13 Social Intelligence and Personality: There's Room for Growth

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We thank our commentators for their thoughtful, constructive, and largely positive critiques of our work on social intelligence. We were particularly pleased to find that many of the responses were not really critiques at all, but more like responses in the spirit of our enterprise, that pick up on various issues and pursue them further than we have. To us, that means that the social-cognitive approach to personality is engaged in a healthy process of growth and development. But that is not to say that the commentators have not raised a number of important criticisms as well. In the space available to us we want to provide some sense of our own perspective on the new issues and attempt to clear up some misunderstandings.

INTELLIGENCE AND SOCIAL INTELLIGENCE

The term *social intelligence* was selected deliberately, but in the end it has caused us no end of trouble and misunderstanding. People naturally confuse our notion with the more familiar concept of "social IQ." This is a confusion that is not confined to academic psychology. Just as we were preparing our response to the commentaries, the *New York Times Magazine* (July 3, 1988) carried an installment of William Safire's syndicated column, "On Language," discussing the various meanings of the word *intelligence*. Safire distinguished between the common attributive meaning of the noun, as a modifier of words like *test* and *quotient*, and the more recent use, as described by H. E. Meyer in his recent book, *Real-World Intelligence:* "information that has not only been selected and collected, but also analyzed, evaluated, and distributed to meet the unique pol-

icy-making needs of one particular enterprise. . . In short, intelligence has become a management tool.''

That's how we use social intelligence: not so much as a thing that people *have* in varying quantities, like height or weight, or even the practical intelligence of Sternberg and Wagner, but rather an ever-evolving resource—a fund of information, and a set of operating procedures that people *use* (and disagree on) in managing their lives. Intelligence, as a rich and diverse fund or resource isn't a static or unidimensional entity; it is the essential ingredient of the active, creative, evolving side of personality. Just as the most interesting thing about self-esteem is not whether you have it or not, but how you use and maintain and enhance it (whatever *it* is); intelligence is a resource to be cultivated and we are all gardeners first and foremost—all engaged in making sense of ourselves and of our worlds.

Sternberg and Wagner seem to prefer an attributive concept of social intelligence when they discuss practical intelligence as a means of achieving success in some domain in life and propose that practical intelligence is a quantity that people possess in varying amounts, and which teachers can inculcate in students. But from our point of view, social intelligence is something everyone has—not just young PhDs or MBAs on-the-make. We don't dispute the value of their approach (though we doubt that our readers, as ostensibly successful academics, would agree on the answers to their test question–and we wonder how a person who isn't identified with a coherent program of research ever got hired by a prestigious department), but we think they are too sanguine about their results on consistency. Putting aside the question of whether correlations of 0.30 or 0.40 are modest or substantial (see also Baron), a correlation of 0.60 between practical intelligence tests for making-it-in-psychology and making-it-in-business won't make the case for consistency. From a logical point of view, the fact that people who know the ''correct'' answers in one domain also know the ''correct'' answers in the other does not mean that practical intelligence in one domain actually carries over into the other. To make that claim, one needs evidence that the same answers are correct in the two situations. But we don't really want to carp about the degree of consistency and predictability in behavior: Our approach considers them to be empirical questions rather than pretheoretical assumptions; and besides, Sternberg's theory of practical intelligence doesn't require them.

Specificity and Generality

As several of the commentators noted, we do, however, emphasize specificity over generality in the analysis of personality. We see a number of virtues in starting an analysis from the ''bottom-up''; though we certainly have nothing against finding generality. First, the specificity approach is consistent with the architecture of human cognition, and ours' is a cognitive theory of personality. Linville and Clark thoughtfully elucidate the fine-grained structure of declarative

and procedural knowledge. Second, the problem-solving process that underlies what Bergen and Dweck aptly called the actualized side of personality starts with individuals' appraisals of particular problems or tasks in specific settings. Given this framework, it makes most sense to start with analyses of reactions to context-specific tasks, as one might start with a chess master's handling of certain prototypic game situations. Of course, it is frequently preferable to consider several situations at once in light of a common task that the person sees in them. This process of aggregation, however, must still be taken with care, and thus the delineation of equivalence classes of situations is a central part of the assessment technology of a cognitive approach (e.g., Cantor et al., 1987; Wright & Mischel, 1987). Those equivalence classes frequently change for groups of individuals over time, as one of us has seen in a longitudinal study of the transition to college life, and almost always there are important interindividual differences in the sets of specific situations that are seen as mapping onto a life task (see Cantor & Langston, 1988; Zirkel & Cantor, 1988). Nevertheless, principles of generalization, based on individuals' perceptions of similarity of *task* across situations, fit well with our approach.

In fact, the social-intelligence view is not embarrassed by findings of generality—as a learning theory it is built on principles of generalization such as those described by Linville and Clark. We simply do not want to assume generality without testing its empirical validity for individuals in their particular life contexts. Whereas findings of discriminativeness and *specificity* are often dismissed by others as bothersome noise or unreliability of measurement, evidence of *generalization* is not theoretically inconsistent with the social-intelligence perspective. In that respect, despite its acknowledged complexity and more than occasional vagueness, we see a social-intelligence theory as in principle more parsimonious than traditional trait-based alternatives.

We look for generality in personality in the schemata, tasks, and strategies that individuals repeatedly bring to bear in their different life situations. Several commentators point to ways in which these consistencies are built. As Stewart and Pervin note, the autobiographical record provides a flexible source of self-continuity, and self-schemata can "expand" to accommodate quite a diverse array of self-data. Likewise, as Read and Miller and Emmons and King emphasize, individuals often have a penchant for seeing their most salient tasks and concerns as relevant to a broad collection of life events and situations; people also then choose to be in those task-relevant situations as often as they can. (Smith & Rhodewalt, 1986, provide a beautiful illustration of this generalization process in their analysis of the "hostile" world view and stress-engendering strategy of Type A individuals.) Linville and Clark suggest that strategies can easily be (over)generalized, sometimes from "ignorance" (as when a general principle is used to handle a novel situation or a traumatic event), and at other times because of automatic reliance on strategic "expertise" (as when the novel features of a task are ignored in favor of a more familiar interpretation). In this

vein, it seems critical to underscore, as they do, the extent to which this process of strategy generalization can sometimes be good for people—e.g., it helps when facing a crisis to operate by analogy to past events, even if only to bring oneself past the initial panic and immobilization. Whereas, at other times, individuals might be better off containing their impulse to rely on well-worn, familiar strategies, even when those strategies are quite effective in some situations. For example, effective use of defensive pessimism probably requires that a person makes fine distinctions between situations in terms of their anxiety-provoking properties, to identify those achievement contexts that simply are not worthy of exerting much preparatory effort (Cantor & Norem, 1988). Strategy generalization can lead to rigid, inflexible, or even self-destructive consistency; a consistency that is typical of much *mal*adaptive responding.

Social Intelligence and "Success"

Several of the commentators suggest that we have not made sufficient efforts to test and to apply the social-intelligence approach in clinical contexts. Carver and Scheier, for example, chide one of us for conducting research on "successful" students; whereas Pervin warns us that we may be overestimating flexibility in personality by ignoring clinical cases of resistance and unawareness. These critiques are very well taken and appropriate. A theory has to start somewhere, and we chose to start with the adaptive behavior of undisturbed, mature individuals. Although we agree that it is incumbent on theories of personality to address clinical material eventually, we don't see any principled reason to *start* with psychopathology. Some theories of personality, such as Freud's and Eysenck's, have made good use of clinical material, but in so doing they too have risked some generalizability. To take an example: even if Freud's insights into Dora, Schreber, and their confreres were accurate, that doesn't necessarily have anything to do with the rest of us. From our point of view clinical cases are *special* cases, and special cases—almost by definition—are going to be unrepresentative of the norm. In particular we suspected that analyses of clinical cases might miss some important features of social behavior. A chief feature of the interpersonal behavior of many individuals is that it can be flexible and optional rather than rigid and obligatory. The social-intelligence view, like its counterparts in the cognitive-social-learning tradition, seeks to explain this discriminativeness; and we preferred therefore to start with people who more often than not vary their behavior to fit the "requirements" of the situation. As these commentators wisely note, however, it is now time to venture into these other domains.

Although we do appreciate the prod in the clinical direction, at the same time we want to emphasize that observation of mature, undisturbed people also often reveals the breakdown of flexibility in social behavior. For example, Cantor has observed considerable struggle, pain, and self-defeating behavior on the part of

those "successful" honors students. Unfortunately, they do not always achieve their goals, feel good, or flexibly adapt their behavior to increase pleasure and decrease pain. In many respects, these students have taught us as much about the limits on adaptive social behavior as about its actualization.

Social Intelligence and Prescriptions for Living

In a related vein, many of the commentaries contained a plea for this approach to take more of the traditional role of intelligence theories to evaluate individuals and to prescribe solutions to problems in living. Baron, for example, notes that our approach is only descriptive and feels that we should pay more attention to the normative and prescriptive aspects of theorizing; that is, we should have a theory of character that informs people what goals they should have "on reflection" and how to achieve them. Baron may well be right, but his complaint is based on a view of social intelligence that is somewhat different from ours'. He argues that intelligence is an evaluative concept, and that the criteria for social intelligence should be prescriptive rules that bring people to those good goals that they would have on reflection.

This is not exactly what we mean by social intelligence. From our point of view, social intelligence consists of the cognitive resources—the knowledge and skills—that people bring to bear on social problem solving. Our attempt is to begin from a morally neutral stance, focusing on a person's goals (proximal and distal) and the cognitive means by which he or she achieves them. This is not because we disavow the study of self-defeating and maladaptive social intelligence; but rather because everything we know about social intelligence makes us very cautious about adopting uniform standards for success or for moral worth. To adopt a normative or evaluative point of view necessarily involves imposing the investigator's constructs and values on the subject, and this we are hesitant to set out to do, though we all end up doing some evaluation in the end. It seems important to come to those evaluative conclusions about the effectiveness and worthiness of individuals' goals and strategies only after having the chance to view the person in action for some time, and to listen to his or her viewpoint. Perhaps this is what Baron intended, and if so then we applaud the suggestion of standards derived "on reflection." We fear, however, that too much evaluation goes on in our field, *before the fact,* and without sufficient understanding of the individual's perspective. Moreover, we suspect that too often the evaluation enterprise takes over, clouding our view of the complexity inherent in most strategic "solutions." In our experience, there is rarely a strategy that is all "good" or all "bad" for all people; and furthermore, strategies that seem to work well at one point in time sometimes become, over time, less appealing as *the* solution to a pressing life problem. The problem with evaluative standards is that they don't typically encourage people to be flexible and to change their minds.

Thus, we agree with Baron in principle that psychologists should be able to help people to anticipate the goals that they would want to achieve on reflection. Nonetheless, we see this as a much trickier, more time-consuming task than he does. First, to evaluate the effectiveness of a strategy for an individual, a thought experiment has to be played. The costs of using the strategy have to be compared against those that would accrue from not using it. In this regard, for example, some defensive pessimists would be immobilized by anxiety without their strategy (Cantor & Norem, 1988); and most socially anxious individuals would probably stop socializing altogether from fear of embarrassment, without the self-protective security afforded by a social-constraint strategy (Langston & Cantor, 1988; Read & Miller, this volume). Cost-benefit analyses on life-task strategies are not easy to perform. Second, as Baumeister notes, the things that we do that turn out to be "good" for us are often buttressed by illusions that we hold about the self and others; and these are not likely to be the prescriptions for behavior adopted on detached or "wise" reflection. Pervin underscores the illusions that clients hold about themselves and about their real goals and motives. We want also to raise the possibility that psychologists will give bad prescriptive advice because we will not necessarily see clearly what the best solution will turn out to be. In this regard, we think that the best advice is for constant monitoring and updating: People mainly need to be encouraged to be flexible. It may simply be too risky to prescribe a standard course.

Therefore, we shy away from the normative and evaluative standards that most would associate with the study of social intelligence. When we evaluate, we try hard to take a relativistic stance, evaluating individuals' goals and strategies against a self-standard as much as against a social criterion. Taking this stance is by no means easy, and we have been guilty frequently of taking the simpler path of normative comparisons. It is a great deal simpler to say that defensive pessimism is bad because optimists experience less stress, than to say that the pessimists are still better off than they would be without the strategy. Similarly, there is little doubt that "extraverts have more fun" than do the socially anxious individuals who embrace a social-constraint strategy. Nonetheless, those anxious people are still striving for their social goals; it is not altogether clear that they would do so without that "self-defeating" strategy. So we want to try to be true to this relativistic stance, even though we don't always succeed in avoiding promoting uniform standards of adaptive behavior.

There is, however, one prescription that we too believe comes close to being generally applicable. Both Baron and Stewart stress the importance of individuals' roles as agents of choice in framing tasks and choosing strategies that facilitate active confrontation with the environment. Our prescription is to heighten that sense of choice and active confrontation whenever possible; even in the face of real social-environmental (or biological) constraints on behavior. A heightened sense of self-agency and of self-focused reflectiveness may well be a critical precursor to important personality change (Carver & Scheier; Stewart,

this volume). In fact, this is surely part of the reason for feelings of panic and of challenge experienced in times of traumatic stress, transitions, therapy, personal failures, or social censure, when there is at least the hope of effecting some real change (e.g., Stewart; Linville & Clark; Pervin; Read & Miller, all this volume). If as Baumeister notes, striving for self-agency is a major aspect of the process of giving meaning to life, then it can only help to encourage that process (Bandura, 1986). Therefore, a "complete" theory of personality and social intelligence must not shrink from participating in this one prescriptive enterprise.

What Are the Individual's Life Tasks?

A number of commentators appear to agree that our analysis is good as far as it goes, but that certain important things have been left out. This seems to be particularly true of our treatment of life tasks. We are criticized, somewhat paradoxically, for not being attentive enough to the larger meanings and origins of life tasks, and at the same time for ignoring the idiographic variation in the role played by age-graded life tasks in the lives of individuals. Our reaction to these critiques is mixed. We think that we give more attention to each of these considerations than may have come across in our target chapter, and yet there is clearly room for improvement here.

First, as to the broader meanings of life tasks: Baumeister thinks our analysis is incomplete, and that people have life tasks that go beyond the kinds of issues outlined by us and by Klinger and by Plutchik. We were impressed by his analysis and do agree that people need a purpose in life, and a sense of efficacy, legitimation, and moral worth, and that these needs (among others, like competence motivation; Harackiewicz, Abrahams, & Wageman, 1987) are important aspects of personality. It is not difficult to see these broader pursuits at the basis of many of the life tasks and strategies that we observe on the part of college students. In fact, it is reasonable to see our approach as organized around the need for meaning—the need to make sense of life; and self-esteem maintenance is certainly a core task served by individuals' strategies. We don't doubt the relevance of these global categories of life tasks; and we think that some tasks, such as the search for legitimation and moral worth, do get too little attention in our work.

Still, we would be skeptical of a system that strived too relentlessly to link individuals' life tasks to only a few "basic" and abstract pursuits. By way of illustration, we are more comfortable with the diversity of Erikson's scheme of age-graded psychosocial tasks than with Freud's rather exclusive reliance on sex and aggression as the core human concerns. Here, as in most cases, we take the position that more is better; and when one of us, for example, smoothes over some of this individual variation in the service of nomothetic comparisons on age-graded life tasks, it is not without awareness that this is a compromise (Cantor, 1988). (We also wish to note in passing that we were a little perplexed

by the idea raised by Emmons and King that a life-task approach somehow induces an achievement orientation towards motivation and behavior. In this regard, we see our work as following in the tradition of Erikson, 1950, and of Murray, 1938. Life tasks, like Brian Little's 1983 personal projects, guide individuals' purposive striving and problem solving in numerous different life domains. The domains of applicability of tasks and projects surely range at least from work to play and from intimacy to health, and probably elsewhere as well.)

In empirical work, we think that it is vital to stay as close as reasonably possible to the specifics of individuals' life-task concerns. We do think, however, that a theoretical scheme such as that provided by Baumeister can lead one to study important domains of life tasks that might otherwise be ignored. Emmons and King urged us to attend to the origins of life tasks in goals that have evolutionary significance (as in the work of Cantor's colleague, David Buss, 1986); and we have tried this in forging theoretical links to Plutchick's model. Here too, we can see considerable value in focusing on these adaptive pursuits as part of a hypothesis-generation process—e.g., mate-selection strategies become especially interesting if you believe that reproductive success is a central human goal. However, as Baumeister argues, delineation of such "basic" evolutionary goals cannot replace analysis of the diverse meanings that are overlaid on those shared goals. In this way, Buss's (1985) analysis of actual mate-selection strategies follows this dual-level approach to the meaning of evolutionary tasks. Somewhat more generally, Vallacher's and Wegner's (1987) analysis of action identification—i.e., of "what people think they are doing"—provides a framework for uncovering levels of meaning in human behavior, as Baumeister rightly notes.

This discussion brings us then to the other, flip side of the critique of our treatment of life tasks: the view that we ignore idiographic variation in favor of nomothetic analyses of age-graded tasks. In particular, Emmons and King and Read and Miller criticize the work of Cantor and her colleagues (e.g., Cantor et al., 1987) for failing to emphasize sufficiently interindividual variation in the *importance* and *meaning* of particular age-graded life tasks. Although we see their point, the critique does not seem entirely justified. It is certainly true that we do try to find ways to combine individuals' different construals of age-graded life tasks in order to see the diversity of their "solutions" to those tasks (e.g., college students try to "get good grades" or to "make friends" in very different ways). But we also try to point out that those strategic solutions are anchored in importantly different interpretations of the task-at-hand, with different underlying personal goals to be met (e.g., the defensive pessimist is working to harness anxiety before the test and the optimist is busy protecting the self after it; one shy person focuses on potential risks of social embarrassment, whereas another works on assertiveness). With full awareness of the pitfalls of nomothetic aggregation, we have generally tried to be guided by our data as to the appropri-

ateness of making those simplifying assumptions. This approach doesn't work perfectly, and we certainly do miss many of the nuances of the meaning of goals to particular individuals. At the same time it is not the case that we routinely gloss over such idiographic variation.

Actually, Cantor and her colleagues have found that age-graded life tasks vary in the extent to which individuals uniformly attach importance to them (Cantor & Langston, 1988). For example, not surprisingly, most of their college-student sample were very involved in "getting good grades" and in "making friends," and they were in substantial agreement as to the activities and situations that tapped into these academic and social goals. (They differed, nonetheless, in their appraisals of the ease or difficulty of achieving their task goals, and in the strategies that they embraced to work on these shared tasks.) In contrast, the life task of "being independent, on my own, away from family," a traditionally important age-graded task for college students, really seemed to mean very different things and to serve very different functions for different students (Zirkel & Cantor, 1988). Accordingly, analysis of this life task has remained more closely focused at the idiographic level of task construal, and on variations in task involvement.

For some students, this is a life task that they acknowledge but one that they almost "relegate" exclusively to the realm of mundane life activities—activities such as "doing my own laundry" or "managing my money," which they also feel are relatively easy to accomplish. In turn, this life task does not appear to serve as an organizing force in their life activities, nor does progress on this task predict much about their subsequent adjustment to college life. This pattern is very different, however, for another group of students in the sample (approximately one-third of the sample). These students, the "independence schematics," are very engrossed in this life task, imbue those mundane activities with more personal meaning and significance than does the typical student, and choose to be in activities that they view as relevant to striving for independence more frequently than most students. In turn, their feelings about their progress on the independence task are critically important to their later experiences of stress and satisfaction in college and even influence their satisfaction with academic success. For these students, the task of independence serves as a pivotal organizing force in their lives, influencing not only their general well-being but also their reactions to and activities in their other important life tasks (see also Stewart & Healy, 1985).

The analysis of these different patterns of striving for independence at college show clearly the necessity of taking into account individual differences in meaning and centrality of age-graded life-task goals—differences that several commentators pointed to as fundamental components of goal-based theory and assessment in personality research (Emmons & King; Read & Miller; Pervin). We wholeheartedly agree with the desire to remain faithful to these goal-based indi-

vidual differences. We also find it intriguing that the tasks themselves seem to differ somewhat in the amount of important idiographic variation that they actually reveal.

What Prospects for Assessment?

A number of commentators(Bergen & Dweck; Carver & Scheier; Emmons & King) expected us to have more to say about the problem of personality assessment. This expectation was natural given the title of the target chapter (frankly, we are running out of variations on the terms *personality* and *social intelligence*); and it is also natural in view of the dominance of traditional trait- and motive-based approaches to personality, for which assessment is central. But interesting theories of personality do not have to be focused on assessment: Freud, for example, never considered developing a questionnaire or projective technique.

Nonetheless, personality assessment has its rightful place, of course, and in our separate research programs (on defensive pessimism or hypnosis, for example) we have often been concerned with the measurement of individual differences on one dimension or another (Cantor & Norem, 1988; Register & Kihlstrom, 1986). One of us has contributed to some fairly detailed proposals for converting laboratory cognitive tasks into instruments for clinical assessment (Kihlstrom & Nasby, 1981; Nasby & Kihlstrom, 1986); he has also applied those methods to the study of both universal structure and idiographic content in autobiographical memory (Kihlstrom & Harackiewicz, 1982). Meanwhile, the other has been engaged in a 4-year longitudinal assessment of students' tasks and strategies for mastering the transition through college (Cantor et al., 1987). In pursuing these patterns of goal-based individual differences she has drawn on a variety of methods familiar to our commentators: self-reports of tasks, activities, and plans; observer Q-sort profiles of strategies from videotaped interviews; experience-sampling reports of on-line life task activities and emotional reactions; task conflict grids in which students consider the interrelations between their various task goals (after Little, 1983), symptom checklists and self-reports of daily life stress, and more.

In these contexts, in the clinical and semiclinical memory laboratory and in the longitudinal field study, we too have strived for idiographic precision, whereas remaining ever concerned with reliability and validity of measurement. In contrast, for other kinds of questions (the structure of social categories, for example, or the mechanisms underlying the self-reference effect), these idiographic assessments were less relevant, because we wished to study general processes that are widely shared if not ubiquitous (Cantor & Mischel, 1979; Klein & Kihlstrom, 1986). Throughout this work, however, respect and attention has been accorded to the scientific study of individual differences. We assume that the comments of Emmons and King on the absence of such an assessment

program in the cognitive social-learning tradition simply reflect inattention to our mentors' empirical work. In fact, we model our assessment approach after Mischel's studies of person and process variables in self-control and Bandura's analyses of the dynamics of self-efficacy motivation; interweaving, *as they do,* the experimental analysis of process variables with longitudinal data on personality in vivo (Bandura, 1986; Mischel, 1983).

But the social-intelligence viewpoint also necessarily puts severe constraints on the process of assessment. If, as we believe the evidence shows, social behavior is flexible across situations, and personality is continually constructed through social interaction, then personality assessments are going to have to be context sensitive as well. We really doubt that assessments of personality in the abstract, without regard for context, are of much practical or theoretical use. For example, even expressly clinical assessments have a specified purpose. People don't just come in off the street to have their personalities tested: rather, they subject themselves to personality assessment because they are having some problem in living. And a proper clinical assessment is geared precisely toward that problem and interprets the client's test performance in the context generated by it. The same responses, in the context of another presenting complaint, might have an entirely different meaning.

In a similar vein, we do not pursue this level of specificity in our analysis, charting a path, for example, from life-task construal to strategy to outcomes, simply to achieve descriptive completeness. We truly believe that understanding, not to mention predictive power, in personality psychology comes through recognition of complex cognitive-behavioral routes that bring individuals closer to their goals, and the data on defensive pessimism and on social constraint, among others, support this position. (The position is reminiscent of Freud's contentions about ego defense mechanisms; though we do not necessarily accept other aspects of his doctrine.) If we as a field focus only at the structural level of the "big five," then we risk missing much that is active (vs. reactive) and inventive (vs. stereotyped) about the concrete processes of human personality (Cantor, 1988). It is true that we should do both; but frequently the latter enterprise gets jettisoned, perhaps because it is expensive, messy, intensive, difficult, nonparsimonious, and so forth. It may also be very worthwhile.

On the idiographic–nomothetic issue, we admit that we back off, just as Allport had to. Our view, like Allport's (and Mischel's), dictates that proper assessments are idiographic—in which people are allowed to speak for themselves about their own lives and are not forced into the Procrustean bed of the investigator's favorite constructs. It's fine as humanistic doctrine, but, as Allport found out, it's difficult when it comes to getting research done. We want our research to tell us something that generalizes beyond the individual case, and in order to generalize some compromises have to be made. Sometimes we have to evaluate everybody according to the same (nomothetic) standard, accepting the

fact that a certain amount of noise will creep in. This compromise with idiographic ideals is not an embarrassment, but it *is* something we have to worry about.

We can entertain some other compromises that keep us close to individual lives but permit a considerable degree of generality. One strategy is to study relatively homogeneous subject groups, like honors college freshmen, who are presumably all at pretty much the same stage of life, with many shared goals and common resources. Another, which veers close to the semiclinical, is to enroll the subjects as active collaborators in the research enterprise. One of us, for example, is very interested in the possibility that autobiographical memory is organized temporally into relatively large "chunks" of time. But how are these chunks to be defined? Periods of 5 years, beginning at birth? Preschool, elementary school, secondary school, college, career, and retirement? What about people who don't go to college? Or who went to college after raising a family? Or who don't have a career? Or have had lots of different jobs? The only way to determine a proper organizational scheme is by means of a sensitive inquiry at the individual level, as Stewart suggests, and the most likely outcome is that the best organizational rubric will be different for each person. That's fine by us, because the essential prediction is that, however autobiographical memory is organized at the individual level, the effects of organization on memory processing should be the same for everyone. We look forward to creative combinations of idiographic and nomothetic research strategies, but our mathematical colleagues are either going to have to come up with some innovative techniques for data analysis, or our reviewers are going to have to accept some statistical sloppiness.

Awareness and Control

Almost from the beginning of psychology, there has been an unfortunate misidentification of things cognitive with things conscious. The idea of people as (fairly) rational social problem solvers sometimes gives the implication that every social interaction is the product of deep thought, in which people consciously evaluate their goals and select their strategies, and leads to the image of the person as lost in thought at the choice point. Moreover, at least since Kelly (1955) introduced the notion of constructive alternativism, we have had to struggle with the interpretation that there is no objective reality, and that behavior can be changed by a simple act of thought. Pervin is especially concerned that we may be too glib about what is conscious, and too sanguine about the ease of behavior change.

Pervin is right that clinical work frequently involves people who seem not to know what they're doing, or who have insight but seem unable to change. And insofar as our approach is to have any clinical application (as indeed we hope it does), we should have something to say about that. In fact, as he notes, one of us has been centrally concerned with the nature of nonconscious mental life

(Kihlstrom, 1984, 1987, 1988). And information-processing concepts such as automaticity may provide some explanation for why some aspects of personality and behavior are difficult to change. This is work for the future, and we appreciate the prod.

Where's Emotion? Where's Motivation? Where's Temperament?

An even larger group of commentators (Baron; Baumeister; Bergen & Dweck; Emmons & King; Klinger; Pervin; Read & Miller)—in fact, almost everybody who talks to us—raises the question of where emotion and motivation fit into our scheme of things. It's a natural question, because sometimes cognitive psychology seems to treat emotion and motivation as beyond the pale. We want to make it clear that we think that emotions and motives are important, and that any comprehensive approach to personality and social interaction should have something to say about how cognition, emotion, and motivation relate to each other. There's no doubt in our mind that emotions and motives affect cognitive processing, just as some emotions and motives are cognitively constructed, and that the "trilogy of mind" (Hilgard, 1980) works as an integrated system to produce behavior. In other places (Kihlstrom, 1988; Showers & Cantor, 1985) we have tried to expand on this point of view. Integrating emotion and motivation with cognition is work for the future, but we agree that it's important work.

Thus, we think that a very reasonable agenda for the future is to try to hook these specific structures of social intelligence to more global or generalized individual differences in motivation and emotion. This agenda applies to all the structural units that we have considered: (1) self-schemata and autobiographical memories might be importantly linked to and shaped by generalized expectancies (Carver & Scheier) and by current psychological stances (Stewart); (2) current life tasks can and do serve diverse purposes, reflecting species-general evolutionary goals, culturally defined prescriptions or moral guides to individual life, or highly personalized sources of self-worth and of meaning (Baron; Baumeister; Emmons & King); (3) strategic preferences are likely to be very much influenced by individuals' chronic or transient states of arousal, activity level, sensation seeking, and emotionality (Carver & Scheier; Klinger; Pervin; Stewart). Nevertheless, the thrust of these analyses should still be to capture the many ways that common dispositions are actually expressed in personal tasks and strategies, rather than to label people as abstract types.

For example, in several commentaries the links between defensive pessimism and need for achievement, and between social constraint and social anxiety or shyness, were correctly noted. Yet, in this regard, the dispositional constructs of shyness and social anxiety have been intriguing researchers as much because of the richness and diversity of their expression, as because of their unitary structure or *direct* predictive power (Cheek, Melchior, & Carpentieri, 1986). The Lang-

ston and Cantor (1988) analysis of social constraint, as only one possible path from social anxiety to social dissatisfaction, is based on this "fuzzy sets" view of shyness (see also Arkin, Lake, & Baumgardner, 1986). The diverse meaning of motives like need for achievement or fear of failure in the actual lives of real individuals provides another good illustration of this point (Stewart, this volume; Veroff, 1983). Thus, it would certainly be accurate to say that the defensive pessimist's construal of achievement tasks reflects a need for achievement balanced by a fear of failure (as Emmons & King suggest). In fact, they exhibit many of the features of a fear of failure profile *before* testing situations (Norem & Cantor, 1986). The critical feature of their task construal, however, is that it calls forth a strategy for initially overcoming the debilitating effects of that fear of failure. For these individuals, defensive pessimism is an active response to anxiety and, as such, they look less and less like "classic cases" (of fear of failure), the more they use it effectively. For others, or at other times, defensive pessimism may be a strategy invoked more to engender anxiety than to harness it—i.e., to energize and guard against overconfidence (Brehm et al., 1983). In that case, the resemblance to a fear of failure profile is just that—a resemblance. Either way, the strategy follows from the person's specific construal of the achievement task; and it makes more sense when viewed in that way, rather than as a mere reflection of an abstract motive of need for achievement or of fear of failure, or of some combination of both.

Thus, on one hand we see much to be gained in linking social intelligence to some underlying stylistic and motivational structures. At the same time, we also see much to be lost in the analysis of personality without detailed follow-through on the specific and diverse ways that those common dispositions are expressed in different individuals' lives. We still see a need to keep doing what Carver and Scheier aptly labelled as *idiographic process* analysis. And it was for this reason that we made the plea for increased attention to the "doing" side of personality, as a complement to the somewhat more popular broad-based structural distinctions in individual difference research (Cantor, 1988; cf. Emmons & King).

A related question comes up about temperament. Carver and Scheier suggest that individual differences in activity level, emotionality, and sociability are biologically based, but we're not so sure that's the whole story. The literature on gender identity and gender role (e.g., Money & Ehrhardt, 1972) may provide a better model for understanding temperament than any simple theory of genetic/hormonal endowment. A neonate's activity level, emotionality, or gregariousness may be initially determined by his or her biological endowment, but these behaviors also occur in a social environment that either supports or modulates them. In Money and Ehrhardt's terms, the program for temperament passes, at birth, from the genes to the environment. Later, as children become aware of their actions, their impact on the environment, and others' expectations of them, they begin to regulate their own behavior in accordance with perceived social norms and their developing self-concepts. In Money and Ehrhardt's terms, the

program for temperament is now continuously exchanged between the person and the environment. From this point of view, whatever continuities exist between child and adult temperament are as much a function of the environment and the person's self-concept as they are of some *autonomous* genetic/biochemical processes. Moreover, these are environmental effects that do not vanish with the wave of a behavior geneticist's hand. The program for temperament differentiation, like the program for gender differentiation, is uniquely structured in the specific interactions between child and parents and siblings and teachers; it is bound to be different for each child in the family, as behavior genetic studies of "nonshared environmental" effects reveal. So although it's true that a truly comprehensive theory of personality ought to have something to say about the biological substrates of temperament, it's also true that a theory of temperament ought to have something to say about cognitive and social processes.

Is This Really a Theory?

Baron raised the question of what kind of theory ours' is, and Bergen and Dweck ask how well it meets certain formal standards for a theory. It's a good question, and we're willing to concede that maybe ours' is only a viewpoint, or an approach, or a model. Whatever it is, it is also, as Bergen and Dweck note, a little vague here and there. By way of conclusion, let us at least try to clarify two problems.

Concerning structure: Social intelligence consists both in what the person knows about the self and his or her social world (declarative knowledge) and the skills, rules, and strategies by which he or she applies that knowledge to solve the problems encountered in social interaction (procedural knowledge). The only constraints on this structure are the constraints on the representation of knowledge in the cognitive system. We admit that this is not a simplifying structure, but we doubt that any *very* simple structure—like a three-or five-factor model of personality, for example, is any better. Moreover, some of the confusion between the "structural" and the "actualized" elements of personality in our model is intentional. We view the cognitive system as a fluid, evolving resource or fund; new life tasks and strategies, for example, are continuously constructed in vivo, and some of them then get "permanently" represented in the declarative and procedural structures of personality.

Concerning formal specificity: There's not a lot of rigor in the present account, if a theory's adequacy is measured by whether it can be translated into an operating computer simulation. It's too early for that, but when the time comes it seems likely that Linville and Clark will have been on the right track with ACT* or some related production-system architecture (like SOAR; Newell, 1988). In our view, social interaction involves problem solving, and personality contributes a repertoire of structured knowledge—declarative and procedural—that an individual can use in "solving" problems. Explicitly or implicitly, an ACT-like

architecture pervades both our book and target chapter and appears in some of our other work as well (e.g., Kihlstrom, 1985, 1987).

Even if our ideas are not expressed in LISP programs, in principle they could be. One of us, at least, is certain that an interesting computer simulation of an individual personality could be designed like an expert system, linking nodes and writing productions that would represent the individual's fund of knowledge in some domain, and that would effectively mimic his or her actual behavior in some domain. Here is another place where the idiographic–nomothetic tension rears its ugly head. The architecture of personality, in terms of how socially relevant declarative and procedural knowledge is structured, is universal. The particular content of personality, in terms of the social knowledge represented by nodes and productions, is highly idiosyncratic. A model of people-in-general will never adequately simulate an individual's behavior. But just let us tinker with the parameters a little.

REFERENCES

Arkin, R. M., Lake, E. A., & Baumgardner, A. B. (1986). Shyness and self-presentation. In W. H. Jones, J. M. Cheek, & S. R. Briggs (Eds.), *Shyness: Perspectives on research and treatment* (pp. 189–203). New York: Plenum Press.

Bandura, A. (1986). *Social foundations of thought and action: A social cognitive theory.* Englewood Cliffs, NJ: Prentice-Hall.

Brehm, J. W., Wright, R. A., Solomon, S., Silka, L., & Greenberg, J. (1983). Perceived difficulty, energization, and the magnitude of goal valence. *Journal of Experimental Social Psychology, 19,* 21–48.

Buss, D. M. (1985). Human mate selection. *American Scientist, 73,* 47–51.

Buss, D. M. (1986). Can social science be anchored in evolutionary biology? Four problems and a strategic solution. *Revue Europeenne Des Sciences Sociales, 24,* 41–50.

Cantor, N. (under review 1988). From thought to behavior: "Having" and "doing" in the study of personality and cognition. *American Psychologist.*

Cantor, N., & Langston, C. A. (in press 1988). "Ups and downs" of life tasks in a life transition. In L. Pervin (Ed.), *The goals concept in personality and social psychology.* Hillsdale, NJ: Lawrence Erlbaum Associates.

Cantor, N., & Mischel, W. (1979). Prototypes in person perception. In L. Berkowitz (Ed.), *Advances in experimental social psychology* (Vol 12, pp. 3–52). New York: Academic Press.

Cantor, N., & Norem, J. K. (in press). Defensive pessimism and stress and coping. *Social Cognition.*

Cantor, N., Norem, J. K., Niedenthal, P. M., Langston, C. A., & Brower, A. M. (1987). Life tasks, self-concept ideals, and cognitive strategies in a life transition. *Journal of Personality and Social Psychology, 53*(6), 1178–1191.

Cheek, J. M., Melchior, L. A., & Carpentieri, A. M. (1986). Shyness and self-concept. In L. M. Hartman & K. R. Blankstein (Eds.), *Perception of self in emotional disorder and psychotherapy* (pp. 113–131). New York: Plenum.

Erikson, E. H. (1950). *Childhood and Society.* New York: Norton.

Harackiewicz, J. M., Abrahams, S., & Wageman, R. (1987). Performance evaluation and intrinsic

motivation: The effects of evaluative focus, rewards, and achievement orientation. *Journal of Personality and Social Psychology, 53*(6), 1015–1023.

Hilgard, E. R. (1980). The trilogy of mind: Cognition, affection, and conation. *Journal for the History of the Behavioral Sciences, 16,* 107–117.

Kelly, G. (1955). *The psychology of personal constructs.* New York: Norton.

Kihlstrom, J. F. (1984). Conscious, subconscious, unconscious: A cognitive perspective. In K. Bowers & D. Meichenbaum (Eds.), *The unconscious reconsidered* (pp. 149–211). New York: Wiley.

Kihlstrom, J. F. (1985). Posthypnotic amnesia and the dissociation of memory. In G. H. Bower (Ed.), *The psychology of learning and motivation* (Vol 19, pp. 131–178). Orlando: Academic Press.

Kihlstrom, J. F. (1987). The cognitive unconscious. *Science, 237,* 1445–1452.

Kihlstrom, J. F. (1988). Cognition, unconscious processing. In G. Adelman (Ed.), *Yearbook of the encyclopedia of Neuroscience.* Boston: Birkhauser.

Kihlstrom, J. F., & Harackiewicz, J. (1982). The earliest recollections: A new survey. *Journal of Personality, 50,* 134–148.

Kihlstrom, J. F., & Nasby, W. (1981). Cognitive tasks in clinical assessment: An exercise in applied psychology. In P. Kendall & S. Hollon (Eds.), *Cognitive-behavioral interventions: Assessment methods* (pp. 287–317). New York: Academic Press.

Klein, S. B., & Kihlstrom, J. F. (1986). Elaboration, organization, and the self-reference effect in memory. *Journal of Experimental Psychology: General, 115,* 26–38.

Langston, C. A., & Cantor, N. (in press). Social anxiety and social constraint: When "making friends" is hard. *Journal of personality and social psychology.*

Little, B. (1983). Personal projects: A rational and methods for investigation. Environmental Behavior, 15, 273–309.

Mischel, W. (1983). Delay of gratification as process and as person variable in development. In D. Magnusson & V. P. Allen (Eds.), *Human development: An interactional perspective* (pp. 149–165). New York: Academic Press.

Money, J., & Ehrhardt, A. A. (1972). *Man and woman, boy and girl: Differentiation and dimorphism of gender identity from conception to maturity.* Baltimore: Johns Hopkins University Press.

Murray, H. (1938). *Explorations in personality.* New York: Oxford Press.

Nasby, W., & Kihlstrom, J. F. (1986). Cognitive assessment of personality and psychopathology. In R. E. Ingram (Ed.), *Information-processing approaches to clinical psychology* (pp. 217–239). New York: Academic Press.

Newell, A. (1988). *Unified theories of cognition.* The 1987 William James Lectures, Carnegie Mellon University.

Norem, J. K., & Cantor, N. (1986). Defensive pessimism: "Harnessing" anxiety as motivation. *Journal of Personality and Social Psychology, 51*(6), 1208–1217.

Register, P. A., & Kihlstrom, J. F. (1986). Finding the hypnotic virtuoso. *International Journal of Clinical and Experimental Hypnosis, 32,* 84–97.

Showers, C., & Cantor, N. (1985). Social cognition: A look at motivated strategies. In M. Rosenzweig & L. W. Porter (Eds.), *Annual Review of Psychology* (Vol. 36, pp. 275–305). Palo Alto, CA: Annual Reviews.

Smith, T. W., & Rhodewalt, F. (1986). On states, traits, and processes: A transactional alternative to the individual difference assumptions in Type A behavior and physiological reactivity. *Journal of Research in Personality, 20*(3), 229–251.

Stewart, A. J., & Healy, J. M., Jr. (1985). Personality and adaptation to change. In R. Hogan & W. Jones, (Eds.), *Perceptives on personality: Theory, measurement, and interpersonal dynamics* (pp. 117–144). Greenwich, CT: JAI Press.

Vallacher, R. R., & Wegner, D. M. (1987). What do people think they're doing. Action identification and human behavior. *Psychological Review, 94,*3–15.

Veroff, J. (1983). Contextual determinants of personality. *Personality and Social Psychology Bulletin, 9,*331–344.

Wright, J. C., & Mischel, W. (1987). A conditional approach to dispositional constructs: The local predictability of social behavior. *Journal of Personality and Social Psychology, 53*(6), 1159–1177.

Zirkel, S., & Cantor, N. (1988). *Independence and identity in the transition to college life.* Paper presented at Meetings of the American Psychological Association, Atlanta.

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