The Journal of Mind and Behavior
Summer and Autumn 2006, Volume 27, Numbers 3 and 4
Pages 275–300
ISSN 0271–0137

The Practical Dangers of Middle-Level Theorizing in Personality Research

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Personality research has functioned under the prevailing influence of middle-level theorizing sufficiently long to justify consideration of the effects of this approach. Despite improvements in precision and testability of hypotheses, with resulting increases in volume of research, the pervasive effect of several practical dangers of middle-level theorizing are identified. These involve the unappreciated failure to test comprehensive theories when concepts from them have been extirpated, overly-weak justification of research methods, a vanity of small differences, and insufficient theoretical precision in framing empirical efforts. Ways of avoiding these dangers are explored, and it is concluded that the most promising is a comparative analytic stance toward inquiry that reconsiders comprehensive theorizing without courting ambiguity and imprecision.

Keywords: middle-level theorizing, personality, comparative analysis

For some time now, the research study of personality has been dominated by middle-level theorizing. This approach avoids elaborate assumptive systems concerning matters far removed from immediate observation and makes no pretense of comprehensiveness concerning human behavior (Conant, 1947). The emphasis of middle-level theorizing on concreteness and simplicity was fueled in the personality area by the joint and powerful impact of the sociologist, Merton (1957, 1968), and the psychologist, Skinner (1953). Both being major forces influencing academics, Merton offered a declaration of independence for researchers overwhelmed by the daunting complexities and ambiguities of elaborate, comprehensive theories, and Skinner insisted that to theorize at all was an unscientific obfuscation of the observables that constitute the only facts.

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Consequently, the hypotheses arising from middle-level theorizing have been concrete enough to be readily tested in research and often have the ring of common sense. These characteristics of testability and common sense have seemed advantageous to scientifically-minded psychologists for whom the comprehensive personality theories popular in the past seem hopelessly abstract, ambiguous and imprecise. That these comprehensive theories were inspirational, aiming to provide prescriptions for the betterment of human living, swayed clinicians in their favor more than researchers. Indeed, researchers suspected the prescriptions as being somewhere between dangerously ineffective helping attempts and outright tricks with which clinicians manipulate their clients. Middle-level theorizing, though less grand in its aspirations, seemed a needed antidote to the vagaries of more comprehensive attempts (e.g., Mendelson, 1993).

Middle-level theorizing has held sway in personality research long enough now that attempts at evaluating its effectiveness are warranted. Has it led to more convincing findings? Has it decisively increased our understanding of personality? These questions are admittedly too large to be answered by any one person in any one article. But, it may be useful to make a start. Certainly, middle-level theorizing has dramatically increased the amount of personality research that gets done. The concreteness in such approaches has lent some clarity to hypotheses and research strategies, and minimized the potential ambiguities attending elaborate interpretation of results. What we conclude, we appear to know conclusively. I do not dispute the value of these advances.

What I should like to do, however, is explore the pitfalls particular to middle-level theorizing that jeopardize how convincing is the research following from it, and raise doubts about how much we are learning about personality in the process. Having done this, I will reconsider whether and how more comprehensive theoretical approaches might help after all.

How Middle-Level Theorizing Leads to Research That Fails to be Convincing

Extirpation of Parts of Comprehensive Theories

There are, of course, quite a few comprehensive theories of personality, such as that of Freud (1901/1960), Jung (1933), Rogers (1961), Adler (1927), and Frankl (1960). Making many assumptions and considering the full range of their interrelationships, these theories and others like them aim to give a rather complete picture of the thoughts, feelings, and actions of persons throughout their lives. The presence of these comprehensive theories complicates matters for anyone attempting middle-level theorizing. It is reasonably likely that middle-level theorizing will cover the same or similar ground traversed by comprehensive approaches. The danger here is that middle-level approaches may appear to have empirically supported or disproven some of the conceptualizations of the related comprehensive approach without actually having done so.

This danger is greatest when the assumption focused upon in the middle-level approach has been extirpated, as it were, from the comprehensive approach. Because of the extirpation, there appears no necessary reason why the empirical procedures employed by the middle-level research should be appropriate to the wider implications of the comprehensive approach. Nonetheless, it is so seductive to believe that the results obtained from such middle-level research is somehow relevant to the implications of the more comprehensive approach.

There are many examples of this mistake in personality study. Although it is dated now, one example is so famous and yet so blatant that is may suffice to make my point. The importance of this experiment by Mowrer (1940) lay in the behavioristic claim that through laboratory investigation with simpler forms of life than humans the psychologist could clarify what there was of empirical value in Freudian formulations. It is a hallmark of the brave attempt to approach comprehensive, and maddeningly elusive psychoanalytic formulations of personality through the light of pragmatism and common sense.

Following through on this formulation, Mowrer extirpated a familiar defense mechanism, regression, from the broad and complex context of psychoanalytic theorizing. He used small numbers of rats in an experiment involving electric shocks and cage equipment. One group was permitted to learn by trial and error that the shock through the floor grid on which they stood could be turned off by pressing a foot pedal. Another group was given shocks through the floor grid for an approximately equal amount of time, but had no pedal to press. Eventually, these latter rats learned to minimize shock by sitting up on their hind legs. When sitting up had been learned well, a foot pedal was inserted and they were permitted to learn how to use it to turn the shock off completely. From this point on, both groups were then frustrated by having shock introduced to this foot pedal, so that when they tried to turn off the floor shock they got a shock from what had been the instrument of salvation. The rats that had originally learned to minimize shock by standing on their hind legs almost immediately "regressed" to that behavior, while the other rats persisted in pushing the pedal despite its ineffectiveness.

What is unconvincing about this experiment as a demonstration of regression? The major problems all stem from failure to take into account that regression is a notion inextricably embedded in a more comprehensive theoretical framework. In other words, the very middle-level theorizing that prompted this experiment is its own undoing! For psychoanalysts, regression involves going from a later stage of psychosexual development to an earlier one. Conceptually, this regressive movement involves both fixation and frustration. By fixation, Freud (1901/1960) meant the overly strong attachment to a particular, earlier stage of psychosexual development, and by frustration he meant the current prevention of instinctual gratification in whatever stage persons find themselves. In regression, when frustration becomes strong, retreat to a previous, fixated psychosexual stage may take place.

Now, it is possible, I suppose, that rats have psychosexual development in some rudimentary sense. At least, their mating behavior improves with practice. But, even if this were considered to qualify the rat as an organism on which the concept of regression can be well tested, it seems clear that no test has actually been made in Mowrer's study. After all, standing on your hind legs or pushing a pedal to avoid shock are not actions that convincingly express psychosexual development. Notice that Mowrer (1940) felt little need to justify the choice of rats as participants, and of standing up or pushing a pedal as psychosexually relevant measures of behavior. All this experiment has demonstrated is that a newly learned habit may be relinquished, when no longer effective, for a previously learned and still reasonably effective one. One thing is clear: if defensiveness is to be studied on non-human organisms, they must at least have some form of psychosexual development analogous to humans, and the specific behaviors measured must be convincing analogues of the human behaviors focused upon by psychoanalytic theory. Were it not that middle-level theorizing tends to dull our awareness of these requirements, we might not be so quick to study personality concepts in non-human organisms using the simple and contrived responses common in many laboratory experiments.

Weak Justification of Research Methods

Fortunately, middle-level approaches do not always suffer from the shadow of related comprehensive theories. But, there is still the danger inherent in the weak justification of research designs and measures that surrounds middle-level theorizing. As Merton (1957, 1968) made clear, middle-level approaches typically start with one or perhaps two theoretical assumptions and proceed to apply these assumptions to various areas of observation in a pragmatic fashion, without elaborate justification of just why *this* observation and not *that* one. This reveals that the task of middle-level theorizing is significantly inductive. However freeing this sounds, we should also recognize that the empirical plans thus generated will not require much theoretical justification of designs and measures. It is not surprising that the designs and measures generally employed in middle-level approaches have been justified primarily on the grounds of feasibility and precision, without much concern for whether they fully fit the denotations and connotations of the theoretical task at hand.

Sometimes, this weak justification of designs and measures has led to studies that defy even common sense and general credibility, to say nothing of relevance to comprehensive theories. There are, unfortunately, many examples of this problem. Let us consider a few.

First, I will review the contributions by Bem and Funder (1978), and Funder (1983) to the ongoing question of whether behavior shows consistency across situations. They put forward the view that in order to find behavioral consistency, one must match the personality of the person to the "personality" of the situation. When these "personalities" match, person variables should be predictive of behavior in the situation. This certainly sounds like a welcome voice of rationality and theoretical sophistication following the ravishes of the person–situation debate of the late 1960s and early 1970s. But, Bem and Funder focus on what they regard as a major implication of their view, namely, that when situations have similar "personalities" we should expect to find that people will display the same behaviors in them. They do not seem to recognize that once you have decided to go beyond superficial appearances to the underlying meaning of situations, you must be prepared to do the same thing for the behaviors involved. Ostensibly similar behaviors may mean different things in different situations and to different persons. And, ostensibly different behaviors may mean similar things in similar situations and to similar persons. Prediction is simply more complex than the blithe assumption that uninterpreted behaviors in similar situations will be highly intercorrelated. The trouble may well be that the notion of personalities for situations as well as for persons seems to have arisen more out of the concrete studies framing the person–situation debate than out of any comprehensive theory of persons and situations. Given the common sense, practical, middle-level theorizing that framed the enterprise, it is not surprising that these researchers somehow failed to follow through on the implications of their position.

Despite not grouping responses by their underlying meaning, Bem and Funder (1978) report findings from the comparison of two situations that appear conceptually equivalent, but emerge on the basis of Q-sorts to be functioning quite differently. They then suggest that the absence of behavioral similarity shown across these two situations supports their view. But, as Mischel and Peake (1981) point out, these findings are actually irrelevant because a different set of participants performed in each of the two tasks, even though the logic of the position would seem to require the same set of participants. Further, the clearest test of the position would have involved two or more situations that appeared dissimilar but emerged as functionally similar in the Q-sorts. Then one could at least have tested whether behavior was consistent across them, though, of course, the problem of behavioral meaning mentioned before would have persisted.

Mischel and Peake attempted a more adequate test of the Bem–Funder position by utilizing the same set of participants in two performance situations, with many other features of the procedure the same or similar. They found that not only did the two situations appear similar to the investigators who planned them, but they also emerged as functionally similar in the Q-sorts. The significant but low level of behavioral consistency across situations appeared, therefore, to disconfirm the Bem–Funder prediction. Mischel and Peake seem almost surprised at these results, and spend effort trying to understand why behavioral consistency did not emerge. In critiquing aspects of sampling procedure and data combination, they indicate that one should not do this kind of research without specific, articulated theoretical reasons to guide decisions. And these researchers appear to have influenced Bem (1983) in this direction as well.

I could not agree more, and would indeed go beyond the points raised by Mischel and Peake. There are many basic questions of appropriateness of procedure that have gone at least unanswered and perhaps unasked in these studies. Why were the mothers' Q-sorts about their children and the situations used, when it was actually the children who performed the tasks? Although the mothers' judgments might well have accurately depicted their views of the situations, it is the children's personalities and views of the situations that are relevant, as it is their behaviors that were scrutinized for consistency. The pragmatic, middle-level answer that mothers are adults and therefore can be more articulate than children about things on Q-sorts, is not in any sense convincing. It only raises the further question of why children are being used as participants in a study of behavioral consistency in the first place? Some personality theories consider children to have relatively unformed personalities and, hence, to be less stable in their behaviors and less definite in their construal of situations than they will be at a later age. Having gone this far, the question that comes next to mind is why the delay of gratification performance involved in these studies seemed a particularly relevant one with which to test the behavioral consistency notion. Does not this behavior require a high level of ego control? On these bases alone, one would not expect much behavioral consistency, to say nothing of the crucial question already raised about the discrepancy between ostensible and underlying meanings of the behaviors measured (Mischel and Peake's approach to behavior being similar to that of Bem and Funder in this regard).

Unless we wish to be implicit in our theorizing, we must realize the large number of assumptions that have to be made in order to answer any one of the questions. Not much progress will be made in this task with middle-level formulations. And, as the whole set of questions needs to be answered in a mutually compatible manner for the sake of internal consistency, what is called for is a comprehensive theory of personality, situations, and behaviors.

By taking such an approach, it will be possible to decrease the likelihood of making the procedural decisions framing our research in an inconsistent or unconvincing manner.

I do not wish to give the impression that problems in the design and measures of research traceable to middle-level theorizing are restricted to some content areas. To suggest that this is not the case, consider two more examples concerning psychotherapy outcome research. The two studies involved have fostered the conviction among many investigators that improvement resulting from psychotherapy is largely a function of whether clients believe that the procedures they undergo can help them. In other words, what gets called beneficial outcome may be little more than a placebo effect.

In the first study, Barkovec and Nau (1972) attempted to show that treatment procedures appear more credible as vehicles for personal improvement than do control procedures typically used in therapy outcome research. Volunteer undergraduate participants rated the credibility of descriptions and rationales of two therapy, three placebo, and one other control procedure frequently used in psychotherapy outcome research. The rating scale was intended to yield information about both the credibility and improvement expectancy generated by the various rationales. The participants were 450 undergraduates enrolled in an introductory psychology course.

In general, the various control procedures emerged as less credible than the therapy conditions. The conclusion is reached that when research appears to have demonstrated the value of a therapy condition by comparison with a control condition, what may really be going on is the triumph of a credible rationale over a non-credible one.

Why is this conclusion not particularly persuasive? First, there is the age-old problem of using undergraduate participants conscripted for the task because they are students in a class. If the study were to have used persons applying for therapy due to some felt personal problem, their need might have led them to find all described procedures a lot more credible, including the control conditions. Secondly, credibility ratings were made merely on the basis of the written rationales provided. One way in which this is problematic is that all therapy and control treatments would likely have become more credible if participants had actually experienced them rather than just read short verbal descriptions. There is no way of telling whether the treatments would have been differentially credible if they had actually been experienced, and such experience is more germaine to the issue of what produces therapeutic change than is the mere rating of a short description.

The other problem is that the written descriptions were taken by the investigators from the research papers describing the various therapy and control conditions they had used. It seems likely that investigators writing up positive results concerning some therapy they believe in will take more trouble in rendering it credible to the reader than they will a control condition having little theoretical interest. It is not clear that the descriptions used by Barkovec and Nau (1972) bore much resemblance to the experienced impact of the therapy and control conditions involved in the original research studies from which they abstracted verbal descriptions. In short, these investigators may have unintentionally favored their conclusion in their procedure and, hence, taught us very little about the effects of psychotherapy.

The second study I wish to discuss builds on that of Borkovec and Nau (1972). In it, Shaw and Blanchard (1983) attempted to show that the instructional set given to clients in a stress management program determines the program's effectiveness. The same stress management program was presented to a "positive demand set" group of eight participants as having previously proven very effective, and to a "neutral demand set" group of eight participants as being experimental and hence of unknown effectiveness. Then, these two groups were trained over five sessions in a conglomerate approach to stress management. These groups were compared to a "waiting list" control group of six participants.

Results showed that although the positive and neutral demand set groups did not differ in initial expectation of therapeutic benefit, the former group increased in this expectation as the latter decreased during the course of stress management training. In addition, the positive demand set group had consistently higher expectation of future benefit (after the training was over) than did the neutral demand set group. Although these groups did not show any differences on various standardized psychological tests, the positive demand set group did show some tendency from pre- to post-training testing to increase in reported ability to handle stressful situations, though both groups also reported themselves as improving over time. Finally, though the groups did not differ on several physiological measures, the positive demand set group did show less systolic blood pressure reactivity to a laboratory stress test than did the neutral demand set group. This may have been due to the fact that the positive demand set group reported practicing the stress management techniques they were taught twice as often as did the neutral demand set group. On the basis of these results, the conclusion is reached that "giving participants a high initial expectation of therapeutic benefit from stress management training has significant benefit in terms of self-report of change and reduced physiological reactivity, and that these improvements are mediated at least in part by increased compliance with home practice instructions" (Shaw and Blanchard, 1983, p. 564).

Rather, I question whether it was the set-inducing procedure that could have had even the small effects reported. First, the instructional set differentiating groups amounted to only two sentences. The positive demand set group heard, "The treatment program you are about to undertake has been in extensive clinical use for some time with highly successful results. The purpose of our study is to chart the course of your progress as you learn the skills for management of your stress" (p. 558). In contrast, the neutral demand set group heard, "The project you are about to undertake involves having you learn various relaxation techniques. By and large, these procedures are derived from laboratory experiments, and our aim is to assess their validity" (p. 558). It is to me hard to believe that these brief instructions, in some ways not that different, would have an effect stronger than participation in a bonafide, five-week stress management program of once-weekly sessions of one and a half hours each. If the stress management course was as engrossing as such things can be for persons and literally the same for both groups, would not whatever preliminary effects there might have been due to the instructional set have been wiped out?

Let us turn to the stress management program actually employed to find an answer to this question. Of initial importance is that it involved bits and pieces of commonly employed approaches. Included was "didactic material on the nature of stress and its effects" (p. 559), then a bit of training in progressive relaxation, meditation, breathing exercises, cognitive and action coping procedures, assertiveness skills training, and a general review of all other approaches. There could not have been time to go into any depth regarding these various procedures, to say nothing of the likelihood that some of them are at variance in rationale and actions with others. It is quite possible that this pastiche, euphemistically called a combination by the authors, had little deep or lasting effect on participants. Indeed, an extensive review of outcome research (Lehrer, 1982) indicates that relaxation training tends not to work with less than five sessions carefully adapted to the needs of the individual participants. And, relaxation training was only one of the many approaches included in the pastiche.

So, then, what led to the increased expectancy of benefit, greater practice of stress management techniques, and lowered systolic blood pressure from pre- to post-testing in the positive demand set group? It becomes apparent in the write up of this study that the one experimenter who trained both groups was not blind as to which group he was dealing with! The authors say, "Although at a theoretical level it might have been interesting to have an experimenter/trainer who was 'blind' to the instructional set given each group, this did not seem practical or realistic" (p. 558). Whereas a two sentence instructional set may not reasonably be expected to strongly influence long-term performance, subtle but continuing training pressures consistent with these instructions could very well have that effect over five, $1\frac{1}{2}$ hour sessions.

The pair of studies just critiqued are flawed. Nonetheless, they went through a peer review, were published in reputable journals, and have convinced some readers that instructional set is a (perhaps the most) potent factor in beneficial psychotherapeutic outcome. We seem to be operating with rather low standards for justification of research designs, measures, and inference. At least some of this, it seems to me, can be traced to the laxity that is a danger of middle-level theorizing. If there are strong, comprehensive theories underlying the various stress management treatments, for example, would anyone have found the five-session pastiche contrived by Shaw and Blanchard adequate?

A Vanity of Small Differences

A definite implication of middle-level theoretical approaches is that many discrete hypotheses regarding the same or similar phenomena can exist concurrently in an area. This is because each investigator may well start with just one assumption (e.g., Durkheim's famous hypothesis that suicide rate is a function of the normlessness of a society), applying it in one or more concrete contexts by procedural elaborations that seem dictated by methodological concerns or common sense. One positive feature of having many discrete hypotheses is that it leads to a lively empirical competition. When this happens, many papers get published and the area becomes hot.

But, there is an insidious danger here that may well show itself. Signs of it are the entrenchment of several hypotheses, each of which has a certain amount of empirical evidence in its favor. A more advanced stage of the difficulty is when the hypotheses, surrounded by followers, promote the development of schools of thought, increasingly insulated from each other, though the differences between them seem small to outsiders. There is a vanity in all this insofar as a more concerted attempt to theorize comprehensively might collapse hypotheses together, on such grounds as being special cases of each other, or differing expressions of the same underlying process.

A research area that has included such small differences is intrinsic motivation. The basic phenomenon studied is the paradoxical decrease in performance of a preferred activity, which takes place when an external reward is imposed. Many investigators have proposed explanations of this phenomenon. Deci (1971) offered the cognitive evaluation hypothesis that if a reinforcement is perceived as an external source of control, then intrinsic motivation to perform is reduced. Also, resentment toward the reinforcer builds (Assor, Roth, and Deci, 2004). Kruglanski (1975; Fishbach, Friedman, and Kruglanski, 2003; Shah and Kruglanski, 2003) proposed endogenous versus exogenous attribution, similarly focusing on the reasons for instead of the causes of behavior. Then, there is the over-justification hypothesis of Lepper and his colleagues (Lepper and Greene, 1975; Henderlong and Lepper, 2002): behavior originally justified by intrinsic motivation becomes over-justified with the addition of extrinsic reinforcements. Reiss and Sushinsky (1975) offered a competing response hypothesis suggesting that participants will be less interested in playful activities to the extent that extrinsic rewards elicit responses that interfere with playful responses. There is even a delay of gratification hypothesis (Ross, Karniol, and Rothstein, 1976), which focuses on the inhibiting effects of delay between promise and receipt of reward.

The empirical studies done in furtherance of these hypotheses are intriguing and sophisticated. Each hypothesis has its own kind of empirical support. But, as DeCharms and Muir (1978), and Reiss (2004) concluded in their reviews, the great bustle of activity in this field has led to little consensus or accumulated sense of understanding, and the problem may be the insufficient theoretical elaboration of the many proposed explanations. The many hypotheses are by and large the result of middle-level theorizing. As such, they are rich in unelaborated and implicit assumptions. I suspect that at the underlying level of implicit assumptions, there is considerable similarity among these hypotheses. In order to reveal this underlying relatedness, we will have to theorize more comprehensively about persons, situations, and their effect on behaviors. Perhaps the long-awaited accumulation of discrete facts into overall understanding without any more comprehensive effort on our part, which has been promised by advocates of middle-level theorizing (Merton, 1968), will just not happen.

Insufficient Theoretical Precision

Insufficient theoretical precision is another danger of middle-level theorizing. It comes out of a willingness to state a hypothesis without fully elaborating all of the implicit implications involved, and to accept rough and ready formulations as long as they lead one quickly to empirical investigation. Sometimes, middle-level theorizing supports the unscrutinized belief that we all know what is meant by familiar theoretical concepts, and that common sense can be our guide.

It is, of course, virtually impossible to delineate just how much theoretical precision is enough. It is much easier, however, to identify when theorizing is insufficiently precise to be convincing. At times like this, the theoretical enterprise seems awfully arbitrary and ambiguous, in a manner that does not just reflect whether we happen to agree with a line of argument. Typically, in the case of insufficient precision, we cannot even get to the point of determining if we agree or not.

An article by Schroeder and Costa (1984) is a good example. The study is a methodological critique of measures of stressful life events, and aims to question whether the accumulating evidence for stress-related disorders can be believed at all. The authors theorize that measures of stressful life events are suspect because they include items contaminated by illness, ambiguity, and neuroticism (which is their term for negative affectivity). In truth, the early framers of stressful event checklists included a few items relating to illness (e.g., "Your health worsened") on the grounds that illness events are themselves stressful. Schroeder and Costa are justified in worrying about this methodologically (when studies are to determine the correlation between stressful events and illness symptoms), and it is fortunate that investigators of stress-related disorders now routinely delete illness items from stressful event checklists. Their critique also seems justified regarding ambiguous events, as responses to them might express a host of biases too complex for the investigator to unravel. Schroeder and Costa are sufficiently clear about what constitutes an ambiguous item with which the reader can straightforwardly agree or disagree. Once again, it is fortunate that investigators in this area have taken to deleting or amending stress items that were ambiguously worded when originally introduced.

The major theoretical difficulty in the article by Schroeder and Costa (1984) concerns the rationale that some stressful event items are contaminated by neuroticism (negative affectivity). The authors use "psychological adjustment" as the opposite of "neuroticism" and indicate that event items can be contaminated by neuroticism in two ways. Items

> may be direct symptoms of psychopathology (e.g., "major change in eating habits," "sexual difficulties") . . . or they may represent events that could be the result of neurotic traits or tendencies, such as divorce or being fired from a job. In other circumstances, occurrence of the event could reflect maladjustment by the individual along with environmental changes. (Schroeder and Costa, 1984, p. 854)

Is a major change in eating habits such a clear sign of psychopathology? Depending on the circumstances involved, might not getting divorced be as likely a sign of mental health as of neuroticism? May it not be that getting fired from a job is as likely to reflect economic circumstances or even someone else's neuroticism as one's own? Questions such as this never get posed much less answered by the authors, who assume that readers will all share one unambiguous view of neuroticism that is so obvious as not to need discussion. This insufficient elaboration of theory falls far short of convincing that stressful event checklists may be just another measurement of psychopathology.

To their credit, Schroeder and Costa list in an appendix their grouping of typical stressful event checklist items into those which are supposedly uncontaminated and those with the various sorts of contamination they have suggested. But, what we find here is not at all reassuring. They rate "Laid off the job," "Unemployed for at least a month," and "Quit your job," as uncontaminated, whereas "Job demotion," "Falling behind in payments on a loan or mortgage," "Repossession of merchandise," and "Credit rating difficulties," are all regarded as expressive of neuroticism (negative affectivity). If you are unneurotically laid off from or quit a job, and are hence unemployed for at least a month, are you not rather likely to fall behind in payments on a loan or mortgage, and thereby risk credit rating difficulties and repossession of merchandise? At what point, by what theoretical stratagem, does one suddenly become neurotic? Surely not very likely just because one is incurring the social consequences of previous social pressures or actions. If Schroeder and Costa mean this, then they must develop a theory to cover it, though I think it would be difficult. If they do not mean this, then what do they mean? Obviously, it would not be very convincing to say that the grouping of event checklist items was done empirically, because the initial argument they present requires a conceptual rather than empirical classification. Whatever their response, it must be concluded that the article as published does not make its case unless we are willing to accept a vague and unsubstantial conceptualization. Middle-level theorizing makes it more likely that we will accept their ambiguities, and therein is one of its dangers.

Insufficient theoretical elaboration also attends the idea that because a stressful event item may or may not be endorsed a particular way by persons with negative affectivity, that item should be dropped as having no legitimate worth. This assumes that endorsement of such an event item is not a reliable gauge of whether the event actually happened (it could be a figment of a neurotic imagination), or whether it could have happened to a non-neurotic person (its occurrence somehow being restricted to the circumstances surrounding negative affectivity). Schroeder and Costa would have to provide a convincing rationale for this assumption, as the event items they rated as contaminated with neuroticism do not make their case immediately apparent. Most of the items they classified as contaminated are definite enough (e.g., "Job demotion," "Falling behind in payments on a loan or mortgage," "Credit rating difficulties") that there would seem little question of whether they happened to the person or not. As to whether such events only befall persons with negative affectivity (or neuroticism), we would have to contend that persons without this difficulty never have "Problems with parents," or "Other troubles with your children," or "Argument or fight with close friend or neighbor." On the face of it, without any further theoretical ado, the position taken by Schroeder and Costa is simply not convincing.

Lurking behind their position is the whole questionable quest for pure measures of complex behaviors. If a measure of dominance correlates at .30 with a measure of intelligence, should we not rest easily until we have partialled intelligence out? When we do this, however, we are left with a measure of dumb dominance, which may not be nearly as socially potent as intelligent dominance. When we use our emaciated measure of dominance in studies, are we really learning more about this phenomenon as it occurs in the real world?

If two psychological measures were correlated at about .70, we might be tempted to conclude that there was really only one variable involved. But, height and weight are very likely correlated that highly, and in that physical realm, we do not conclude that there is only height, or only weight, or even that the best expression of the way things are is that there is only one composite variable, such as mass. We continue to believe in the reality of height and weight because both seem cogent observationally, make a big difference in life activities and decisions, and involve measurement operations that are very different (rulers as opposed to scales). Is it too much to expect of psychologists that they can believe in both stressful events and negative affectivity, when these seem observationally cogent, have different effects, and are measured by different operations? Schroeder and Costa (and they are hardly alone) do not seem to think it necessary to contend with this logic. And, this contention would not even be close to the theoretical elaboration of neuroticism required by the stance they have taken (Costa and McCrae, 1995).

The tendency not to elaborate theory sufficiently to deal with the task at hand is fairly widespread in our field, and this may be the result of a commitment to middle-level theorizing of sufficiently long standing to have influenced the scholarly endeavors of a generation of personality psychologists. In reviewing an edited volume (Cantor and Kihlstrom, 1981) meant to display the state of the art in personality study, Hogan (1982, p. 852) felt it necessary to point out "that the cognitive social learning theorists repeatedly criticize positions that do not exist. Along the way, however, they are guilty of a range of other factual and logical errors." Social learning theory is, of course, one of the outgrowths of a middle-level rejection of comprehensive theorizing in the personality area. According to Hogan (1982, pp. 851–852), the book he reviewed failed to meet its modest theoretical requirements, much less showing some understanding of such comprehensive theories as that of Freud. So, a penchant for middle-level theorizing, continued long enough, may lead toward mediocre scholarship.

Nor do we seem to be learning from the ineffectiveness of middle-level theorizing. Even the recent resurgence of an emphasis on positive psychology (Carver, Lehman, and Antoni, 2003; Isaacowitz, Vaillant, and Seligman, 2003; Seligman and Csikszentmihalyi, 2000; Snyder and Lopez, 2001) has been marred by insufficient conceptualization in its emphasis on happiness, the foundation stones of which are optimism (e.g., Peterson, 2000; Seligman, 1991) and a sense of subjective well-being (e.g., Diener, 2000; Diener, Sue, Lucas, and Smith, 1999). The major justification, currently, for the centrality of optimism and subjective well-being is the extensive research on the relationship of these two human qualities to various signs of good psychological and physical health (cf., Diener, 2000; Peterson, 2000).

Once one delves into the research literature, however, many unresolved issues arise. As to optimism, there is ambiguity as to what are its main characteristics, and how they should be measured. Most involved psychologists emphasize that it is a feeling that things will turn out for the best, and a tendency to see oneself in the best possible light (cf., Armor and Taylor, 2003; Peterson, 2000; Peterson and Park, 2003). It is not clear, however, whether optimism is expressive of active efforts or complacency. Indeed, Taylor (1989; Armor and Taylor, 2003) has even gone so far as to consider optimism a "positive illusion," rather than something reflecting coping efforts.

Further, there are other unresolved issues concerning the optimism concept. One is the difference between little versus big optimism (cf., Peterson, 2000). In being optimistic, it is a very different thing to believe that you will find a convenient parking space, and to believe that we are on the verge of world peace. Optimism researchers are not sure how, or if little and big optimism go together. Another issue has to do with whether optimism is an expectation exclusively, or something that operates in combination with emotions and motivations (cf., Peterson, 2000; Peterson and Park, 2003). If the latter, optimism may lead to coping efforts. But, in the former case, optimism may be an end in itself, or even eliminate the motivation for protective behaviors.

Another issue concerning both optimism and subjective well-being involves the relationship between the positive emotions emphasized in these two stances, and negative emotions that obviously also exist. Optimism researchers usually regard optimism and pessimism as negatively related expressions of each other. Similarly, subjective well-being researchers tend to consider this stance to be negatively related to unhappiness about one's life. But, some findings indicate that positive and negative emotions are empirically unrelated (Bradburn and Caplovitz, 1965; Carver, Lehman, and Antoni, 2003; Scheier and Carver, 1985), suggesting the need for a more complicated conceptual approach. This also has research implications for the stances of both optimism and subjective well-being.

There are also other similar issues to those in optimism that remain unresolved with regard to subjective well-being (cf., Carver, 2005; Diener, 2000). For example, is the measurement of subjective well-being best approached through global judgments of one's life, or satisfaction with specific domains (e.g., marriage, work). This is especially problematic, as Schwartz and Strack (1999) have shown that global measures of life satisfaction can be influenced by one's momentary mood in responding to a questionnaire, and by socially desirable responding. It is also unclear as to whether the measurement emphasis in subjective well-being research should be on the frequency or intensity of pleasant emotions, as some findings show that feeling mildly pleasant emotions most of the time may be sufficient for reports of happiness (Larsen and Diener, 1985).

What is to be made of the ongoing definitional and measurement issues surrounding optimism and subjective well-being? It is my belief that they will not be resolved by a continuing commitment to middle-level theorizing, and the hope that further accumulation of research steeped in that approach will somehow clarify everything. Instead, conceptual effort needs to be expended in determining how optimism and subjective well-being fit into the process of daily living. This effort needs to incorporate comprehensive theorizing about personality and its interaction with social circumstances. Through such effort we will be able to determine not only how best to measure and test these expressions of happiness, but also whether they are the concepts on which we should be expending our research efforts.

Actually, relevant comprehensive theories of personality have been available for quite some time, but overlooked by researchers on optimism and subjective well-being. In particular, the theories of Rogers (1961) and Maslow (1968) incorporate optimism and subjective well-being into their conceptualizations of the ideal personality type, and also delineate the developmental process whereby this and the non-ideal personality types come about. In addition, the personality theories of Adler (1927) and Frankl (1960) are relevant in contending that optimism and subjective well-being are either less important than, or even subordinate to courage (cf., Maddi, 2002, 2004). There is much in these comprehensive theories that could have guided (and could still guide) the planning and interpretation of research in a manner to resolve, and perhaps even avoid unimportant controversies, to say nothing of integrating the positive psychology research movement (Maddi, 2006a).

What Shall We Do?

The position I have taken is that personality research can be surprisingly unconvincing and that the laxity inherent in middle-level theorizing is at least partially to blame. Although only a few examples were considered above, many more could have been included. The examples were chosen to cover many years in personality research, suggesting that the passage of time alone has not stifled research efforts driven by middle-level theorizing. Although some early examples are so obviously extreme that the particular approach was stifled (e.g., Mowrer, 1940), other less blatant limitations have been repeated with regard to other personality variables. In any event, the research that has been discussed involves substantial investigators publishing in reputable journals. All the more reason for us to consider whether corrective steps need to be taken, and if so, which would be most effective.

Should We Continue to Assume that Problems Automatically get Resolved in the Research Process?

An expression of the assumption that the research process automatically takes care of all problems is the conclusion that the situation in personality research these days is normal enough, and that no corrective action is needed. After all, particular papers will always have strong and weak points. The weakest studies will fall by the wayside, and over time, the strongest will accumulate sufficiently so that understanding will emerge without any fancy theoretical effort. This is a cardinal tenet of the middle-level approach, argued spiritedly by such as Merton (1968). It is also an article of faith.

Where are the breakthroughs in personality psychology that can be attributed to the mere accumulation of facts? Are we even close to any? If there has not been enough time, how much more time must there be?

If the complacency inherent in middle-level theorizing were not bad enough, the dangers chronicled earlier raise further doubts as to whether the supposed facts observed will accurately reflect objective considerations. In other words, how will we be able to distinguish the phenomenologically stronger and weaker studies? And, if we cannot do that accurately, on what basis can we be assured that the weaker ones are indeed falling by the wayside? What typically happens in our field is that studies are judged strong if they seem methodologically sound. But, by those criteria, all of the studies critiqued earlier would be regarded as strong. It is on theoretical, not methodological grounds that they emerge as weak. And, if we are busy restricting theorizing to the middle-ground, how will the studies that should fall away ever do so? However much studies and journals in which to publish studies proliferate, I am not optimistic that understanding of personality will grow unless there is improvement in the quality of the research. And that is not likely to happen if middle-level theorizing continues to guide the process.

Should Improvements in Research Methods be Added to the Middle-level Approach?

Continuing reliance on middle-level theorizing, efforts could be increased to improve the methodological quality of research studies. If research studies were more sound in designs, procedures, samples and data analyses, psychologists might feel more comfortable with the article of faith that understanding will emerge at some future time from the accumulation of facts. But, consider what would be involved in the methodological improvement of research. Inevitably, one would have to theorize more elaborately about the phenomena to be studied, and this would lead in the direction of comprehensive approaches.

It is important that giving stronger justifications for research designs and measures be less a matter of procedural and psychometric standards than of conceptual appropriateness. In order to be convincing here, the personality researcher would be led inevitably closer to comprehensive theorizing. Think of the implicit assumptions Schroeder and Costa (1984) must have been making when they grouped stressful event items as contaminated or uncontaminated by neuroticism (negative affectivity). And, think of the theoretical elaboration that would be necessary in order to expose the underlying assumptions of the various hypotheses concerning instrinsic motivation so that it can be determined how some or all of the hypotheses may be interrelated conceptually. The danger attending extirpation of an assumption from a comprehensive theory may appear different, but bears similarities nonetheless. To improve extirpation studies, it is necessary that inferences made from findings not involve various parts of the comprehensive theory lurking in the background but not tested. In this case, Mowrer would have had to conclude that his study was merely about response substitution under stress, not about the defense mechanism of regression. I strongly suspect that the experiment (and others like it) would then have been much less interesting to him and others like him. Mowrer would probably not have done a study of mere response substitution. This indicates the submerged but nonetheless powerful appeal of comprehensive formulations.

It becomes apparent that the path to avoidance of the dangers of middle-level theorizing leads toward comprehensive theorizing. It is difficult to be sure just when the transition is made from the one to the other kind of approach. Merton (1957, pp. 5–6) describes middle-level approaches as "theories that lie between the minor but necessary working hypotheses that evolve in abundance during day-to-day research," and comprehensive approaches as "all-inclusive, systematic efforts to develop a unified theory that will explain all the observed uniformities of social behavior, social organization and social change." Although this distinction appears clear in the abstract, it emerges that Merton regards some rather elaborate thinkers, such as Weber, to be enacting a middle-range approach. But, if one records the enormous number of implicit assumptions that stand behind Weber's (1930) deceptively simple hypothesis that Protestantism led to the rise of capitalism, then it is questionable whether what we see is middle-level theorizing. Further, any consideration of all of Weber's works renders it even more unreasonable to consider him a middle-level theoretician.

It is not possible to settle here the ambiguity as to when a theory is elaborate enough to shade over from middle-level to comprehensive. Suffice it to say that the attempt to avoid the dangers of middle-level theorizing leads toward comprehensive theorizing and that the dividing line between the two is unclear. This suggests just how tentative is middle-level theorizing, which carries within it the seeds of its own demise.

The Importance of Reconsidering Comprehensive Theorizing

However much it may make some psychologists shutter, it might make sense to return to comprehensive personality theorizing. This approach was given up primarily because of three major dangers: comprehensive theories (1) tend to provide abstract formulations so far removed from concrete realities as to be difficult to test, (2) often become the vehicle for charismatic leaders to develop movements too insulated from empirical evidence to be of scientific value, and (3) disagree with one another to a degree that seems dismaying.

But, is there a way of engaging in comprehensive theorizing without inordinately provoking the limitations just mentioned? Relevant to trying to answer this question are two recent returns toward comprehensive theorizing (Mayer, 2005, 2006; McAdams and Pals, 2006). In his approach, Mayer does not consider it constructive to steep oneself in the traditional theories put forward by particular personality theorists. Rather, he emphasizes working with the agreements across theorists and researchers in thinking about various aspects of personality, and using these agreements as a springboard to an overall (what he calls standard) view. In discussing what needs to be done, he argues that such a concept as defensiveness should only be part of a standard view if personality theorists are in agreement on what it is, and how it comes about. The end result of such an approach will be to discard theoretical disagreements (rather than resolve them in research), and dispense with the content emphases that may have been contributed by one or two theorists alone. This approach emphasizes popularity too strongly, and individual insights insufficiently (Maddi, 2006b). Similarly general is the approach of McAdams and Pals, which suggests that five principles be emphasized, namely, evolution and human nature, the dispositional signature, characteristic adaptations, life narratives, and the differential role of culture. These authors also believe that an emphasis on how personality thinkers have already addressed these topics in their particular, comprehensive theories would be stultifying rather than informative (Maddi, 2007). They seem to believe that it is the role of research to identify how these topics relate to each other, if they do. It appears to me that the approaches of Mayer, and of McAdams and Pals are too general to help much in organizing and integrating personality research, and are surprisingly consistent with middle-level approaches in their avoidance of theorizing about the overall, specific content of personality (Maddi, 2006b, 2007).

By comparison, the existing comprehensive theories are more complete and integrated in their understanding of the content and parts of personality and how they interact in producing patterns of thoughts, feelings, and behaviors. It is this specificity and precision that can be helpful in producing incisive research directions. And, disagreements among theories can add to rather than detract from definitive research (Maddi, 1968/1996). In considering comprehensive personality theories, it is certainly relevant to start with the ones that already exist, though new ones can be added, provided that they too are complete and integrated.

In the difficult process of grasping clarity, comprehensiveness, and integration in personality theories, I have proposed a comparative analytical approach

(Maddi, 1968/1996). The first step is to discern the necessary components of a complete personality theory, and the respective roles of these components in hypothesizing about people. To be comprehensive, personality theories need peripheral, developmental, and core propositions (Maddi, 1968/1996). The peripheral propositions cover the major lifestyles or personality types that it is possible to encounter, and the particular, learned dispositions (e.g., traits, motives, habits, defenses) that combine to form these types. The dispositions constitute bases for perceiving situations in particular ways and acting accordingly. In that sense, they are interactional variables. There are also implications for the meaning of particular behaviors contained in the disposition concept and in the organization of dispositions into the more genotypical types. In contrast, the core propositions of a personality theory cover the essential, unlearned nature of human beings, structured as the overall directional tendencies and whatever entities or characteristics these include. The developmental propositions cover the manner in which interaction with the environment and other people (fueled by expression of core tendencies and the response of others to them) leads to particular learned peripheral dispositions and types. Locked in the developmental propositions are many assumptions about the nature of societies, groups, the family, and the learning process.

Research that is to be informed by comprehensive personality theories must take into account that such theories constitute an integration of core, developmental, and peripheral propositions. It is poor practice, therefore, to study an isolated disposition here, or a partial developmental hypothesis there. All aspects of research — including sampling, raw data, data collection procedures, data reduction, procedural design, data analysis, and inferences from findings — must reflect the investigator's sensitivity to the assumptions of the overall theoretical approach at hand. In this fashion, the dangers of middle-level theorizing described earlier will be avoided. Further, there will be a more solid (because comprehensive) basis for studying the complex, naturally-occurring phenomena that should be at the heart of personality psychology.

To some readers, what I am advocating will appear to be worsening, rather than improving the research process. How is it a gain to avoid the dangers of middle-level theorizing by risking the dangers of comprehensive theorizing? Fortunately, there is a way of taking the comprehensive approach that avoids its risks. This way involves the rest of comparative analysis (Maddi, 1968/1996), as I suggest below.

The second step in comparative analysis starts with being as complete as possible in identifying the list of theories that purport to explain the phenomenon under consideration. This second step continues with the attempt to organize these theories into classes (or models) on the basis of the kind of core propositions they adopt, and the implications of these propositions for what is regarded as ideal and non-ideal styles of life that are subsequently learned through the developmental process. For example, in my attempt at comparative analysis for personality theories (Maddi, 1968/1996), I identified three models for theorizing, namely, the conflict, fulfillment, and consistency approaches, each with two subclasses. In the conflict model, there is the psychosocial variant (e.g., Freud, Erickson) and the intrapsychic variant (e.g., Jung, Rank). In the fulfillment model, there is the actualization variant (e.g., Rogers, Maslow) and the perfection version (e.g., Adler, Frankl, Maddi). And in the consistency model, there is the cognitive dissonance variant (e.g., Kelly) and the activation version (e.g., Fiske and Maddi). Doing this second step is obviously a matter of judgment, and involves one in the parsimonious task of adopting as few classes as seems to do justice to the essential thrust of the theories.

The third step in comparative analysis is to pinpoint the issues that separate the classes identified in the second step. Examples of issues are: "Does personality show radical change after the childhood years have been passed?" and "Is all, or only some behavior defensive?" (Maddi, 1968/1996). The fourth step is an attempt to resolve the issues arising in the third step by marshalling of relevant research evidence. If research shows radical change in personality after childhood, this would favor fulfillment theories over the others. If research shows all behavior to be defensive, this would favor conflict theories over the others. There are, of course, other research-resolvable issues as well. Although much personality research has been done, little of it has had comparative analytic aims in mind. When possible, it is best, therefore, to carry out new research planned explicitly to bear on issue resolution. Perhaps it is too much to expect that there will be many crucial studies, but with several carefully integrated studies, one can go a long way toward resolving issues.

The ideal strategy of comparative analysis is perhaps too formidable for many of us, given limitations of time and resources. But, it is still expressive of comparative analysis, though on a smaller scale, to identify and attempt to resolve one or more issues separating two particular theories (rather than all that there are in the relevant models) as they attempt to explain some phenomenon. Such studies would involve measuring one or more dependent variables important to both theories, and the particular independent variables in each theory that are deduced to influence the dependent variables. Then, the independent variables can be evaluated in their relative role in influencing the dependent variables.

An example of this approach involved three studies aimed at comparing the relative effectiveness of hardiness and optimism in facilitating coping efforts to resolve the stressfulness of circumstances (Maddi and Hightower, 1999). Deriving from existential personality theory, hardiness is hypothesized to be the courage and motivation to cope with stressors by transforming them from potential disasters into growth opportunities (Maddi, 2002). Although it does not appear to derive from a comprehensive personality theory, optimism has research support as an expression of happiness that is related to psychological and physical health through the mediating effect of coping (e.g., Peterson, 2000). In all three studies (Maddi and Hightower, 1999), hardiness and optimism were treated as independent variables in regression analyses attempting to predict both effective (transformational) and ineffective (regressive) coping. The first two studies tested undergraduates confronted with the everyday stresses of college life, whereas the third study tested adult females whose stressor involved waiting for the results of a biopsy examination of their breast lumps. The results of all three comparative analytic studies showed that hardiness is the better predictor of transformational coping and avoidance of regressive coping. If further studies support these comparative analytic results, it will emerge that hardiness (courage) is more important in dealing effectively with stressors than is optimism (happiness).

Comparative analysis is neither mysterious nor new, but it can be very helpful in gaining the value, while avoiding the complexities of comprehensive theorizing. Whether the comprehensive theories started with are already in existence or devised newly, much careful, critical thought is necessary in order to carry out the classificatory and issue-identifying steps of comparative analysis. Rest assured that whatever ambiguities exist in the theories will surface rapidly in taking these steps. The ambiguities will have to be resolved conceptually. In the case of existing comprehensive theories, that may involve listening carefully and critically to the attempts of experts in these approaches to resolve the ambiguities. In the case of theories we may have devised, taking the classificatory and issue-identification steps may be the occasion of needed but overlooked theoretical elaboration or clarification on our own parts. By definition, the issues posed in comparative analysis must be specific and concrete enough to lend themselves to empirical resolution through research. Thus, adopting a comparative analytic stance fights "those lazy white elephants of the mind" (Murray, 1959) that have exemplified one danger of comprehensive theorizing.

The danger expressed in the movements led by charismatic leaders is also decreased by comparative analysis. It is possible to believe in one theory above the others and still adopt a comprehensive analytic stance. But, that stance forces one to also believe in the importance of demonstrating the superiority or inferiority of theories pitted against each other empirically. Accepting comparative analysis amounts to putting one's beliefs on the line, and that certainly fights the tendency toward scientifically unsupported movements. There will certainly be believers in various theories who decline to adopt a comparative analytic stance, but if there are others who do adopt it vigorously, the emergence of issue-resolving research will have an impact on the theories involved nonetheless. Over some period of time, theories will come to be relied on which have a sound basis of empirical support. The others will drop by the wayside, not due to some mysterious process of disinterest, but by dint of human ingenuity in comparing them with those that emerge as more promising.

The final danger associated with comprehensive theorizing is the dismay resulting from major differences among theories addressing similar phenomena. An example of this is the horror expressed by Fiske (1974) and Sechrest (1976) over the fact that a concept called by the same name may have different meanings in different theories. Comparative analysis also helps to resolve this dismay. After all, the various concepts in a comprehensive theory are interrelated and therefore influence each other as to meaning. It is not surprising, therefore, that semantically similar concepts may differ. Further, it has to be odd for theoretical disagreements to be used as the basis for fears that the explanatory value involved is not scientific. If the emphasis shifts from the single concepts of middle-level theorizing to the integrated approach of comprehensive theorizing, it becomes possible to deal with disagreements constructively through comparative analysis. Pinpointing issues and enacting research that could resolve them is an optimistic basis for avoiding the danger of disagreement that goes along with comprehensive theorizing.

A Final Thought

When all is said and done, adopting a comparative analytic stance may well have another advantage in personality study. Research done in this fashion is often difficult to enact alone. Bringing the theories involved to the point of clarity necessary to pinpoint the issues that need empirical resolution, and carrying out the research that is truly relevant are elaborate and taxing efforts. Often, others will have to be consulted, informed, or collaborated with, in order to get the work done. These practical pressures toward working together will find an ideological justification in the shift toward defining the personality field in terms of seminal issues to be resolved (psychologists who disagree as to which theory is best for explaining a phenomenon can still work together constructively in resolving the issues joined by their favorite theories). Thus, though it might seem at first blush that the competitive implications of comparative analysis will further fragment the field, the opposite is actually the case. Adopting a comparative analytic stance and enacting it will bring us into closer working relations and give us a much needed, shared intellectual framework and strategy of inquiry.

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