
TARGET ARTICLE

A Critical Analysis of Current Trait Theory

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Many current trait theorists suggest that a consensus is emerging around the Big Five as the basic structure of personality. This target article gives critical consideration to the nature of the evidence supporting such a view as well as to more fundamental issues concerning the conceptual status of the trait concept. It is argued that the evidence is less supportive and convincing than is suggested by trait enthusiasts, that there are fundamental problems with the trait concept, and that the trait model is not the only personality model to recognize consistency and coherence in functioning.

The personality field is witnessing a resurgence of interest in traits. From the threats to its conceptual foundations, dating back to the 1960s and most exemplified in the works of Mischel (1968) and Peterson (1968), trait theory has returned with a vengeance such that some see the field as having arrived at a consensus concerning the basic structure of personality. What is striking concerning the statements of many trait enthusiasts is that progress in trait theory and research has come to be equated with a "consensus" concerning the "structure" of personality, thereby virtually equating a particular trait model with trait theory and trait theory with the field of personality. If this statement seems a bit exaggerated, consider the following sample of quotes to which many others could be added:

If there is to be a specialty called personality, its unique and therefore defining characteristic is traits. (A. H. Buss, 1989, p. 1378)

Personality psychology may be defined as the study of individual differences. (Brody, 1988, p. 1)

Taken together, they (the Big Five) provide a good answer to the question of personality structure. (Digman, 1990, p. 436)

The five-factor model of personality has brought order to the competing systems of personality structure by showing that most traits can be understood in terms of the basic dimensions of Neuroticism, Extraversion, Openness, Agreeableness, and Conscientiousness. (McCrae & Costa, 1990, p. 176)

We believe its long history, cross-cultural replication, and empirical validation across many methods and instruments makes the Five Factor Model a basic discovery of personality psychology; core knowledge

upon which other findings can be built. (McCrae & John, 1992, p. 207)

Just as cartographers eventually settled on a standard system with north-south and east-west axes, so personality researchers must settle on a standard set of locations for the Big Five dimensions. (Goldberg, 1993, p. 30)

My opinion is that the five-factor model of personality ... is largely sufficient for characterizing normal and abnormal personality functioning. (Widiger, 1993, p. 82)

One can further extend these statements to the suggestion that the five-factor model (FFM) and the associated NEO Personality Inventory (Costa & McCrae, 1989) are standards against which the validity of other comprehensive personality questionnaires can be assessed (Piedmont, McCrae, & Costa, 1991).

A few years ago, Waller and Ben-Porath (1987), in an *American Psychologist* comment on a McCrae and Costa (1986) article, questioned whether the field of personality had arrived at a consensus concerning the FFM as representative of the structure of personality. Suggesting that other models were available, Waller and Ben-Porath viewed such claims as premature. I agree with their assessment but go beyond it here in considering the evidence in detail and in raising several questions concerning not only the FFM but the trait approach more generally. Although this target article touches upon issues raised by critics such as Mischel, it is not the intent here to re-raise issues that have been reviewed and dealt with elsewhere (Kenrick & Funder, 1988; Pervin, 1983, 1985). Rather, the intent is to examine the evidence used by current trait enthusiasts in support of their position, to suggest that the evidence

for the above conclusions is not nearly as compelling as some trait pronouncements would have us believe, and to suggest that there are fundamental problems with the trait view of the person that limit the utility of the view as a fundamental model of personality.

In approaching these issues, it is clear that trait theory is not a monolithic enterprise. Indeed, one of the arguments to be made is that there are fundamental differences among those who consider themselves to be proponents of the trait approach. Nevertheless, there would appear to be sufficient commonalities for these individuals to be considered as trait theorists in contrast with proponents of alternative points of view.

A Critical Consideration of the Evidence

In their recent introduction to the *Journal of Personality* special issue on the FFM, McCrae and John (1992) raised the question of the rationale for the model. Their answer is "We believe it is simply an empirical fact, like the fact that there are seven continents on earth or eight American Presidents from Virginia" (p. 194). We may ask, then, what the evidence is of this empirical fact. The trait approach has a long history in the field, and the idea of five basic personality factors was suggested at least 20 years ago, perhaps as much as 50 years ago (Digman, 1990; Goldberg, 1990, 1993; John, 1990). Has there been some fundamental paradigmatic shift, some new methodology, some new data of great import that account for the new consensus? Even its adherents would appear to suggest that such is not the case but rather that "we have attained marginally increased understanding of the person" (Brody, 1988, p. viii) and "a gradual improvement in the methodological quality and sophistication of the research, especially in the realm of multivariate procedures" (John & Robins, 1993, p. 221). What, then, is the nature of the evidence, and what conclusion or conclusions can be drawn?

Genetic Evidence

We shall argue that evidence for genetic influences in traits provides one of the strongest sources of support for assigning a central role in personality to traits. (Brody, 1988, p. 69)

A prime source of personality traits is inheritance. (A. H. Buss, 1988, p. 20)

Most trait theorists have argued in favor of the strong heritability of most psychological traits (Bouchard, Lykken, McGue, Segal, & Tellegen, 1990; Loehlin, 1992; Plomin, Chipuer, & Loehlin, 1990). Although Funder (1991) suggested that traits are learned, much

more typical has been the claim that traits have a genetic basis (John & Robins, 1993). The evidence in support of the heritability of some important aspects of personality seems to me to be compelling—a view I would not have expressed 20, perhaps even 10, years ago. At the same time, it seems to me that two points need to be kept in mind. First, to my knowledge no one has argued that inheritance is all important or even more important than environment, if such a comparison is even meaningful. Indeed, current thinking in the field suggests that the environment is important, that it accounts for at least half the variance in personality traits, and that there is a danger that the rush from the environment will go too far (Dunn & Plomin, 1990; Plomin et al., 1990). Second, evidence of the heritability of personality varies considerably from characteristic to characteristic, being strongest for traits associated with temperament (e.g., emotionality, activity, sociability; A. H. Buss & Plomin, 1984) and weakest for attitudes and beliefs.

Accepting the above evidence, from a personality standpoint the question becomes how much of personality is best understood in terms of temperament-like variables and how much in terms of belief-type variables. And, even where temperament-like variables are important, are they alone sufficient to account for many of the phenomena of greatest interest to personality psychologists—a point to which I return after a further review of the evidence.

Stability Over Time

Personality traits change over time, but for most traits and for most individuals, the changes are unlikely to be large enough to deny stability. (A. H. Buss, 1988, p. 40)

Somewhere between age 21 and age 30 personality appears to take its final, fully developed form. ... To quote James "... In most of us, by the age of thirty, the character has set like plaster." (Costa & McCrae, 1994, pp. 21, 34)

Most trait theorists have suggested that evidence of longitudinal stability of traits provides support for the trait model (Brody, 1988; Funder, 1991; McCrae & Costa, 1990). Interestingly enough, both social cognitive theorists and psychoanalysts would agree with the assumption of longitudinal stability, although the explanations offered are different from those of trait theory as well as different from one another. Here one can ask two questions. First, if our longitudinal data reflect stability only, isn't it important to consider whether some significant aspects of personality are being neglected? After all, with all of life's crises and chance events, wouldn't it seem that a theory that suggests that "personality apparently changes little after age thirty in

most people" (McCrae & Costa, 1990, p. v) is perhaps missing something? As a minimum, wouldn't it make sense to distinguish among parts of personality—with the suggestion that some parts change less than others (e.g., core personality structure or character as opposed to values, individual acts, etc.)? Second, how are we to account for the stability that does exist? Is all of this due to genetic influences—in which case, how much does the theory really have to say about development, or is it due to other influences and processes as well? One can think of processes that maintain stability that are consistent with trait theory (e.g., traits lead to the selection and shaping of maintaining situations; others respond to people with trait-maintaining behaviors), but that is different from the theory specifying such factors or detailing when one would and would not expect stability. In the current context, one could argue that we now have evidence concerning the stability of important temperament variables, largely genetically determined. Surely personality and personality development are about more than that.

As I have emphasized elsewhere (Pervin, 1994), there is at least as much evidence of personality change as there is of personality stability, and the more important question is one of process. For some time, we have known that there is greater evidence of stability over shorter periods of time than over longer periods of time and that the potential for change tends to be greatest during periods of rapid development (Bloom, 1964). We also have known that there are large individual differences in stability of personality characteristics (Block, 1971). In addition, we have known that even small shifts in parts of a system can have dramatic consequences for the functioning of the system as a whole. Finally, we have known that there are many reasons why people continue to function as they do and that such stability does not mean that change is not possible. Therefore, it seems rather extreme to conclude that "in most of us, by the age of thirty, the character has set like plaster" when the supportive data are based on a limited sampling of personality variables suggesting that "three-fifths of the variance in true scores for personality traits is stable over the full adult life span" (Costa & McCrae, 1994, p. 33).

Emphasis on the aggregate stability of temperament-related traits, after age 30, likely exaggerates the stability of personality. It ignores individual differences in stability and the potential for even small changes in some elements to result in dramatic change in the overall organization of personality functioning. It ignores nontrait aspects of personality functioning that maintain stability over time as well as aspects of the environment that maintain consistency and resist change. Most important, as noted, it fails to address the issue of the factors that account for stability and change in various aspects of personality functioning. Despite

this, evidence of stability is taken as evidence in support of the trait conceptualization of personality.

Agreement Among Measures—The Magic Number Five, Plus or Minus Two

Dating back to Cattell (1959), trait researchers have attempted to demonstrate agreement among personality measures within and across data sources. As the introductory quotes suggest, trait enthusiasts (in particular, proponents of the FFM) have suggested that basically the same five factors are recovered from the factor analysis of different personality questionnaires. Although evidence in support of this statement exists, considerable debate remains concerning how many factors best describe personality, the extent of agreement among results when comparable sources of data are used, and the extent of agreement when different data sources are used (i.e., self-reports, ratings, behavioral indices). As with so many other aspects of trait research, part of the problem here revolves around what constitutes sufficient evidence in support of a statement concerning, for example, agreement between self-report and observer ratings. Do average correlations of .50 between self-ratings and peer ratings and .56 between self-ratings and spouse ratings constitute adequate evidence of agreement and validity (McCrae & Costa, 1990)?

To what extent has a consensus emerged concerning how many and which traits form the basic structure of personality? Current trait enthusiasts have suggested that such a consensus has emerged. If this were true, it would indeed represent an advance beyond earlier controversy. Other criticisms would still apply, but at least one could speak in terms of a consensus. Despite the suggestion that five seems "just right" (McCrae & John, 1992), some have argued in favor of three factors (Cloninger, 1987; Eysenck, 1990; Gray, 1987; Tellegen, 1985, 1991; Zuckerman, Kuhlman, & Camac, 1988) or seven factors (Tellegen, 1993) or, more broadly, that "the bottom line is that the most widely accepted personality traits may be too inclusive" (A. H. Buss, 1988, p. 246). Guilford (1975) attempted to address this question in terms of levels, as have others, but there does not appear to be agreement in this regard or concerning the relations of factors at one level to factors at another level.

Disagreement concerning the number of traits inevitably complicates the issue of comparability. Despite heroic efforts to map traits from different schemes onto one another and suggestions that there is substantial agreement in this regard (Borkenau & Ostendorf, 1987; John, 1990; McCrae & Costa, 1990; Wiggins & Pincus, 1992), questions remain concerning the comparability

of factors across instruments and data sources. In the words of Briggs (1989), a proponent of trait theory, "the resemblance is more fraternal than identical" (p. 248). Or, in the words of Tellegen (1993), another proponent of trait theory, "after half a century of painstaking psycholexical research, the quasi-consensual Big Five portrayal of these dimensions is still substantially incomplete and ambiguous" (p. 126). To such disagreement we may add the suggestion by Goldberg (1990) that, "when one turns from single terms to multi-word statements, the picture is far less clear" (p. 21).

The Fundamental Lexical Hypothesis

... the fundamental lexical hypothesis—namely that the most important individual differences in human transactions will come to be encoded as single terms in some or all of the world's languages. (Goldberg, 1990, p. 1216)

Two questions can be raised concerning the fundamental lexical hypothesis: How much support is there for the hypothesis? What is the status of concepts taken from everyday language relative to scientific concepts? Concerning the former, there is evidence of cross-cultural replicability of the Big Five factors. Reviewing the relevant evidence, Digman (1990) suggested that "the Big Five have appeared now in at least five languages, leading one to suspect that something quite fundamental is involved here. Is this the way people everywhere construe personality, regardless of language or culture?" (pp. 433–434).

When I read the early reports of such efforts, I was skeptical that they would replicate across non-Indo-European-language cultures. In other words, I accepted part of the fundamental lexical hypothesis to the extent that there was something encoded in the Indo-European languages that had to do with shared cultural ways of viewing the world but that this would not transpose to other cultures that did not share the same language or ways of viewing the world. Clearly, this is a complicated issue, and "conclusions about the linguistic, or even cultural, universality of the Big Five would be premature" (John, 1990, p. 78). My own reading of the literature suggests surprising (to me) replicability across non-Western languages, even when one starts with data from these other languages, but that there is not an absolute correspondence of factors (Yang & Bond, 1990).

This is an important development in research, and cross-cultural replicability would indeed provide support for a possible evolutionary interpretation of the way individual differences have become encoded as personality categories into natural language (John, 1990, p. 77). If the data continue to support the hypothesis, then we would have to conclude that something of

significance in human adaptation is going on and needs to be understood. However, that something (e.g., common human tasks and interpersonal issues) need not be the only or best way of describing human personality structure or processes. Cognitive anthropologists interested in this area have emphasized the relevance of personality descriptor categories to basic themes in folk interpretations of social behavior. In addition, they have emphasized the strong evaluative component of trait categories and their similarity to Osgood's evaluative and potency categories (D'Andrade, 1985; White, 1980).

Before turning to the second question, it is worth noting that Goldberg's emphasis is on single words and adjectives at that. The extent to which such restrictions bias the results remains to be determined. Tellegen (1993) emphasized that Allport and Odbert (1936) had left out evaluative terms, as have subsequent lexical analyses, thereby precluding the emergence of a positive factor and a negative factor. Beyond this, these analyses do not leave room for alternative conceptualizations of the world, such as one based on motives or goals. In research I conducted with Adrian Furnham (Pervin & Furnham, 1987), it was clear that individuals conceived of human action in motive or goal-related terms. That is, subjects clearly viewed themselves and others as having goals that remained somewhat stable but also varied according to situational factors.

Another study illustrates the differing results that are possible when subjects are allowed to use free-response descriptors. Shweder and Bourne (1984) asked subjects in India and the United States to describe the personality of a close acquaintance. Responses were coded according to whether they referred to an abstract trait, an action, or an evaluative term. They also were coded for whether contextual qualifications were used for the descriptors. The data indicated that Americans are more likely to use trait terms, whereas Indians are more likely to describe people in terms of their actions. In addition, the Indians were much more context dependent in their person descriptions. The suggestion made was that the Indian view of the person is much more embedded in a social matrix, whereas the individualistic American culture treats the person as separate from group contexts and social norms. Along with the importance of such cultural differences in person descriptions, this research illustrates how free-response descriptions provide for different results than are the case when only single-word adjectives are used.

Data and interpretations such as these bring us to the second question—that concerning the relation between concepts derived from lexical analyses and scientific concepts. Although Goldberg (1993) titled his recent article "*The Structure of Phenotypic Personality Traits*" (italics added), he moved to discussion of the scientific study of individual differences and suggestions con-

cerning the basic structure of personality. In other words, movement is quickly made from phenotypic, lexically derived personality traits to the basic structure of personality. Note that even some proponents of the FFM have been cautious in this regard. Thus, McCrae and Costa (1985) suggested that "no one would argue that an analysis of common English terms for parts of the body would provide an adequate basis for the science of anatomy, why should personality be different?" (p. 711). Yet, proponents of the FFM apparently have been prepared, nevertheless, to proceed as if such is the case.

Another proponent of trait theory, Tellegen (1993), was more circumspect in this regard. Tellegen distinguished between natural-language constructs and psychological constructs of individual differences—that is, between "folk concepts" and "psychological concepts." Folk concepts are viewed as common-sense ideas or folk wisdom concerning personality that is shared within a culture. In contrast to this, psychological concepts are viewed as scientific concepts advanced to describe or explain personality phenomena. Tellegen suggested that both are worthy of investigation but that they are conceptually distinct and that the criteria for judging their adequacy are different.

In sum, we remain without conclusive answers to the two questions raised in relation to the fundamental lexical hypothesis. There is good evidence developing to suggest that many of the same constructs are used cross-culturally to describe people, but this correspondence is not perfect, and the analyses for the most part have been restricted to single-word adjectives. The relation between folk concepts and psychological concepts would appear to be much more problematic.

Prediction: Back to the .30 Barrier?

I am impressed with the extent to which a limited number of common traits predict behaviors in a wide variety of situations. Mischel's personality coefficient of .30 is clearly a fiction derived from badly designed research and measurement error. (Brody, 1988, p. 133)

... reviews of the literature have concluded that personality measures when classified within the Big-Five domains, are systematically related to a variety of criteria of job performance. (Goldberg, 1993, p. 31)

Prediction clearly is a problematic area for any theory of personality, and I am not sure that it is the best measure of a theory. A theory may explain a lot but may be poor at predicting certain events. We may understand a great deal about a phenomenon but have great difficulty predicting specific events with great accuracy (e.g., weather, economic events). However, trait enthusiasts have accepted predictive utility as a measure of their success, and, as indicated in the above quotes,

many are impressed with the predictive utility of trait measures. Others, of course, have suggested that the picture is more mixed (A. H. Buss, 1989, p. 1379) and that the approach has a difficult (impossible?) time predicting events such as the passive person who commits an act of violence (Wiggins, 1973). Many current supporters have made the useful distinction between band-width and fidelity, suggesting that narrower traits are useful in making specific predictions, whereas broader traits are useful in making broader predictions (Funder, 1991; John & Robins, 1993; Zuckerman et al., 1988).

In the above quote, Goldberg (1993) clearly suggested the utility of the trait approach—most specifically, the Big Five model—in personnel selection. Goldberg referenced studies suggesting that measures of Conscientiousness are most likely to be "valid predictors" for all jobs, that measures related to Agreeableness are "most highly related" to job performance, and that personality measures provide "substantial" incremental validities over cognitive measures for the prediction of job-related criteria. At the same time, Goldberg noted the inconsistency in findings (e.g., the importance of Conscientiousness as opposed to Agreeableness), which he described as "befuddling."

In addition to this "befuddling" lack of agreement between major reviews of the literature, one can ask how much evidence there actually is of validity. In the Barrick and Mount (1991) study described as demonstrating that measures of conscientiousness are "systematically related" to performance and are "valid predictors" of performance, the reported estimated true correlations ranged from .20 to .23 ($M = .22$) across five occupational groups! In the Schmidt, Ones, and Hunter (1992) review of the literature, these were described as "moderate" correlations. It should also be noted that the estimated true correlations for the other four trait factors with the five occupations ranged from .00 to .18 ($M_s = .13$ for Extraversion, .08 for Emotional Stability, .07 for Agreeableness, and .04 for Openness to Experience).

Turning to the issue of incremental validity over cognitive measures, reference is made to the McHenry, Hough, Toquam, Hanson, and Ashworth (1990) study of the prediction of performance on nine Army jobs. The data indicate an incremental validity for temperament/personality measures over cognitive measures, but whether this is "substantial" is open to question. In this study, there were five measures of job performance. The incremental validity of adding Temperament/Personality measures to General Cognitive Ability measures ranged from .02 to .21. A striking aspect of these incremental validities, however, is that the incremental validity for the two measures of core job performance were .02 and .02, whereas those for the remaining three measures were .11, .21, and .21. What is unclear in the

data is just how much of a rating bias, based on observations of subjects' personalities, entered into the improved validities. What is clear, however, is that the personality measures did not show incremental validities over measures of cognitive ability for the measures of core job proficiency.

How one reads and evaluates such data partly depends on one's point of view. For proponents of trait theory and users of trait measures in personnel selection, such results may give encouragement and support for the utility of the model and associated measures. My own reading suggests a more cautious interpretation. I am struck with the inconsistent results and what I consider to be less than impressive increases in incremental validity on nonrating measures of performance. In areas such as this, I'm still not sure that we have gone much beyond the .30 correlation barrier between trait measures and measures of behavior.

Some Fundamental Questions Concerning Trait Theory and Research

My own reading of the above literature leads me to the conclusion that this is an impressive effort, the results of which must be taken seriously. Lest the above critical analysis be misleading, let me state that I believe that the various data suggest that something is there and that personality theorists of all persuasions must come to grips with the data and what that something is. At the same time, my own reading suggests questions concerning each component of the evidence as well as more fundamental issues. The suggestion here is that beyond individual studies and areas of research, there are fundamental questions involving the entire trait enterprise, both conceptual and methodological.

Conceptual Status of the Trait Concept: Definition

Just what is a trait? Perhaps the answer to this question is obvious to others, but to me it is not. Indeed, Borgatta (1968) came to the same conclusion in his review of trait theory more than 20 years ago: "What is to be included in a definition of 'traits' is not self-evident" (p. 510). Probably most would agree that *trait* represents a disposition to behave expressing itself in consistent patterns of functioning across a range of situations. As I understand it, the key words in the definition are *disposition* and *consistent*. Concerning disposition, it is clear that no trait theorist would suggest that a trait will manifest itself regardless of the situation, so a trait represents a disposition to respond.

Concerning consistency, it is clear that trait theorists emphasize covariation as the key element of the definition of a trait (Tellegen, 1991).

I think that all trait theorists would agree with the preceding definition. However, it is deceptively simple, broad, and ambiguous. That is, I suspect that all personality psychologists, regardless of orientation, would agree that people have dispositions to behave in consistent ways—otherwise there would be no need for the field of personality and perhaps not even for psychology! Indeed, I find it fascinating that the term *disposition* is used by trait theorists (Tellegen, 1991), social cognitive theorists (Mischel, 1990), and social psychologists (Ajzen, 1985).

So what is central to the definition of *trait*—what distinguishes the concept from other personality concepts and unifies trait theorists as a distinct group? The major question that concerns me here is what is meant by *functioning* or *behavior*. Most trait theorists specifically refer to behavior in their definition of a trait (Funder, 1991; John & Robins, 1993; Tellegen, 1991). I have always taken this to mean overt behavior, as, for example, in the act-frequency approach (Botwin & D. M. Buss, 1989; D. M. Buss & Craik, 1983). According to Tellegen (1991), *traits are inferred from behaviors and are the basis for inferences to other behaviors*. Brody (1988) suggested that "the sine qua non for postulation of a trait is its expression in varied situations" (p. 7).

What I find quite remarkable today is that for some the definition of *trait* has been broadened to include thoughts, feelings, and motives as well as overt behavior (McCrae & Costa, 1990, p. 23). Now perhaps there is no reason not to include such other phenomena within the definition of a trait, but my question is whether there is consensus in this regard and the implications of including thoughts, feelings, and motives as well as overt behavior.

In raising this question with trait theorists, it has been suggested to me that "no trait theorist has ever confined the term to a consistent pattern of overt behavior" (R. R. McCrae, personal communication, February 14, 1991). I believe, however, that trait theorists have been ambiguous in this regard. Most trait psychologists refer to behavior, with, as noted, the implication of overt behavior. At the same time, it is clear that personality trait inventories refer to attitudes, values, desires, and so forth as well as overt behavior (Werner & Pervin, 1986). Indeed, this has often struck me as one possible reason for problems in prediction to overt behavior from such inventories, because attitudes and values have a complex relation to behavior. In addition, many trait psychologists distinguish among different kinds of traits. For example, Guilford (1975) distinguished among temperament traits, motivational traits, and attitudes, Cattell (1965) among cognitive-ability traits,

temperament-stylistic traits, and dynamic traits, and Allport (1937) between expressive-stylistic traits and motivational traits.

The question of the relation of traits to motives is of particular concern to me. I suggest that, despite impressive and valuable efforts to establish links between traits and motives (Costa & McCrae, 1988; Read, Jones, & Miller, 1990) and despite efforts by trait theorists to include motives in their concept of a trait, the two concepts are fundamentally distinct. The point is perhaps made most clear in relation to the noted personologist Henry Murray. Although Murray has been included by many among the list of trait theorists (A. H. Buss, 1989; Digman, 1990; Funder, 1991; McCrae & Costa, 1990; Tellegen, 1991; Wiggins, 1991), he specifically dissociated himself from this point of view. In comparing trait and motive (need) concepts, Murray (1938) rejected the trait concept as being too concerned with consistent patterns of overt behavior, thereby neglecting needs that might not generally be manifest in overt behavior but that could account for sudden inconsistencies of the very kind noted by Wiggins (1973) as problematic for the trait position—the passive person who suddenly commits an act of violence. Thus, despite the argument of some trait theorists that the distinction between traits and motives is artificial and without basis in common sense (R. R. McCrae, personal communication, February 14, 1991), I would suggest that it is fundamental and of considerable import.

Conceptual Status: Descriptive or Explanatory?

Too many psychologists write as if a trait were an effective cause of behavior. This is quite incorrect and misleading. ... The trait, then, should be considered descriptive, but not explanatory. The causes of human behavior insofar as we can identify them are the motives which impel us to act and the environment which shapes our actions. (Stagner, 1948, p. 144)

I find Stagner's quote of interest because it considers both the former question of the relation between traits and motives as well as the question now before us: What is the conceptual status of the trait concept? In its simplest terms, one can ask, as Allport (1937) did, whether traits are real. Allport concluded, of course, that traits are real structures within the person—a position held by many current trait theorists (Funder, 1991; Tellegen, 1991). Beyond this, however, my own reading of the literature suggests two fundamentally different positions, one suggesting that traits are descriptive concepts and the other suggesting that traits are explanatory concepts—the exact distinction drawn by Stagner more than 40 years ago! A third position, that traits are

folk concepts, can also be noted—a position that I would read as being similar to the traits as descriptive position. A fourth position, not frequently seen by me, suggests that traits as descriptive concepts and traits as explanatory concepts are not truly distinct views (Funder, 1991, p. 35).

Some trait theorists have taken pains to suggest that traits are explanatory concepts that are not circular in nature (Eysenck, 1990). Here traits are seen as real and causal of behavior. Another position, however, is that traits are descriptive and require explanations rather than offering explanations for behavior: "Traits are 'lost causes'; their existence requires, rather than provides, a scientific explanation" (Wiggins, 1973, abstract). As noted in an earlier section, another position suggests that traits are real, but those derived from natural-language analyses are best understood as folk concepts (Tellegen, 1991, 1993; Tellegen & Waller, in press). This position is specifically rejected by John and Robins (1993), who suggested:

it is important to realize that the Big Five factors, although derived from ratings of common language trait terms, are *not* folk concepts, that is, "everyday variables that ordinary people use in their daily lives to understand, classify, and predict their own behavior and that of others" (e.g., Gough, 1987). (p. 221)

At the same time, John and Robins (1993) suggested that the FFM does not take an explicit causal stance. In other words, although traits are viewed as explanatory concepts, explanations are not offered in other than trait terms (R. R. McCrae, personal communication, February 14, 1991; McCrae & Costa, 1990, p. 25). Finally, one may note the view that "personality traits may be caused, causal, or both" (A. H. Buss, 1989, p. 1386).

At this point, one may be tempted to ask for the true trait theorist to stand up. Not only are there definitional differences among trait theorists but also differences concerning conceptual status. To their credit, many trait theorists have been aware of this problem and have called attention to it (Briggs, 1989; John & Robins, 1993; Wiggins, 1973). However, in the current enthusiasm for traits and the FFM, it is tempting to forget or dismiss these fundamental issues of definition and conceptual status.

Dynamic, Patterned Aspects of Personality Functioning

In the past, Mischel (1968) raised questions concerning the consistency of personality, and Lamiell (1981) raised questions concerning the utility of an individual-differences model for the study of personality. Although I share their concerns, my focus here is on two

different but related issues—the extent to which the trait point of view can capture the dynamics of personality functioning and the extent to which the trait point of view can capture the pattern and organization of personality. I approach this part of my critique from a dynamic, systems point of view (Pervin, 1978, 1983) emphasizing the interplay among motives (as well as other components of personality) and emphasizing systems principles such as multidetermination, equipotentiality, and equifinality. Similar issues have been addressed by others, both critics as well as proponents of the trait point of view (Briggs, 1989; McAdams, 1992).

As I suggested earlier, Murray (1938) drew a fundamental distinction between the trait and motive (need) concepts. Writing at the same time as Murray, Allport (1937) struggled with the relation between traits and motivation. Basically, Allport's struggle was with the relation between traits (motives) that initiated behavior and traits that were more stylistic or expressive in nature. This was related to his concern with the genotypic–phenotypic distinction—that is, with the fact that the same behaviors in different people could have different roots (equifinality) and that seemingly different behaviors in the same person could have the same root (equipotentiality). Ultimately, Allport (1960) concluded that it was the motivational, more genotypic traits that were “the very springs of conduct” (p. 326). As he expressed it in a 1939 address at Berkeley, “the organism is, after all, but a living system of interdependent motives” (Allport, 1939, p. 108).

Stagner (1937), another early trait theorist, considered traits to be descriptive rather than explanatory of behavior. Stagner suggested that motive concepts were necessary for the latter. In the third edition of his personality text, he conceptualized personality as a goal system (Stagner, 1948).

Others (e.g., McClelland, 1951) have distinguished among traits, schemas, and motives as personality variables. In agreement with Murray, McClelland suggested that this was a conceptual issue rather than a purely terminological issue. I find myself in strong agreement here with Murray and McClelland. It seems to me that traits and motives are different concepts, associated with different referents, and historically associated with different assessment devices. Not being a trait theorist may not have made Murray a better personality psychologist, but it did speak to a fundamental difference in his sense of what personality is about. Historically, the motive concept has been used to account for varying levels of activity that exist in the organism, the directed nature of organismic activity, and the varying responsiveness of the organism to environmental stimuli—the energizing (activating), directing, and preparedness-for-response aspects of the concept (Pervin, 1983). Motives may or may not be

reflected in behavior, may combine in complex ways with one another, and may be in conflict with one another. After a time during which the concept was virtually banished from the literature, it has come back to play a vital role in the thinking of personality and social psychologists (Pervin, 1989). It is different from the trait concept, not a substitute for it and certainly not to be replaced by it!

Turning now to the question of pattern and organization, it often has struck me that trait theorists, who so often have focused on individual differences, have so rarely focused on individuals. In the words of McAdams (1992), it is “the psychology of the stranger.” Surprisingly, few case studies have been reported from a trait perspective, and surprisingly little consideration has been given to the question of pattern and organization. Allport was particularly concerned with synthesis, organization, patterning, and the unity of personality. Significantly, the 1961 revision of his 1937 classic was titled *Pattern and Growth in Personality*. Unfortunately, this area remains neglected by trait theorists, perhaps because other groundwork needed to be done, perhaps because the model is inherently non-organizational and nonsystemic. It is true that some trait psychologists have been aware that “personality descriptions based on a small set of common traits provide a limited and even an impoverished descriptive system,” yet they have been prepared to reach the conclusion that “it is a serviceable system and there is no compelling empirical justification for the rejection of such a system” (Brody, 1988, p. 122).

I am struck with the increasing use of the term *coherence* in the field. I think that it is a good term, different from and better than that of *consistency* in that it speaks to the importance of pattern and organization in personality functioning. Individuals have patterns of stability and change in their behavior, both over time and across situations. The concept of coherence focuses attention on how the parts hang together, on the organization of the system. If we focus exclusively on individual differences, and if we aggregate only over situations, I fear that we will miss the essence of personality—the dynamic interplay among the parts of a system that can be characterized by varying degrees of complexity, organization, and integration.

Conclusion

In this target article, I have attempted to give serious consideration to the current status of trait theory and its place in the field of personality. My focus has been on the nature of the evidence and conceptual issues relative to the claims being made. I have not considered at all here problems associated with the method of factor analysis, so fundamental to the FFM, although for more

than 30 years there have been critics of the method, both within and outside trait theory (Allport, 1958; Loevinger, 1993; Tomkins, 1962). What concerns me most of all about the method, as is true for much of the trait model, is that it presents a static picture of the individual and "washes out" as residuals potentially interesting and significant data that are not readily associated with other data. The "odd" piece of behavior, the violent act committed by the otherwise passive person, is treated as a residual rather than as something telling about the person's personality and personality functioning more generally.

So where are we in relation to the current status of trait theory and its place in the field of personality? I applaud the gains being made in trait research. At the same time, I express concern about what I see as premature statements of a consensus in the field and potentially dangerous equations of personality with trait theory generally and the FFM in particular.

I think that trait psychologists are coming up with interesting findings and interesting ideas. Trait psychologists, their results, and their ideas need to be taken seriously by proponents of other points of view. At the same time, I think that they speak to a very restricted component of personality. My own guess is that this component consists mainly of temperament variables and the least common denominator of adjective descriptions of personality. In terms of temperament, the traits emphasized are close to the three temperament dimensions noted by A. H. Buss and Plomin (1984)—Emotionality, Activity, and Sociability. In terms of adjective descriptions of personality, it probably has parallels with the three semantic differential dimensions—Evaluative, Activity, and Potency.

Rather than being a serviceable system, the trait model is, I would suggest, fundamentally flawed in terms of its ability to come to grips with the issues of personality dynamics and personality pattern and organization. As noted, some of these problems are likely inherent in the factor-analytic model that is at the root of current trait theory. Beyond this, however, I am suggesting that the trait concept is fundamentally static in nature and therefore cannot be adequate to the task of describing or explaining personality in terms of the functioning of a dynamic system.

Most of all, what I want to make clear in this target article is that trait theorists express only one of many possible models of personality. It is not true that Mischel (1968) or other social cognitive theorists have suggested that individuals have no personalities (Goldberg, 1993), and social cognitive theory is not the only alternative to trait theory. In sum, it seems to me to be premature to suggest that we have discovered the basic foundations of personality and that all subsequent efforts must be based on this core foundation.

Notes

This target article is based on an address given at the Catholic University of Leuven, Belgium, March 18, 1993, as part of a memorial lecture series in honor of Professor Willem Claeys.

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