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The first problem that arises in any attempt to evaluate "trait theory" is to figure out what it is. Pervin's creative and challenging target article encounters that problem repeatedly. In one place, his essay frankly acknowledges that "trait theory is not a monolithic enterprise"; in another place, it asks plaintively, "So what ... unifies trait theorists as a distinct group?" The observation is an understatement, and the question is a darned good one.

Pervin's "critical analysis of current trait theory" seems really to have two targets. The first is the Big Five. Pervin expresses doubts about the robust ubiquity and sufficiency of five traits—maybe any five, certainly these five—to describe human personality. In my own research, although I have found the Big Five useful as a partial summary of the list of 100 traits I personally prefer (the California Q-set; J. Block, 1978), I share many of Pervin's doubts. In particular, some of the salesmanship of the Big Five may have been excessive, other alternative summary models exist and are viable, and the development of more and different models should not be precluded. But doubts like these are shared by many "trait theorists," including J. Block, Tellegen, Eysenck, Loevinger, and many others. So a critique of the Big Five, however useful it may be, is not really a critique of trait theory more generally.

The second target of Pervin's essay is trait theory more generally. The essay finds three particular aspects to criticize, so it seems reasonable to presume that these are central to what Pervin views as trait theory.

One aspect of trait theory that comes in for criticism is that it "likely exaggerates the stability of personality." In particular, it ignores individual differences in stability, ignores the way small changes in the environment can (sometimes) lead to large changes in personality, and fails to describe the mechanisms that promote stability and change.

The charge that trait psychology exaggerates stability strikes me as unfortunately reminiscent of those half-full/half-empty arguments that used to plague personality psychology's consistency controversy (Kenrick & Funder, 1988). Our understanding of consistency versus discriminativeness in behavior failed to get anywhere for the longest time because those who focused on one of these phenomena somehow could seldom resist the temptation to devalue the importance of the other. To say something about the usefulness of traits, you had to claim that behavioral fluctuations are less important than "situationists" think. To say something about the exquisite discriminativeness of human

behavior, you had to emphasize how consistency tends to be exaggerated by those interested in traits.

So let's not start again in what is really only a slightly different context. Emphasis on stability is going to de-emphasize change, and vice versa. This is a given. But that does not mean that to study one of these we must belittle the other. In particular, it does not mean that to focus on stability is necessarily to "exaggerate" it. Empirically, the inescapable finding remains that personality assessments (in terms of personality traits) made at one time often can predict independent assessments as well as direct measurements of behavior years later. Consider, for example, the work on delay of gratification by Funder, J. Block, and J. H. Block (1983) and Mischel (1984). Results like these show that there is something integral to human psychology that maintains its behavioral effectiveness over long periods of time. We could call that *personality*.

But this is not to deny that the environment has its own continuities and that people often change, sometimes dramatically. (Pervin's essay mentions the [hypothetical] case of a sudden, "violent act committed by the otherwise passive person." Notice how his phrase "otherwise passive" is a trait description that provides an indispensable context for interpreting the violent act.) Nor does the observation of consistency itself explain the mechanisms underlying personality stability or change. That is a further question that has yielded valuable research (e.g., J. Block, 1971, 1993; Caspi & Bem, 1990; Helson, 1993).

Pervin's second complaint seems to renege somewhat on his earlier promise not "to re-raise issues that have been reviewed and dealt with elsewhere." The complaint is embodied in his section title, "Prediction: Back to the .30 Barrier?" If there were one issue already dealt with elsewhere and years ago, one could be forgiven for thinking this was it. Trait measures can be used to predict behavior, and the degree to which they can do this is both practically important (e.g., Rosenthal & Rubin, 1982) and comparable to the degree to which other psychological variables—including situational variables—can do the same thing (e.g., Funder & Ozer, 1983). Nobody disputes this anymore, so far as I am aware, and that fact is perhaps one of the major concrete advances "soft" psychology can report over the past two decades (Kenrick & Funder, 1988).

Still, it remains possible to be discouraged, as Pervin apparently is, about how traits cannot predict behavior perfectly. Sometimes their predictive validity is smaller than one might desire. But anybody with a better way

to predict behavior (if, as Pervin notes, you even want to play that game) is welcome to use it. The world will surely beat a path to his or her door.

The third complaint Pervin's essay makes about trait theory is the most serious and telling. Trait theory, Pervin argues, does not offer a sufficient account of human personality. The insufficiencies are multifaceted. For example, as already noted here, trait theory—whatever it is—does not explain how personalities exhibit both continuity and change over time. It seems to me that this question is precisely what occupies the research time of some important trait theorists.

A second conceptual deficiency of trait theory is that it focuses too much on overt behavior and especially on consistencies in overt behavior. Now Pervin does quote trait theorist McCrae as saying there is no reason why traits can't include thoughts and feelings, but he rejects the comment. He writes that "trait theorists have been ambiguous in this regard." And he has a valid point. Sometimes trait theorists measure thoughts, feelings, and attitudes—especially on their ubiquitous questionnaires—but still seem to regard the ability to predict behavior as the *sine qua non* of the trait measurements that result.

If I may speak as a trait theorist—and Pervin does call me one—I think this "ambiguity" results because we trait theorists believe (a) thoughts and feelings are important to a large degree because of their effects on what people do and say and, (b) more fundamentally, the only way, besides ESP, to know what somebody else thinks and feels is to watch what he or she does or says. This latter fact—and it is a fact—does blur the operational distinction between behavior on the one hand and thoughts, feelings, and motives on the other, but I don't see how it is helpful to think of the necessarily inferential relation of outer and inner manifestations of personality as an ambiguity. It's just the way the world is.

Which brings us to the nub of Pervin's critique—the place where he states an issue is "of particular concern to me" and where he makes his most compelling argument. Traits do not by themselves explain behavior. They describe patterns and consistencies in behavior, but they don't explain where those patterns and consistencies come from either developmentally or in terms of their proximal causation. What traits do not tell you, in particular, is why somebody is doing something—that is, his or her motives.

Guilty as charged. Trait concepts on the one hand and motive concepts on the other hand are indeed, as Pervin states, distinct concepts. For one important thing, motive concepts can be used to explain trait concepts, but not vice versa. Murray, McClelland, Wiggins, and Pervin are all correct to insist that patterns of behavior and motivations for behavior must be kept distinct. And they are further correct to argue that, for that reason,

traits can never offer a full account of behavior and, to that extent, in Wiggins's vivid phrase, remain forever "lost causes."

Still, to say that something is not a complete explanation is not to say that it is no explanation at all. Complete explanation is a pretty remarkable thing to demand, after all. So you did something because of your motive. So where did the motive come from? By themselves, motives aren't complete explanations either, and neither is any other kind of concept. To paraphrase myself (Funder, 1991), to any answer to any question, one can still ask, Why? Every small child knows this, as anybody who has ever been interrogated by one can attest.

I don't mean to be critical of motives. They're useful for explanation, despite their inevitable insufficiency. But so are traits. I have a friend and colleague who has served as a faculty member in several departments. In each job, he's been miserable. He can enumerate very persuasively why each of his department chairs was an unfair tyrant, why each teaching load was excessive, how office space and salary were unfair and paltry, and so forth. He really is persuasive. But you know what? I think he'd be unhappy anywhere. I think he's got a trait. He exhibits a behavioral pattern (of complaining) from which I infer an emotional pattern (of feeling miserable) that I think does explain why he is saying such nasty things about his latest department. And this explanation is—in an almost psychodynamic way—different from the explanation he would offer himself.

To explain a behavior in terms of the broader pattern of which it is a part—as I have just done—can be a legitimate and useful step in the (infinite) explanatory regress. I know, it's not complete. We still don't know why my friend is such a negativistic cynic. But identifying him as such provides (a) insight into his current actions (and feelings, which we infer from his actions), (b) a basis for predicting his future actions and feelings, and (c) a useful analytic rest stop. We have explained his behavior; take a breath; now we need to explain the explanation. And—fair warning—that explanation, whatever it turns out to be, will need explanation too, and on to the end of time. This is the argument (made in my 1991 article) that Pervin notes is "not frequently seen by me." I don't see it very often either, but that doesn't make it wrong.

A final comment. Pervin's commentary raises a large number of important points about which it is useful to think and debate. But I hope it does nothing to push the enterprise of conceptualizing and measuring personality traits further into the corner of psychology in which it seems perpetually in danger of being relegated. Some trait psychologists seem all too happy to talk and write in a way that is of interest and sometimes even comprehensible only to one another. Worse, I am running into a lot of non-trait psychologists who seem to regard the

conceptualization and measurement of traits as a limited and archaic enterprise.

What they don't realize is that it's an enterprise they can't escape. For example, one psychologist of my acquaintance is interested in behavioral genetics and is intrigued by heritabilities, niche-picking, and so forth. Yet he has been heard to say, "I think personality assessment is overemphasized." But what do people inherit, if not traits, and what is the result of choosing a societal niche, if not a stable pattern of behavior, and how can you measure either of those without personality assessment?

A wide variety of modern research studies individual differences in people's goals, in their implicit theories of the world, or in how they characteristically cope with stressful life situations (e.g., optimistically vs. pessimistically). This work is valuable and indeed constitutes an important new wave of personality research emphasizing the proximate causal processes underlying behavioral regularities. But this work, too, essentially consists of the conceptualization and measurement of *traits*, however heroically its investigators sometimes try to avoid the term. And such avoidance sometimes has the results that the measurement of these individual-difference variables fails to take full advantage of the available technology and that the overlap (if not sometimes redundancy) between these new individual-difference variables and other, more venerable ones not infrequently goes unexamined and unknown.

So, I hope none of Pervin's target article is taken to mean—and I don't think he wants it to mean—that trait conceptualization and measurement should in any way be de-emphasized or, just as bad, isolated from the rest of psychology. Nearly all psychologists wish to learn what it is about our subjects that will affect their future outcomes. And, even more basically, we also want to know what our subjects are doing (and feeling) in general. If you ask questions like these, you are a trait theorist too, and welcome to the club.

Note

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