

Comment

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Values, Politics, and Psychology

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Kendler's (October 1999) traditionally positivist critique, "The Role of Value in the World of Psychology," has consequences that call for close consideration, because it is indirectly a sermon against American Psychological Association (APA) involvement in social advocacy. Others can respond more appropriately to his use of Anne Harrington's (1996) treatment of holism in German culture, which tarred Gestalt psychology with the brush of "enchanted science." In her view, this is a romantic misconception of science that made it vulnerable to the unwarranted intrusion of Nazi values; in his, it lends itself to the equally illicit support of humanistic ones. In my reading, when Köhler (1938) wrote about the "objective requiredness of values," he was referring to their phenomenal quality of objectivity, as compared with the phenomenal subjectivity of tastes and preferences. This distinction is important for the part people's values play in

their lives, but it seems to me irrelevant to the objective or empirical justification of values, which in spite of some obscurity of exposition I think Köhler neither claimed on this basis nor achieved.

Be that as it may, I have to object to Kendler's (1999) interpretation of my justification for a socially activist psychology. He recognized that I know about the distinction between facts and values that positivists like to emphasize (Smith, 1961). He quoted me, in part, that

just as in our scientific roles we should and most do act *as if* there were truth "out there" that we only have to be clever enough to discover . . . in our personal, political, and psychologist roles we also do well to act *as if* there are objective right and wrong, better and worse choices and policies. . . . [W]e are adrift as persons and useless as citizens if we do not try to find and pursue the right and if we do not take our own convictions—and those of our opponents—seriously as attempts to advance the right and give it reality. We become literally "de-moralized." (Smith, 1991, p. 189)

He read this as showing my "fundamental error" (Kendler, 1999, p. 831) of confusing my position as a scientific psychologist with that of a democratic philosopher like Walter Lippman (1955), whom I had drawn upon—moral choice being in the domain of philosophy, not science. I am not confused.

Advocates of supposedly value-free science commonly assume that values and morals find their justification in some privileged other realm of religion, philosophy, or tradition. However, many educated participants in the contemporary world, particularly those influenced by science, are skeptical of the claims that each of these resources provide a firm basis for values and moral choice. Those who do not simply let their choices be guided by personal whim, by convention, or by one or another sort of authority have to make more or less informed judgments about what is good for people. Empirical facts cannot fully determine these judgments, but they certainly can inform and often lead these thinkers to modify them. Psychologists are

in an advantageous position to bring such facts to bear. So I have argued that psychologists, as scientists and professionals, have just as much justification as anybody else, and more than many, to enter into democratic controversy about value choices.

Here I can applaud one of Kendler's (1999) recommendations. He said that in an ethically pluralistic society, such as that found in the United States,

moral guidelines are needed . . . but they cannot be set in stone. They require constant evaluation to determine their consequences. . . . A continuous surveillance of the consequences of the guiding moral principles will be needed to elevate the acceptability and effectiveness of social policies. (Kendler, 1999, p. 832)

Evaluating consequences does not span the gap between facts and values, but it gives the empiricism of science a distinctive role in the rhetoric of value controversy.

Because psychologists have citizen and human responsibilities in their capacities as psychologists, members of organized psychology in Nazi Germany are blamed for not standing forth against Hitler's evils: They did not need a research base for such a risky but humanly desirable stand. APA policy positions on controversial matters like affirmative action, abortion, and nuclear freeze, to which Kendler objected, do not imply that "psychology and psychologists have a pipeline to moral truths" (Kendler, 1999, p. 831). They imply that the democratic political process of APA governance has led the organization to adopt the position, usually with the conviction that psychological research and professional experience have relevant contributions to make to public debate on the issues. The more fully developed the research evidence or the conceptual analysis is, the more likely APA's involvement will make a difference.

REFERENCES

- Harrington, A. (1996). *Reenchanted science: Holism in German culture from Wilhelm II to Hitler*. Princeton, NJ: Princeton University Press.

- Kendler, H. H. (1999). The role of value in the world of psychology. *American Psychologist*, 54, 828-835.
- Köhler, W. (1938). *The place of value in a world of facts*. New York: Livewright.
- Lippman, W. (1955). *The public policy*. Boston: Little, Brown.
- Smith, M. B. (1961). Mental health reconsidered: A special case of the problem of values in psychology. *American Psychologist*, 16, 299-306.
- Smith, M. B. (1991). *Values, self, and society: Toward a humanist social psychology*. New Brunswick, NJ: Transaction.

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Is Value-Free Science Possible?

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In this comment, we criticize some of the positions taken within Howard H. Kendler's (October 1999) article "The Role of Value in the World of Psychology" that seem problematic for the future of psychology and that seem to have overlooked similar inquiries that have appeared in the *American Psychologist* in the past 30 years. We also suggest an alternative approach, which acknowledges the inevitability of values and suggests a way of treating them.

What is Kendler's (1999) message? Primarily it is this: Because particular points of view are often wrong or self-serving, values should never be promoted by research psychologists (p. 831). Further, if psychologists take a position or try to influence policy, they endanger the potential social contributions of proper psychological science (which is objective, fact-based, and agnostic with respect to values; p. 833). This is because the potential authority of science is eroded in the public eye when scientific research is aligned with particular points of view (p. 831). Moral pluralism is an inescapable result of the impossibility of determining the relative worth of competing moral systems, and

thus judgments of value can only arise from democratic dialogue, not from particular theoretical perspectives or research programs (p. 832).

But is value-free science really possible? Howard (1985) discussed in detail the problems with the standard positivist view, most pertinently the unacknowledged fact that all research is inevitably value laden (Feyerabend, 1975; Kuhn, 1977; Schwartz, 1990). Empirical "facts" can support many incompatible theoretical positions and are always theory dependent and thus value dependent. That is, one can never choose between competing theories by simply looking to the brute facts, as Kendler (1999) proposed (p. 833). Instead of trying to act as neutral truth seekers following a value-free pursuit of knowledge, psychologists should acknowledge and accept the place of values in research, according to Howard.

In a similar essay on science and values, Miller (1969) argued that by abdicating their responsibility to align their work with their role as citizens, psychologists risked leaving control of scientific practice with industrial or bureaucratic elites, who may have vested interests far more pernicious than those of psychologists (p. 1068). Bevan (1980) also addressed the relation of science and government policy, saying that science today is rarely a value-free inquiry; instead, it is often used as a tool by special interest groups for accumulating political power (p. 782). In this light, psychologists cannot afford to retreat from these realities and struggles but instead must clearly articulate their own visions of "the good" so that these visions may inform and perhaps influence the debates.

Kendler (1999) used Harrington's (1996) distinction between *enchanted* (holistic, value encompassing) and *disenchanted* (mechanistic, positivistic) science as a framing device for his article, arguing that only disenchanted science is appropriate science. What Kendler did not seem to recognize is that even mechanistic theories are suffused with values. For example, behaviorism, arguably one of the most disenchanted theories in history, certainly has influenced values and social policy. Its strong emphasis on rewards and punishments supports the existing capitalistic economic structure, vertical-hierarchical conceptions of control and reinforcement, and the heavy emphasis on grades and punitive sanctions found within many American schools.

Furthermore, we question Kendler's (1999) criticism of holism. Showing that holistic argumentation has been misused in history for justifying terror systems does

not allow the conclusion that holism or enchanted theories are inappropriate; of course, any tool can be misused. In defense of holism as a useful theoretical lens, Sperry (1988) argued that emergent or top-down properties of complex systems are just as real and causal as the mechanistic or bottom-up processes described by disenchanted theories. Thus, although holistic perspectives undoubtedly place strong challenges on existing scientific methods, they may well be worth the effort. Ironically, Kendler's willingness to rely on the product of the democratic process to determine societal values is itself an endorsement of holism: His position implies that the emergent collective will is more valid than the beliefs of any one individual and should be trusted as the best estimate of "the good" (p. 832).

Of course, Kendler (1999) was correct to insist that scientists (holists and reductionists alike) must meet the objective epistemological standards of science. How can scientists bring this requirement and the desirability of acknowledging the role that values and preferences play in scientific research under the same umbrella? One perspective is that practitioners' values are already fairly obvious, in the very questions they have chosen to study and the factual conclusions they endorse (e.g., Kendler's own values are fairly clear in his article). However, if values are to be made even more explicit, research reports could contain a conclusory note or footnote in which the researcher acknowledges the underlying purposes connected with the research, his or her own moral and ethical preferences, and the "take-home" message (if any) he or she would like to promote. By including such a note, researchers' implicit wishes and foundational beliefs can become more transparent and available for open discussion and criticism. Of course, the researcher's values and beliefs will make a larger impression, to the extent that the reader is convinced by the research methods and results.

In summary, we believe there is no avoiding the fact that scientists are people, motivated to do research in part by a desire to confirm their own values and beliefs. Because data collection is guided by theories, which lie midway between values and facts, facts will always be influenced by values. We suggest that acknowledging this will ultimately better serve Kendler's (1999) goal of enhancing democratic dialogue than will the futile effort to retain an illusory separation between facts and values, an effort which will fail for all but the most pedestrian or descriptive research. Kendler quoted At-

kinson as saying psychologists should “not disguise political efforts by cloaking them in the framework of psychological research” (p. 833). We agree, and we add that psychologists should not disguise their values by cloaking their findings as objective facts. It would be better if everyone “came out of the closet.”

REFERENCES

- Bevan, W. (1980). On getting in bed with a lion. *American Psychologist*, 35, 779–789.
- Feyerabend, P. K. (1975). *Against method*. London: Redwood Barn.
- Harrington, A. (1996). *Reenchanting science: Holism in German culture from Wilhelm II to Hitler*. Princeton, NJ: Princeton University Press.
- Howard, G. (1985). The role of values in the science of psychology. *American Psychologist*, 40, 255–265.
- Kendler, H. (1999). The role of value in the world of psychology. *American Psychologist*, 54, 828–835.
- Kuhn, T. (1977). *The essential tension*. Chicago: University of Chicago Press.
- Miller, G. (1969). Psychology as a means of promoting human welfare. *American Psychologist*, 24, 1063–1075.
- Schwartz, B. (1990). The creation and destruction of value. *American Psychologist*, 45, 7–15.
- Sperry, R. W. (1988). Psychology's mentalist paradigm and the religion/science tension. *American Psychologist*, 43, 607–613.

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Beyond the Naturalistic Fallacy

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I strongly agree with Kendler (October 1999) in his warning to psychologists against committing the naturalistic fallacy. He maintained that the American Psychological Association has made unwarranted moves from neutral facts to support for morally loaded policies (e.g., regarding abortion) and cautioned that this kind of behavior could undermine public trust in psychologists as scientists. However, Kendler failed to acknowledge the extent to which his overall position rests on a denial of the possibility of ethical knowledge—not just for psychologists, but for scientists and ethicists alike. In turn, this failure con-

tributed to a distorted presentation of Köhler's (1938) work on values and its relation to Nazism.

Underlying Value Skepticism and the Naturalistic Fallacy

Kendler (1999) wrote, “Ethics, unlike science . . . has no extrinsic criterion, shared by all, that can be used to judge the validity of moral principles. Moral truths are restricted to the domain of the ethical system that endorses them” (p. 832). These statements amount to authoritative pronouncements, not to the effect that people disagree about ethics, but that there are no transcendent ethical truths that can be discovered. This claim extends significantly beyond the fact of pluralism. Arguably, making such a claim also extends beyond the sphere of competency of psychologists qua psychologists.

Elsewhere, Kendler (1999) committed the naturalistic fallacy as he tacitly moved from the fact that societal value beliefs change to a loaded claim about morality itself. He referred to “democratic processes that, over time, have modified what is legally and morally correct” (p. 834). However, from the fact that today's moral judgments contradict earlier ones, nothing can be deduced about what is morally correct, not even that what is correct has been modified (Kohlberg, 1971).

Kendler on Köhler

Kendler (1999) identified Köhler as one of those enchanted scientists who thinks that science has “a pipeline to moral truth” (p. 831) or that “science can reveal moral principles that are right for humankind” (p. 832). However, he cited no evidence that Köhler held such views. First, as Kendler himself noted, when dealing with Köhler's work, one has to distinguish between the empirical and the philosophical claims he made. Köhler clearly stated that the purpose of his book *The Place of Value in a World of Facts* was philosophical, not scientific (1938, p. x and p. 280). Second, nowhere in that book did Köhler defend moral principles like “always love your neighbor” or “never steal,” much less allege that such principles are right for all of humankind. The book is neither overtly nor covertly a text in ethics. Third, Köhler stated that the “phenomenon of value as such remains, whether or not there is agreement about definite values” (1938, p. 53), thereby showing awareness of the fact people disagree about values (a fact Kendler often invoked against the enchanted scientists). Finally, Köhler (1938) wrote,

If we were . . . to establish a direct relation between values and something in nature, our theory would undoubtedly commit the very error that is attributed to naturalism; such a theory would be an attempt to reduce values to indifferent facts, and would thus contradict the very simplest phenomenological observations. (p. 276)

Köhler was thus in agreement with Kendler that one cannot derive values from indifferent facts.

Köhler's (1938) project with values was never meant to be prescriptive; it was meant to be descriptive. In his writings, Köhler attempted to describe how it is that people perceive values against the background of the world. Further, his statements on values were not meant to prove that there are values (much less specific values); rather, they reflect Köhler's assumption that values exist. Kendler (1999) actually suggested that “the whole gamut of moral behaviors . . . is fair game for scientific investigations” (p. 835), but apparently only if investigators assume at the outset that moral behaviors cannot really be known to be correct or incorrect.

Dubious Claims About Nazism

Kendler (1999) claimed that the “same epistemological process that allows holism and humanistic psychology to generate a psychologically demanded morality has also justified Nazi and Communist ideology” (p. 828). Was the Nazi ideology justified? Nazis invoked many things in an attempt to justify the unjustifiable. Nothing follows from this. More interesting questions are, which ideologies are most amenable to the Nazi ideology, and might any moral beliefs have causally contributed to the rise of the Nazi regime? Frankl (1986), who lived through six Nazi concentration camps, attributed the gas chambers of Auschwitz, Treblinka, and Maidanek not to work done in some ministry or other in Berlin, but rather to the work done “at the desks and in the lecture halls of nihilistic scientists and philosophers” (p. xxvii). Frankl's logotherapy would have something to say about how a nihilistic worldview might causally contribute to hostile behaviors, but this is not the place to explore such ideas.

In any case, the nihilistic ethic of Nietzsche (1887/1999) is clearly more amenable to Nazism than is the value philosophy propounded by Köhler (1938). The nihilistic ethic actually condones a “production of values” by the strong to be imposed on the weak and the sick, among whom Jews and Christians are to be counted. It might thus be argued, contrary to Kendler, that in maintaining, as Frankl

(1986) and Köhler did, that values can be perceived as being required, one precisely opposes the nihilism that may have played both a causal and a rationalizing role in the development of 20th century totalitarian regimes. It is interesting to note that Frankl—who put values at the center of his psychological theorizing perhaps more than any other recent thinker—insisted that psychologists not attempt to prescribe specific values to patients but only encourage them to seek out what is required in their concrete life situation. Of course, he believed that if everyone did this, the world would never again witness anything like the Nazi Holocaust. He also thought this was compatible with a pluralistic democratic society and with sound science.

REFERENCES

- Frankl, V. E. (1986). *The doctor and the soul*. New York: Vintage.
- Kendler, H. H. (1999). The role of value in the world of psychology. *American Psychologist*, 54, 828–835.
- Kohlberg, L. (1971). From is to ought: How to commit the naturalistic fallacy and get away with it in the study of moral development. In T. Mischel (Ed.), *Cognitive development and epistemology* (pp. 151–235). New York: Academic Press.
- Köhler, W. (1938). *The place of value in a world of facts*. New York: Liveright.
- Nietzsche, F. (1999). *On the genealogy of morals* (D. Smith, Trans.). Oxford, England: Oxford University Press. (Original work published 1887)

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Let's Be Realistic!

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My critics incorrectly read into my position an espousal of a so-called value-free science, a discipline that is free of all values. The decisions to become a psychologist, to choose a specialty, and to investigate a research problem all involve value judgments. Moreover, natural-science methodology itself possesses a value system of its own. Three obvious values are honesty, logical consistency, and the maintenance of the political freedom essential for scientific inquiry. A major thrust of my

article (Kendler, October 1999) was not that all of science is value free but instead that scientific data are value neutral; there is no logical connection between the natural *is* and the moral *ought*.

How then is it possible for the American Psychological Association to endorse pro-choice and affirmative action policies? I suggest that the justification for political action begins with Köhler's attributing a factual status to the experience of a value judgment. Although he did not propose a universal moral system (I regret not making this clear), Köhler nevertheless created a phenomenological climate that encouraged the view that psychology could serve as a moral authority. With a similar orientation, Kurt Lewin (Kendler, 1989) designed action research to make people behave better. This served as a catapult for Chein, Cook, and Harding (1948) to implement action research on a broad scale. In some manner never clearly stated but dependent on the conflation of facts with values, psychologists become capable of identifying policies, including intensely controversial ones, that are of "maximal social utility" (Chein et al., 1948, p. 44). This line of thought is carried a step further by Smith (2000, this issue), who suggested that psychologists pretend that there is an objective way to differentiate right and wrong, although he should know better. This pretension led him to encourage "psychologists, as scientists and professionals, . . . to enter into democratic controversy about value choices" (Smith, 2000, p. 1151).

Of course, psychologists have a right to enter into value debates but not by deceiving themselves and their audiences into believing that they have special moral insights or that their conclusions are supported by the science of psychology. A more productive approach for psychologists is to investigate the consequences of social policies such as preferential treatment in affirmative action programs (Kendler, 2000). Then, citizens of a democracy can make their policy decisions in light of empirical evidence without being seduced into believing that psychology can offer moral guidance. Psychologists who wish to influence political decisions can combine with their like-minded colleagues to form committees (e.g., Concerned Psychologists for Affirmative Action) to espouse their political convictions without implying that the science of psychology justifies their position or that all psychologists share their views.

The plaintive justification for Smith's (2000) position surprises me; he argued that one literally becomes demoralized if

one does not pursue the right and "give it reality" (Smith, 1991, as quoted in Smith, 2000). Must one assign a reality to one's personal view and, by implication, an unreality to an opposing opinion? Must one be forced into believing that a particular side of the abortion or affirmative action debate is evil or unreal? By stubbornly believing in the reality of one's moral beliefs, one loses one's sensitivity to one's obligations and responsibilities to science and society.

The final argument Smith (2000) offered is that organized psychology has moral responsibilities to withstand the evils of Nazism. In response, I cannot avoid referring to my own flirtation with Marxism, a flirtation that terminated in the late 1930s after historical evidence and political action convinced me that Nazism and communism were opposite sides of the coin of totalitarianism. Although the intrinsic evil of Nazism seems matchless, a body-count criterion of victims places communism ahead of Nazism in its wickedness, as demonstrated by replicated studies of Lenin, Stalin, Mao Zedong, Pol Pot, Kim Il Sung, and minor associates (Courtois et al., 1999). I suggest that the evidence from the 20th century supports the contention that those psychologists who resisted the conflation of psychology and politics repulsed totalitarian influences more effectively than those who thought that psychology has the ability to identify the moral right.

Although agreeing with me that the American Psychological Association has no right to support "morally loaded policies," DuBois (2000, this issue, p. 1152) criticized me for believing that "there are no transcendent ethical truths that can be discovered" (p. 1152). Natural-science psychology can discover moral predispositions (e.g., parental love) but cannot validate a moral principle (Kendler, 2000). This does not mean that individuals cannot adopt, by faith, transcendent moral truths. My point is that an unbridgeable chasm separates the epistemology of science from that of religion. DuBois is mistaken in suggesting that I implied that Köhler indirectly supported Nazi values, but I forgive him.

Sheldon, Schmuck, and Kasser's (2000, this issue) equating of behaviorism with unbridled capitalism reflects a misunderstanding of behaviorism, a rejection of the fact/value dichotomy, and a conflation of the views of Watson, Skinner, and Tolman (Kendler, 1987). The idea that democracy is holistic frightens me if it means that votes for candidates will be judged to be greater than the sum of their parts.

Sheldon et al.'s (2000) suggestion that research reports should include the value preferences of the author makes a mockery of science. If scientific knowledge is inevitably contaminated by value commitments, how could natural-science methodology have succeeded in revealing reliable and consistent empirical laws in a broad range of disciplines? The key question is whether psychology should be interpreted as some vague combination of the humanities, postmodernism, critical theory, hermeneutics, social constructionism, and multiculturalism, with perhaps a smattering of natural-science ideas, that will have infinite appeal to those who yearn to experience an integrated and fulfilling world, or whether psychology should be conceptualized as a natural science that perceives the world as it is, not as it ought to be, and that can offer an accurate picture having great pragmatic value.

A symbiotic relationship integrates democracy, natural science, and moral pluralism. Suggesting that psychology can replace the political processes of democracy while ignoring the impact of moral pluralism will create a divisive, squabbling discipline. Psychology will forfeit professional respect, and society will lose a source of potentially useful knowledge.

REFERENCES

- Chein, I., Cook, S. W., & Harding, J. (1948). The field of action research. *American Psychologist*, 3, 43–50.
- Courtois, S., Werth, N., Panné, J.-L., Paczkowski, A., Bartosek, K., & Margolin, J.-L. (1999). *The black book of communism: Crimes, terror, repression*. Cambridge, MA: Harvard University Press.
- DuBois, J. M. (2000). Beyond the naturalistic fallacy. *American Psychologist*, 55, 1153–1154.
- Kendler, H. H. (1987). *Historical foundations of modern psychology*. Pacific Grove, CA: Brooks/Cole.
- Kendler, H. H. (1989). The Iowa tradition. *American Psychologist*, 44, 1124–1132.
- Kendler, H. H. (1999). The role of value in the world of psychology. *American Psychologist*, 54, 828–835.
- Kendler, H. H. (2000). *Amoral thoughts about morality: The intersection of science, psychology, and ethics*. Springfield, IL: Charles C Thomas.
- Sheldon, K., Schmuck, P., & Kasser, T. (2000). Is value-free science possible? *American Psychologist*, 55, 1152–1153.
- Smith, M. B. (1991). *Values, self, and society: Toward a humanist social psychology*. New Brunswick, NJ: Transaction.
- Smith, M. B. (2000). Values, politics, and psychology. *American Psychologist*, 55, 1151–1152.
- Improving peoples' understanding of the complex relationship between cigarette smoking and affective distress is critical, as such knowledge necessarily informs prevention, intervention, and public policy efforts. As noted by Parrott (October 1999), virtually all smokers attribute their smoking, at least in part, to its alleged anxiolytic properties. The question of whether these attributions are truly valid, however, remains unanswered. In his thoughtful and provocative article, Parrott (1999) took the discussion one step further by posing the question, Does cigarette smoking cause stress? The answer that emerged was an unequivocal yes. To support his argument, Parrott pointed out that (a) adolescent smokers report increasing stress levels as their smoking increases, (b) regular smokers are more stressed than their nonsmoking counterparts, and (c) smokers experience an overall reduction in stress when they quit smoking. On the basis of these observations, Parrott argued that repeated administration of nicotine by means of cigarette smoke inevitably leads to the emergence of nicotine dependence and withdrawal symptomatology, conditions that are stressful in and of themselves. A vicious cycle then develops, whereby the smoker smokes as a means of alleviating the psychological stress caused by withdrawal. According to this perspective, then, smoking (nicotine) presumably has no direct, beneficial effect on mood. Rather, smoking is only calming within the context of withdrawal relief.
- Parrott (1999) was clearly correct when he claimed that nicotine provides relief from the subjective distress evoked by withdrawal, as ample evidence supports this contention. However, his assertions that (a) smoking has no direct effect on affective distress and (b) smoking actually causes stress must be questioned. First, causation cannot be inferred from the fact that smokers are more stressed than nonsmokers; such an observation is simply correlational and, as such, might have multiple determinants. Correspondingly, Parrott seemed to imply that smokers are randomly distributed throughout the population, that is, that an individual's choice to become a smoker is arbitrary.
- Mounting evidence suggests, however, that predisposing individual differences in psychopathology (e.g., depression, schizophrenia), personality (e.g., neuroticism), and nicotine responsivity (e.g., initial sensitivity to nicotine) all mediate smoking heritability (Gilbert & Gilbert, 1995). Thus, one can just as easily assert that differences in affective distress between smokers and nonsmokers predate smoking onset. In fact, several longitudinal studies suggest just that (see Gilbert, 1995).
- Second, similar logic can be applied to interpreting the observation that adolescents who smoke are more stressed. In addition to the well-documented relationship between disorders of affect and smoking onset, evidence points to strong associations (assessed cross-sectionally and longitudinally) between conduct disorder—which itself is likely to be a stressful condition—and both smoking initiation and nicotine dependence (Riggs, Mikulich, Whitmore, & Crowley, 1999). Moreover, it is important to remember that a significant proportion of adolescents who smoke do not progress to nicotine dependence (Kassel, 2000). Again, there is strong reason to believe that predisposing differences in emotional and behavioral characteristics selectively set the stage for who becomes a smoker.
- Third, although the finding that smokers report less stress after quitting is consistent with Parrott's (1999) argument, other interpretations of these data are equally plausible. It may be that successful quitters feel a well-justified sense of pride and satisfaction over their accomplishment and that this phenomenon—rather than a pharmacological explanation—accounts for their reduced stress and overall sense of well-being. Moreover, once again there is reason to believe that individual differences, particularly in depressive symptomatology, are predictive of who actually quits smoking. Thus, analogous to the process through which some people become smokers, smokers who quit may be different from those who do not.
- Fourth, it is important to note that even nondependent smokers (chippers) report that they smoke to cope with negative affect on occasion (Shiffman, Kassel, Paty, Gnys, & Zettler-Segal, 1994). Similarly, there is reason to believe that adolescent smokers often attribute their smoking to mood regulation motives and do so, in all likelihood, before the onset of nicotine dependence (Kassel, 2000).
- Finally, in line with his assertion that smoking actually causes stress, Parrott (1999) argued that “there is no empirical evidence that nicotine does alleviate stress”

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(p. 819). However, put simply, such evidence does exist. Some studies have reported that within certain contexts, smoking can produce a beneficial effect on affective distress, independent of withdrawal alleviation (see Gilbert, 1995). For example, there is reason to believe that smoking reliably narrows the focus of attention and therefore may reduce anxiety by facilitating distraction from an impending threat (Kassel, 1997). This hypothesis received strong support in a series of studies demonstrating that those who smoked and rated slides experienced a significant reduction in anxiety, whereas those who smoked in the absence of distraction experienced no significant change (Kassel & Shiffman, 1997; Kassel & Unrod, 2000). Consistent with Parrott's (1999) argument, these findings impugn the notion that smoking is inherently anxiolytic. At the same time, they dispel the belief that smoking has no effect on negative mood outside of withdrawal relief. These studies identified a critical factor—distraction—whose presence mediates smoking's effect on negative mood. Although recent discussions have taken a "does it or doesn't it" attitude with respect to whether smoking alleviates psychological distress, our findings suggest that "it does *and* it doesn't," depending on the availability of concurrent attention-eliciting stimuli.

Identifying the contexts in which cigarette smoking exerts its effects on behavior, cognition, and emotion is crucial to furthering the understanding of why nicotine is so reinforcing. There is strong reason to believe that variability across both situational factors (e.g., predrug mood state, availability of benign distraction, distal or proximal aspects of stressors) and more enduring personal factors (e.g., trait anxiety, depression) plays a crucial role in determining the effect of nicotine on behavior (Gilbert, 1995). As such, the time has come to look for moderators and mediators of the relationship between smoking and emotional response and, in so doing, explore the influence of contextual factors rather than continue asking true–false questions. In the end, it may be that Parrott (1999) is correct in asserting that the emergence of nicotine withdrawal over time results in heightened stress levels among smokers, at least in some smokers. Even if this assertion is true, it is premature—and I would argue inaccurate—to regard this notion as a fait accompli or to hold that smoking has no effect on subjective stress other than to provide withdrawal relief. Given the costs to individuals and society at large brought on by cigarette smoking,

it is imperative that the door to examining the reinforcing mechanisms, particularly with respect to affect regulation, that govern this destructive behavior not be closed.

REFERENCES

- Gilbert, D. G. (1995). *Smoking: Individual differences, psychopathology, and emotion*. Washington, DC: Taylor & Francis.
- Gilbert, D. G., & Gilbert, B. O. (1995). Personality, psychopathology, and nicotine response as mediators of the genetics of smoking. *Behavior Genetics*, 25, 133–147.
- Kassel, J. D. (1997). Smoking and attention: A review and reformulation of the stimulus-filter hypothesis. *Clinical Psychology Review*, 17, 451–478.
- Kassel, J. D. (2000). Are adolescent smokers addicted to nicotine? The suitability of the nicotine dependence construct as applied to adolescents. *Journal of Child and Adolescent Substance Abuse*, 9, 27–49.
- Kassel, J. D., & Shiffman, S. (1997). Attentional mediation of cigarette smoking's effect on anxiety. *Health Psychology*, 16, 359–368.
- Kassel, J. D., & Unrod, M. (2000). Smoking, anxiety, and attention: Support for the role of nicotine in attentionally mediated anxiolysis. *Journal of Abnormal Psychology*, 109, 161–166.
- Parrott, A. C. (1999). Does cigarette smoking cause stress? *American Psychologist*, 54, 817–820.
- Riggs, P. D., Mikulich, S. K., Whitmore, E. A., & Crowley, T. J. (1999). Relationship of ADHD, depression and non-tobacco substance use disorders to nicotine dependence in substance-dependent delinquents. *Drug and Alcohol Dependence*, 54, 195–205.
- Shiffman, S., Kassel, J. D., Paty, J., Gnys, M., & Zettler-Segal, M. (1994). Smoking typology profiles of chippers and regular smokers. *Journal of Substance Abuse*, 6, 21–35.

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Does Smoking Amortize Negative Affect?

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In Parrott's intriguing recent contribution to *American Psychologist* (October 1999), he argued that nicotine dependence leads to increased stress by imposing a cycle of repetitive abstinence effects across the day, souring smokers' diurnal affective experiences. We agree that the nature and pat-

tern of affect manifest during ongoing smoking is a vital and comparatively neglected area of inquiry. However, we contend that, owing to inferential constraints that arise from self-selection in both smoking initiation and maintenance, the notion that smoking increases stress is only one of an array of possible explanations for the observed patterns of stress–smoking relations. We highlight this indeterminance with an illustrative example of one alternative concept: the idea that smoking redistributes or amortizes stress.

A note regarding terminology is required at the outset. An assumption we make in this comment is that Parrott's (1999) use of the term *stress* is essentially interchangeable with the constructs of negative mood and negative affect. In our discussion, we use the term *affect* rather than *stress*, because the former allows greater contact with additional literatures and theories. For instance, Solomon's (1977) theory of the affective dynamics of drug addiction yields predictions that are consonant with Parrott's findings.

The Problem of Latent Affective Destiny

Parrott's (1999) argument that smoking causes increased negative affect is based on several observations. The first is that periodic oscillations in negative affect occur during regular smoking and that these oscillations are tied to the smoking schedule such that mood generally improves immediately upon smoking and worsens between cigarettes. A second observation is that negative affect is generally higher among smokers than among nonsmokers in cross-sectional research. A third premise is that smoking initiation occasions an increase in tonic negative affect in young smokers. Finally, some studies suggest that smoking cessation results in decreased tonic negative affect among former smokers. Taken together, this family of observations would seem to point to a causal role for cigarette smoking in the amplification of negative affect.

However, as Parrott (1999) noted, the integrity of the causal account arising from these observations depends crucially on the assumption that smokers and nonsmokers are comparable in terms of affective or stress liability. There are grounds for questioning this assumption. For instance, mounting evidence suggests that susceptibility to negative affect is a diathesis for the development of nicotine dependence (e.g., Anda et al., 1999; Kandel & Davies, 1986).

Comparing smokers' and nonsmokers' affective patterns is fraught with po-

tential pitfalls. Of course, what is really needed is information about what smokers' affective experiences would have been had they not become dependent smokers. The appropriate but admittedly elusive comparison group would consist of individuals with equivalent presmoking characteristics who did not become smokers. Of course, this condition is essentially intractable: Why would a putatively vulnerable person not become a smoker despite an environment in which cigarettes are ubiquitous and in which youth routinely experiment with smoking?

For similar reasons, it is difficult to make strong inferences from cross-sectional comparisons of smoking and nonsmoking adults or from longitudinal data from adolescents. Even if only a subset of smokers become tobacco dependent to self-medicate a preexisting affective vulnerability, the population of current smokers might reasonably be expected to report increased negative affect relative to nonsmokers. Likewise, the increasing negative affect observed in adolescent smokers could reflect a dampened version of what might have happened to them had smoking not been interposed. In both cases, changes in negative affect could only really be evaluated relative to the smoking population's affective destiny in the absence of the uptake of smoking.

Smoking Cessation: A Differential Filter?

Smoking cessation studies can provide vital information regarding the role of smoking in affect regulation. However, cessation studies cannot cleanly substitute for information about presmoking affective characteristics or latent affective destiny. The intervening dependence process necessarily muddies inference because the physiological and associative residues of dependence are likely to influence affect for many months postcessation.

Recent research suggests that affect improvement or stress reduction following smoking cessation is not the rule for all or even most smokers. For instance, we have shown that a substantial proportion of smokers (including complete abstainers) report prolonged or exacerbating affective symptoms long after cessation and that these individuals are at particularly high risk of later relapse (Piasecki, Fiore, & Baker, 1998).

Issues of sampling are also vital in interpreting the smoking cessation literature. Our studies, like another discounted by Parrott (1999; i.e., Gilbert et al., 1998), allow individuals who lapse after the tar-

get quit date to contribute affective information to data analyses. This method is a necessary complement to traditional approaches in which analyses of affective reports are limited to complete abstainers. Experiencing decreased negative affect after smoking cessation may serve as a permissive factor for smoking cessation in a subset of individuals. Thus, traditional research methods may unwittingly filter from the data those smokers likely to display worsening affect over the postcessation period.

An Amortization Account

If smoking presents an "affective raw deal," why do people do it? It may be worthwhile to consider an account for at least a subset of smokers that complements Parrott's (1999) proposal. This hypothesis is based on the following beliefs: (a) Affectively vulnerable persons are more likely to begin smoking than are others, (b) these individuals are less likely to quit smoking successfully, (c) the latent affective destiny for many of these individuals would likely have been tumultuous, (d) smoking acutely and repeatedly mitigates negative affect instigated by external stressors or arising from internal person variables, and (e) smokers are reasonably accurate perceivers and cost-benefit analysts of at least average skill.

This conjunction of beliefs leads us to propose that there may be individuals for whom smoking is an attractive alternative because it *amortizes* negative affect, staving off severe, unexpected periods of negative affect in favor of predictable, manageable bouts of it. Both accounts—smoking increasing or amortizing stress—are speculative propositions, because each depends on particular assumptions regarding the smoker's latent affective destiny. Furthermore, the two ideas are not mutually exclusive. For instance, the amortization idea implies something of a bet against affective destiny: An individual smoker could experience either more or less total negative affect over the smoking career than would otherwise have occurred. The possibility of incurring excess tonic negative affect may represent an acceptable risk to some smokers when weighed against the prospect of severe, phasic affective bouts (e.g., exacerbating depression symptoms following smoking cessation in the formerly depressed; Covey, Glassman, & Stetner, 1990). This risk may become even more acceptable because of the immediacy of nicotine's effects on mood, given the tendency for immediate rewards to be highly desired and for de-

layed rewards to be discounted (e.g., Rachlin, 1989).

Parrott's (e.g., 1995) experimental work is very interesting and has been valuable in highlighting the rhythm of affect in dependent smoking. Much of value can be learned by contrasting this pattern of affect with those observed postcessation and those seen in variously constituted groups of nonsmokers and former smokers. However, it is important to bear in mind that affective profiles have diverse constituents (e.g., elevation, scatter, trajectory) that may be independently influenced by drug use and misuse. It is also important to recognize that nature has not provided researchers with ideal control conditions for making strong inferences about the impact of smoking on these dimensions of affective display.

REFERENCES

- Anda, R. F., Croft, J. B., Felitti, V. J., Nordenberg, D., Giles, W. H., Williamson, D. F., & Giovino, G. A. (1999). Adverse childhood experiences and smoking during adolescence and adulthood. *JAMA*, 282, 1652-1658.
- Covey, L. S., Glassman, A. H., & Stetner, F. (1990). Depression and depressive symptoms in smoking cessation. *Comprehensive Psychiatry*, 31, 350-354.
- Gilbert, D. G., McClernon, F. J., Rabinovich, N. E., Plath, L. C., Jensen, R. A., & Meliska, C. J. (1998). Effects of smoking abstinence on mood and craving in men: Influences of negative-affect-related personality traits, habitual nicotine intake, and repeated measurements. *Personality and Individual Differences*, 25, 399-423.
- Kandel, D. S., & Davies, M. (1986). Adult sequelae of adolescent depressive symptoms. *Archives of General Psychiatry*, 43, 255-262.
- Parrott, A. C. (1995). Stress modulation over the day in cigarette smokers. *Addiction*, 90, 233-244.
- Parrott, A. C. (1999). Does cigarette smoking cause stress? *American Psychologist*, 54, 817-820.
- Piasecki, T. M., Fiore, M. C., & Baker, T. B. (1998). Profiles in discouragement: Two studies of variability in the time course of smoking withdrawal symptoms. *Journal of Abnormal Psychology*, 107, 238-251.
- Rachlin, H. (1989). *Judgment, decision, and choice*. New York: Freeman.
- Solomon, R. L. (1977). Addiction: An opponent-process theory of acquired motivation: The affective dynamics of addiction. In J. D. Maser & M. E. P. Seligman (Eds.), *Psychopathology: Experimental models* (pp. 66-103). San Francisco: Freeman.

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A Smoke Cloud of Confusion

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We believe the cumulative empirical evidence to date does not support the conclusions made by Parrott (October 1999) in his article "Does Cigarette Smoking Cause Stress?" His conclusions that smoking increases negative affect and that quitting smoking decreases negative affect are based on a highly selective review of studies that were for the most part not designed to accurately characterize these effects. Below, we review these studies, provide alternative interpretations of their findings, and note important misrepresentations of relevant work.

Characterizing Effects of Quitting Smoking—The Huge Problem of Selective Relapse

In smoking cessation studies, the individuals who experience the largest increases in abstinence-related negative affect are most likely to relapse (Covey, Glassman, & Stetner, 1990). Typically, these individuals are removed from study samples because they are no longer abstinent. The result of this selective dropout is group statistical mean affect scores representing only individuals who do not relapse. In comparison with relapsers, such individuals have repeatedly been found to experience fewer abstinence symptoms and less stressful environments. Inevitably, the mean values of negative affect are downwardly biased. That is, only the fittest (least stressed and most psychobiologically robust) are included in the reported mean values.

The potential degree of such biasing can be recognized by considering the fact that a majority of quitters in studies referenced by Parrott (1999) relapsed and were not included in group means. Excluding the 12% dropout rate in Gilbert et al. (1998), the dropouts at one month ranged from 31% to 87%, and those at six months or a year ranged from 85% to 92%. Parrott's interpretations fail to account for or even to note this high dropout problem.

The Repeated Measurements Effect—Another Confounding Problem

Researchers have observed significant decreases in mean self-reported negative af-

fect (Sharpe & Gilbert, 1998) and pre-quit baseline smoking withdrawal questionnaire scores (McChargue & Collins, 1998) as a function of repeated measurements. These decreases can occur during a baseline period in the absence of any intervention and can simulate an intervention-caused decrease in negative affect when in fact the intervention results in either no change or an actual increase. For this reason, smoking cessation trials in which few baseline measurements are taken may result in decreased negative affect means because of the repeated measurements effect. Of the studies Parrott (1999) reviewed, only two used more than one baseline (Gilbert et al., 1998; West & Hajek, 1997), and only one included a no-quit control group (Gilbert et al., 1998). Importantly, if this study had used a single baseline without a randomly assigned no-quit control group, the findings would have almost certainly been interpreted as supporting, rather than contradicting, Parrott's position.

Failure to Consider Methodological Problems of Cited Studies

Parrott (1999) failed to address the above-noted dropout and repeated measurements effect problems in studies he cited as supporting his view. Interestingly, Parrott suggested problems only with the one study (Gilbert et al., 1998) whose results were strongly contrary to his view. We believe the results of this study are more valid than those of others because the study (a) minimized dropouts (only 12%), (b) included a randomly assigned no-quit control group, and (c) included six pre-quit baseline sessions. Parrott not only failed to recognize these strengths but also erroneously claimed that abstinence was not biochemically confirmed. As clearly stated in Gilbert et al. (1998), carbon monoxide concentrations were assessed at 48-hour intervals throughout the abstinence period, and nicotine and cotinine levels were assessed 3, 10, 17, and 31 days after quitting, as well as when any carbon monoxide concentration exceeded four parts per million.

Parrott (1999) also claimed that participants in the Gilbert et al. (1998) study were allowed to lapse and still be considered quitters. Although it is true that the quit-group participants were allowed to smoke a total of no more than 10 cigarettes over the 31-day protocol and still be included in the quit group, only a minority of quitters smoked any cigarettes at all during this time, and the withdrawal patterns of this group did not differ significantly from abstainers.

Further, Parrott (1999) did not cite empirical findings and theoretical reviews

suggesting that nicotine and smoking can reduce negative affect under some conditions (Gilbert, 1995; Kassel & Shiffman, 1997). There are also problems with his suggestion that the failure of smoking to reduce negative affect levels of smokers to below those of never-smokers means that smoking simply alleviates nicotine withdrawal symptoms. This argument fails to consider the fact that smokers are genetically and environmentally disposed to negative affect (Gilbert, 1995). Parrott also argued that the higher stress levels of adult smokers are indicative of the deleterious effects of smoking. We believe this is as erroneous logically as inferring that antidepressant medications increase depression because the depression levels of antidepressant users are higher than those of nonusers. Finally, contrary to Parrott's claim, there is a great deal of evidence supporting a neurochemical rationale for why nicotine and smoking should alleviate negative affect (reviewed by Gilbert, 1995).

Conclusions

With rare exceptions, studies to date have been designed to effectively assess treatment outcomes but have not allowed for the accurate characterization of the intermediate and long-term effects of quitting smoking. Extremely high rates of selective dropout, a lack of no-quit control groups, and the failure to account for the repeated measurements effect make valid interpretations impossible. A variety of methodologically sound studies not cited by Parrott (1999) have found acute smoking and nicotine to reduce negative affect. Our view of the literature suggests that smoking and nicotine can reliably decrease negative affect and stress under specific conditions. It is clear that much research is required before the effects of nicotine and smoking on affect can be accurately characterized. Current evidence suggests that these effects are highly situation- and trait-dependent (Gilbert, 1995; Gilbert et al., 1999) and that new and rigorous experimental methods are needed to make progress in understanding them (Gilbert et al., 1998, 1999).

REFERENCES

- Covey, L. S., Glassman, A. H., & Stetner, F. (1990). Depression and depressive symptoms in smoking cessation. *Comprehensive Psychiatry*, 31, 350-354.
- Gilbert, D. G. (1995). *Smoking: Individual difference, psychopathology, and emotion*. Washington, DC: Taylor & Francis.
- Gilbert, D. G., McClernon, F. J., Rabinovich, N. E., Plath, L. C., Jensen, R. A., & Meliska, C. J. (1998). Effects of smoking abstinence on mood

- and craving in men: Influences of negative-affect-related personality traits, habitual nicotine intake and repeated measurements. *Personality and Individual Differences*, 25, 399–423.
- Gilbert, D. G., McClernon, F. J., Rabinovich, N. E., Dibb, W. D., Plath, L. C., Hiyane, S., Jensen, R. A., Meliska, C. J., Estes, S. L., & Gehlbach, B. A. (1999). EEG, physiology, and task-related mood fail to resolve across 31 days of smoking abstinence: Relations to depressive traits, nicotine exposure, and dependence. *Experimental and Clinical Psychopharmacology*, 7, 427–443.
- Kassel, J. D., & Shiffman, S. (1997). Attentional mediation of cigarette smoking's effect on anxiety. *Health Psychology*, 16, 359–368.
- McChargue, D. E., & Collins, F. L., Jr. (1998). Differentiating withdrawal patterns between smokers and smokeless tobacco users. *Experimental and Clinical Psychopharmacology*, 6, 205–208.
- Parrott, A. C. (1999). Does cigarette smoking cause stress? *American Psychologist*, 54, 817–820.
- Sharpe, J. P., & Gilbert, D. G. (1998). Effects of repeated administration of the Beck Depression Inventory and other measures of negative mood states. *Personality and Individual Differences*, 24, 457–463.
- West, R., & Hajek, P. (1997). What happens to anxiety levels on giving up smoking? *American Journal of Psychiatry*, 154, 1589–1592.

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Cigarette Smoking Does Cause Stress

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The three commentaries (Gilbert & McClernon, 2000, this issue; Kassel, 2000, this issue; Piasecki & Baker, 2000, this issue) on my article (Parrott, October 1999) raised a number of important issues. I respond by focusing on several articles published during the past year.

The main comment was that smokers may be predisposed to suffer from stress. It is certainly true that cigarette smokers are overrepresented in many disadvantaged groups, including the poor and those with psychiatric and behavioral disorders. But the crucial question is, Does smoking help them control their stress, depression, or other problems? The empirical evidence shows that it does not. The recent study by Anda et al. (1999) demonstrated this, although Piasecki and Baker (2000) cited this study as showing that “negative affect is a diathesis for the development of nicot-

tine dependence” (p. 1156). Anda et al. found a positive association between the incidence of adverse childhood experiences (ACEs; e.g., verbal and physical abuse) and the incidence of depression in young adults. However, within each ACE level, significantly more smokers than nonsmokers reported feeling depressed. This occurred among those who had suffered multiple ACEs and among those who had not experienced any ACEs. This shows that nicotine dependency constitutes an additional source of distress, which simply adds to those of environmental or experiential factors (Parrott, 2000b). In another recent study, mental health, behavioral problems, and psychoactive drug use were monitored longitudinally in New Zealand youngsters. Early socioeconomic disadvantage led to an increased likelihood of later smoking, but taking up smoking increased the incidence of psychological problems three years later: “Smoking at age 18 elevated the risk of anxiety/depressive disorder” (McGhee, Williams, Poulton, & Moffitt, in press).

If smoking does not help relieve stress or depression, why are smokers overrepresented in these disadvantaged groups? One possible answer is that they develop nicotine dependency more readily as a result of stronger abstinence symptoms. People predisposed to neuroticism (i.e., with emotional lability) are likely to develop nicotine withdrawal symptoms characterized by tenseness and irritability, whereas people predisposed to depression may suffer withdrawal dominated by sadness. When they smoke they feel normal, but in between cigarettes their feelings of anxiety or depression are worse than if they were not nicotine dependent. Individuals with depression or a neurosis therefore develop nicotine dependency more readily and find cessation difficult, but they do benefit strongly from nicotine replacement (Kinnunen, Doherty, Militello, & Garvey, 1996).

Piasecki and Baker (2000) suggested that smoking has an amortizing function. I had to look this word up in the dictionary, and it seems to derive from a medieval French concept for reducing current debt by means of a permanent debt relieved only by death—possibly a very accurate parallel for tobacco smokers! But Piasecki and Baker used this term to suggest that smoking allows moods to be actively controlled and managed, and thus *possibly* lessened. While I agree that mood modulation is central to cigarette addiction, I must emphasize that there is little evidence for genuine mood improvements, although patterns of smoking and withdrawal certainly interact with the environmental conditions. Slater and I have recently shown

that nicotine withdrawal symptoms were significantly worse under high environmental stress than under low environmental stress, while post-cigarette relief was also correspondingly greater (Parrott & Slater, in press). Kassel (2000) and Gilbert and McClernon (2000) mentioned the extensive literature showing that smoking interacts with environmental conditions, but again it does not show that nicotine generates real mood advantages (compared with nonsmokers). This data can all be interpreted, with equal parsimony, as showing that abstinence symptoms and post-cigarette relief are closely related to the environmental circumstances.

Gilbert and McClernon (2000) pointed out that many of those attempting to quit fail and cheat. I should emphasize that mood gains will only occur with complete abstinence. If a “former” smoker has an occasional cigarette, his or her cholinergic neurotransmitter system will remain in a state of marked withdrawal: His or her stress levels should remain high. The best advice for smokers is to simply stop. Passive smoking is thus a particular danger for recent quitters, which is why cessation rates are so low when partners or colleagues continue to smoke. Gilbert et al.’s 1998 study, which I criticized, also only covered one month, but mood gains over such a short period are unlikely, as withdrawal symptoms and cravings will still predominate. Those studies showing significant mood improvements covered three to six months of continual abstinence (see Parrott, 1999). Gilbert and McClernon also suggested that the mood gains are an artifact of only unstressed smokers being successful at quitting, but the empirical evidence shows that this is not the case. Cohen and Lichtenstein (1990) reported near-identical levels of high baseline stress in those who quit and those who failed. My data (Parrott, 1995) were very similar: Those who quit for six months (and reported steadily decreasing stress) had very similar levels of self-rated stress at baseline as those who failed. Gilbert and McClernon also suggested that the decline in stress is an artifact of repeated testing. However, the steady smokers in Cohen and Lichtenstein (1990) reported consistently high stress over several repeated sessions. Further, we have found no evidence for this repeated measurements effect at the University of East London.

Kassel (2000) suggested that successful quitters may experience “a well-justified sense of pride and satisfaction over their accomplishment” (p. 1155), which may account for their reduced stress. Although former smokers often show improved self-

esteem, again I would like to offer an alternative explanation, namely, that self-esteem is normal in nonsmokers but impaired in smokers (as it is in many forms of drug dependency). Smokers who are quitting the habit should therefore experience a range of psychobiological improvements. This leads to another point: Not only does nicotine dependency cause stress (Parrott, 1999), it also leads to greater depression (Parrott, 2000a) and probably many other problems. Yet, there is surprisingly little research into the disorders caused by nicotine dependency. This clearly reflects the dominance of tobacco industry funding for nicotine research. Hence, many journals are dominated by research articles methodologically designed to show nicotine in a good light. Comparatively few research articles are concerned with abstinence, withdrawal, or the problems of nicotine dependency. This bias has also led to numerous convoluted models, based on the notion that that nicotine must have some positive functions. But once one grasps that nicotine is a psychologically damaging drug of addiction, its effects become far more straightforward to understand (Parrott, 1998, 2000a, 2000b, 2000c).

REFERENCES

- Anda, R. F., Croft, J. B., Felitti, V. J., Nordenberg, D., Giles, W. H., Williamson, D. F., & Giovino, G. A. (1999). Adverse childhood experiences and smoking during adolescence and adulthood. *JAMA*, 282, 1652-1658.
- Cohen, S., & Lichtenstein, E. (1990). Perceived stress, quitting smoking, and smoking relapse. *Health Psychology*, 9, 466-478.
- Gilbert, D. G., & McClemon, F. J. (2000). A smoke cloud of confusion. *American Psychologist*, 55, 1158-1159.
- Gilbert, D. G., McClemon, F. J., Rabinovich, N. E., Plath, L. C., Jensen, R. A., & Meliska, C. J. (1998). Effects of smoking abstinence on mood and craving in men: Influence of negative-affect-related personality traits, habitual nicotine intake and repeated measurements. *Personality and Individual Differences*, 25, 399-423.
- Kassel, J. D. (2000). Smoking and stress: Correlation, causation, and context. *American Psychologist*, 55, 1155-1156.
- Kinnunen, T., Doherty, K., Militello, F. S., & Garvey, A. J. (1996). Depression and smoking cessation: Effects of nicotine replacement. *Journal of Consulting and Clinical Psychology*, 64, 791-798.
- McGhee, R., Williams, S., Poulton, R., & Moffitt, T. (in press). A longitudinal study of cannabis use and mental health from adolescence to early adulthood. *Addiction*.
- Parrott, A. C. (1995). Smoking cessation leads to reduced stress, but why? *International Journal of the Addictions*, 30, 1509-1516.
- Parrott, A. C. (1998). Nesbitt's Paradox resolved? Stress and arousal modulation during cigarette smoking. *Addiction*, 93, 27-39.
- Parrott, A. C. (1999). Does cigarette smoking cause stress? *American Psychologist*, 55, 817-820.
- Parrott, A. C. (2000a). Nicotine/tobacco dependency: A direct cause of depression. *International Journal of Neuropsychopharmacology*, 3(Suppl. 1), S324.
- Parrott, A. C. (2000b). Smoking and adverse childhood experiences [Letter to the editor]. *JAMA*, 283, 1959.
- Parrott, A. C. (2000c). Tobacco and nicotine dependency: A direct cause of psychobiological distress. *Psych-Talk*, 24, 25-31.
- Parrott, A. C., & Slater, M. (in press). Nicotine withdrawal symptom development during conditions of high and low environmental stress. *Proceedings of the British Psychological Society*.
- Piasecki, T. M., & Baker, T. B. (2000). Does smoking amortize negative affect? *American Psychologist*, 55, 1156-1157.

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Who Is Rich? Who Is Happy?

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In the October 1999 issue of the *American Psychologist*, Mihaly Csikszentmihalyi argued that the relationship between material and subjective well-being is ambiguous, with wealthy individuals no more likely and sometimes less likely than impoverished ones to report they are happy. This is a comforting message for those who have been disturbed by aspects of the U.S. economy, in which over 35 million Americans, including one fifth of U.S. children, live in poverty (Mishel, Bernstein, & Schmitt, 1999). If poverty has no consequences for the well-being of individuals, this is good news for America.

In contrast to the studies Csikszentmihalyi (1999) cited, however, decades of research show economic status to be a significant correlate of psychological distress and diagnosable mental disorders (Belle, 1990). High levels of depressive symptoms are particularly common among those experiencing low income and economic stress, especially mothers with young children (Belle, 1990). Economic hardship often takes a toll on relationships among family members, increasing conflict between spouses and diminishing their capacity for supportive, attentive, and consistent parenting (Belle, 1990; McLoyd, 1998). Poverty is associated with elevated

rates of threatening and uncontrollable life events, noxious life conditions, marital dissolution, infant mortality, many diseases, violent crime, homicide, accidents, and deaths from all causes (Belle, 1990; McLoyd, 1998; Wilkinson, 1996). These experiences, surely, are not reflective of or conducive to happiness, in any meaningful sense of the word.

It is not just the well-being of those below the threshold of poverty that income affects. Rather, across the entire income spectrum, decreases in income are associated with increases in distress, morbidity, and mortality in a dose-response fashion. As Adler and her colleagues (1994) noted, "Not only do those in poverty have poorer health than those in more favored circumstances, but those at the highest level enjoy better health than those just below" (p. 15). Evidence for such a gradient has been found in studies of stress, depression, and hostility, as well as in numerous studies of mortality (Adler et al., 1994).

Csikszentmihalyi (1999) thought it significant that the self-reported happiness of Americans did not increase between 1960 and 1990, while at the same time the adjusted value of after-tax personal income more than doubled. Such a characterization of the U.S. economy ignores changes within this 30-year period as well as the crucial question of how that after-tax income was distributed. The real purchasing power of U.S. wages generally stagnated or fell in the 1980s, while poverty rates increased (Mishel et al., 1999). In 1990, to maintain their earlier standard of living, American workers increased their hours of employment (Mishel et al., 1999). During this same period, many employers reduced health and retirement benefits, and public investments in mitigating poverty through subsidized housing and direct income support declined (Folbre & The Center for Popular Economics, 1995). Most Americans, therefore, were neither richer nor more secure in 1990 than they had been in 1960.

Recent decades have also witnessed striking increases in the concentration of income and wealth in the United States. Corporate profits, the stock market, and chief executive pay are at record levels (Mishel et al., 1999). Yet the median U.S. family actually owns less wealth today than it did in 1989, and almost one in five U.S. households has zero or negative wealth (Mishel et al., 1999).

Inequalities such as these are themselves injurious to health and well-being, as Csikszentmihalyi (1999) briefly noted. Within the industrialized world, income inequality is associated with elevated rates of homicide, violent crime,

alcohol-related deaths, traffic fatalities, heart disease, infant mortality, poor educational outcomes, and overall mortality (Kawachi & Kennedy, 1999; Wilkinson, 1996). Indexes of income inequality in the industrialized nations are excellent predictors of life expectancy in those nations, with the most egalitarian societies, not the wealthiest societies, having the longest-lived citizens (Wilkinson, 1996). The United States, which leads the industrialized world in income inequality, ranks behind 19 other nations in life expectancy, including Costa Rica, Greece, and Spain (Kawachi & Kennedy, 1999).

The deadly effects of income inequality appear to be mediated by the stresses of life in a winner-take-all economy, losses in social cohesion and trust, and the skewing of social policies in favor of the wealthy at the expense of the poor and middle class (Kawachi & Kennedy, 1999; Wilkinson, 1996). It is not coincidental that the United States ended 60 years of guaranteed economic assistance to poor families as the Dow Jones average reached new heights. As a classic Tom Tomorrow cartoon put it, "If the poor don't like it, let 'em buy their own senators!" (Folbre & The Center for Popular Economics, 1995, p. 1.8). Or as Supreme Court Justice Louis Brandeis put it, "We may have democracy, or we may have wealth concentrated in the hands of a few, but we can't have both" (Loneragan, 1941, p. 4).

Certainly the experience of happiness is not limited to the wealthy, nor do riches ensure happiness. Yet wealth, poverty, and economic inequality have profound implications for the well-being of individuals. It would be unfortunate if Csikszentmihalyi's (1999) dismissal of such implications in the case of self-reported happiness led us, as psychologists or as citizens, to imagine that they do not.

REFERENCES

- Adler, N. E., Boyce, T., Chesney, M. A., Cohen, S., Folkman, S., Kahn, R. L., & Syme, S. L. (1994). Socioeconomic status and health: The challenge of the gradient. *American Psychologist*, 49, 15-24.
- Belle, D. (1990). Poverty and women's mental health. *American Psychologist*, 45, 385-389.
- Csikszentmihalyi, M. (1999). If we are so rich, why aren't we happy? *American Psychologist*, 54, 821-827.
- Folbre, N., & The Center for Popular Economics. (1995). *The new field guide to the U.S. economy: A compact and irreverent guide to economic life in America*. New York: New Press.
- Kawachi, I., & Kennedy, B. P. (1999). Income inequality and health: Pathways and mechanisms. *Health Services Research*, 34, 215-227.
- Loneragan, R. (1941). Brandeis' glorious career ended. *Labor*, 23(9), 4.
- McLoyd, V. C. (1998). Socioeconomic disadvan-

tage and child adjustment. *American Psychologist*, 53, 184-204.

Mishel, L., Bernstein, J., & Schmitt, J. (1999). *The state of working America: 1998-1999*. Ithaca, NY: Cornell University Press.

Wilkinson, R. G. (1996). *Unhealthy societies: The afflictions of inequality*. London: Routledge.

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Human Individuality, Happiness, and Flow

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Csikszentmihalyi's (October 1999) interesting analysis of happiness put forth research issues psychologists should attend to in greater detail. However, he made a number of errors in logic and paid inadequate attention to human individuality.

Csikszentmihalyi (1999) made a logical error when he concluded that happiness is the only intrinsic goal that people seek for its own sake. According to the mathematician Bertrand Russell (1945), happiness is sometimes a nonmotivational by-product of satisfying human desires. In other words, happiness is a common consequence of satisfying motives—it is not the cause of what motivates people. As J. S. Mill put it, happiness cannot be found by directly aiming for it, only by aiming to satisfy basic needs and taking pleasure en passant. In drawing the conclusion that happiness is the ultimate goal, Csikszentmihalyi confused a common consequence of satisfying desire (happiness) with the primary aim or cause of desire itself.

Csikszentmihalyi (1999) stated that Aristotle reduced all motives to happiness. This is misleading—Aristotle (1976) recognized that human happiness, motives, and pleasures are multifaceted, meaning that they differ in kind. He taught, "As activities differ in kind, so their pleasures" (p. 322). Aristotle was especially fond of the pleasures of friendship, justice, beauty, and learning. He did not discuss these pleasures in terms of a common element, such as *flow*. The issue is important because some psychologists try to reduce all motives to 1, 2, or a few categories, whereas others recognize 10, 20, or more fundamentally different motives. Aristotle belongs in this latter group. When one considers how many thousands of genes

affect behavior, the larger number of motives may seem more valid, and psychological theories generally may recognize far too few motives and basic needs to address the complexity of human experience. Flow is at most only one goal, and so researchers need to determine how it relates to other goals.

My psychometric work on human desires, which is only now being disseminated (see Reiss, 2000a), calls into question the construct validity of flow. I have found 15 to 16 significant intrinsic motives that cannot be reduced to a category called flow. In fact, these motives are largely uncorrelated with each other. Because the correlations are very low, no element (not even flow) can be a common motivator.

Csikszentmihalyi (1999) emphasized the similarity between the constructs of flow and intrinsic motivation. However, the analogy is not valid because ego control is lost in flow but strongly maintained in intrinsic motivation. According to Csikszentmihalyi, flow occurs when the ego becomes lost in experience. Losing the sense of autonomy and ego are crucial for experiencing flow, which is the historical position of mysticism and a number of religions (Armstrong, 1993; Reiss, 2000b). In contrast, the whole point of intrinsic motivation theory is that internal perceptions of control determine intrinsic motivation (Eisenberger, Pierce, & Cameron, 1999). In flow, all is one, so nothing can be separately seen as the controller versus the controlled. Thus, flow is incompatible with intrinsic motivation, or the idea of the self as controller and master of fate.

Finally, Csikszentmihalyi's (1999) analysis of flow does not give adequate attention to human individuality. Does everybody really need to be creative or become a scholar to enjoy flow? Are skill, concentration, and perseverance—the three values touted by Csikszentmihalyi—values for all? Can people with mental retardation experience flow? I suspect that flow researchers could significantly strengthen their theory by analyzing human individuality in detail. Psychologists must be careful not to put forth a model of human nature that leaves behind certain people or implies that scholars and creative people are superior specimens of humanity. Some people value skill acquisition much more strongly than others (Reiss, 2000a, 2000b). People differ significantly in how important achievement is for their happiness.

In conclusion, Csikszentmihalyi's (1999) analysis of the psychological experience of mysticism (loss of autonomy) is an interesting and important topic for future research. However, Csikszentmihalyi made a number of common philosophical

errors when he extended his work to a theory of human motivation. These errors are mostly the result of confusing cause and consequence. Further, individual differences must be recognized. Flow may be a valid analysis of what makes some people happy, but I do not believe it can be extended to all or even to most people.

REFERENCES

- Aristotle. (1976). *The ethics of Aristotle: The Nicomachean ethics* (J. A. K. Thomson, Trans.). New York: Penguin Books.
- Armstrong, K. (1993). *A history of God: The 4,000-year quest of Judaism, Christianity, and Islam*. New York: Ballantine Books.
- Csikszentmihalyi, M. (1999). If we are so rich, why aren't we happy? *American Psychologist*, 54, 821-827.
- Eisenberger, R., Pierce, W. D., & Cameron, J. (1999). Effects of reward on intrinsic motivation—Negative, neutral, and positive: Comment on Deci, Koestner, and Ryan (1999). *Psychological Bulletin*, 125, 677-691.
- Reiss, S. (2000a). *Who am I? The 16 basic desires that motivate our behavior and define our personality*. New York: Tarcher.
- Reiss, S. (2000b). Why people turn to religion: A motivational analysis. *Journal for the Scientific Study of Religion*, 39, 47-52.
- Russell, B. (1945). *A history of western philosophy, and its connection with political and social circumstances from the earliest times to the present day*. New York: Simon & Schuster.

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Cultural Values and Happiness

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Csikszentmihalyi (October 1999) has reminded social scientists that they cannot shrink from challenging the validity of their most cherished values, including the fundamental nature of happiness. He cited research affirming that material wealth does not correlate with happiness and then presented data correlating happiness with the experience of *flow*. However, in making this leap Csikszentmihalyi confused correlation with causation. If losing oneself in a project, relationship, or dream is followed by a very positive condition, it does not mean that the experience itself caused happiness. It is equally likely that *losing oneself* is the causative factor. In looking at

his data through the common Western values of individualism, rationalism, and the Protestant work ethic, Csikszentmihalyi may have overlooked the substance of happiness that has been frequently described in other cultures as experiencing the absence of the self by being in a state of intimate connectivity with others. From this perspective, the lack of relationship between materialism and happiness is explained without invoking the construct of flow: Any value that emphasizes the self obviates connectivity with others.

Research supports the tenet that the less people focus on themselves, the happier they are (e.g., Nolen-Hoeksema & Davis, 1999). Moreover, the quality of a person's connection with others is often the best predictor of therapy outcome and mental health (Hubble, Duncan, & Miller, 1999). With so much evidence supporting the importance of connectivity and de-emphasis of self, the fact that so much research in psychology emphasizes intrapsychic variables seems to indicate that the individualistic bias present in Csikszentmihalyi's (1999) article pervades the entire field (e.g., Sue & Sue, 1999).

REFERENCES

- Csikszentmihalyi, M. (1999). If we are so rich, why aren't we happy? *American Psychologist*, 54, 821-827.
- Hubble, M. A., Duncan, B. L., & Miller, S. D. (Eds.). (1999). *The heart and soul of change: What works in therapy*. Washington, DC: American Psychological Association.
- Nolen-Hoeksema, S., & Davis, C. G. (1999). "Thanks for sharing that": Ruminators and their social support networks. *Journal of Personality and Social Psychology*, 77, 801-814.
- Sue, D. W., & Sue, D. (1999). *Counseling the culturally different: Theory and practice*. New York: Wiley.

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Why Can't We Measure Happiness?

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There is obvious merit to the discussion of the origins of happiness and well-being presented by Csikszentmihalyi (October 1999). Focusing increasing attention on

the search for an adequate theoretical framework to clarify the elusive origins of subjective well-being is an important service within the job description for psychologists. In his description of *flow*, Csikszentmihalyi directed readers' attention to a phenomenological experience, which in turn suggested a phenomenological approach to understanding the subjective experience of well-being. This approach holds much promise, as the more traditional objective approaches to analyzing subjective well-being, such as measuring linear correlations of various aspects of material well-being, have clearly failed to provide significant understanding.

However, it seems that Csikszentmihalyi (1999) abandoned the phenomenology of flow midway through his article, when he suggested some linear correlation between flow and well-being. He noted that autotelic persons, who are more often in flow, "tend . . . to report more positive states overall and to feel that their lives are more purposeful and meaningful" (p. 825). This implies that less autotelic persons must report less positive states, purpose, and meaning. Although researchers are often tempted to increase the legitimacy of their arguments by offering objective evidence in support of their position, I submit that in citing this linear correlation, Csikszentmihalyi made the same error that has confounded previous attempts to delineate subjective well-being: that of objectifying the subjective experience. When researchers try to pin down the phenomenological experiences of flow or of happiness, they are inevitably disappointed with the elusive quality of the construct and the paucity of the correlations.

I base this argument on results of my own research (Sink, 1999), which suggest that the quantitative approach, when used to quantify flow or some other source of well-being, will fail to find significant correlations sufficient to explain more than a relatively small amount of well-being. I used a composite of several self-report instruments in an attempt to quantify a relationship between spiritual experience or religious belief and satisfaction with life or positive affect, surveying over 200 workers at a large psychiatric treatment facility and another 200 adult graduate students. Contrary to my expectations, the correlations between spirituality and well-being that I found remained at the same low level to low moderate level of significance ($r = .22$ to $.26$) that had typically been found over the years for correlations between well-being and various material resources, health, job satisfaction, or socialization. There were some indications that an instrument free of language bias and

religious value biases and more specifically focused on transformative spiritual experiences might yield slightly higher correlations with satisfaction with life and positive affect. However, another interpretation of the results is that the linear correlational methodology itself is inadequate to identify the phenomenological experiences of happiness and well-being. That is, taken objectively, almost anything can become a source of happiness or well-being, but happiness and well-being are seldom, if ever, derived from any single objective factor.

Also, flow can explain only a limited amount of happiness and well-being. It is likely that almost everyone has experienced flow during various periods of concentrated activity. The experience is certainly well-known in sports, where athletes refer to being "in the zone" and excellent performances are produced in an almost unconscious fashion. Very few people, however, are able to sustain flow over an extended period of time, and few, if any, can remain in the zone as a way of life.

I have reviewed the data that I had previously collected, looking specifically for indicators of flow as described by Csikszentmihalyi (1999), and I have found flow falls short of any high correlation with well-being. For example, several test items in one of the self-report instruments (the Spiritual Transcendence Scale; Piedmont, 1999) refer to experiences at least vaguely similar to the experience of flow as described by Csikszentmihalyi. A test item referring to a sense of having been so engrossed in an activity (in this test item, prayer and meditation) as to have become temporarily oblivious to external events correlated only very weakly with satisfaction with life ($r = .11, p < .05$) and with positive affect ($r = .11, p < .05$). Another test item that asked about having had (spiritual) experiences during which the participant had lost track of where he or she was or of the passage of time also correlated only very weakly with either satisfaction with life or positive affect. Although these test items were not constructed specifically to identify flow experiences, they do nevertheless seem to at least come close to some of the described characteristics of flow experiences, and they suggest that the correlations with satisfaction with life and positive affect may likely be no more conspicuous than are correlations with any variety of other constructs, including material resources and demographic variables.

The point of this discussion is not to deny the significance of Csikszentmihalyi's (1999) work on flow and happiness or of

my own, for that matter, on transformative spiritual experience and subjective well-being. Rather, my sense is that the process of reducing phenomenological experience to objective descriptions is the wrong direction for psychologists to take in pursuing their self-avowed goal of "discovering what promotes happiness" and "bringing this knowledge to public awareness" (Csikszentmihalyi, 1999, p. 827). Psychologists will do a greater service for society if they stick to studying the experience itself and not try to reduce the experience to quantified terms. Until psychologists focus on the phenomenological experience of happiness and the subjective sense of well-being, they will fail to achieve more than the most superficial understanding of optimal human development.

REFERENCES

- Csikszentmihalyi, M. (1999). If we are so rich, why aren't we happy? *American Psychologist*, 54, 821-827.
- Piedmont, R. L. (1999). Does spirituality represent the sixth factor of personality? Spiritual transcendence and the five-factor model. *Journal of Personality*, 67, 985-1013.
- Sink, J. R. (1999). *Religiousness, spirituality, and satisfaction with life: A structural model of subjective well-being and optimal adult development*. Unpublished doctoral dissertation, The Fielding Institute, Santa Barbara, California.

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Happiness, Flow, and Economic Equality

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Reading the comments on my article "If We Are So Rich, Why Aren't We Happy?" (October 1999), I realize how hopeless it is to try summarizing in a few pages 30 years of research and writing. Most of the criticisms commentators raised have been anticipated in and dealt with elsewhere (especially in Csikszentmihalyi, 1990, 1993). I naively thought that these issues did not need to be developed again at length. Apparently that was a mistake, so let me address some of these points after the fact.

To begin, let me restate something I thought I had made clear in the article,

namely, that I do not claim that flow is the only means of reaching happiness. I included flow with a variety of other means for controlling subjective states, such as learned optimism, hope, and more traditional forms of mental discipline. If I stressed flow, it was because this is the topic I know most about.

Timothy B. Smith (2000, this issue) wrote that losing oneself is the causative factor in happiness, and the less people focus on themselves, the happier they are. This formulation is a bit too simple: One can lose oneself in an orgy or at a Nazi rally without necessarily being happier as a result. To avoid the individualistic bias he complained about, it does not help to fall into a collectivistic bias; as I have argued in Csikszentmihalyi (1993) and elsewhere, the ideal is to integrate the two. In any case, even if Smith were right and losing oneself was the proximal cause of happiness, one would still have to ask, how does one lose oneself? My answer is that one way this happens is by becoming involved in flow experiences.

Steven Reiss (2000, this issue) raised some original points. He claimed I made a logical error in concluding that happiness is the only intrinsic goal people seek for its own sake. This because "happiness is a common consequence of satisfying motives" (p. 1161) and thus cannot be a cause in itself. I am afraid Reiss takes logic too seriously. In human psychology it is quite common for causes also to be consequences and vice versa. If I feel happy after hiking in the mountains, chances are I will want to experience that happiness again and take another hike. Reiss also chided me for not realizing that "human happiness, motives, and pleasures are multifaceted" (p. 1161). Here he failed to note the distinction so often made in my writings between the *activities* that produce flow, which are indeed multifaceted, and the *experience* of flow, which seems remarkably similar across activities. He also misunderstood what the "loss of ego" implies. It certainly does not mean losing "internal perceptions of control" (p. 1161; in fact, the sense of control is one of the main symptoms of flow, as already described in Csikszentmihalyi, 1975/2000). Try to imagine a rock climber or a race car driver not having an internal perception of control! What is lost in flow is not the "I" but the "me," to use the distinction made by William James (1890/1950) and George H. Mead (1934). Finally, Reiss asked the rhetorical question, "Does everybody really need to be creative or become a scholar to enjoy flow?" (p. 1161). If Reiss had read some of the original work on which

Psychophysiology and Racism

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Csikszentmihalyi (1999) was based, he would have noted that most of the examples of flow do not come from creative scholars but from inner-city teenagers, assembly-line workers, welders, Alpine farmers, an Egyptian hobo, a Chinese cook, and so on. I am tempted to add "whatever," given that it seems to matter so little what one writes; some readers will distort it for their purposes anyway.

The last sentence above applies nicely to John R. Sink's (2000, this issue) comments. I am not sure what to make of them. I sympathize with his failure to objectify subjective experience in his own research, but that does not mean that the task is impossible (see, e.g., Csikszentmihalyi & Patton, 1997; Csikszentmihalyi & Schneider, 2000; Moneta & Csikszentmihalyi, 1999). As far as knowledge goes, nothing is more objective than subjective experience. I do not know that the Earth is round or that electrons whirl inside atoms: I have to take these facts on faith. But I surely know when my shoe pinches or when I am amused or content. Therefore, I do not see why scientists cannot deal with phenomenology as they do with other objective processes: objectively.

Of all the comments, I found the one by Deborah Belle, Joanne Doucet, Jacob Harris, Joy Miller, and Esther Tan (2000, this issue) the most thought provoking. This response was built on serious scholarship as well as common sense and sound values. It would indeed be unfortunate if my article were to be interpreted as a defense of economic inequality. I take their rebuke as seriously as it was written, and I admit that I could have been more explicit in my article about the issues they raise. Nevertheless, let me make two points in my defense.

Belle et al. (2000) described how poverty is associated with a variety of ills (marital dissolution, infant mortality, crime, etc.) and then concluded, "These experiences, surely, are not reflective of or conducive to happiness, in any meaningful sense of the word" (p. 1160). Of course, I never claimed that such outcomes reflect or are conducive to happiness. I only tried to ask the question, given that rich people do not have all these problems, why aren't they happier? I honestly believe that this is still an important and unresolved question.

The second point refers to the last paragraph of the comment (Belle et al., 2000): "Yet wealth, poverty, and economic inequality have profound implications for the well-being of individuals" (p. 1161). As I tried to point out in my article, it is not poverty per se that seems to cause unhap-

pininess, but the relative deprivation the poor feel in contrast with the rich. Thus, I would not agree that poverty and inequality should be conflated. Inequality is an important issue, and if I had been writing on public policy, it would have had to be a central one. Even so, I should have stressed it more. However, I do not change my basic position, namely, that material wealth in itself seems to have remarkably little to do with happiness. Looking for happiness in wealth is not only useless but threatening to the physical and social environment. However, I think Belle et al. are right that struggling to achieve fairness and an equitable division of resources is a fundamental strategy to increase happiness.

REFERENCES

- Belle, D., Doucet, J., Harris, J., Miller, J., & Tan, E. (2000). Who Is Rich? Who Is Happy? *American Psychologist*, 55, 1160-1161.
- Csikszentmihalyi, M. (1990). *Flow: The psychology of optimal experience*. New York: HarperCollins.
- Csikszentmihalyi, M. (1993). *The evolving self: A psychology for the third millennium*. New York: HarperCollins.
- Csikszentmihalyi, M. (1999). If we are so rich, why aren't we happy? *American Psychologist*, 54, 821-827.
- Csikszentmihalyi, M. (2000). *Beyond boredom and anxiety* (25th Anniversary ed.). San Francisco: Jossey-Bass. (Original work published 1975)
- Csikszentmihalyi, M., & Patton, J. D. (1997). Le bonheur, l'expérience optimale et les valeurs spirituelles: Une étude empirique auprès d'adolescents [Happiness, the optimal experience, and spiritual values: an empirical study of adolescents]. *Revue Québécoise de Psychologie*, 18, 167-190.
- Csikszentmihalyi, M., & Schneider, B. (2000). *Becoming adult: How teenagers prepare for the world of work*. New York: Basic Books.
- James, W. (1950). *Principles of psychology* (Vol. 1). New York: Dover. (Original work published 1890)
- Mead, G. H. (1934). *Mind, self, and society from the standpoint of a social behaviorist*. Chicago: University of Chicago Press.
- Moneta, G. B., & Csikszentmihalyi, M. (1999). Models of concentration in natural environments: A comparative approach based on streams of experiential data. *Social Behavior and Personality*, 27, 603-637.
- Reiss, S. (2000). Human individuality, happiness, and flow. *American Psychologist*, 55, 1161-1162.
- Sink, J. R. (2000). Why can't we measure happiness? *American Psychologist*, 55, 1162-1163.
- Smith, T. B. (2000). Cultural values and happiness. *American Psychologist*, 55, 1162.

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Clark, Anderson, Clark, and Williams's (October 1999) offering of a biopsychosocial model for the study of racism and health is a call to action for psychologists. Particularly useful is their demonstration that the racism does not have to be objectively real to affect health outcomes; thus, consensual definitions of racism and discrimination become inessential. Historically, psychologists studying racism have been put to the Sisyphean task of proving again and again that racism exists. To value Clark et al.'s model, one must simply concede that people have been treated badly on the basis of race (a hard proposition to argue, given the United States of America's history of slavery, Jim Crow laws, and segregation) and that people who have historically experienced racial discrimination may reasonably perceive current life hardships as influenced by these events, leading to differential stress and health outcomes.

Clark et al.'s model (1999) and the questions it raises can and should inform empirical investigations of racism and health. The model can be situated in the debate over the diversification of psychology, the social meaningfulness of research, and bias in scientific research and practice. Contributions of racist stimuli to widespread health problems have been largely ignored by mainstream psychological science. Several factors account for this oversight. Politically conservative factions within health science have shaped the research agenda of federal agencies like the National Institute of Mental Health, agencies that promote biogenic over social or environmental explanations of mental disorders (Albee, 1996) and favor criteria for acceptable research based on traditional empirical models: objectivity, reductionism, and stringent control of variables. For various reasons, the study of racism and health is not a good fit with this agenda. Sue (1999) recently stated that scientific psychology has valued procedures—for instance, internal validity—that have "differentially hindered the development of ethnic minority research" (p. 1070). Harrell (1999) has also questioned the restrictive ways psychology "come[s] to know the world" (p. 60), arguing that an interdisciplinary and syn-

thetic approach, embracing research methods of philosophy, sociology, political science, and literature, must inform the study of racism, both because these methods can yield valuable qualitative data and because a congruent body of psychological literature on the topic has not appeared. (Hoshmand, 1999, and Seligman [in a roundtable discussion; see Morgeson, Seligman, Sternberg, Taylor, & Manning, 1999] have made similar arguments for an emphasis in community psychology and personality research on inclusion and synthesis rather than on scientific methodology.)

Designing an experiment on racism poses a challenge for researchers seeking answers (and funds!) under present criteria: how to operationalize racism according to an acceptable definition, how to ethically treat it as an independent variable, how to isolate and measure its effects separately from the mediator and moderator variables identified by Clark et al. (1999). Notwithstanding these challenges, some very ingenious work on racism has been done in the lab. Jones, Harrell, Morris-Prather, Thomas, and Omowale (1996) reported that exposure to videotaped and imagined scenes of racial stimuli, mediated by the level of intensity of the racial material presented, resulted in significant physiological responses (increases in blood pressure and heart rate, as well as facial electromyographic and pulse rate changes) in African Americans. Morris-Prather et al. (1996) found that although female African American participants reported more subjective distress when viewing videotaped negative encounters with authority figures (Caucasian or Black law enforcement officers), African American male participants showed greater increases in blood pressure yet reported less distress. These findings are congruent with the Krieger and Sidney findings (as cited in Clark et al., 1999) that pointed to denial as a coping response with negative health effects for African Americans. Demonstrations of the impacts of gender and racial experience may explain why males suffer from psychophysiological diseases in a disproportionately fatal manner and may lead to effective treatment strategies. Yet psychologists should not engage in this research only because it is the morally correct stance; they should do so because it will advance scientific practice. If there is to be a revolution in how psychologists practice science, the methods of psychophysiology, applied to racism, may emerge as a paradigm that bridges the gap between

the laboratory and the real world. With adequate funding, psychophysiological methods incorporating scientific measurements and control factors can be applied even in community settings.

In a roundtable discussion (Morgeson et al., 1999), Seligman recently stated that psychology historically had three goals that were intended to change the world: to cure mental illness, to make the lives of normal people more productive and fulfilling, and to nurture talent. Figuring out how racism affects health outcomes and how to prevent or treat the problem will do all three for the rapidly increasing proportion of ethnic minorities in the population. Psychologists should now create a scientific community dedicated to the study of health, racism, and discrimination; here are some ways this can be done. First, psychologists must find value in nonconservative research approaches and topics (the study of racist stimuli has occurred eclectically, outside the pale of traditional research; this can be changed). Second, because researchers study those issues best that are closest to their hearts, psychologists must help nurture into being the critical mass of researchers concerned with minority issues necessary to create a sense of scientific excitement and collaboration. Third, psychologists must realize that although there is something comforting about the positivistic study of biogenetic factors (genes are material and controllable; scientists can decode them, alter them, and eradicate disease), there is evidence that the less material yet no less real construct called society can also be altered in positive ways. (At the right social moment, psychologists' work can have great impact; consider Kenneth and Mamie Clark's seminal, if flawed, doll studies and the *Brown v. Board of Education* desegregation decision.) Fourth and perhaps most important, psychologists must support the commitment of significant research funds for widespread study of the psychophysiology of racism and other environmental stressors over biogenic factors. Genetics directly accounts for far less disease in the total population than do factors such as racist stressors, poverty, malnutrition, gender discrimination, environmental toxins, and childhood chaos and abuse. Genes do, of course, play a large role in the intergenerational development of disease processes in certain families, and study designed to alleviate human suffering is worthwhile. However, on the balance sheet, the lack of consistent, federally funded attention to environmental factors—like sys-

tematic racial discrimination—points to a significant weakness in social and scientific policy. An immeasurably positive impact on health outcomes for a huge segment of the population can be secured if psychologists take up the implicit challenge offered by Clark et al. (1999). Iijima Hall (1997) pointed out that psychology will become obsolete if it does not begin to address issues of concern to minority populations in research, education, and practice. Historically, psychology has alienated Blacks, Latinos, poor people, and others not only by passively ignoring their concerns but also by actively contributing to oppressive forces. Now is the time for the field to actuate a paradigm shift and lend its energies to the study, treatment, and eradication of racism and other forms of social injustice.

REFERENCES

- Albee, G. W. (1996). Revolutions and counter-revolutions in prevention. *American Psychologist*, 51, 1130–1133.
- Clark, R., Anderson, N. B., Clark, V. R., & Williams, D. R. (1999). Racism as a stressor for African Americans: A biopsychosocial model. *American Psychologist*, 54, 805–816.
- Harrell, C. J. (1999). *Manichean psychology: Racism and the minds of people of African descent*. Washington, DC: Howard University Press.
- Hoshmand, L. T. (1999). Locating the qualitative research genre. In M. Kopala & L. Suzuki (Eds.), *Using qualitative methods in psychology* (pp. 15–24). Thousand Oaks, CA: Sage.
- Iijima Hall, C. C. (1997). Cultural malpractice: The growing obsolescence of psychology with the changing U.S. population. *American Psychologist*, 52, 642–651.
- Jones, D. R., Harrell, J. P., Morris-Prather, C. E., Thomas, J., & Omowale, N. (1996). Affective and physiological responses to racism: The roles of Afrocentrism and mode of presentation. *Ethnicity and Disease*, 6, 109–122.
- Morgeson, F. P., Seligman, M. E. P., Sternberg, R., Taylor, S. E., & Manning, C. M. (1999). Lessons learned from a life in psychological science: Implications for young scientists. *American Psychologist*, 54, 106–116.
- Morris-Prather, C. E., Harrell, J. P., Collins, R., Jeffries Leonard, K. L., Boss, M., & Lee, J. W. (1996). Gender differences in mood and cardiovascular responses to socially stressful stimuli. *Ethnicity and Disease*, 6, 123–131.
- Sue, S. (1999). Science, ethnicity, and bias: Where have we gone wrong? *American Psychologist*, 54, 1070–1077.

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Historical Racism: Implications for Native Americans

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We are thankful for Clark, Anderson, Clark, and Williams's (October 1999) article outlining the role of racism as a stressor for African Americans. We agree that more research needs to address how intergroup and intragroup racism affects the biopsychosocial outcomes for African Americans. Furthermore, we believe that the model presented constitutes an effective starting point for this journey.

After reading this article, we were struck by the similarities that exist between this cultural group and Native Americans. Native Americans have historically been and currently are highly affected by intergroup racism, racism being the existence of "beliefs, attitudes, institutional arrangements, and acts that tend to denigrate individuals or groups because of phenotypic characteristics or ethnic group affiliation" (Clark et al., 1999, p. 805). One long-standing example of intergroup racism that continues to have pervasive effects is historical racism. It is our belief that historical racism has had and continues to have a profound impact on Native Americans. We outline below some possible ways in which historical racism constitutes a stressor with biopsychosocial implications for American Indians.

The concept of historical racism is an outgrowth of the fact that American Indian people have long experienced racism and oppression as a result of colonization and its accompanying genocidal practices (Brave Heart & DeBruyn, 1998). It is estimated that the population of Native American peoples was decreased to only 10% of its original number by the end of the 18th century (Sue & Sue, 1990). The massive loss of lives, land, and culture is believed to have resulted in a long legacy of chronic trauma and unresolved grief for Native Americans (Brave Heart & DeBruyn, 1998). Similar to the lasting effects of slavery on African Americans, the historical legacy of trauma and unresolved grief experienced by Native American peoples because of historical racist acts has become an unfortunate foundation of the American Indian experience. Also similar to the experiences of African Americans, this foundation has had tragic ramifications on the well-being of Native American peoples.

As stated elsewhere, "the trauma and intergenerational grief and despair associated with these experiences is still readily evidenced in most tribal cultures and is still taking a toll in many tragic ways" (Sommers-Flanagan & Sommers-Flanagan, 1999, p. 376).

In addition to the overt racism and discrimination experienced by Native Americans, many American Indians continue to encounter more subversive racial discrimination. Examples of institutionalized discriminatory practices abound. One illustration of this practice exists in the area of health care for American Indian peoples. Despite the fact that Native Americans are plagued by disproportionately high rates of suicide, homicide, accidental deaths, domestic violence, child abuse, alcoholism, and mental health problems (Brave Heart & DeBruyn, 1998; Indian Health Service, 1995), Native Americans are both an underserved and an underrepresented health care population.

The mental health services offered are particularly incongruous with the estimated need for services among American Indian people (U.S. Congress, Office of Technology Assessment, 1986). Indeed, mental health services are nearly unavailable to many American Indians. According to *Indian Adolescent Mental Health* (U.S. Congress, Office of Technology Assessment, 1990), only 1% to 2% of the Indian Health Service's budget was allocated to mental health services, and only 3% of the staff were mental health providers. Correspondingly, there are also few Native American mental health providers (U.S. Congress, Office of Technology Assessment, 1990).

Inadequate health care, along with the aforementioned high morbidity and mortality rates, is exacerbated and perhaps precipitated by the high poverty and unemployment rates that exist for Native Americans. Perhaps the best indicator of the current Native Americans health status is the fact that American Indians do not live as long as other U.S. populations. Heart disease, liver disease (cirrhosis), diabetes mellitus, and accidents constitute leading causes of death for this population (U. S. Congress, Office of Technology Assessment, 1990). The bleak current health status of Native Americans leads to the question, What stressors are contributing to the high rates of morbidity and mortality?

Clark and his colleagues (1999) have outlined the manner in which racism may constitute a stressor with negative biopsychosocial ramifications for African Ameri-

cans. Because of the inherent similarities between the experiences of racism of African Americans and Native Americans, we believe that such a model helps explain why Native American health is marked by high morbidity and mortality rates. By gaining a better understanding of the way in which racism as a stressor can negatively affect the biopsychosocial functioning of Native Americans, clinicians may be able to formulate more effective therapeutic and preventative tools.

Of course, perceived and historical racism can play an important role within the therapeutic setting for Native Americans. The role perceived and historical racism plays in the biopsychosocial functioning of Native Americans needs to be taken into account and elucidated.

In terms of historical racism, the atrocities and mistreatment, such as broken treaties and attempted genocide, have fostered in many American Indians a great deal of mistrust for both the government and many non-American-Indian people. Past exploitation is a frequently given reason for Native American people being suspicious. Because of the historical experience of racism, many American Indian clients "may perceive all non-Indians (including non-Indian counselors) as potentially racist and interfering until they prove themselves to be otherwise" (LaFromboise, Trimble, & Mohatt, 1990, p. 632). Trust is widely viewed as the key to therapeutic relationships, and this unfortunate legacy of mistrust could negatively affect the therapeutic relationship and the client's expectations for outcome.

We believe that it is the clinician's job to be aware of the effects of historical racism. Ignorance of this aspect of Native American reality inadvertently serves to promote historical racism in the therapeutic relationship. It is vital for clinicians intending to work with Native Americans to become culturally competent so as to minimize the effect of historical racism. Hillary Weaver (1997) has outlined three components of cultural competency for clinicians working with Native Americans. These components are (a) having knowledge of the client's cultural context, including history and worldview; (b) being aware of personal assumptions, values, and biases; and (c) using appropriate intervention strategies and skills. We believe that establishing cultural competencies is a crucial initial step that must be taken to defuse the presence of historical racism in the therapeutic relationship.

Once the clinician has become culturally competent, we believe that he or she will be well equipped to begin to unravel

the role of perceived racism and how it may act as a contributory stressor affecting biopsychosocial functioning of his or her Native American clients. We believe that the model set forth by Clark and his colleagues (1999) promises to be an effective way to begin this process.

REFERENCES

- Brave Heart, M. Y. H., & DeBruyn, L. M. (1998). The American Indian holocaust: Healing historical unresolved grief. *American Indian and Alaska Native Mental Health Research*, 8, 56-78.
- Clark, R., Anderson, N. B., Clark, V., & Williams, D. R. (1999). Racism as a stressor for African Americans: A biopsychosocial model. *American Psychologist*, 54, 805-816.
- Indian Health Service. (1995). *Trends in Indian health*. Washington, DC: U.S. Department of Health and Human Services.
- LaFromboise, T. D., Trimble, J. E., & Mohatt, G. V. (1990). Counseling intervention and American Indian tradition: An integrative approach. *Counseling Psychologist*, 18, 628-654.
- Sommers-Flanagan, R., & Sommers-Flanagan, J. (1999). *Clinical interviewing* (2nd ed.). New York: Wiley.
- Sue, D. W., & Sue, D. (1990). *Counseling the culturally different* (2nd ed.). Somerset, NJ: Wiley.
- U. S. Congress, Office of Technology Assessment. (1986). *Indian health care*. Springfield, VA: National Technical Information Service.
- U. S. Congress, Office of Technology Assessment. (1990). *Indian adolescent mental health*. Washington, DC: U.S. Government Printing Office.
- Weaver, H. (1997). The challenges of research in Native American communities: Incorporating principles of cultural competence. *Journal of Social Service Research*, 23, 32-39.

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