of which the absolute value is less than 1.00, is knowably false for all individuals. The problem is that such a statement is not knowably true for any individual, and this is because the statement is certainly false for some individuals—though we could not say which ones without investigating the matter case by case—and possibly false for all of the individuals. Within a discipline in which the overriding objective is to explain and understand the behavior/psychological functioning of individuals, it is difficult to imagine an epistemologically worse state of affairs. That the person after whom personality psychologists' most prized statistic has been named would have been
untroubled by this state of affairs is properly seen not as a failure of discernment on the part of Karl Pearson (1857–1936) but instead as a reflection of the fact that he was not interested in individuals:

It is almost impossible to study any type of life without being impressed by the small importance of the individual. . . . Evolution must depend upon substantial changes in considerable numbers and its theory therefore belongs to that class of phenomena which statisticians have grown accustomed to refer to as mass phenomena.

(Pearson, 1901/1902, p. 3; quoted in Porter, 1986, p. 306)

It is arguably one of the great ironies of late twentieth century psychology that Pearson’s most visible legacy has become the linchpin of a paradigm widely fancied as appropriate for handling questions about—of all things—human individuality! But irony or not, since the problem identified above is a logical and not an empirical one, it is not going to vanish or wilt in the face of any findings of any empirical study. One either recognizes that knowledge about individual differences variables is neither equivalent to nor substitutable for knowledge about individuals or one violates logic. There are no other choices, even where there seem to be.

For example, many (perhaps most) contemporaries are pleased to believe that the traditional paradigm can be rescued from this critique by using group means or other aggregate statistics as the basis for probabilistic statements about individuals (see, e.g., Paunonen & Jackson, 1986a, pp. 471–473). Let us consider the matter from an epistemological standpoint.

Take a statement of the form, “The probability is \(p\) that Smith will do X.” The question is, under what values of \(p\) could such a statement be empirically verified or falsified as a claim to knowledge about Smith?

Clearly, the values 1.0 and 0 will “work” here, because substituting either one of those values for \(p\) would amount to an assertion of certainty that Smith will (if \(p = 1.0\)) or will not (if \(p = 0\)) do X. As a claim to knowledge about Smith, either statement logically admits the possibility of disconfirmation in the face of evidence that Smith does or does not do X. Brief reflection will reveal, however, that the values 1.0 and 0 are the only values that will “work” in this context. Let \(p = .9\), for example. As a claim to knowledge about Smith, how is the statement “The probability is .9 that Smith will do X” possibly to be empirically evaluated? If Smith in fact does X, is the statement to be considered verified or falsified, and in either case why? If Smith does X, it is not possible that the probability that he would have done so was 1.0 all along, and not .9? If so, then what is the truth value of the assertion that the probability was .9? Alternatively, if Smith does not do X, would this disconfirm the assertion that the probability was .9 that he would? If so, why? After all, the assertion made no claim to certainty about what Smith would do. And yet if not, why not? If Smith has in fact not done X, then perhaps the probability that he would do X was in fact 0 all along and not .9 at all. Again, when probabilistic statements about individuals are based on the results of individual differences research, how are such statements to be evaluated as claims to scientific knowledge about those individuals?
The answer, of course, is that they are not to be so evaluated. The reason is that they cannot be so evaluated, and that is the problem. When it is based on the findings of individual differences research, a statement such as "the probability is .9 that Smith will do X" simply means that given Smith and 99 other individuals identical to Smith with respect to the predictor variable(s), 90 will do X and 10 will not. Just which 90 will and which 10 will not is a question left untouched by the probability statement. This means that not only is the probability statement not really a claim—least of all a scientific one—to knowledge about Smith, it is not really a claim to knowledge about any one of the other 99 individuals either. It is quite literally a claim to knowledge about no one, and that is why it fails to get at anything consequential in the domain of personality theorizing.

Now some would point out, as did Paunonen and Jackson (1986b, p. 472, footnote 1), that a statement of the form "The probability is .9 that Smith will do X" might be based on evidence pertaining to Smith's behavior across many situations, where it has been observed that in 90% of those situations Smith has engaged in behavior X and in 10% of those situations he has not. Under these conditions, there is a sense in which the probability statement could be empirically verified or falsified as a claim to knowledge about Smith. But that is because all of the data from which the statement issues refer to observations about Smith, which means that we are no longer discussing individual differences research, either as the source of the statement or as the locus of its subsequent verification/falsification. In other words, to achieve this sort of knowledge about individuals one must step outside of the individual differences framework.

B. Individual Behavior Is Not Caused by and Cannot Be Explained in Terms of the Difference(s) between That Individual and Others

For all of the foregoing, many will cling to the intuitively appealing notion that the psychological differences between individuals are relevant to and must therefore somehow be incorporated into explanations for their respective actions. As compelling as this notion seems to be, it is found wanting on close inspection.

Let us say, for example, that among his other personality characteristics Smith is an extraverted individual, and that Jones is, among his other personality characteristics, an introverted individual. Thus, along the dimension of individual differences known as introversion–extraversion, Smith's status is E while Jones' status is I. Let us further suppose that as a direct result of his "E-ness," Smith's behavior is consistently chatty (C) at social gatherings, and that as a direct result of his "I-ness," Jones' behavior is consistently quiet (Q) in such settings.

Now from a theoretical standpoint, the concern in this little scenario would be to explain Smith's "C-ness" in terms of his "E-ness," and to explain Jones' "Q-ness" in terms of his "I-ness." In individual differences research, however, what an investigator is actually looking at on the psychological ("predictor variable") side is neither E nor I per se, but instead and quite literally at the
difference between the two, \([E - I]\). Similarly, what the investigator is actually looking at on the behavioral ("criterion variable") side is neither \(C\) nor \(Q\) per se, but instead and quite literally at the difference between the two, \([C - Q]\). As a result, the focus in individual differences research is actually on the relationship between the two \(differences\), and it is finally for this reason that, in such research, one's vision of the individual persons between whom differences are being studied becomes blurred.

The problem is this: How can the \textit{difference between} the respective psychological constitutions of Smith and Jones possibly be said to influence, determine, or cause Smith's behavior \textit{or} Jones' behavior? If one adheres to the logic of the traditional individual differences approach, one is eventually forced to concede that the \textit{difference} between Smith's extravertedness (\(E\)) and Jones' introvertedness (\(I\)) exists psychologically \textit{neither} for Smith \textit{nor} for Jones. Presumably, what exists psychologically for Smith is simply his extravertedness (\(E\))—and not the difference between his extravertedness and Jones' introvertedness, \([E - I]\). Similarly, what presumably exists psychologically for Jones is simply his introvertedness (\(I\)), and not the difference between his introvertedness and Smith's extravertedness, \([E - I]\). Hence, to try to explain Smith's and Jones' respective behaviors by reference to \([E - I]\) is to ground each of the respective explanations in an entity which, though it might well be said to exist in some sense \textit{for an onlooker} of Smith and Jones, cannot be said to exist for either Smith or Jones.

Of course, one might object at this point that the entity \([E - I]\) could serve some sort of explanatory function if one sets as one's task explaining neither Smith's chattiness (\(C\)) nor Jones' quietude (\(Q\)) per se, but instead the difference between the two \([C - Q]\). The response to this objection is not merely to concede it but to underscore it as the point: the coherence that such individual differences research explanations can \textit{in principle} ever offer requires that the discussion be limited to the \textit{differences between} individuals, and that it never be permitted to lapse over into a discussion about \textit{individuals}. What the "laws" embodying such "explanations" would "explain" is merely between-person variance in the criterion variable(s), and the "explanation" would be between-person variance in the predictor variable(s). Such "explanations" might well serve to advance purely demographic or actuarial agendas. They might even serve to advance theories of \textit{data}. But they will never advance theories of \textit{persons}, and this is troublesome because that happens to be what personality theories are.

No person's psychological constitution \textit{is} between-person variance on the predictor-variable side of a regression equation, and no person's behavior \textit{is} between-person variance on the criterion side of a regression equation. Indeed, as they have been conceptualized by the ersatz "nomotheticists" of our discipline, it is necessarily the case that \textit{individual differences variables do not exist for individuals}. Hence, no discussion of individual differences variables can be a discussion of individuals. Moreover, would-be general laws, the terms of which are individual
differences variables, cannot possibly be laws that explain individual behavior. Because such laws pertain to individual differences, and because individual differences do not exist for individuals, one is forced to conclude that such laws pertain, quite literally, to no one. Laws that pertain to no one cannot possibly be laws that pertain to everyone, and it is to the latter that any nomothetic personality psychology worthy of the name would aspire.

II. SOME ISSUES IN NEED OF CLARIFICATION

A. Relationship of the Present Argument to Allport's Views

Many readers will detect in what has been said thus far traces of the so-called "nomothetic versus idiographic" controversy which has dogged the field now for over 50 years. Following the German philosopher Wilhelm Windelband, Gordon Allport (1937) introduced the terms "idiographic" and "nomothetic" into the discourse of personality psychologists as a way of highlighting, among other things, the difference between the study of persons on the one hand and the study of person variables, that is, variables with respect to which persons have been differentiated, on the other. Allport noted, quite properly, that mainstream personality research was thoroughly dominated by studies of the latter sort—which in his view qualified as nomothetic—and he steadfastly insisted that a proper understanding of personalities would require that the knowledge yielded by such studies be supplemented by knowledge that could be obtained only through studies of the former sort, that is, studies of the sort he labeled idiographic. Given (a) the points of convergence between Allport's views and my own, and (b) the fact that shortly before his death in 1967 Allport quite explicitly "cried uncle and retired to his corner" (cf. Allport, 1966, p. 107), the reader might wonder what reason(s) I might have for presuming that some gain could be realized through yet another seance with Holt's (1962) "Teutonic ghost." Put briefly, my presumption in this regard stems from the conviction that, as forceful and incisive as Allport was in certain respects, he failed in several important ways to prosecute effectively and/or correctly the case against the established individual differences framework.

To begin with, I doubt that Allport aided his own cause by labeling the sort of inquiry he advocated "idiographic." For even if his usage of this term was consistent with Windelband's (1894/1904), the latter had coined the expression to refer to knowledge about unique, historically configured events or phenomena, had identified such knowledge as the goal of inquiry within the humanities (die Geisteswissenschaften), and had explicitly distinguished such knowledge from the sort of knowledge sought within the natural sciences (die Naturwissenschaften). At a time when mainstream academic psychologists were still quite sensitive about their credentials as scientists—on the natural science model, of course—Allport
wittingly or otherwise invited the charge of "antiscientist," and it is one which his critics pressed swiftly (e.g., Skaggs, 1945) and sometimes harshly (e.g., Nunnally, 1967).2

But if Allport's use of the term "idiographic" was ill-advised, his use of the term "nomothetic" was simply wrong in the sense that it was not consistent with the meaning that Windelband had intended. For Windelband, the term nomothetic referred to knowledge that could be expressed in the form of allgemeine Gesetze—general laws. Any such law specifies was immer ist—what always is—in some specified empirical domain. It specifies, in other words, what obtains in each and every recurrent instance of the event or phenomenon it putatively governs, what is thus common to all of those recurrent instances, and it is precisely a law's alleged generality that is thrown into doubt by its failure to perform in this way.

The German expression for general is allgemein, a word which itself derives from the expression allen gemein—common to all. Without doubt, this is the meaning Windelband attached to the term "nomothetic." It is most unfortunate that this meaning was not preserved when Allport branded as "nomothetic" the kind of knowledge about personality produced by inquiry conducted within the traditional individual differences framework. We have already noted that such inquiry produces aggregate statistics that can be interpreted in a scientifically meaningful way for no individual (cf. Danziger, 1990). Such "laws" as can be formulated on the basis of such statistics, therefore, cannot possibly be regarded as laws found to hold recurrently for each of many persons, and for this reason cannot possibly be nomothetic laws of personality in the sense intended by Windelband (1894/1904).3

Nevertheless, Allport called the traditional individual differences approach to the study of personality "nomothetic," and in so doing only threw his intellectual adversaries into the proverbial briar patch. For given Windelband's conception of nomothetic knowledge as knowledge of the sort sought by and produced within the natural sciences psychology was trying to desperately to emulate, Allport's contemporaries could scarcely have wished for better than to be accused (sic) by a critic of pursuing their subject matter in a way that conformed to the methods and knowledge objectives of the natural sciences. Had Allport more fully appreciated the foregoing considerations (or more vigorously pursued their logical implications), he would never have labeled the traditional paradigm "nomothetic" to begin with, and the entire history of the nomothetic versus idiographic controversy might have

2 Allport himself seems to have been sensitive to this charge, judging by his attempt in 1962 to substitute the term "morphogenic" (borrowed from the scientifically respectable discipline of biology) for the term "idiographic." It seems that by then, however, the damage had been done. The proposed terminological graft never took, and despite his efforts Allport never succeeded in altering the widespread perception of idiographic inquiry as antiscientific.

3 That Windelband himself would have seen no contradiction whatsoever in speaking about nomothetic knowledge within the domain of personality is clear from other portions of the original text. Alas, but without doubt, the number of authors who have written on the nomothetic versus idiographic controversy exceeds greatly the number of those who have read Windelband's text or familiarized themselves with its actual contents (Windelband, 1894/1904; cf. Lamiell, 1992a).
taken a different (and rather more productive) course. But all speculation on this count aside, I personally do not see how the fact can any longer be ignored that what Allport called the “nomothetic” approach to the study of personality is not now and has never been anything of the sort under the meaning of the term nomothetic intended by Windelband.

B. The Need to Distinguish between Uniqueness and Individuality

In my view, Allport and others who have followed his thought have made yet a third strategic error of sufficient conceptual consequence to warrant separate discussion. The difficulty to which I allude here is nicely illustrated by the following passages, which appeared in a monograph published in 1955 within a section subtitled “The Dilemma of Uniqueness”:

If there is to be a science of personality at all it should do better than it has in the past with the feature of personality that is most outstanding—its manifest uniqueness of organization.

(p. 21, emphasis added)

Nor is it helpful to take refuge in the example of other sciences . . . (On the contrary . . .) we should refuse to carry over the indifference of other sciences to the problem of individuality.

(p. 22, emphasis added)

What I would highlight in these passages is the ease with which Allport moves from the term “uniqueness” to the term “individuality,” with no apparent inclination to draw any distinction between the two. Space permitting, countless other examples of this phenomenon could be cited, and not only from the writings of Allport.

By making such an issue of uniqueness as he argued his case against the adequacy of traditional “nomothetic” inquiry, I think that Allport succeeded—unfortunately—in creating the impression that, in his view, the inability of such inquiry to accommodate the possibility that in certain respects each person would be found to be like no other person was its critical flaw. But as confirmed “nomotheticists” have long well known, the individual differences paradigm is in fact not logically incapable of accommodating the phenomenon of uniqueness, at least in a certain sense of that term.

Historically, “nomotheticists” have been guided in their work by the notion that “the” human personality is structured by a finite number of underlying attributes in some amount of which every individual is endowed by nature and/or nurture. In accordance with this notion, it has been assumed that once the elements or components of the presumed generic structure have been isolated, the particular features of any one individual’s personality will be comprehensively specifiable as that individual’s measured coordinates or “location” within the structure (consider, for example, the recent work of McCrae & Costa, 1986, 1987; see also Angleitner, 1991; Asendorp, 1995; McCrae & Costa, 1995).
Since it is logically possible that each measured individual would be found to take up a position occupied by no other measured individual within the multidimensional space, it is clear that, in this view, there is at least one sense in which uniqueness—and individuality if that just means uniqueness—is a phenomenon that can be accommodated by traditional "nomotheticism." Thus have many "nomotheticists" found license to proceed with business as usual without having to either concede the validity of Allport's assault on conventional practices or explicitly reject his thesis concerning individuality qua uniqueness.

Not altogether satisfied with this ploy, Allport's strategy was to point out that "nomotheticism's" accommodation of uniqueness would "work" only insofar as the putative elements or components of the presumed generic structure were known to be universally applicable, and only insofar as those elements or components could be specified comprehensively. If either or both of these conditions failed to obtain, an investigator would inevitably fail to capture Smith's uniqueness because (a) the investigator would characterize Smith in terms that did not apply to Smith, (b) the investigator would fail to characterize Smith in terms that did apply to Smith, or (c) both.

The mainstream response to this position has typically been that the issues raised are empirical matters best handled in accordance with the established principles of construct validation as set forth by Campbell and Fiske (1959; see also Cronbach & Meehl, 1955). The notion here has been that competently executed research focused on the reliability, convergent validity, and discriminant validity of various individual differences constructs would eventually reveal those attribute dimensions necessary and sufficient for identifying any given individual's personality characteristics (again, see the previously cited work by McCrae & Costa for current examples).

Now, for reasons already mentioned, investigations into the reliability, convergent validity, and discriminant validity of various measures of individual differences cannot logically resolve the aforementioned concerns of Allport, no matter how competently those investigations are executed, and no matter what findings they unearth. The questions Allport raised concerned the grounds on which it would be determined whether or not some putative personality attribute X could or could not be meaningfully applied to any given individual. The correlation coefficients in which evidence concerning reliability, convergent validity, and discriminant validity subsists simply beg such questions, because when they are less than +/-1.00—and they always are—they are uninterpretable for individuals. Thus, the traditional "nomothetic" response to Allport's concerns regarding the celebrated assumption of universal applicability (Bem & Allen, 1974) is inadequate at its very epistemological core—persistent and widespread beliefs to the contrary notwithstanding (see, e.g., Paunonen & Jackson, 1986a, 1986b; cf. Lamiell & Trierweiler, 1986)—and Allport need not have retreated one inch on this score.

But if I would criticize traditional "nomotheticists" for pretending to meet the challenges Allport mounted when in fact they never have done and never can do so, I would also criticize Allport himself for making such an issue of uniqueness
To see why, let us grant for just a few moments, and solely for discursive purposes, the validity of McCrae and Costa's (1987) claim to have isolated five basic dimensions necessary and (for now at least) sufficient as terms in which to describe the major features of any given individual's personality. Let us now suppose that the requisite five measurements have been made of the person Smith.

Now if these five measurements serve their intended purpose at all, then they convey valid information about what is, rather than what is not, the case as regards Smith's personality. They do this in virtue of the fact that the intersection of the five measurements within the multidimensional space locates Smith at a particular point and not at any of the other possible points within that space. Ensembled, the measurements state that Smith is "here" and not "there" or anywhere else in the space. It is for statements of just this sort, that is, statements about what is, rather than what is not, the case as regards the personality of a specific individual, that I would propose we reserve the term *individuality*.\(^4\)

But now with this putative knowledge about Smith's individuality at hand, in what research direction would we be led by concerning ourselves with the question of Smith's uniqueness? In the spirit of traditional "nomotheticism," for example, suppose that we wished to know whether the location occupied by Smith in the five-dimensional space proposed by McCrae and Costa is or is not also occupied by one or more other individuals.

It is important to see here that in posing this question a distinction between Smith's individuality on the one hand, and his uniqueness on the other, has already and necessarily been made, if only implicitly. For in order to determine if what is the case regarding Smith's personality is the case for Smith and no one else—i.e., is the case for Smith uniquely—one must first have at hand some knowledge of what is the case for Smith. Thus, knowledge about Smith's individuality—what is rather than what is not the case concerning Smith—is both distinct from the logically prior to any knowledge about Smith's uniqueness or lack thereof.

A second crucial observation to be made is that if knowledge about Smith's individuality is at hand, then some basis exists for pursuing questions concerning how Smith came to be as he is, what keeps him as he is, and what might change the way he is, and how answers to these questions might be used to explain why Smith currently acts as he does and, perhaps, to predict how he will act in certain future circumstances. Hence, given some initial knowledge about Smith's individuality, an investigator might well opt *not* to pursue the question of uniqueness at all, and to pursue instead questions of the sort that Leon Levy (1970, p. 29) identified as central to personality

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\(^4\) I should note here that on this score Allport's views were very much in line with those of Windelband, for whom the thought that he might *not* be unique was utterly abhorrent.

\(^5\) This is not to say that I regard the traditional individual differences paradigm as well-suited to the formulation of such statements. Indeed, I do not. However, since this issue is tangential to our immediate concerns, I let it pass here.
psychology's overriding theoretical concerns. By the same token, if the question of uniqueness (which, it may be noted, is not to be found among those cited by Levy) is to be pursued, then the questions concerning personality dynamics (development, maintenance, change, etc.) must inevitably be held in abeyance.

Bearing the above in mind, let us now suppose with McCrae and Costa (1986), and in traditional "nomothetic" fashion, that the five attribute dimensions in terms of which Smith's individuality has been articulated can be meaningfully applied to all of the other individuals with reference to whom the question of Smith's uniqueness is to be settled. Of course, in granting this assumption we already finesse one of Allport's major concerns with respect to the dilemma of uniqueness (see above). But ignoring this fact for now, let us focus instead on the fact that even if the assumption is granted, another very serious problem immediately arises, namely, that of determining how many—and which—other individuals should be compared with Smith before concluding that he is or is not unique in the sense under discussion.

To say that someone is unique is to say that there is no one else just like him. But is the phrase "no one else" to mean, literally, not one other human being who has ever lived, is now living, or who will ever live? If so, then in taking up the question of Smith's uniqueness we have, to say the least, set a rather formidable task for ourselves. On the other hand, if this is not what the phrase "no one else" is to mean, then the question of what the phrase is to mean remains. Furthermore, any answer that we might offer to this question will demand a rationale, that is, an explanation for why the phrase "no one else" should be given any meaning other than its literal meaning as expressed above.

But just to be sporting, let us suppose that somehow all of these matters have been resolved: (1) that the population of individuals with reference to whom the question of Smith's uniqueness is to be settled has been specified in a conceptually defensible and practically workable way; (2) that the assumption that McCrae and Costa's five attribute dimensions can be applied meaningfully to every individual within that population has been justified properly, and (3) that measurements of each one of those individuals with respect to the five attribute dimensions are at long last available.

Now let us suppose that, as it has turned out, none of the other individuals within the investigated population has been found to occupy the same position in the multidimensional space where we had previously located Smith. At long last, empirical grounds would exist for claiming that, in at least one sense of the term, Smith is unique.

But now a new question arises: What do we know about Smith's personality?

Incidentally (and as if we were not in this up to our necks already), how long should we expect the data-gathering process to take? And if and when it is completed, should we expect to find Smith at just that location in the multidimensional space where we left him? Or might he have moved by then? And if by then he has moved, what, if any, problems would this create? As important and difficult as these questions are, it would be a shame to let them block our passage at this late stage of our discussion, so let us pretend that these questions, too, can be satisfactorily answered: that all of the needed measurements have been gathered within a reasonable length of time, and that Smith, God bless him, has stayed put at the precise coordinates within the multidimensional space where we had located him originally.
that we did not know before the question of his uniqueness was settled? Is he any 
more the person we originally found him to be by virtue of the subsequently 
established fact that there is no one else just like him? Surely, the answer to this 
question must be "no," for if it is "yes," then it follows that Smith is no longer the 
person he was, in which case the validity of the conclusion that he is unique is 
thrown back into doubt. Alternatively, suppose that our investigation has turned 
up indisputable evidence that there exists at least one other individual, Jones, whose 
personality is organized exactly like Smith's. The question is, is Smith any less the 
person we originally found him to be by virtue of the subsequently established fact 
that he is not unique in this regard? Once again, the answer to this question must 
be "no," for if it is "yes," then our original measurements of Smith no longer 
represent his individuality, in which case he really might be unique after all, and 
in any case can no longer be regarded as identical to Jones.

The lesson here is not difficult to see: whatever the fidelity of our original 
assertions concerning Smith's individuality, that fidelity can be neither compromised 
nor enhanced by the results of inquiry into Smith's uniqueness. Smith cannot be any 
more the person he is simply by virtue of our establishing (as if, somehow, we ever 
really could) that he is unique, and he cannot be any less the person he is even if it 
is discovered that he is not unique. Smith's individual personality—his individuality—is 
what it is whether he is unique or not. Moreover, the questions of genuine theoretical— 
and practical—consequence cited earlier concerning personality development, mainte-
nance, and change remain whether in the important features of his personality Smith 
is, to paraphrase Murray and Kluckholm's (1953) much too celebrated observation, 
"like all other persons, like some other persons, or like no other persons."

What Allport called "the dilemma of uniqueness" should be laid permanently 
to rest. It is not only unproductive but actually counterproductive to continue to 
level against "nomotheticism" the charge that it fails to accommodate the possibility 
of individual uniqueness. In the first place, there is at least one logical sense in 
which the charge simply is not true, and committed "nomotheticists" will always 
be quick to reassert that fact (see, e.g., Jackson & Paunonen, 1988). In the second 
place, while there is also a sense in which the charge is true, it is also true that, 
when all is said and done, the point is moot. It is the fact of individuality, not the 
altogether separate and finally inconsequential matter of uniqueness, that under-
mines conventional "nomotheticism" as a framework for the advancement of per-
sonality psychology's pantheoretical agenda.\(^7\)

\(^7\)This is as good a place as any to point out that in calling for an approach to the study of 
personality that respects the \textit{individuality} of psychological phenomena, one \textit{need not}—and I \textit{do not}—seek 
to promote a kind of \textit{individualism} in the traditional sense of that term (see in this regard the excellent 
article by Sampson, 1988). In this connection Alfred Adler's individual psychology comes immediately 
to mind as a very clear example of a view which is at once respectful of individuality but is anything 
but a celebration of what Sanford calls "self-contained individualism." Indeed, Adler's concept of \textit{das} 
\textit{Gemeinschaftsfühl} is the very antithesis of such individualism, and is in fact much more in the spirit 
of what Sanford calls "ensembled individualism." Similarly, William Stern's \textit{critical personalism} (Stern, 
1906, 1917, 1918, 1924; cf. Lamiell, 1996, 1992b) offers a comprehensive framework for conceiving of 
the human person in a way that is mindful of individuality yet disdainful of individualism.
III. "IDIOTHETIC" INQUIRY AS AN ALTERNATIVE TO TRADITIONAL "NOMOTHEΤICISM"

Bem (1982) observed that, historically, the fatal problem with recommending something like what Allport thought of as idiographic (or morphogenic) inquiry has always been that "one is never quite sure what to do next" (p. 23). In view of this problem, and as my own views are clearly akin to Allport's—even though, as I have tried to make clear, they deviate from Allport's in several significant respects—I intend to focus in the remainder of this chapter on some basic principles of "idiothetic" inquiry which I believe can and should guide us as we move from the traditional individual differences paradigm toward a more adequate alternative. Following Rorer and Widiger's (1983) worthwhile recommendation, I will proceed by relating my views to a very sobering but insightful epistemological commentary offered sometime ago by Paul Meehl (1978).

A. Synopsis of Meehl's Views on the Slow Progress of "Soft" Psychology

The following passage nicely conveys Meehl's views concerning the notion that theoretical assertions in the so-called "soft" areas of psychology—among which he explicitly included personality psychology—can be corroborated adequately by means of conventional tests of statistical significance carried out against the null hypothesis, in accordance with the inferential principles set forth by the renowned Sir Ronald Fisher:

I suggest to you that Sir Ronald has befuddled us, mesmerized us, and led us down the primrose path. I believe that the almost universal reliance on merely refuting the null hypothesis as the standard method for corroborating substantive theories in the soft areas is a terrible mistake, is basically unsound, poor scientific strategy, and one of the worst things that ever happened in the history of psychology.

(1978, p. 817)

The major problem here, Meehl notes, is the fact that the null hypothesis is almost always false, and knowably so with virtual certainty a priori (see also on this point, Bakan, 1966). Population means are virtually never precisely equal, and population correlation coefficients are virtually never precisely zero. Consequently, a statistical relationship large enough to reach that magical \( p < .05 \), and thus to reject the null hypothesis, is virtually guaranteed regardless of the substantive merits of the theoretical proposition under putative test, provided only that one is able and willing to make a sufficient number of observations. Meehl quite properly concludes from this that much of what has historically passed for theory testing in "soft" psychology reduces to "meaningless substantive constructions on the properties of the statistical power function" (p. 823).
Adopting what might be termed a “neo-Popperian” philosophy of science, Meehl thus argues that, for all practical purposes, it is simply oxymoronic to speak of “risking” theoretical propositions against the possibility of failing to reject the null hypothesis. His conception of a more apposite approach is conveyed through the following example:

If I tell you that Meehl's theory of climate predicts that it will rain sometime next April, and this turns out to be the case, you will not be much impressed with my “predictive success.” Nor will you be impressed if I predict more rain in April than in May, even showing three asterisks (for $p < .001$) in my $t$-test table! If I predict from my theory that it will rain on 7 of the 30 days of April, and it rains on exactly 7, you might perk up your ears a bit, but still you would be inclined to think of this as a “lucky coincidence.” But suppose that I specify which 7 days in April it will rain and ring the bell; then you will start getting seriously interested in Meehl's meteorological conjectures. Finally, if I tell you that on April 4th it will rain 1.7 inches (66 cm), and on April 9th 2.3 inches (90 cm) and so forth, and get seven of these correct within reasonable tolerance, you will begin to think that Meehl's theory must have a lot going for it. You may believe that Meehl's theory of the weather, like all theories, is, when taken literally, false, since probably all theories are false in the eyes of God, but you will at least say, to use Popper's language, that it is beginning to look as if Meehl's theory has considerable verisimilitude, that is, truth-likeness. . . . An unphilosophical chemist or astronomer or molecular biologist would say that this was just good sensible scientific practice, that a theory that makes precise predictions and correctly picks out narrow intervals or point values out of the range of experimental possibilities is a pretty strong theory.

(Meehl, 1978, pp. 817–818, emphasis and parentheses in original)

With Meehl's thoughts in mind—and most especially those expressed in the last statement of the above quotation—let us now turn to a consideration of studies that I and various colleagues have been conducting over the past several years in the area of subjective personality judgments.

B. Studies in the Epistemology of Subjective Personality Judgments

First, a bit of background. If one digs beneath the reliability and validity coefficients (and, occasionally, other aggregate statistical indices) issuing from studies of individual differences to the conceptual core of the paradigm, one finds at bedrock an unwavering conviction that meaningful statements about the extent to which any given individual's personality is endowed with one or more underlying attributes (traits, predispositions, etc.) can be derived only by comparing that individual with others. In this view, our knowledge of who Smith is is necessarily and inextricably tied to our knowledge of who others are. Though manifest in a variety of ways, this conviction finds its most visible and
formal expression in the logic of the normative measurement operations which have long been regarded as the *sine qua non* of scientifically negotiable statements about personality.\(^8\)

Several years ago, however, I arrived (via studies focused on the so-called “illusory correlation” phenomenon; see Lamiell, 1980; Lamiell, Foss, & Cavenee, 1980; see also Shweder, 1975, 1980) at the hypothesis that, in formulating and expressing subjective judgments about their own and one another’s personality characteristics, lay persons do not routinely rely on a normative reasoning process. Instead, I hypothesized, Smith relies on what I later came to appreciate as an essentially *dialectical* reasoning process by which his judgments of, say, Jones, are framed not by contrasting Jones with others, but by contrasting Jones with a conception of who Jones is not but might otherwise be, i.e., with a conception of the *negation* of Jones.

A question which often arises here is, would not Smith’s conception of who Jones is not itself require prior knowledge (existing “now” as memory traces of previous experiences) of who others are? A “yes” answer here would, of course, imply that the judgments Smith makes of Jones (or, for that matter, of himself) are grounded in normative considerations after all. My answer, however, is “no.” The thesis is that Smith’s ability to conceive of who Jones is not but might otherwise be does not require prior knowledge of who others are. Indeed, the contention here is that were it not for the capacity to frame judgments dialectically, and hence independent of considerations about individual differences, it would be impossible for knowledge about individual differences to be framed. The claim here, in other words, is that it is dialectical reasoning that makes normative (and for that matter, ipsative) reasoning possible, and not the other way around. Nor, if this is true, could it be so only for the lay person. To the contrary, it would have to be true as well for personality investigators themselves, and elsewhere I have tried to explain both that and why this is the case (Lamiell, 1987, chap. 5). Suffice it for now to say that it is here where one finds the most important methodological implications of this line of inquiry for those investigators who are interested in trait measurement and in search of a viable technique (see Equation 2 in Lamiell, 1981, p. 282) with which to replace the heretofore favored normative operations.\(^9\)

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\(^8\)To be sure, investigators have long recognized the possibilities offered by ipsative measurement. However, prevailing wisdom has always been that individual differences research, and hence inquiry grounded in the logic of normative measurement, is a logical precursor to any viable attempt to measure individuals ipsatively (see, e.g., Beck, 1953; Eysenck, 1954; Falk, 1956).

\(^9\)Warning: those investigators who are merely looking for a better way to measure individual differences (as appears to be the case, for example, with Paunonen & Jackson, 1986a, 1986b; see also Asendorpf, 1988) will simply have to look elsewhere. The objective, in my view, is not to improve individual differences research but to abandon it. In this same vein, the notion should be resisted that my proposed methodological alternatives can properly be evaluated according to the psychometric criteria of the individual differences paradigm. The realization that those criteria are inadequate to the task(s) at hand is what gave rise to the proposed methodological alternatives in the first instance.
Given the hypothesis that lay persons reason dialectically rather than normatively in formulating subjective personality judgments, the challenge, à la Popper, was to devise a means of exposing that hypothesis to the risk of disconfirmation in empirical observations. To this end, several studies have to date been carried out (Lamiell & Durbeck, 1987; Lamiell, Foss, Larsen, & Hempel, 1983; Lamiell, Foss, Trierweiler, & Leffei, 1983).

In all of those studies, the subject's task was very simple: he or she was presented with a series of 30–40 activity protocols and told that each such protocol conveyed valid information about the extent to which one of his or her peers typically invests his or her time or effort in each of a number of activities. The subject was requested to consider the information displayed in each protocol, to form a judgment about the degree to which the indicated activity pattern reflected each of a number of underlying personality attributes (e.g., warm versus cold, sociable versus unsociable, industrious versus lazy), and then to express each judgment by marking a rating scale.

Now if in a task of this sort the subject's judgments are of a normative nature, then the actual ratings made of the targets by a particular subject should be well predicted by formal measurements of those same targets derived nonactuarially (Lamiell, Trierweiler, & Foss, 1983; cf. Conger, 1983; Woody, 1983) via the arithmetic proper to normative measurement operations. Alternatively, if the subject's reasoning process is not of a normative nature but is instead dialectical, and hence patterned after the logic of what Cattell (1944) once called interactive measurement, then measurements of the targets derived nonactuarially via the arithmetic proper to such measurement should better predict the subject's actual ratings of those targets.

Note carefully the approach that was taken here: on the basis of two competing and precisely articulated theoretical conceptions of the psychological process engaged by the experimental task, specific point predictions were derived nonactuarially for where a given subject's ratings of the targets would literally fall on a specified scale. The sensible thing to do at this point was not to fashion some sort of a null hypothesis test, and certainly not to set about analyzing individual differences in the subjects' perceptions of the targets (e.g., by engaging one or another of the various data analysis procedures discussed by Schneider, 1973). The sensible thing to do was to check, for each subject, the degree of correspondence between each of the two sets of predicted ratings and that subject's actual ratings. Using for this purpose the well-known index of profile dissimilarity devised by Cronbach and Gleser (1953), the findings obtained with one subject who participated in the study by Lamiell and Durbeck (1987) are displayed in Table I.

Focusing for the moment on the dissimilarity indices shown in the first row of Panel IV in the table, one can see that, for Target 1, the index resulting from a comparison of the subject's actual ratings to the interactively derived point predictions (.11) was lower (indicating less dissimilarity) than that resulting from a comparison of the subject's actual ratings to normatively derived point predictions (.55). The dialectical theoretical conception of the subjective judgment process thus showed greater verisimilitude in this instance, and it is important to see that this conclusion
TABLE I
Predicted Ratings, Actual Ratings, and Proportional Profile Dissimilarities (One Subject, 40 Targets)

<table>
<thead>
<tr>
<th>Target</th>
<th>Panel I: Predictions from normative model (N) Attribute</th>
<th>Panel II: Predictions from interactive model (I) Attribute</th>
<th>Panel III: Actual Ratings (A) Attribute</th>
<th>Panel IV: Profile dissimilarities</th>
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Means of the profile dissimilarity values
Standard deviations of the profile dissimilarity values
i value for differences between correlated means (N vs I)

does not appeal to any $p$ value, or to any inferential statistic of any sort. There is no null hypothesis here to reject or fail to reject.

Of course, a critic might object that the results just discussed represent those obtained in one instance only. And so they do. But by scanning the remainder of Panel IV in the table, one can see that point predictions based on the interactive (dialectical) model approximated actual ratings better than did point predictions based on the normative model in 31 instances, and failed to do so in only 9 instances. Here, the possibility for putting a test of the null hypothesis to good use does arise. For example, a $t$ test comparing the two arrays of dissimilarity values might be carried out (see bottom of Table I). Alternatively, a simple chi-square analysis might be conducted, in which we would enter 31 tallies in one cell of the chi-square table to represent the 31 “hits” for the dialectical theory, and 9 tallies in another cell to represent the 9 “misses” for that theory. The distribution of “hits” versus “misses” thus obtained for this individual subject could in turn be tested for statistical significance against chance expectations, which in this example would be a distribution of 20 “hits” and 20 “misses.”

For the record, the obtained value of chi-square in this instance equals 12.1, a value which, at one degree of freedom, would occur by chance alone much less often than 1 time in 100. It is vitally important to recognize, however, that in this context the statistical analysis did not serve as a test of the substantive theoretical proposition. It served instead as a means of determining whether or not tests of that theoretical proposition already accomplished by other and entirely independent means have confirmed or disconfirmed the proposition with a degree of regularity sufficient to regard it as empirically corroborated.

But now what of the objection that, for all of this, we have still considered but one subject? The argument, of course, is that scientific theories or theoretical propositions cannot stand or fall on results obtained with just one subject. And so they cannot. But suppose that I had at hand evidence (and I do; see Lamiell & Durbeck, 1987) that in the study from which the data displayed in Table I were obtained, which involved a total of 67 subjects (investigated individually, of course), there were 57 for whom the hypothesis that the subjective reasoning process is dialectical was confirmed, 10 for whom that hypothesis was not clearly confirmed, and none—not one—for whom the competing hypothesis (that the subjective reasoning process is normative) was confirmed instead. This distribution of “hits” and “misses” could likewise be submitted to a chi-square analysis, but to what end? The obtained ratio of “hits” to “misses” appears to be overwhelming for the simple and very good reason that it is, and any conventional statistical test at this point would be merely gratuitous.

C. Implications

One thing I certainly do not wish to suggest by the foregoing is that studies of subjective personality impressions should now take center stage in the activity of personality investigators. Those studies do serve, in my view, the worthy function
of demonstrating that individuals can be characterized, in terms of their salient personality attributes, through the exercise of a reasoning process that is at once coherent and exceedingly systematic, but which at no point requires the comparison of one individual to another. With respect to the enterprise of trait measurement, the question raised by these findings is, If lay persons can proceed in the manner just described as they formulate and express their “subjective” impressions, then why could not personality investigators proceed in like fashion as they formulate and express their “objective” impressions, in the form of personality profiles? The answer is that they could, and in my view they should.

It is also to be hoped that the studies mentioned above will awaken at least some contemporaries to the theoretical possibilities that are opened up by taking seriously the concept of dialectical reasoning. The possibility suggests itself, for example, that the subjects of our inquiries reason in this fashion not only when they are engaging a fairly sterile experimental task involving personality ratings, but routinely, in the appraisal of the circumstances of their day-to-day lives (consider, for example, the dialectical themes that emerge in Frijda’s [1988] discussion of the laws of emotion; see also Rychlak, 1981, 1988). In short, there is, potentially at least, a great deal more of genuine theoretical consequence here than might immediately meet the eye, and it is unlikely that those possibilities are going to be vigorously explored so long as the majority of investigators within the field are busy searching for—or celebrating the “discovery” of—personality psychology’s answer to the periodic table of elements.

I believe, with Meehl, that if basic research in the psychology of personality is ever to prove successful in advancing personality theory, there is going to have to be a sharpening of hypotheses to the point that they enable one to make point predictions, or at least narrow interval predictions, from the range of experimental possibilities which are presented to the subjects. When one is positioned to do this, as we were in our studies of subjective personality judgments, tests of statistical significance are, as we have just seen, either obviated altogether or relegated to an epistemological role entirely different from—and decidedly more limited than—that which such tests have played heretofore.

The major advantages of this alternative approach vis-à-vis the objective of fashioning a genuinely (rather than merely pseudo-) nomothetic paradigm for basic personality research are not difficult to see. First of all, and as the studies mentioned above illustrate, it is possible to carry out theoretically relevant studies of individuals without either resorting to radical behaviorism or compromising methodological rigor. The “trick” here, if that is what it is, is to bring two or more divergent and well-articulated theoretical propositions to bear on the task of predicting what Smith will literally do in specified circumstances. Hypothesis testing is then a matter of investigating the correspondence between those divergent predictions and what Smith actually does. A test of this particular sort need engage no inferential statistics because it is not a test of a null hypothesis.

Second, since empirical research designed in this way neither involves nor invites any comparison between what Smith does and what others do, the investiga-
tion of each individual becomes a coherent study unto itself. This is just as it should be in a discipline that would advance our theoretical understanding of individuals.

Third, the foregoing logically compromises not at all the search for general or nomothetic principles. To the contrary, in a discipline where individuals are the entities over which generality is sought, the rigorous examination of theoretical propositions at the level of the individual is not only not antithetical to the quest for generality but is in fact logically central thereto. If reliance on conventional null hypothesis testing procedures is one of the worst things that has ever happened in the history of psychology, then another is surely the ascendance of the notion that genuinely nomothetic principles of personality could somehow be established without studying individuals.\(^\text{10}\)

Fourth, nothing in the approach I am advocating requires one to reject the fact of individual differences. As mentioned in the introductory comments to this chapter, the crucial difference between “idiothetic” inquiry and the traditional paradigm is that while the former simply accommodates individual differences, the latter makes those differences the focus of investigation. For example, in the impression formation studies discussed previously the subjects most certainly did differ in their ratings of the targets, and no feature of the employed methodology rendered inadmissible the possibility (nay, the certainty) that such would be the case. Nor, however, was the study of those differences the point of the research. The point of the research was to empirically evaluate alternative theoretical conceptions of the reasoning process through which each individual subject arrived at his or her ratings, and there is simply no analysis of the differences between the ratings that could have shed light on that question (see Lamiell, 1987, chapter 6, for a further discussion of this point). Here and elsewhere I have discussed the problems which arise when individual differences are made the focus of investigation. I stand by the conviction that those problems are fatal to personality psychology’s overriding scientific objectives, and that those same problems are irremediable within the traditional paradigm (cf. Dar & Serlin, 1990; Lamiell, 1990a, 1990b; Ozer, 1990).

Finally, I should not fail to acknowledge that as attempts to proceed along the lines I have sketched are put into practice, great challenges are bound to arise. It is, after all, one thing to generate theoretically based point predictions in studies of subjective personality judgments—even that was not so simple as it might now appear to have been—and quite another thing to generate comparable predictions in many of the other substantive areas in which personality investigators have legitimate and important concerns. At the very least, however, I hope to have provided the outlines of an ideal toward which I believe we should be

\(^{10}\) It is interesting to note that, at its beginnings, experimental psychology was very much an “\(N = 1\)” affair in the domain of research methodology even at its objective was to discover general laws of human psychological functioning. The notion that one either seeks such knowledge or studies individuals but not both is a myth that developed later on (see Danziger, 1990).
striving. Knowing what one is trying to achieve can be very helpful, even if one cannot at a given point in time claim to have achieved it fully.

When all is said and done, perhaps the most pressing current problem with personality psychology is that, over the years, the majority of those who animate the field has gradually lost the ability to distinguish between the genuinely scientific business of formulating and testing theoretically derived propositions concerning the behavior/psychological functioning of individuals on the one hand and the merely actuarial business of accounting for variance in countless measures of individual differences in behavior/psychological functioning on the other. Or perhaps the ability to make this distinction remains, but the role of the actuary is simply preferred in much larger numbers. In any case, I for one am greatly disturbed by the witting or unwitting ascendance of the notion, evidence of which abounds in the literature, that our subjects are not so much beings to be understood, to the end of enlightening us not only about them but about ourselves, as they are objects to be wagered on (cf. Paunonen & Jackson, 1986a, pp. 471–472), like so many horses. “Idiothetic” inquiry is not about placing bets, with the objective of maximizing payoffs in the long run. It is not about regarding persons as things or as mere matter (see the above cited works by Stern). It is about the serious business of advancing theoretical conceptions of individual behavior–psychological functioning, toward the end of improving our understanding of ourselves and one another. I do not know for certain that the framework as I have sketched it up to now will get us where we want to go. I do know that the long-dominant individual differences paradigm will not. It is time to move on. The epistemological basis for doing so is at hand, even if an exquisitely detailed map of all of the territory is not.

References


