AGGRESSIVE BEHAVIOR
Volume 23, pages 59–68 (1997)

Book Reviews

THE ECOLOGY OF AGGRESSION, by A.P. Goldstein. New York, Plenum Publishers, 1994, 196 pp.

The Ecology of Aggression begins with the statement, "Every act of aggression is a person-environment event. The singular goal of this book is to provide supportive substance for this assertion." In the 200 pages that follow, I hold to and deliver on this interactionist, transactional perspective. At several levels of human functioning, the contributions of both social and physical ecological variables to the onset and maintenance of aggressive behavior is described and examined. In far too much of our research, theorizing and applied attempts at aggression reduction, aggression is still explicitly or implicitly conceptualized as in the perpetrator, by the perpetrator, and because of the perpetrator alone.

Tedeschi's dismissive review is both disappointing and puzzling. He claims the book makes "no attempt at reviewing a body of research," yet the book in its totality is an in-depth, critical examination of aggression-relevant ecological research—on site, neighborhood, and regional dimensions of the physical ecology of aggression (i.e., micro, meso, and macro-level variables) and, in parallel, on the victim, group, and larger collective dimensions of its social ecology. Both causation and intervention are considered at length through these several ecological lenses.

In two instances, he challenges the factual accuracy of conclusions I have drawn based on reviews of existing investigations. In both instances he is clearly incorrect, and reflects in his assertions a lack of awareness of bodies of work that have become available during the past dozen years, and/or a strong selective bias in his decisions about which findings to cite. Regarding the behavior impact of television violence, he states, " . . . There would be few researchers who would claim more than a small effect," citing Freedman's [1984] long outdated views as his evidence. Not only have by far the majority of investigators examining the same, older data as Freedman draw quite opposite conclusions [Friedrich-Cofer and Huston, 1986; Huesmann et al., 1991], but more relevantly, post-1984 studies and analyses [Donnerstein et al., 1994; Hoberman, 1990], as well as meta-analytic efforts [Comstock and Paik, 1994; Hearold, 1986], have each shown such effects to be substantial, and to include impacts on perpetrators, victims, bystanders, and others.

A major section of the book is devoted to examining research and theory on environmental design because it influences the likelihood of the occurrence of an aggressive act, and as it has been and might be employed to curtail such behavior. Its many operational implementations, supportive findings, and functional strengths and limitations are each explored. I conclude that this strategy rests on a strong and growing base of evidence [Brantingham and Brantingham, 1991; Clarke, 1992; Jeffery, 1977; Poyner

© 1997 Wiley-Liss, Inc.

and Webb, 1991]. Brushing such evidence aside, Tedeschi dismisses both its substantial existing accomplishments or its potential applications.

The reviewer further writes, regarding my analysis of violent crime as a function of location in the home: "I wonder how much we learn about aggression by knowing that more homicides occur in the bedroom than the living room, and that the bathroom is the safest place to be in the home. . . . Perhaps, as a practical precautionary measure, one should, to be safe, sleep in the bathtub." Such sarcastically offered trivializing is deeply insensitive to the realities of the ecology of aggression as it manifests itself in the real world of many of our urban children. As the hearings held by the American Psychological Association's Commission on Violence and Youth [Eron et al., 1994] as well as other interview and anecdotal sources have reported, a not insignificant number of such youngsters *do* sleep in bathtubs or other protected niches in the home, in order to be protected from being in the line of fire from stray bullets. Beyond such matters, and as I make quite explicit, such locational studies of violent behavior are a rich source of information and speculation regarding the instigating role of arousal, intimacy, sexuality, family dynamics, and more.

Having reviewed and critically examined the existing social and physical ecological literature bearing upon the causes and control of aggression optimally viewed as a person-environment duet, the book's final chapter seeks to point to additional domains relevant to aggression that might benefit from an ecologically oriented reworking of their underlying theories or constituent procedures. Topics considered in this manner are the prediction of aggression, audience effects, and the presence of weapons – three aggression domains of considerable social and theoretical significance, each in need of further clarification. Yet for Tedeschi, this chapter—its future directions purpose apparently not understood by him—is too "vague." Nothing is said to the review's readers about its actual purpose or contents.

These are but a few of a number of instances of an unfortunate "slam-bang" quality to Tedeschi's appraisal of my book. Offering little or no evidence, elaboration, or justification for his swipes, he pins on parts of it such labels as "somewhat misleading," "sketchy," "uncritical," and the like. This author, and I believe the readers of his review, would have been more fairly served and better informed by his review if he had devoted greater effort to reporting and objectively examining what is actually in the book, rather than to his exploitating of it as a dart board for his largely unspecified biases and unsupported musings.

Arnold P. Goldstein
Center for Research on Aggression
Syracuse University
Syracuse, New York

REFERENCES

Brantingham PJ, Brantingham PL (eds) (1991): "Environmental Criminology." Prospect Heights, IL: Waveland Press.

Clarke RV (1992): "Situational Crime Prevention: Successful Case Studies." New York: Harrow and Heston.

Comstock GA, Paik H (1994): The effects of television violence on antisocial behavior: A meta-analysis. Communication Research 21: 516–546.

Donnerstein E, Slaby RG, Eron LD (1994): The mass media and youth aggression. In Eron LD, Gentry JH, Schlegel P (eds): "Reason to Hope: A Psychosocial Perspective on Violence and Youth." Washington, DC: American Psychological Association: 219–250.

Eron LD, Gentry JH, Schlegel P (eds) (1994): "Reason to Hope: A Psychosocial Perspective on Violence and Youth." Washington, DC: American Psychological Association.

Freedman J (1984): Effects of television violence on aggression. Psychological Bulletin 96: 227–246.

Friedrich-Cofer L, Huston AC (1986): Television violence and aggression: The debate continues. Psychological Bulletin 100:364–371.

Hearold S (1986): A synthesis of 1043 effects on television on social behavior. In Comstock G (ed): "Public Communication and Behavior." San Diego: Academic Press.

Hoberman HM (1990): Study group report on the impact of television violence on adolescents. Journal of Adolescent Health Care 11:45–49.

Huesmann LR, Eron LD, Berkowitz L, Chaffee S (1991): The effects of television violence on aggression: A reply to a skeptic. In Suedfeld P, Tetlock PE (eds): "Psychology and Public Policy." New York: Hemisphere: 191–200.

Jeffery CR (1977): "Crime Prevention Through Environmental Design." Newbury Park, CA; Sage.

Poyner B, Webb B (1991): "Crime Free Housing." Oxford: Butterworth Architecture.

Effect Sizes, Prediction, Causal Mechanisms, and the Ecological Validity of Research on the Effects of Viewing Television Violence: Some Thoughts Stimulated by Goldstein, by James T. Tedeschi

In his rejoinder Dr. Goldstein reiterates several claims about what he has accomplished in his book on *The Ecology of Aggression.* He feels I did not fairly represent the content to the reader in my review of it. He characterizes my review as having a "slam-bang" quality, as making unsubstantiated "swipes," and as exploiting his book as a "dartboard for largely unspecified biases and unsupported musings." It might serve my own interests somewhat to engage in repartee with Dr. Goldstein regarding these charges, but it would be best for those who are interested to read his book and then my review and make their own judgments.

There is an allegation by Goldstein that I was incorrect about two factual conclusions in my review. However, I could only find one specific case cited in his rejoinder. I stated that few researchers would claim more than a small effect of viewing television violence on aggressive behavior. Goldstein claims that I must be unaware of important research on this topic or have a strong selective bias regarding what to cite, because according to him substantial effects have been found. What should be made clear at the outset is that one cannot conclude from effect size whether a relationship is causal or not. So, whether I am right or wrong in concluding that the effect of viewing violence on television is small is a separate issue from whether the relationship is causal.

It so happens that I have recently reviewed the relevant literature along with a co-author [Tedeschi and Felson, 1994, Chapter 4]. Our examination of the literature indicated reliable effects in laboratory experiments, but mixed results in field and longitudinal studies. The unreliability of nonlaboratory findings might indicate that there are contextual features in uncontrolled (open) settings which negate effects found in controlled

(closed) laboratory conditions. Or, it could be that there are special conditions introduced in the laboratory which make the interpretation of the findings in terms of aggression ambiguous [see Tedeschi and Quigley, 1996, for a full exposition of this point]. Nevertheless, Tedeschi and Felson [1994] concluded that "exposure to television violence probably does have a small effect on violent behavior" (p. 122). Eron [1992] drew the same conclusion in his testimony before a congressional committee when he stated that "the causal effect of television violence on aggression, even though it is not very large, exists" (p. S8539). The meta-analyses referred to by Goldstein also reveal small but significant effects [Comstock and Paik, 1994; Hearold, 1986; Wood et al., 1991]. It is possible that Goldstein makes a different judgment about the effect sizes than I do or that he wants to draw the inference that small research effects may translate into important social consequences and so we should not trivialize them. Certainly, this is a valid point and has been substantiated by Rosenthal [1986]. We could conclude, therefore, that we are both right. The research effect is small, and the social consequences could be substantial.

However, as indicated above, effect sizes do not settle the issue of causal inference. It should be noted that the conclusions of Tedeschi and Felson, and by Eron differ in two important ways. First, Tedeschi and Felson are uncertain and conclude only probably, while Eron is certain about the effects of viewing violence on television. Second, Tedeschi and Felson refer to an "effect" while Eron states that there is a causal relationship. What is meant by an effect is an association of one event with another, which may be indicated statistically by a correlation (or an effect size). The two events (A and B) may be causally related, or some third factor (C) might cause both. Freedman [1986] suggests that we must do research to rule out various possible third factors to establish a causal relationship between viewing violence on television and aggressive behavior. Another strategy would be to re-examine what we mean by "cause" and attempt to empirically establish it in future research.

Hume [1961, p. 18] defined cause in terms of invariant antecedent-consequent relationships. Thus, if anytime A occurs B subsequently occurs, it might be said that A is a cause of B. However, consider that day always follows night. No natural scientist would want to conclude that night causes the day. But the invariant association of the two suggests that there might be some mechanism by which night gets transformed into day. Of course we all know that the mechanism involves the positions of the sun and a rotating earth. According to Harre and Madden [1975], the specification of the mechanism is a description of a cause. While correlations provide us with a basis for prediction, such associations need to be explained in terms of causal mechanisms.

Simple effects research is often carried out in social psychology where the design is constructed around some intuition about a possible causal process, and based on assumptions about the process it is predicted that manipulations of a specific kind will produce specific outcomes. Unfortunately, without some indications of the mediating processes, such research is open to a large number of alternative interpretations. Finding reliable effects may be a first exploratory step that suggests there may be a causal relationship between events, but until evidence is provided about the mechanism by which the antecedent event brings about the consequent event, it is reasonable to remain skeptical that a causal relationship exists.

Among the causal mechanisms for the relationship between viewing violent television and aggression examined by Tedeschi and Felson [1994] were imitation of media

models, vicarious reinforcement and legitimations, cultural spillover, cognitive priming, desensitization, unrealistic fear, and routine activities. No convincing evidence has been provided for these or any other process that mediates between viewing violence on television and aggressive behavior. Thus it could be concluded that no causal relationship has been established for this relationship.

A problem in examining a large body of research that is not guided by scientific theory but instead by social values is that there is a tendency to keep a tally of hits and misses. Thus, meta-analyses essentially tell us that over *n* studies, which may vary somewhat in procedures, designs, measurements, and sampling populations, we get an average effect size and then a statement can be made about the likelihood that such an effect would occur by chance. For example, averaging across field studies might indicate a small significant effect. However, evaluating scientific research is not a baseball game in which we keep score in this way. Negative findings, particularly in studies that are methodologically sophisticated, like Milavsky et al. [1982], might be given more weight than the positive findings in a study with methodological shortcomings, such as by Eron et al. [1972]. Stronger weightings for negative findings fits into the view that theory choice in science is based on falsification [Popper, 1957, p. 19], and that confirmation of hypotheses based on evidence is not logically possible. While meta-analyses can be adjusted to account for various weights or quality of studies [Hunter and Schmidt, 1990], they have not yet been done with respect to the effects of television violence.

Finally, I tend to be very cautious about applications of what we "know" about social psychology. The motivations of social psychologists are often prosocial in nature and there is a desire to help resolve important social problems. But we all know where the road of good intentions leads. In the case of television violence there are conflicting values. On the one hand, there is a question of free expression and the First Amendment, and on the other hand, there is a concern about safety and the brutalization of women and children (not to speak of men). Those of us who develop knowledge in the closed system of scientific theories and laboratories should be very cautious of pontifications to lay people about how to make political decisions in open systems, particularly when no fully evaluated theory is available as a backdrop to justify practical recommendations. The generalization of scientific knowledge to open systems must be based on theories. Ecological validity refers to the application of theory and not simple generalization from research findings [Chow, 1987; Greenwood, 1982; Manicus and Secord, 1982]. A reliable effect of viewing television violence on the aggressiveness of viewers may help us predict that a viewer will be more aggressive, but it does not imply that removing the predictor will eliminate the effect. Removal would be effective only if viewing is a cause of the effect. While the felt urgency to contribute to reducing violence in society is meritorious, the rush to provide the aura of science without sufficient basis and without the humility associated with the limitations of science is likely to have unpredictable and perhaps lamentable results.

The best way to resolve arguments of the sort illustrated by the present case is to develop causal models regarding the effects of viewing violence on television and antisocial behavior of viewers. Skeptics can develop models showing that viewing violence and aggressive behavior are caused by some third variable, and those who believe there is a direct effect can propose and test causal models of how this effect is produced. Arguments about effect sizes are no substitute for having a good scientific

theory regarding the causal mechanisms that produce the effects. Furthermore, we should be cautious about making recommendations for public policy without a sound theoretical basis.

James T. Tedeschi, Professor
Department of Psychology
University of Albany, State University of New York
Albany, New York

REFERENCES

Chow SL (1987): Science, ecological validity, and experimentation. Journal for the Theory of Social Behavior 17:181–194.

Comstock GA, Paik H (1994): The effects of television violence on antisocial behavior: A meta-analysis. Communication Research 21:516–546.

Eron LD (1992): Testimony before the Senate committee on governmental affairs. Congressional Record 88:S8538–S8539.

Eron LD, Huesmann LR, Lefkowtiz MM, Walder LO (1972): Does television violence cause aggression? American Psychologist 27:253–263.

Freedman JL (1986): Television violence and aggression: A rejoinder. Psychological Bulletin 100:372–378.

Goldstein A (1994): "The Ecology of Aggression." New York: Plenum Publishers.

Greenwood JD (1982): On the relation between laboratory experiments and social behavior: Causal explanation and generalization. Journal for the Theory of Social Behavior 12:225–250.

Harre R, Madden EH (1975): "Causal Powers: A Theory of Natural Necessity." Oxford: Basil Blackwell.

Hearold A (1986): A synthesis of 1043 effects of television on social behavior. In Comstock G (ed): "Public Communication and Behavior. Vol. 1." San Diego, CA: Academic Press, pp 65–113.

Hume D (1961): "A Treatise of Human Nature." Garden City, NY: Doubleday.

Hunter JE, Schmidt FL (1990): "Methods of Meta-Analysis: Correcting Error and Bias in Research Findings." Newbury Park, CA: Sage.

Manicus PT, Secord PF (1983): Implications for psychology of the new philosophy of science. American Psychologist 38:399–413.

Milavsky JR, Stipp HH, Kessler RC, Rubens WS (1982): "Television and Aggression: A Panel Study." San Diego, CA: Academic Press.

Popper KR (1957): "The Logic of Scientific Discovery." London: Hutchinson.

Rosenthal R (1986): Media violence, antisocial behavior, and the social consequences of small effects. Journal of Social Issues 42:141–154.

Tedeschi JT (1996): Review of "The Ecology of Aggression" by Goldstein AP. Aggressive Behavior 22:227–232.

Tedeschi JT, Quigley BM (1996): Limitations of laboratory paradigms for studying aggression. Aggression and Violent Behavior: A Review Journal 1:163–177.

Tedeschi JT, Felson RB (1994): "Violence, Aggression, and Coercive Actions." Washing DC: American Psychological Association.

Wood W, Wong FY, Chachere JG (1991): Effects of media violence on viewers' aggression in unconstrained social interaction. Psychological Bulletin 109:371–383.

IMPULSIVITY AND AGGRESSION, edited by E. Hollander and D.J. Stein. New York, John Wiley and Sons, 1995, 372 pp.

This volume is divided into an Introduction and five major sections: Phenomenology, Neurobiology, Disorders of Impulse Control, Impulsive Aggressive Personality

Disorders, Related Disorders, and Treatment Strategies. Many of the 34 chapter authors are well known (e.g., Emil Coccaro, Robert Plutchik, Herman van Praag) and have been working in the fields of emotions or major mental disorders for many years. However, only four or five are women and this may have influenced the topics chosen and the analyses made. Overall, while much is to be learned from reading this volume, I found it to be variable in quality and to focus on a limited segment of the available literature. In addition, while most of the authors seem to be biomedically trained, they are listed simply as "Dr." I like to know about the training, discipline, and experience of authors with whom I am unfamiliar since scholars from different disciplines slice the behavioral dysfunction pie into different pieces. Knowing the discipline of the authors helps to evaluate what is written. On a positive note, although 25 of the chapters authors are working in the United States, the rest come from Denmark, Spain, the Netherlands, and South Africa. Thus, we do get a much appreciated world view on the topics covered.

Part 1, Phenomenology, consists of three chapters. The first, by Plutchik and van Praag, is about the definition and nature of impulsivity, and relations to aggression. It sets the pace for my reaction to the rest of the book. This is a volume written primarily (but not exclusively) about severe aggressive disorders, mostly by psychiatrists who likely work in inpatient settings. The book devotes little space to problems of lesser intensity such as the impulsive shopper, premarital and marital anger and aggression, verbal abuse of elders within intact families, etc. Throughout the book, much is made about relating impulsivity and aggression to that notorious DSM-III diagnosis—the borderline personality. Given the lack of interrater reliability of this diagnosis, I was often concerned about the validity of what I was reading. In addition, it seemed strange that some chapters referred to DSM-III while others referred to DSM-IV. I gather this book took some time to put together and the authors probably did not read each other's chapters (this, of course, is common in edited volumes).

Throughout the volume there is a great deal of semantic imprecision. In the Introduction, Hollander and Stein note that ". . . impulsivity and aggression contribute to major public health problems such as crime, violence, homicide, suicide, substance abuse, and sexual dyscontrol" (p. 1). Thus, they basically note that aggression contributes to aggression! On page 14, reference is made to a "recent review" by Polis that was published in 1981—15 years ago! On page 15, and in other sections, there is mention of aggressive "impulses" that, we are told, should be distinguished from "aggressive behavior (or violence)," and on page 18 mention is made about "higher levels of impulsivity." Well, there is really no way to measure an "impulse" and I would prefer that these scholars talk instead about the frequency, intensity, and duration of the behaviors under consideration.

Part of this first section reviews Plutchik's well-known psychoevolutionary theory of emotions. Some of it seems quite out of date, such as the studies quoted that measure "ego defenses." Again, while my behavioral learning is surely showing, I think no one has been able to measure anything reliably and validly about the ego! Some psychometric scales are presented in this and in later sections, with little or no indication given about their reliability or validity. Thus, I continually yearned for more hard data. However, the reader is introduced to the names of many tests and this may be useful to those who are looking for instruments for their practice or research.

I do not mean to focus on this chapter in particular. However, it set the stage for my

reaction to the rest of the book. Plutchik and van Praag, and many of the other chapter authors, fail to discuss some very important findings. There is little mention of reinforcement theory, modeling, sociocultural contexts for the acceptable display of impulsivity and aggression, anger as a motivator for aggression, etc. Although Plutchik sees aggressive behavior as part of our evolutionary past, Averill [1982, 1983, 1993] has suggested that anger (or what some call verbal aggression) exists simply because there are current payoffs (i.e., reinforcements). Others [e.g., Russell, 1991, 1994] have noted that in some cultures there is either no impulsivity or aggression, or that members of those societies tolerate it without defining it as a problem. Almost no mention is made about the important work of Berkowitz [1993], which I initially found to be astounding. But, then I realized the problem. The editors and chapter authors do not define with any degree of clarity what phenomena are under consideration of this volume. Spielberger et al. [1995] have repeatedly made it clear that the field cannot move forward until we have clear definitions of anger, aggression, violence, hostility, etc., and this volume would have been much improved with precise definitions of the phenomena under consideration.

The next two chapters address measurement and epidemiology. The measurement chapter was uninspiring. We are told that obsessive people are "anti-aggressive," that depression scales are recommended for measuring "inward aggression," and that Spielberger's work has demonstrated that aggression can be measured as a state or a trait. However, Spielberger et al. differentiate anger from aggression and their work has focused on the former, not the later. Finally, we are told (p. 36) that global scales of aggression have "high validity, but low reliability." Wow! I thought we taught undergraduates that reliability is a prerequisite for validity! In contrast, the chapter on "The Epidemiology of Aggression" was excellent. Clear definitions and data are given for homicide, physical assault, child abuse, domestic violence, etc. My only concern is that the author, Lee Robins from the Washington University School of Medicine, does not refer to the widely recognized and important data presented by O'Leary and Murphy [1992] when domestic violence is discussed. Finally, I wondered why these three chapters that discuss definitions of aggression (a behavior), measurement, and behavioral epidemiology are grouped together under the heading of Phenomenology?

Part II, Neurobiology, consist of three chapters that address findings from animal models of aggression and biological studies of impulsive and aggressive behavior. They are well written and discuss the role of septal brain lesions, the 5-HT system, and selected breeding studies, as they relate to impulsivity and aggression. As a psychologist, I learned much from reading them and would recommend them to my colleagues.

Part III consists of two chapters, one on impulse control disorders (ICDs) and a second on compulsive gambling. The first reviews the history, definition, and clinical description of problems such as pyromania, kleptomania, "buying mania," various paraphilias, compulsive gambling, etc. Associated psychopathology is discussed, as are family studies. Biological studies, we are told, have consistently found that people with ICDs have abnormalities in serotonergic neurotransmission. As it true throughout the volume, much more space is devoted to a discussion of why biological therapies might be effective, with less attention given to behavioral or psychosocial treatments. The authors conclude that ICDs have received relatively little study, remain poorly understood, and should be considered as a separate group of disorders. I was happy to see the next chapter which devotes exclusive attention to the problem of pathological gambling, given that many see it as the next dysfunctional behavioral epidemic likely

to emerge throughout the world. It is well written, with coverage of the clinical picture, prevalence and course, family studies, comorbidity, and pathogenesis. Brief reviews are presented of psychoanalytic, learning cognitive, and addictive theories with a discussion of whether gambling can be seen as an affective disorder, a disorder of impulse control, and/or a simple sensation-seeking behavior. Although I generally agreed with most of what was presented, some statements were perplexing. For example, the authors report that "What differentiates gambling from other operant behaviors . . . is the variable nature of reinforcement, without a fixed interval schedule" (Isn't this also true of fishing?), and they quote Custer [1982] as a source of this information. While Custer's descriptive stage model for the development of pathological gambling is well known, I do think B.F. Skinner had something to say about schedules of reinforcement! We are also told that gambling does not seem to be affected by negative consequences, with no data to back this statement up and no explanation presented as to why it might be true. In contrast, much more information is presented about dexamethasone response, noradrenergic function, serotonergic function, and platelet monoamine oxidase activity. A multifactorial behavioral treatment approach is recommended, with the conclusion that only a few isolated reports have shown pharmacological therapy to be beneficial.

The chapters in Part IV discuss the borderline personality disorder, and impulsivity and aggression in the antisocial personality disorder, while Part V presents information about a variety of "related disorders" including self-mutilation, sexual impulsivity, neurotransmitters and psychopharmacology of impulsive aggression in children, and organic mental disorders and impulsive aggression. My reaction was mixed. Statements such as "psychopathy is the *sine qua non* of predatory killers . . ." have a circularity to them, and I felt like I was in a time warp when I read that "reconstructive psychotherapy" [an old term promoted by Wolberg in 1954] has been abandoned because highly specialized therapists are required (p. 179). It also seemed pejorative to read that short-term or "superficial psychotherapy" has led to some changes in social skills and self-esteem although they are not effective in the long run. The fact is that little has been shown to be effective in the long run with these problems.

In contrast, the chapters on self-mutilation and sexual impulsivity are filled with interesting facts, clear definitions, and research findings. They present significant data on the efficacy of group psychotherapies, cognitive therapy, behavior therapy, and psychopharmacology. Likewise, the chapters on impulsive aggression in children and organic mental disorders are filled with new and useful information. However, only a single paragraph is devoted to a discussion of behavioral treatments which are concluded to be "highly effective" (p. 255) for treating patients with organic aggression and may be useful when combined with psychopharmacology. Again, I yearned for more data.

The final section consists of four chapters that discuss pharmacotherapy, preclinical/clinical studies of the role of 5-HT receptors, psychotherapy, and legal and ethical issues. They are generally informative, although borderline personality disorder is practically reified with (again) no discussion of the lack of reliability of this diagnosis. Markovitz, from Case Western Reserve, notes that if a dysfunction in the 5-HT system is involved in the behaviors under consideration, then they are likely to occur comorbidly, and discussing impulsivity and aggression as discrete behaviors may thus be misleading. This was the closest discussion of convergent-discriminant validity I found in the book, and it was refreshing. Mak and colleagues from the Netherlands conclude

that the efficacy of 5-HT agonists (particularly buspirone and eltoprazine) has not been satisfactorily proven. Stone, from Columbia University, concludes that we must think in terms of years of psychotherapy when working with impulsive and aggressive patients. I wonder how the managed care people would respond to that? Regrettably, while the pioneering work of Marsha Linehan is noted, there is little or no mention of the work of Gerald Patterson, Joseph Wolpe, Eva Feindler, Ray Novaco, Jerry Deffenbacher, Albert Ellis, Len Berkowitz, Rowell Heusmann, and others who have contributed so much to our understanding of aggression in children, adolescents, and adults. The final chapter presents legal definitions and case examples that will be of great help to those psychologists and psychiatrists who find themselves enmeshed in the legal system when dealing with aggressive patients.

Overall, this book has much to offer and as a psychologist who studies anger as a phenomenological, psychobiological construct [Kassinove, 1995], I learned a great deal. However, I often wanted more data, more behavioral science, and more discussion. Perhaps I would have been more satisfied with a 744-page book rather than one of 372 pages.

Howard Kassinove
Hofstra University
Hempstead, New York

REFERENCES

Averill JR (1982): "Anger and Aggression: An Essay on Emotion." New York: Springer/Verlag.

Averill JR (1983): Studies on anger and aggression: Implications for theories of emotion. American Psychologist 38:1145–1160.

Averill JR (1993): Illusions of anger. In Felson RB, Tedeschi JR (eds): "Aggression and Violence: Social Interactionist Perspectives." Washington, DC: American Psychological Association. pp. 171–192.

Berkowitz L (1993): "Aggression: Its Causes, Consequences, and Control." New York: McGraw-Hill.

Custer RL (1982): An overview of compulsive gambling. In Carone PA, Yolles SF, Kiefer SN (eds): "Addictive Behaviors Update: Alcoholism, Drug Abuse, Gambling." New York: Human Sciences Press.

Kassinove H (ed) (1995): "Anger Disorders: Definition, Diagnosis, and Treatment." Washington, DC: Taylor and Francis.

O'Leary KD, Murphy C (1992): Clinical issues in the assessment of spouse abuse. In Ammerman RT, Hersen M (eds): "Assessment of Family Violence." New York: Wiley. pp. 22–46.

Russell JA (1991): Culture and categorization of emotions. Psychological Bulletin 110:426–250.

Russell JA (1994): Is there universal recognition of emotion from facial expression? A review of the cross-cultural studies. Psychological Bulletin 115:102–141.

Spielberger CD, Reheiser EC, Sydeman SJ (1995): Measuring the experience, expression and control of anger. In Kassinove H (ed): "Anger Disorders: Definition, Diagnosis, and Treatment." Washington, DC: Taylor and Francis, pp 49–65.

Wolberg LR (1954): "The Technique of Psychotherapy." New York: Grune & Stratton.