Who Would Argue?

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At present in the scientific study of emotion, it is likely difficult to find psychologists who would argue against the notion of cognitive mediation, so long as we accept a broad definition of what is “cognitive.” It is not surprising, therefore, that many of us may have little with which to quibble regarding Lazarus’s interesting target article. Could any investigator of human emotion argue with a call for in-depth interviewing of subjects and reduced reliance on one-shot assessments? Would any emotion theorist disagree with the contention that there are both biological universals and cultural sources of variability in the way emotions are aroused and regulated (cf. Ekman, 1994; Russell, 1994)? And wouldn’t all but the hardest headed economist endorse the contention that rationality is not easily defined in a practical way? Moreover, wouldn’t both Freud and Skinner dismiss models of a static person–environment relationship and agree that these interactions are ongoing and changing? These are some of the issues that Lazarus raises, but they do not strike us—at least in the moderate way in which he discusses them—as especially controversial or problematic.

It was nice, as well, to read such an explicit acknowledgment of the important role of nonconscious processes in the generation of emotions. Indeed, interactions between emotion and cognition that are out of awareness are consuming considerable research attention of late, and several excellent summaries of this work have been published (Bornstein & Pittman, 1992; Niedenthal & Kitayama, 1994). Discussed in the Niedenthal and Kitayama (1994) volume, for example, are the ways in which emotion exerts unconscious influences on the processes involved in transforming sensations into mental representations, mutual influences of emotion and perceptual thresholds for certain stimuli, and (most important from the standpoint of Lazarus’s theory) how much cognitive processing is required for perceivers to make judgments about the emotional meaning of events. There is a subtle implication in the target article that such research is not being done—for example, Lazarus calls for a return to the microgenetic techniques used to study perception—but in fact it has become a thriving industry among social, clinical, and cognitive psychologists. We return to this point later.

In studying the nonconscious nature of appraisal, however, we are unsure of the value of bringing back concepts such as ego defense. Surely individuals do not always see what they would prefer not to, but the study of motivated inattention (described even at a macro level by Goffman, 1959, and others) is probably not advanced by the introduction of psychodynamic terminology, lest this new look go the way of the original New Look. It is simply not clear that, even with in-depth interviewing, we could determine reliably whether ego defenses are or are not involved in some cognitive process. In studies utilizing painstakingly videotaped psychotherapy sessions of the same patient over hundreds of hours, collaborators participating in the Program on Conscious and Unconscious Mental Processes at the University of California–San Francisco found it very difficult to agree on the presence of a particular mechanism of defense during one epoch or another. Will scientists ever be able to distinguish ego-defense from nondefensive forms of inattention? We aren’t sure, and that is why we agree, actually, with Lazarus’s bottom line here: We should not equate automatic appraising with the cognitive unconscious or the dynamic unconscious. Perhaps this distinction itself just isn’t very helpful.

Moreover, there are some ambiguities surrounding the distinction between automatic appraising and dynamic unconscious or ego defense. Lazarus asserts that “some automatic appraisals are the result of defenses,” but this conclusion implies that some automatic appraising process must precede the operation of a defense. In Lazarus’s view, an individual must first appraise an event as distressing and generate a negative automatic appraisal; only then is it possible for this person to engage in defense (unconsciously), generating another automatic appraisal or a conscious appraisal. It would seem difficult to disentangle the ongoing processes of automatic appraising and ego defense relying only on in-depth interviewing. In this complicated endeavor, experimental methodologies may be more helpful. Lazarus suggests that sometimes “the person is making two appraisals at the same time—one conscious as a defense, the other unconscious, but both with different emotional outcomes.” In-depth interviewing would not likely uncover this state of affairs, but it can be investigated experimentally. For example, two recent studies (Power & Brewin, 1990; Power, Brewin, Stuessey, & Mahoney, 1991) used an emotional priming task to investigate controlled and automatic processing of emotional material in samples of normal subjects. They varied the stimulus-onset–asynchrony (SOA) between the onset of a prime (positive or negative life-events) and the onset of a probe (positive or
negative self-referent adjectives). Following a negative prime, subjects took significantly longer to endorse as self-descriptive negative adjectives at SOAs of 2,000 msec than at SOAs of 250 msec. In addition, subjects endorsed fewer negative adjectives following a negative prime at SOAs of 2,000 msec compared to 250 msec. Consistent with the literature on positive and negative priming (e.g., Neely, 1977; Tipper, 1985; Tipper & Cranston, 1985), the authors concluded that these findings indicate that regulation of affect is a controlled process. It seems that this kind of experimental paradigm, in which various levels of SOA are compared, would be most able to separate the ongoing processes of automatic appraising and ego defense.

Moving on to the vexing problem posed by asking whether emotions are rational, the discussion here is as much about the difficulty in defining rationality as it is about values. Indeed, a view of human behavior that sees as the only goal the maximization of utility (i.e., wealth) will not provide a useful framework for the study of emotion. It is not irrational to sacrifice oneself for one's loved ones, as Lazarus suggests. We would argue that it might also not be irrational to try to maximize one's happiness and the happiness of others, even if this involves failing to optimize utility by, for instance, engaging in "sunk cost" choice strategies or declining to place a dollar value on one's time (Mayer & Salovey, in press; Salovey, Mayer, & Rosenhan, 1991). Salovey and Mayer (1990) argued that it may be useful to dispense with arguments about rationality and instead to consider the emotions and their regulation as a kind of intelligence. Now, we would be the first to acknowledge that the term intelligence shares much of the same baggage as rationality. Still, emotional intelligence is neither an oxymoron nor a useless construct. Defined as "the ability to monitor one's own and others' feelings and emotions, to discriminate among them and to use this information to guide one's thinking and actions" (Salovey & Mayer, 1990, p. 189), the construct of emotional intelligence allows us to draw together diverse literatures on appraising and expressing emotions in the self and others, regulating emotion in the self and others, and using emotions in adaptive ways. It also provides a descriptive language for assessing and understanding individual differences in these processes.

So we too see value in not drawing sharp distinctions between emotion and reason, and we do not see any need for so-called rational individuals to heed Publilius Syrus's (c. 100 BCE/1961) call to "rule your feelings, lest your feelings rule you." However, we do not agree that the best solution to the problems connected with rationality is "to expunge the concept from our psychological vocabulary" (Lazarus). We think that, with careful definition and use, the concept may have utility. We agree, however, that irrationality should not be equated with maladaptive decision-making, but there are many cases in which, whatever the premises, inference processes do not follow rules of logic (Kahneman, Slovic, & Tversky, 1982). In many cases, illogical inferences produce the kind of maladaptive beliefs that are associated with distressing emotions, even when the consequent inference processes based on these premises are totally logical. For such cases, the concept of irrationality may be useful in the context of emotions so long as we delimit its meaning precisely.

The most vexing problem raised by Lazarus, in our view, concerns how best to study emotion. Lazarus appears to place little faith in laboratory experiments, arguing, instead, for the longitudinal investigation of small samples of individuals in natural settings. Our knowledge of emotion would certainly be enhanced by such studies. However, there is a danger here in being too quick to endorse the value of such fieldwork. Our cognitive colleagues provide an analogy. For years, the verbal learning tradition dominated the study of cognition; experiments generally involved the measurement of memorial processes using relatively simple stimuli in laboratory settings. This state of affairs prompted Neisser (1978) to exclaim, "If X is an interesting or socially significant aspect of memory, then psychologists have hardly ever studied X" (p. 3). Like Topsy, a field dealing with practical aspects of memory called "ecological" or "everyday" memory just grew and grew through the 1980s. And, although much of this work produced fascinating findings, there was a problem: It was taken as an article of faith that just because memory was studied outside the laboratory, in natural settings, with a focus on real-life autobiographical events, by definition such work had superior generalizability over the laboratory tradition.

In an article that needs to be loved, not despised, our colleagues Mahzarin Banaji and Robert Crowder (1989) argued that, with regard to everyday memory, the emperor may indeed be naked. The core of their argument is that just because methods with high ecological validity are used does not confer automatically extra generalizability onto the findings. As they noted, the use of lifelike methods does not guarantee generality of conclusions to real-life situations. They even argued, in a book chapter titled "Experimentation and Its Discontents" (Banaji & Crowder, 1994), that ecologically valid methods may work against such generalizability, a particular psychopathology of everyday memory. Just as principles of learning were unlikely to have been derived by observing rats in the New York City subway rather than, say, in contrived laboratory mazes, exclusive reliance on natural obser-
vation may not prove especially efficient for developing theories or generating stable findings about emotion. Are patients in psychotherapy, families at funerals, or teenagers attending horror films having emotionally important events that are somehow more valid than experimental subjects experiencing a laboratory mood-induction procedure? They might be, but not necessarily; indeed, the laboratory itself is an authentic real-world context (Klatsky, 1991; Mook, 1983). It may be as difficult to generalize from these everyday situations as it is from the laboratory. Of course, we are not arguing against such research (in fact, like Lazarus, we would like to see more of it); rather, we are noting that, just like laboratory methods, conclusions drawn from this research are also limited, especially if the sacrifices in sample size and representativeness suggested by Lazarus are implemented.

Lazarus may be giving up on the laboratory too easily. Indeed, it is possible to design emotionally provocative experiences in laboratory contexts. In our laboratory, for example, we have produced intense and “realistic” feelings, like envy, that seem to be experienced by most of the subjects we recruit (Salovey & Rodin, 1984). The naturally occurring complex of emotions felt by one of us witnessing the televised arrest on a double-murder charge of a childhood football hero is no more authentic and may indeed lend itself less easily to scientific scrutiny than if such feelings were generated using a laboratory imagination procedure. Why should we assume, as Lazarus suggests, that the study of the spontaneous feelings would have any better ecological validity than those elicited in the laboratory?

Finally, we are most interested in the economic and political issues raised by Lazarus at the conclusion of his target article. Lazarus argues that we need to convince “those responsible for funding” that expensive, in-depth, longitudinal studies are “the only way” we are going to overcome the vexing problems that he raises. This declaration strikes us as a bit overstated; such studies are but one of the ways that our knowledge of the emotions will advance. More important, Lazarus implies that such funding decisions are made by some external body over which we have minimal control. But, in this instance, “they” is “us.” Scientists who study emotion sit on panels reviewing grant proposals at several federal agencies. And we are often our own worst enemy. Peer review—in part due to the incredibly low levels of funding available given the number of excellent proposals written—often dissolves into a festival of methodological nit-picking rather than a celebration of what is good about our field. And all over Washington, we psychologists have a reputation for being among the most self-critical (indeed, self-destructive) of all fields in the peer review process. How can we expect successfully to “urge this on our research granting agencies” when, as peer reviewers, we so frequently take the position that nothing is ever good enough? The Super-Conducting Super Collider of emotion will not be built—not that the physicists got theirs either—if there is no consensus among ourselves that our field is worthy of funding.

Note

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References

In Defense of a Nomothetic Approach to Studying Emotion-Antecedent Appraisal

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Richard Lazarus, whose seminal work has strongly influenced many of the current “appraisal theories of emotion,” identifies a number of vexing research problems in the field and suggests potential solutions. Although I generally agree wholeheartedly with his diagnoses, I partially disagree with his proposals for therapy. Given the context of peer commentary, with many other emotion researchers providing commentaries, I take the license to concentrate on my own views and prior work with respect to the issues raised by Lazarus.

Stress

In justifying his focusing on the emotions rather than on stress, Lazarus raises, in passing, an important issue that deserves comment. Because much of Lazarus’s research career has been devoted to the phenomenon of stress and because he must be considered one of the eminent authorities in this area, his suggestion to stop treating stress as a unidimensional variable and to consider it as an emotional subset of distress-related emotions is of great significance. Based on the empirical results of several years of experimental stress research (Scherer, Wallbott, Tolkmitt, & Bergmann, 1985), we have come to the conclusion that stress should be considered a special case of emotional arousal and studied with respect to the specific emotional quality that underlies a stress episode. Rather than limiting stress to a subset of specific emotions, we have suggested that the term stress be used to refer to cases in which a particular emotion is more intense and/or longer lasting than would normally be the case. This occurs when the constellation of emotion-inducing situational factors remains unchanged and the person is unable to cope. Because emotions are emergency reactions and mobilize much of the organism’s available energy, any abnormally intense and prolonged emotional arousal is likely to deplete the organism’s energy and to be subjectively experienced as stress. In a similar fashion, psychogenic affect disorders can be analyzed, at least partially, in terms of a malfunctioning of the normal emotion-generating processes, particularly with respect to appraisal (Scherer, 1987).

I believe that the continuing separation, within and across disciplines, of the research approaches focusing on “normal” emotions (mostly general and social psychologists), stress (mostly medical researchers), and affect disorders (mostly psychiatrists and clinical psychologists) constitutes a research problem worthy of being addressed in greater detail. One of the classic discovery strategies in many disciplines is the study of disorder or malfunctioning of normal processes. One could assume, then, that the study of emotional disturbances and of specific types of stress could be quite revealing for the understanding of normal emotion processes. Conversely, scholars interested in stress and/or affect disorders might find it useful to consider the advances in our understanding of the elicitation and differentiation of normal emotion episodes. This is particularly pertinent in the context of Lazarus’s suggestion to focus more strongly on defense mechanisms in unconscious appraisal and individual coping strategies. Although there are some encouraging signs for this kind of integrative approach, particularly in the area of health psychology, much remains to be done. Given the complexity of the phenomena at hand, only interdisciplinary research programs worthy of this label are likely to provide an adequate solution.

Consciousness

The first vexing problem directly addressed by Lazarus concerns the issue of unconscious appraising.