

PERSONALITY

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CONTENTS

INTRODUCTION	227
ISSUES, PERSISTENT AND OTHERWISE	228
<i>The So-Called Person-Situation Controversy</i>	228
<i>Information Processing</i>	229
<i>Research Strategies</i>	231
<i>Recent Research on Sex and Gender</i>	232
EMERGENT PREOCCUPATIONS: DEPRESSION AND HEALTH	235
<i>The Psychology of Depression</i>	235
<i>Personality and Health</i>	239
ADDITIONAL NOTEWORTHY AREAS SHOWING PROGRESS	244

INTRODUCTION

In the last general review of personality for this series, Lawrence Pervin (1985) directed his attention chiefly to “controversies, issues, and directions” in the field. He began by noting the widespread impression that all was not well with respect to genuine progress in personality research, notwithstanding a few optimistic voices here and there: and—if I read him correctly—he concluded his impressive analysis largely in the pessimist camp himself. In a somewhat plaintive coda, he referred to the “gap” between the richness of persons he experiences as a clinician and the paler and decidedly simpler human organisms he encounters in the research literature.

Being also a clinician, and having spent the past year rather more immersed than usual in that literature, my appreciation of Pervin’s difficulty has a poignant edge. I have lately been troubled about what Meehl (1978) felicitously termed “the slow progress of soft psychology.” Meehl noted the

faddishness of topics that dominate the personality scene for a while only to “fade away,” unresolved yet lacking in any current investigative appeal. To put it plainly, much of what passes as research in personality in the current era is patently trivial and noncumulative, and inspires nothing so much as boredom. As West (1987) notes, the “prototype” article appearing in the *Journal of Personality* continues to describe an empirical study involving self-report measures with college subjects in a single-occasion laboratory or classroom setting. The likelihood of discovering nonobvious, important, enduring truths about humankind by such means must be judged small, at best. I have more to say about these frustrating circumstances, and about what appear to be substantial exceptions to the rule, in what follows.

I begin with an update on the controversies and issues Pervin identified. No attempt is made either here or in later sections to include all pertinent work. In general, my selection criteria have emphasized recency, pertinence to current salient issues in the field, judged importance, and imaginativeness of approach. I have selected topics based chiefly on current activity levels in the most germane primary journals.

ISSUES, PERSISTENT AND OTHERWISE

Pervin (1985) focused on (a) the person-situation “controversy”; (b) information-processing models in personality; and (c) questions of research strategy. He might have added (d) various issues and controversies surrounding sex and gender, but that topic was thoroughly and competently addressed by Deaux (1985) in the same volume. All four of these matters are updated and critically reviewed below.

The So-Called Person-Situation Controversy

I have been baffled for two decades by the debate about whether internal dispositions and external circumstances exclude and oppose each other in determining behavior. At the most fundamental level, it has been argued for many years, that these two classes of entities are not mutually independent either conceptually (e.g. Merleau-Ponty 1963) or empirically (e.g. Zener 1937) at the point of behavior. We attempt to separate them in the design of studies chiefly for reasons of convenience and clarity.

In initiating the contemporary version of this debate—and it does have predecessors (Ekehammar 1974)—it appears that Mischel (1968) confused potency of effects with relative psychometric efficiency. Within the observational limits he employed, imputed “traits” (notoriously difficult to measure) were of course no match for “situations,” whose “reliability” (as measured, for example, by manipulation checks) normally approaches unity. In any event, many, including this reviewer, thought the debate was effectively—

and mercifully—ended in an interactional draw with the publication in 1973 of Bowers's superb analysis of the issues. Alas, it was not to be—a fact that compelled Pervin to rehash them 12 years later.

This unfortunate era in personality research may again be drawing to a close. Studies specifically addressing the controversy are rare in recent literature, and Kenrick & Funder (1988) have published a summary statement that reads like an epitaph. These authors suggest that the debate taught positive lessons. Curiously, these lessons support the empirical reality and potency of personality traits—the position from which most personologists began in 1968. A less charitable appraisal is offered by Kihlstrom (1987), who terms the debate “fruitless” and to a large extent ascribable to antipathy and power struggles between social (i.e. situationist) and personality (i.e. trait) psychologists.

Mischel himself abandoned years ago any radical situationist propensities (e.g. Mischel 1973) in favor of what appears to be a frankly interactionist perspective. He now thinks of traits as conditional probabilities that a particular action will be evoked by a particular environmental state (Wright & Mischel 1987)—still a somewhat mechanical notion, but definitely interactionist.

Information Processing

The cognitive revolution throughout psychology continues to reverberate in the personality domain. In fact, Neisser's (1980) warning against overextension of information-processing models in this domain, reminiscent of Oppenheimer's (1956) caveat against overuse in psychology of the classical-physics model, threatens likewise to go as unheeded. If Neisser's barely disguised contempt for the “passive” and “artificial” model of the person portrayed in much of the earlier (circa 1980) work is still deserved, and if it is shared by outsiders generally, then contemporary personality psychology is indeed in serious trouble. My impression, however, is that the situation has improved appreciably—for example, by increased incorporation of motivational and other dynamic variables into the conception and design of cognitively oriented studies.

Consistent with the trend just noted, much of the recent work conjoining personality and cognitive psychology has been done within the specialized and decidedly affect-involving topics of gender roles and psychological depression, the latter in particular having become an exceedingly popular topic among personality researchers. Consideration of these studies is postponed to later sections. What follows immediately is a sampling of otherwise difficult to classify work at the cognitive/personality interface.

One intriguing recent paper in the cognitive area has a developmental focus and deals less with content than with the structure of the cognitive apparatus. Assume that a prime developmental task of early childhood is that of learning

to appreciate reality through the achievement of an adequate system of mental representation. How is it, then, asks Leslie (1987), that this task is not hopelessly undermined by a concurrent and often substantial investment in pretending? Leslie posits a separate representation system capable of differentiating continuously between "real" and "pretend." He then argues elegantly that such a system depends on the child's discerning that pretending is, in essence, the representation of representation, or *metarepresentation*. The additional observation that pretend play is frequently collaborative leads to the somewhat startling conclusion that the typical child has a generalized "theory of mind" by the time he/she is emerging from infancy.

Pretend-play may thus be seen as a primordial form of the ability to conceptualize (i.e. objectify) mental states—one's own or those of others, and this would appear to be its chief significance for a psychology of personality and personality development. It is conceivable, for example, that our understanding of concepts like self, ego identity, dissociation, "splitting," projection, etc, not to mention some significant perplexities in social cognition, might be enhanced by analysis in terms of the developmental history of representation, an idea whose essential features were advanced by Sullivan (1953) many years ago. It seems clear that any progress in this area would depend on use of a more richly elaborated (hierarchically ordered?) cognitive model than is thus far customary in personality research.

The theme of hierarchical representation may also be pertinent to how adults cognize emotions. Shaver et al (1987), taking a prototype approach to the knowledge of emotions, present evidence that people's conceptions of emotions are in fact organized in this way. If confirmed, such a finding renders inappropriate certain types of data analysis, such as multidimensional scaling, in exploring the domain. The authors comment on the implications of their thesis for the acquisition of knowledge about emotions in childhood, but they do not speculate about effects on the child's developing "theory of mind" as it pertains to the mental states of others. This may be a fruitful area for future investigation.

Finally, recent work has explored the dynamics of cognitive functioning, including selective retrieval from memory and the effects of intentional suppression of mental contents.

Larsen et al (1987) provide an interesting demonstration of a relationship between emotional and cognitive functioning. Hypothesizing that individual differences in affect intensity would be associated with differences in the cognitive processing of emotional stimuli, they in fact found that high-affect-intensity subjects reacted to both positive and negative emotional stimuli with relatively high levels of personalizing/empathic and generalizing/elaborative cognitive operations; emotionally neutral stimuli failed to elicit this effect.

Courtroom judges sometimes instruct juries to disregard information to which they have become privy, an admonition more easily issued than

obeyed. Wegner et al (1987) report two demonstrations of involuntary intrusive thoughts about a "white bear" provoked by attempts to suppress such intrusions. Some success was achieved in controlling these "obsessional" phenomena by teaching subjects to use a specific distracter. Wyer & Budesheim (1987) have studied a related process in the area of person memory. Subjects were provided descriptive information on target persons and then told to disregard some of it. While the accessibility of the presumably suppressed information in a subsequent memory test varied with the experimental conditions, it was generally substantial.

The notion that memory retrieval processes are influenced by personal concerns, motives, and the like is venerable. Defining as "repressors" subjects who scored high and low on the Marlow-Crowne and Manifest Anxiety scales, respectively, Davis (1987) has shown (again) that this type of defensiveness is associated with diminished access to personal memories, especially unpleasant ones. The effect appears limited (as it should) to events involving the self, and appears especially notable for experiences that threaten or provide negative evaluations of the self. The other side of the coin is demonstrated in a recent study by Katz (1987). Here, selectively enhanced retrieval by "creative" subjects for creativity-relevant trait terms occurred only when instructions involved self-concept/schema arousal.

Research Strategies

Explicit attention to fruitful methodology appears to have waned following the period covered by Pervin's (1985) review. That review emphasized the reemergence of an idiographic perspective, continuing questions about the status of self-report, and the trend toward data aggregation (e.g. meta-analysis) as a means of differentiating robust and reliable from conditional and setting-specific findings. Suffice to say, none of these issues has been resolved in the interim. This is hardly surprising, since all of them go to the epistemological heart of a still nascent science of personality.

Another venerable problem, that of identifying the elements or dimensional units comprising "personality," has surfaced again in the contemporary literature. According to one account, the quarry are nothing less than "the biological bases of personality" (Zuckerman et al 1988)! The factorially derived *Big Five* have attracted the most attention. These presumed basic dimensions are, roughly: Introversion-Extraversion; Friendly Compliance-Hostile Noncompliance; Will (sometimes Conscientiousness); Neuroticism (sometimes Emotionality); and Openness to Experience (see, for example, Noller et al 1987). Apparently convinced of the primacy of these variables in personality functioning, McCrae & Costa (1986) urge they be adopted in clinical assessment, whose more typical instruments are said to lack comprehensive coverage of these purportedly basic organizing components.

A much needed caution on such enthusiasms has been offered by Waller & Ben-Porath (1987), who point out that the alleged robustness of the five factors pertains more to their reliability (in paper-and-pencil tests) than to their validity. I would add that, given the taxonomic absurdities of psychiatric diagnosis as represented by DSM-III and its recent revision (but cf McReynolds, this volume), clinicians need to avoid elevating observational reliability above substance observed. It is possible to become too "operational," a strategy that tends to degenerate into arbitrariness. It is not clear that mere number crunching of responses to preselected items on personality inventories, as in many factor-analytic studies, has burst its own confines to make a general contribution to the cause.

If I were to bet on what sort of "basic dimensions" we will eventually settle on in personality research, I would *still* (Carson 1969) expect variables with an interpersonal referent to provide a large share of the successful candidates. To my mind Leary (1957) and his colleagues staked out the basic territory—now generally known as the interpersonal circle. This circumplex space, meaningful both conceptually and mathematically, and defined by bipolar, orthogonal dimensions of "power" and "love," continues to show remarkable resilience. Using the pertinent adjective scales developed by Wiggins (1979), Gifford & O'Connor (1987) present evidence that the circle does more than merely summarize our collective implicit personality theory; it actually does a good job of mapping objective behavioral output. Meanwhile, Wiggins (Wiggins et al 1988) continues to refine and develop the geometric properties inherent in the model and to show its potential for subsuming other purportedly central personality dimensions, such as introversion–extraversion. It may also help clarify some problems in gender-related behavior (Wiggins et al 1988; Wiggins & Holzmuller 1978, 1981), the next topic on our agenda.

Recent Research on Sex and Gender

Little new ground has been broken in the area of sex and gender since Deaux's (1985) review. Themes already well developed in the literature, and for which she provided expert perspective, have been consolidated or elaborated. Unfortunately, much confusion remains. The "tension" she described as pervading the area seems to have diminished, possibly because the evidence for nontrivial sex differences in behavior (in terms of central tendency) has gained acceptance.

The problem of how masculinity and femininity are to be conceptualized as organized trait complexes within personality and the related problem of how to measure them continue to be debated. The original notions of Bem (1974) on gender schemata, the unidimensionality of sex typing, and the mid-dimension (i.e. nontyped) psychological gender equivalence of "undifferentiated" and "androgynous" persons seem to have lost credibility as

investigators become increasingly analytic in approach. In three well-conceived studies, Edwards & Spence (1987) found little evidence for either the unidimensional or bidimensional (i.e. orthogonal instrumental and communal dimensions) views, and only weak suggestions of the operation of gender schemata (see below) in cognitive processing; their data are most consistent with a multifactor model of the organization of psychological gender. The gender schemata hypothesis also fared poorly in a cognitive processing study by Payne et al (1987). Perhaps most damaging is the observation by Paulhus (1987) that the median split method of subject allocation into (psychological) gender-related quadrants produces internally based artifacts with respect to theoretically central dependent variables. Moreover, Paulhus argues that Bem's primary data actually measure the two principal dimensions of the interpersonal circumplex mentioned above, noting the irony that both are bipolar.

It would be premature, however, to conclude (*a*) that individuals do not use biological sex as an important organizing principle in the way they think about persons, or (*b*) that the standard measures of psychological gender typing hopelessly lack construct validity. The intuitive appeal of an appropriately limited version of the gender schemata notion found support in a study by Frable (1987), in which many potentially "noisy" variables were ingeniously controlled—specifically by videotaping target persons garbed in dark clothing marked with reflective tape at the joints as they walked before a dark background. In this study, sex-typed (per Bem) individuals used more gender terms and were more accurate in their guesses as to the sex of targets than nontyped subjects. In the role of target, moreover, the movements of sex-typed individuals were judged by observers to be more distinctively masculine or feminine than those of the nontyped. Interestingly, cross-typed subjects (i.e. those having a disparity of biological sex and psychological gender) moved the way the nontyped did, but were closer to typed subjects in their perceptions of targets.

Among the trait ascriptions assuming prominence in the psychology of gender are those of dominance/submission and achievement striving. Concerning the former, Sadalla et al (1987) report four studies that in the aggregate demonstrate enhanced heterosexual attractiveness of males who engage in (nonaggressive) dominant behavior, whereas similar behaviors do not affect attractiveness ratings of females. Male dominance affected sexual attractiveness, not general likeability. Halberstadt & Saitta (1987) attempted to identify nonverbal cues communicating dominance and submission between the sexes in public settings and in various media materials, focusing on such supposedly submissive gestures as canting the head and body and smiling. Few reliable sex differences emerged, suggesting either that dominance stereotypes in this area are erroneous (a conclusion contrary to much

other evidence) or that the intuitive appeal of the meaning assigned to the canting and smiling behaviors observed is largely mistaken.

As for achievement, Gaedert (1987) has added an important qualification to the common finding that women more than men tend to attribute their objectively real accomplishments to causes (such as luck) that do not enhance their self-esteem. This investigator found that, in permitting subjects to choose their own accomplishments (as opposed to standard ones provided by the experimenter) as the backdrop for reporting goals, standards of performance, and analyses of reasons for success, the previously reported gender bias is reduced to insignificance. Conventional notions of accomplishment may not apply to individuals or groups that do not fully share the values implicit in them.

In any case, the primary observations continue to be supported—in some instances with qualifications. For example, in a psychometrically sophisticated study Marsh et al (1987) present evidence that typical measures of “masculinity” may be interpreted as directly assessing self-esteem. Partialling out social desirability, which was more strongly correlated with femininity than masculinity, did not substantially diminish the masculinity/self-esteem association. Thus self-esteem and social desirability seemed to function here as forms of stereotypic masculinity and femininity, respectively. Continuing the theme, Orlofsky & O’Heron (1987) demonstrated for both sexes, the positive associations of measured masculinity with adjustment and self-esteem. Here, however, femininity also correlated significantly with self-esteem, albeit more weakly. The authors attributed the latter association to the “communal self-esteem” component of femininity, which I suspect is indistinguishable from Marsh et al’s (1987) social desirability. Overall, questions of gender typing (as normally measured) and its meaning with respect to self-esteem, adjustment, and related matters remain amorphous. As Deaux & Major (1987) have recently suggested, gender-related behavior may be far more variable and context-dependent than we have realized.

On the other hand, the relationship is clear between biological sex and the incidence and prevalence of diagnosable depression: Female depressives outnumber male on the order of two to one (Wing & Bebbington 1985), a difference not readily attributable to artifacts or diagnostic biases (Nolen-Hoeksema 1987; Amenson & Lewinsohn 1981). While the general topic is discussed in the section on the psychology of depression, below, some pertinent work on this disorder and its relationship to sex and gender-roles is best considered here.

The enhanced risk of depression among women is apparently attributable in part to the (now increasingly weakly) associated factor of being a nonworking homemaker with exclusive responsibility for the care of young children (Wing & Bebbington 1985); thus traditional gender roles are directly implicated.

Longitudinal data reported by Schaefer & Burnett (1987) cast some light on the possible influence of the spouse upon the likelihood of unfortunate outcomes for such women. This study found a high correlation between wives' perceptions of how well their husbands met their needs for autonomy and relatedness, on the one hand, and the women's sense of well-being, on the other; these correlations tended to increase over time.

What aspect of the traditional female role might correlate with depression? Learned helplessness appears not to be the critical link, according to a searching analysis by Nolen-Hoeksema (1987), who believes that trait-like qualities supposedly differentiating the sexes are more likely to explain the imbalance in the occurrence of serious depression. Specifically, she suggests that men are more likely to cope with depressive feelings by an active type of distraction, whereas women tend to amplify the effects of the mood by ruminating about its causes. It seems to me that this conclusion leads us back to square one.

EMERGENT PREOCCUPATIONS: DEPRESSION AND HEALTH

The current interests of personality researchers in the phenomena of human psychological depression extend far beyond the interface with sex and gender roles. Concern with these phenomena and with relationships between personality variables and physical health has dominated the personality research literature of the past two or three years. This trend has been facilitated by the development of individual assessment instruments that appear (often deceptively) to type persons easily and reliably according to some theoretically or pragmatically attractive variable. We consider first the more general work on depression.

The Psychology of Depression

Some 20 percent of the raw material I assembled for this review dealt explicitly with depression. It appears to be a topic whose time has come. Research has been stimulated in particular by the cognitive work of Beck (Beck et al 1979) and the eventually related directions taken by Seligman's (1975; Abramson et al 1978) learned helplessness theory of depression. The convenience of the Beck Depression Inventory (BDI), widely used to select "depressed" subjects, has doubtless also contributed.

I begin by reviewing recent work on the cognitive variables purportedly underlying the experience of depression.

DYSFUNCTIONAL CAUSAL ATTRIBUTIONS According to the revised form of the learned helplessness theory of depression, the disorder occurs conse-

quent to “negative events” *only* for individuals having the propensity to attribute such events to factors that are internal (“it’s something in me”), global (“the problem is pervasive in my life”), and stable (“and it will always be thus”). The dysfunctional triad is described in the current literature as an attributional or explanatory “style,” and there is even available an Attributional Style Questionnaire (ASQ) said to permit identification of persons who view things this way (Seligman et al 1979). Whether individuals with this “style” could not be said to be depressed already, independent of any experimental manipulations performed on them, has been seriously debated for some time (e.g. Peterson et al 1985) and continues to be (e.g. Wollert & Rowley 1987), without resolution. Nevertheless, researchers persistently explore the unquestioned association (by definition?) between negative thoughts and depressed mood.

Riskind et al (1987) report a study fairly typical for the area. Here an interaction between previously assessed attributional style and expectations of future outcomes predicted BDI-measured depression in college students six weeks later. Complicating matters, however, was a significant effect for attributional style in interaction with initial level of depression. A more straightforward interpretation of results was possible in a comparable study by Peterson & Barrett (1987), where the internal-global-stable explanatory style predicted relatively poor freshman grades and certain other college difficulties independent of initial BDI scores and SAT-assessed academic ability. Of course, such outcomes are not necessarily attributable to depression. Also, it would be interesting to know how these students fared after a failure. A study by Follette & Jacobson (1987) showed “facilitation” (e.g. planning-enhanced attention to studies) following poor academic performance among students with negative attributional styles.

Given the evident perplexities and inconsistencies, it is likely that more can be learned only from correlational studies that attend to process. Ingram et al (1987) took a step in this more analytical direction by examining the “cognitive specificity” of depressed (and anxious) college students on measures of information processing, attributions, automatic thinking, and cognitive interference. They found that depressed students did indeed show distinctive thinking aberrations that seemed likely to produce further maladaptive consequences, but the causal relationships between the cognitive and affective phenomena remained unknown. If one accepts acutely induced moods as comparable to the more naturally occurring variety, there is evidence that depression may affect the cognitive processing involved in person perception (Forgas & Bower 1987).

The issue of causal direction is complicated further by findings that successful treatment of depression by other than cognitive modes (e.g. by antidepressant medication) results in improvement of dysfunctional attribu-

tions. This certainly suggests depressive mood primacy. However, Hollon et al (1987) argue that such a conclusion is unjustified because it confounds a treatment mediator role (which various therapies might produce) with the potential causal role of dysfunctional cognitions. This argument seems strained. In any event, recent work has failed to resolve the causal-direction dilemma, inherent in the revised formulation of the learned helplessness model, now a decade old.

DEPRESSION AND THE SELF Any cognitive role in the inducement of depressive affect involves at some point the historically rich but ephemeral and elusive concept of self. One investigation linking depression with the self, that of Tennen et al (1987), directly targets the dysfunctional-attributions hypothesis discussed above. These investigators assessed college students via the ASQ and multiple measures of depression and self-esteem, and found that self-esteem was a better predictor of attributional style than was depression. In a second study involving psychiatric inpatients, self-esteem and depression proved (not surprisingly) to be highly inversely correlated, both of them predicting ASQ performance well. Even with statistical elimination of social desirability and depth of depression, a significant inverse association remained between self-esteem and the internal-global-stable style of interpreting negative events. Finally, statistical control of self-esteem essentially eliminated the depression-ASQ correlation. Given the character of the field, it would be premature to suggest that these results deliver a mortal blow to the etiologic primacy of dysfunctional cognitions as envisaged in contemporary learned helplessness theory. The image of an outsized coffin nail does suggest itself, however.

Results generally supportive of the role of self-esteem in mitigating depressive affect has also been reported by Pagel & Becker (1987) and Strauman & Higgins (1987; see also Higgins 1987). In the former study, self-esteem exerted its antidepressant effect among spouse caregivers of Alzheimer's patients by inhibiting depressive cognitions. One cannot help reflecting, however, that such effects of the self-esteem variable are hardly unprecedented and that the precise (and otherwise satisfactory) measurement of self-esteem poses its own obdurances.

Meanwhile, other aspects of the self have also figured in recent work on depression. Pyszczynski & Greenberg (1987) offer a self-awareness theory of ("reactive") depression according to which negative self-image is the product of cyclical dysregulation in response to loss and the enhanced self-focusing it brings. The theory is provided some empirical backing in two studies reported by Pyszczynski et al (1987) in which (a) depression (again as measured by questionnaire) was shown to be associated with pessimism about personal outcomes, attributed by the authors to excessive self-focus; and (b) this

pessimism could be reduced to the level of nondepressed subjects by inducements to focus externally. Self-regulatory mechanisms also figure prominently in another theoretical treatise on depression offered by Hyland (1987).

OTHER DEPRESSION-RELEVANT WORK The interpersonal context of depressive behavior continues to stimulate useful observations about the dynamics of depression. Returning for a moment to the depression/helplessness connection, a study by Sacks & Bugental (1987) demonstrates the sometimes considerable interpersonal ramifications entailed in depressive (or helpless) functioning. In this study helpless and nonhelpless (ASQ-defined) women were subjected to contrived social failure and subsequently interacted with "naive" partners. Relative to the nonhelpless, helpless women became depressed and hostile, had elevated voice tension, and engaged in more unpleasant nonverbal behaviors in these second interactions. The partners in these second interactions who were themselves helpless (relative to nonhelpless counterparts) spoke less and were more hostile to partners who had had a prior failure experience, but conversely were less hostile to partners whose prior experience had been (again by contrivance) successful.

The induction hypothesis—i.e. that the depressed person tends to generate depressive affect in others and thereby becomes aversive to those others, perhaps compounding the original difficulty (Coyne 1976)—has amassed sufficient empirical support (see also, e.g., Howes et al 1985) to merit inclusion in any comprehensive theory of depression. At the same time, however, the phenomenon is not so robust as to be routinely replicable, suggesting, as is so often the case in personality research, that we have not yet isolated the controlling variables. At any rate, Stephens et al (1987) report another disconfirmation of the strong version of the induction hypothesis, although the target persons of depressed-acting confederates did reject these confederates. In an interesting twist, the rejection was registered only in subsequent questionnaire responses; overtly, these rejecting subjects were quite responsive to the help-seeking efforts of their "depressed" interaction partners. One wonders, of course, how persistent this responsiveness would be if it failed to have a depression-relieving effect over time—a highly likely scenario in the "real world."

Doubtless there would be individual differences in such persistence, and in this connection Clark et al (1987) report the development of a new "communal orientation scale" that appears to predict especially strong helping responses to "sad" others. The other side of the coin, the sometimes enhanced helpfulness to others of (mildly) depressed persons, is a product of situational factors that increase a sense of responsibility and heighten objective self-awareness, according to an aggregational analysis of the pertinent literature by Carlson & Miller (1987).

Experienced clinicians are careful not to provide too much help to depressed persons because such support may increase the guilt that commonly accompanies serious depressions, thereby producing a negative outcome. Possibly related to the phenomenon of guilt in depression is a recent finding by McGraw (1987): Contrary to intuition, unintended harm-doing produced more guilty feelings in "normal" subjects than did the intentional variety. Using an attributional model, McGraw argues that self-blame (rather than notions of cause or responsibility *per se*) is the critical element in the experience of guilt, the intentional harm doer typically having "worked through" any associated guilt *prior* to commission of the culpable act.

Personality and Health

Recently an entire issue of the *Journal of Personality* (Vol. 55, No. 2, June, 1987) was given over to reports and commentaries on relations between personality variables and sundry aspects of physical health—one measure of the exploding interest in this area manifested by personality researchers. Interest has been stimulated by the establishment of a linkage between the so-called Type A Behavior Pattern (TABP) and coronary heart disease (CHD), still the most lethal of the diseases affecting the population of the United States. As expected, the availability of easily used assessment instruments for the TABP has been contributory, and, as has also become customary, the "second wave" of related research has included much questioning of the adequacy of these instruments.

TYPE A AND CORONARY HEART DISEASE Some stage-setting comments are in order for nonspecialist readers. In the wake of the impressive longitudinal findings of the Western Collaborative Group Study (Rosenman et al 1975) and the Framingham Heart Study (Haynes et al 1980) linking TAPB with CHD, there appeared seemingly well-executed studies that unexpectedly failed to confirm this linkage. Disagreements and difficulties arose regarding how the TAPB should be measured (Fischman 1987). It became clear that Type A, as originally described, was a composite of several not necessarily strongly intercorrelated behavioral characteristics, not all of which—e.g. high levels of achievement motivation—predicted CHD.

Further complicating the picture, different assessments have differentially weighted such components of TABP as time urgency, job involvement, competitiveness, achievement striving, and generalized hostility. To make a long and complicated story short, a plurality of experts in the area now finds the structured interview (SI) method to be the surest way of assessing TABP (Dembroski et al 1978), and suggests that one or another form of hostility is its most CHD-predictive component (Fischman 1987; Wood 1986). At the level of primary assessment many researchers have settled for less, having been especially attracted to the convenience of the Jenkins Activity Survey

(JAS; Jenkins et al 1979). The JAS is a questionnaire that deals minimally with hostility and (see below) seems not to predict CHD.

Much recent research on the A-B typology has sought to identify and differentiate more precisely the supposedly lethal personality factor(s) involved in the TABP. A thorough "quantitative review" by Booth-Kewley & Friedman (1987) of work in this area published through about 1985 shows that some progress has been made. In addition to demonstrating the superiority of the SI over the JAS for predicting CHD, the data reviewed by these authors clearly implicate the negative emotions of depression and anxiety as well as anger/hostility in the correlational network that includes the development of CHD. They suggest that the concept of the coronary-prone behavior pattern be on the one hand broadened to encompass these additional negative affect features, and on the other narrowed to eliminate noncontributory components such as impatience, pressured drive, and workaholism—characteristics strongly represented in the JAS measuring instrument. These conclusions were generally confirmed in a study of 50 post-myocardial infarct men by the same authors (Friedman & Booth-Kewley 1987a), who also describe an expressive variant of the TABP that apparently is not associated with enhanced CHD risk. Type A American and Indian bus drivers who have high accident, reprimand, and absenteeism rates and perhaps blow their horns excessively (Evans, et al 1987) should be reassured by this finding.

The Booth-Kewley & Friedman (1987) review reveals substantial gaps in our information concerning the interrelations among the psychological predictors of CHD, their developmental origins, and the manner in which they may contribute to coronary (or other) arterial blockage. While the last of these remains a near-total mystery, we may be gaining some ground with respect to the other two. Dembroski & Costa (1987), also arguing for component analysis of the TABP, suggest that both the TABP and its apparently critical hostility component are multidimensional in character. Noting that the hostility involved here seems to be of a decidedly antagonistic variety, unmitigated by conflicting tendencies toward agreeableness, they reason that neuroticism as usually conceived and measured may have little contributory role in the "toxic" pattern, and could in some instances be associated with reduced CHD risk.

Meanwhile, Ward & Eisler (1987) have provided a clue to the sources of negative affect in coronary-prone persons. Identifying Type A subjects according to a JAS-like questionnaire developed by the Framingham investigators, they found that these subjects tended (*a*) to set personal goals at a level that invited failure, and (*b*) to use achievement strategies that produced low levels of self-evaluated performance. These findings seem consistent with the self-appraisal model of TABP proposed by Strube et al (1987), according to which Type As are excessively concerned with gaining information (reduc-

ing uncertainty) about their abilities, thereby risking objective failure. JAS-defined Type A subjects generally performed in accord with these expectations. Unfortunately, the meaning of both studies for CHD risk is clouded by their reliance on equivocal measures of the Type A construct.

A similar reservation applies to a newly published longitudinal study of Type A individuals by MacEvoy et al (1988), one of the very few such studies to appear. Using data obtained in repeated evaluations of Swedish children and their mothers over 25 years, and a Swedish, shortened version of the JAS administered at mean age 26.5, these investigators found notable evidence of temperamental precursors to the adult TABP, as measured, and some maternal behavior correlates of the latter such as orderliness and intelligence. Irritability, the JAS factor in this study most closely associated with independent evidence of CHD toxicity, was preceded by lively, sociable (not shy) child behavior, and with having had a poor appetite. The authors acknowledge the evident lack of face validity in these findings.

Overall, we have made genuine progress in this area—notably in sharpening the concept of coronary-proneness beyond the original formulation of the intuitively attractive but unduly inclusive TABP. Should the role of negative affect in general continue to prove to be of primary significance, the field sometimes labeled psychocardiology will doubtless merge with the larger effort under way to understand the influences of personality in all aspects of health and its maintenance.

PERSONALITY, ILLNESS-PRONENESS, AND HEALTH The notion that personality influences health originated centuries if not millennia prior to the Holmes & Rahe (1967) demonstration that reported life-change events were associated with the occurrence of illness. The Holmes & Rahe work, however, stimulated psychologists to take a closer look at the nonphysical parameters of physical pathogenesis. And while this early work on the stress-illness relationship was later subjected to withering and to an extent deserved criticism, it seems to have emerged with its essentials more or less intact (Maddi et al 1987). Stressful events increase the likelihood of subsequent disease. This is not to say, of course, that stress plays a causative role in all diseases.

All persons experience stress, within generally accepted definitions of the term, and their health is variously compromised in undergoing such experiences. It is precisely this variance—insofar as it cannot be accounted for by reference to measurable physical properties of the organism—that justifies the intervention of psychologists in the health arena, an arena whose psychological dimensions are currently expanding at a rate that threatens to exceed the supply of appropriately trained psychological personnel.

If we exclude the classic “neurotic” medical patient, whose often multiple

complaints are not apt to be matched by objectively determined health status (Costa & McCrae 1987; see also Miller et al 1988), issues of personality concern both the "disease-prone" personality and the "hardy" one (to use terms common in the literature). Concerning the former, Friedman & Booth-Kewley (1987b) provided a recent quantitative overview of the field to recent times. Focusing on asthma, arthritis, ulcers, and headaches, in addition to coronary heart disease, they find evidence of a generic disease-prone personality type. The major constituent traits are depression, anger/hostility, and anxiety. Another recent review by Jemmott (1987) suggests the addition of excessive power motives to the list. With the exception of CHD, however, the evidence, though consistent, is not strong. Friedman & Booth-Kewley appropriately caution against overinterpreting their meta-analytic results, especially in view of the fact that in most of the aggregated studies negative affective states may have followed from rather than helped to cause the diseases studied.

As several commentators (e.g. Holroyd & Coyne 1987; Krantz & Hedges 1987; Suls & Rittenhouse 1987) have suggested, we are unlikely to discover (and reliably measure) personality traits and trait-like states that will account for large outcome variances in health status. We may achieve predictive power approaching that of other known risk factors for a given disease, which typically proves less than impressive. Global traits are in this respect far less good bets than more sharply delimited personality variables, especially when the latter can be tied conceptually to the outcomes of interest on a priori theoretical grounds. Some work more nearly approaching this ideal is considered below.

Because psychological depression may increase risk of illness, it is significant that explanatory style (described above) appears itself to be directly associated with an increased likelihood of contracting disease. This finding is the product of an ingenious study by Peterson & Seligman (1987) that made use of the CAVE (Content Analysis of Verbatim Explanations) variation of the Attributional Style Questionnaire on decidedly nonstandard subject materials, such as the media-reported remarks of Baseball Hall of Famers and official records on health status and survival. One is reminded here of the long-standing but to a large extent anecdotal linking of feelings of hopelessness/helplessness to the occurrence and progression of cancer. The latter hypothesis was in fact confirmed in a recent study of breast cancer progression reported by Jensen (1987). Other results of this study, however, seem inconsistent with the suggested portrait of the victim as a ruminative pessimist; rather, tumor growth was associated with a "repressive" personal style that included, among other manifestations, an attraction to comforting daydreams. This finding is also consistent with certain classic views regarding host susceptibility and cancer progression. A more comprehensive un-

derstanding of the psychology involved may eventually resolve this apparent contradiction.

Fortunately, the negative side of the personality-health connection is only part of the story. At least since Kobasa's (1979) introduction of the notion of hardiness, researchers have sought to identify aspects of personality that protect against illness or buffer the pathogenesis of stress (Suls & Rittenhouse 1987). The path of discovery has not proved smooth, as illustrated in Funk & Houston's (1987) scathing review of research using the Hardiness Scale. This device may turn out to be another (inverse) measure of general psychological maladjustment—what is often termed “neuroticism.” As we have seen, the latter's relationship to medical illness is suspect (Costa & McCrae 1987). Hull et al (1987) also offer a critique of both the concept and the measurement of hardiness, and question whether “buffering” effects have been demonstrated.

If hardiness exists, it will likely be factorially complex. A better research strategy might therefore be to focus on simpler and more precisely measurable variables or processes. Linville (1987), for example, measured self-complexity by means of a sorting task and demonstrated its substantial buffering effect for flu and other illnesses following stressful events. Affleck et al (1987) reported the benefits of a sense of control and of disease-course predictability among chronic rheumatoid arthritic patients. In line with both the positive health correlates of a nondysfunctional explanatory style (Peterson & Seligman 1987) and the potency of placebos, the trait of optimism should have buffering qualities, as seems indeed to be the case (Scheier & Carver 1987). Of course, undue optimism in the health domain can also be disastrous (Tennen & Affleck 1987).

RELATED PHENOMENA While we do not know how psychological variables may increase the risk of physical illnesses (including infectious and neoplastic ones), a growing body of evidence suggests that the immune system may be functionally compromised by negative emotional states and events that produce them (Jemmott & Locke 1984; Schleifer et al 1985). Exactly such an effect was recently demonstrated in an exceptionally well-done study by Stone et al (1987). Here, dental students on oral doses of a harmless foreign protein recorded their mood and donated specimens of parotid saliva thrice weekly over a period of 8½ weeks. Secretory immunoglobulin A was assayed from the saliva samples as a measure of immune system response to the antigen analogue. The magnitude of an antigen-specific antibody response was positively correlated with variations in daily mood; the immune systems of these students responded more vigorously to foreign-substance invasion the more they enjoyed a sense of well-being. This interface between immunocompetence and personality functioning deserves maximum attention in future health-related research.

Finally, some recent work on pain control should briefly be noted. Bandura et al (1987) report an elegant experiment on cognitive (self-efficacy) control of the experience of pain that included exogenous manipulation of endogenous opioid action. Self-efficacy training increased pain tolerance with or without inhibition by naloxone of endogenous opioid action, and opioid activation itself was enhanced by a sense of efficacy concerning pain management. Complementing these findings is a demonstration by Litt (1988) that changes in self-efficacy to manage pain are accompanied by corresponding changes in pain tolerance. In this study self-efficacy and perceived control made separable but additive contributions to tolerance.

To summarize the personality/health venture is "on a roll." Problems of intimidating magnitude, some unsuspected, remain to be solved; but the thrust of efforts in this area and the progress already made predict an important and exciting future. A past Surgeon General once remarked that the future and perhaps final cutting edge of medical innovation will reside in human personality and behavior. That forecast seems more credible now than it did only a decade ago.

ADDITIONAL NOTEWORTHY AREAS SHOWING PROGRESS

Space constraints permit only the briefest of comments in three other areas of activity that strike me as especially important: genetic influences on personality, the development and outcomes of childhood peer difficulties, and the self-perpetuating nature of personal dispositions. Concerning the first of these, both Loehlin et al (1987) and Rose et al (1988) present additional compelling evidence on the modest heritability of some personality traits. Interestingly, the latter study estimated separately the concordance effects due to within-twinship social contact (which are typically confounded with genetic ones), and these proved not to be negligible.

Parker & Asher (1987) reviewed the adult sequelae of childhood peer problems, concluding that low social acceptance and excessive aggressiveness, particularly, predict such negative outcomes as dropping out and criminality. Meanwhile, Dodge & Coie (1987) have added to the evidence of information-processing aberrations in reactively aggressive youngsters; specifically, these children assign to signals from the social environment a hostile or threatening meaning that is not consensually valid. But, of course, such autistic expectations may mediate the enactment of behaviors that *do* provoke a hostile environmental response, thus confirming (and presumably strengthening future activation of) the original expectations.

In this manner a maladaptive "fix" becomes self-perpetuating, a process I have described in more general terms as crucial to remediation efforts (Carson

1982). Jones (1986) has recently offered a detailed theoretical treatment of trait-related expectancies as self-fulfilling prophecies in interpersonal behavior generally. This rich idea deserves far more attention from researchers than it has yet received. Meanwhile Buss (1987; Buss et al 1987) has been exploring the apparently considerable tactical efforts persons make to control their interpersonal environments, possibly because of the personal identity issues at stake (Swann 1987).

The field's prospects are somewhat happier than they were when Pervin wrote (1985). There is good and exciting work being done, and one can with effort find it among the barren entities that litter the scene.

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