CHAPTER 6

The Case against the Case against Media Violence

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Even before the introduction of television into everyday life over 50 years ago, the question of whether exposure to violence in the media made the viewer more violent was being debated. But it was the introduction of television into the average American home in the early 1950s that really stimulated an explosion of scientific research on the topic. In this chapter we are not going to review that large body of research in detail; other chapters in this volume do that and show that the accumulated research indicts media violence as a cause of viewers' aggressive behavior. Rather, in this chapter we are going to deal with the writings of those who argue that media violence has no effect on aggression. Specifically, we will (1) summarize some of the most common flaws in their arguments and criticisms, (2) respond in detail to the criticisms of several of the most vociferous "naysayers," and (3) try to explain the psychology of why these naysayers find it so difficult to accept conclusions regarding media violence that are supported by large amounts of evidence while they find it easy to accept conclusions about other threats to public health supported by less compelling evidence. However, to accomplish these goals we need to begin by briefly summarizing what the empirical evidence shows, how the integration of different methodologies leads to a particularly strong indictment of media violence, and what psychological processes explain the effects of media violence on aggression. Let us start with an explication of the psychological processes through which exposure to media violence has an effect on viewers' violent and aggressive behavior. Understanding these processes is the key to understanding what the body of empirical data really shows.

PROCESSES ACCOUNTING FOR EFFECTS OF MEDIA VIOLENCE

To begin with one must realize that different processes explain *short-term effects* and *long-term effects*. Short-term effects are due to (1) priming processes, (2) excitation processes, and (3) the immediate imitation of specific behaviors (Bushman & Huesmann, 2000; Huesmann, 1988, 1998).

Briefly, priming is the process through which spreading activation in the brain's neural network from the locus representing an external observed stimulus excites another brain node representing aggressive cognitions or behaviors (Berkowitz, 1993). These excited nodes then are more likely to influence behavior. The external stimulus can be inherently aggressive, for example, the sight of a gun (Berkowitz & LePage, 1967), or something neutral like a radio that has simply been nearby when a violent act was observed (Josephson, 1987). A provocation that follows a *priming* stimulus is more likely to stimulate aggression as a result of the priming. While this effect is short-lived, the primed script, schema, or belief may have been acquired long ago and may have been acquired in a completely different context.

To the extent that observed violence (real world or media) arouses the observer, aggressive behavior may also become more likely in the short run for two other possible reasons—excitation transfer (Zillmann, 1979, 1983a, 1983b) and general arousal (Berkowitz, 1993; Geen & O'Neal, 1969). First, a subsequent provocation may be perceived as more severe than it is because the emotional response stimulated by the observed violence is misattributed as being due to the provocation (Zillmann, 1979, 1983a). Such excitation transfer could account for a more intense aggressive response in the short run. Alternatively, the increased general arousal stimulated by the observed violence may simply reach such a peak that the ability of inhibiting mechanisms such as normative beliefs to restrain aggression is reduced (Berkowitz, 1993).

The third short-term process, imitation of specific aggressive behaviors, can be viewed as a special case of the more general long-term process of observational learning (Bandura, 1986; Huesmann, 1998). In recent years the evidence has accumulated that human and primate young have an innate tendency to imitate whomever they observe (Butterworth, 1999; Meltzoff & Moore, 2000; Rizzolati, Fadiga, Gallese, & Fogassi, 1996; Wyrwicka, 1996). These theories propose that very young children are likely to imitate almost any specific behaviors they see. Observation of specific aggressive behaviors around them increases the likelihood of children behaving exactly that way (Bandura, 1977; Bandura, Ross, & Ross, 1963). Granted, not all aggression is learned; in children two to four years old, proactive-instrumental behaviors that might be called aggressive (e.g., pushing another child without any provocation to get a desired object) appear spontaneously (Tremblay, 2000), as may hostile temper-tantrums. However, the observation of specific aggressive behaviors at that age leads to the acquisition of more coordinated aggressive scripts for social problem-solving and counteracts environmental forces aimed at conditioning the child out of aggression. As the child grows older, the social scripts acquired though observation of family, peers, community, and mass media become more complex, abstracted, and automatic in their invocation (Huesmann, 1988, 1998). Additionally, children's social-cognitive schema about the world around them begin to be elaborated. In particular, extensive observation of violence around them biases children's world schemas toward attributing hostility to others' actions (Dodge, 1985; Gerbner, Gross, Morgan, & Signorielli, 1994). Such attributions in turn increase the likelihood of children behaving aggressively (Dodge, 1980; Dodge, Pettit, Bates, & Valente, 1995). As children mature further, normative beliefs about what social behaviors are appropriate become crystallized, and begin to act as filters to limit inappropriate social behaviors (Huesmann & Guerra, 1997). Children's own behaviors influence the normative beliefs that develop, but so do the children's observations of the behaviors of those around them including those observed in the mass media (Guerra, Huesmann, Tolan, Van Acker, & Eron, 1995; Huesmann, Guerra, Zelli, & Miller, 1992; Huesmann, 2003). In summary, social-cognitive observational-learning theory postulates long-term effects of exposure to violence through the influence of exposure on the development of aggressive problem-solving scripts, hostile attributional biases, and normative beliefs approving of aggression.

Long-term effects are due to (1) observational learning of social scripts for behavior, of schemas about the world (e.g., is it hostile or benign), and of normative beliefs about the appropriateness of aggressive behavior; (2) emotional desensitization to violence; and (3) justification processes based on social comparisons (Bushman & Huesmann, 2000; Huesmann, 1988, 1998). There is some overlap with short-term effects; long-term effects are also quite likely increased by the habituation process called "desensitization." Most humans seem to have an innate negative emotional response to observing blood, gore, and violence. Increased heart rates, perspiration, and self-reports of discomfort often accompany such exposure (Cline, Croft, & Courrier, 1973; Moise-Titus, 1999). However, with repeated exposure to violence, this negative emotional response habituates, and the observer becomes "desensitized." One can then think about and plan proactive aggressive acts without experiencing negative affect. Consequently, proactive aggression becomes more likely.

One other long-term process is probably important. Social comparison theory suggests that humans evaluate themselves by comparing themselves to others. The aggressive child is generally (with some exceptions) not accepted because others do not like to be around aggressive peers (Anderson & Huesmann, in press). Huesmann (1988, 1995, 1998) has suggested that, to counter this threat to self-worth, aggressive children seek out aggressive media. Observing others behaving aggressively makes the aggressive children feel happier and more justified. Viewing media violence makes them feel happier because it convinces them that they are not alone in being aggressive. Of course, the ultimate consequence of such a turn, toward more exposure to violent media, is more observational learning of aggressive scripts, schemas, and beliefs, and more desensitization to violence.

INTEGRATION OF EMPIRICAL RESEARCH RELATING MEDIA VIOLENCE TO AGGRESSION

Once these processes are understood, the wealth of empirical evidence implicating exposure to media violence as a cause of aggressive behavior does not seem so surprising. However, to understand how compelling the evidence really is, one needs to integrate the evidence from all the different empirical approaches that have been employed.

The methodologies used in studying the relation between media violence and aggression fall into three major classes: (1) experiments in which the researcher manipulates exposure to media violence, (2) correlational studies, or one-shot observational studies in which exposure to violence and concurrent aggressive behavior are measured with surveys or observations, and (3) longitudinal observational studies in which exposure and behavior are measured on the same sample repeatedly over long periods of time. It is critical to integrate the findings of all three bodies of research in reaching any conclusion.

Generally, experiments have demonstrated consistently that exposing children to violent behavior on film and TV increases the likelihood that they will behave aggressively immediately afterward (see reviews by Bushman & Huesmann, 2000; Geen and Thomas, 1986; Paik & Comstock, 1994; Strasburger & Wilson, chapter 4, this volume). The typical paradigm is that randomly selected children who are shown either a violent or nonviolent short film are then observed as they play with each other or with objects. The consistent finding is that children who see the violent film behave more aggressively immediately afterward. They behave more aggressively toward persons (Bjorkqvist, 1985; Josephson, 1987) and toward inanimate objects (Bandura, 1977). The effects occur for all children—from preschool to adolescence, for boys and girls, for black and white, and for normally aggressive or normally nonaggressive. The average size of the immediate effect produced is about equivalent to a 0.4 correlation (Paik & Comstock, 1994). In these well-controlled laboratory studies there can be no doubt that it is the children's observation of the violence that is *causing* the changes in behavior. As described above, the psychological mechanisms operating are priming, excitation transfer, and simple imitation.

The question then becomes whether these causal effects observed in the laboratory generalize to the real world. Do they have real significance in the world? Do they extend over time? Does real media violence cause real aggression in the real world, not just in the short run but in the long run as well?

Empirical correlational studies of children and youth behaving and watching media in their natural environments have demonstrated that the answer to both of these questions is "yes." The great majority of competently done one-shot survey studies have shown that children who watch more media violence day in and day out behave more aggressively day in and day out (Paik & Comstock, 1994). The correlations obtained usually are between 0.15 and 0.30. Such correlations are not large by the standards of variance explained, but they are moderate by the standards of children's personality measurement, and they can have real social significance (Comstock & Scharrer, chapter 11, this volume; Rosenthal, 1986). In fact, as Rosenthal (1986) has pointed out, a correlation of 0.3 with aggression translates into a change in the odds of aggression from 50/50 to 65/35—not a trivial change when one is dealing with life-threatening behavior. Moreover, the relation is highly replicable even across researchers who disagree about the reasons (e.g., Huesmann, Lagerspetz, & Eron, 1984; Milavsky, Kessler, Stipp, & Rubens, 1982) and across countries (Huesmann & Eron, 1986).

Complementing these one-time survey studies are the longitudinal real-world studies that have shown correlations over time from childhood viewing of media violence to later adult aggressive behavior (Eron, Huesmann, Lefkowitz, & Walder, 1972; Milavsky, Kessler, Stipp, & Rubens, 1982; Huesmann, Moise, Podolski, & Eron, 2003; for reviews see Huesmann & Miller, 1994; Huesmann, Moise, & Podolski, 1997). Analysis of longitudinal data has also shown that early habitual exposure to media violence predicts increased aggressiveness beyond what would be predicted from early aggressiveness.

In conjunction with the theories described above, the results from these three kinds of research—experiments showing unambiguous causation, one-shot surveys showing real aggression correlates with concurrent habitual exposure to violent media, and longitudinal studies showing that childhood exposure predicts increased adult aggression independent of childhood aggression—should lead objective scientists to conclude that exposure to media violence increases a child's risk for behaving aggressively in both the short run and long run. So why is there still a body of public intellectuals who refuse to accept this conclusion?

THE PSYCHOLOGY OF DENIAL

To begin with, one must note that there is a clear consensus of opinion among scholars who actually do research on the topic that exposure to media violence causes aggression. Most major health professional groups have issued statements citing exposure to media violence as one cause of youth violence. Two Surgeon Generals of the United States (in 1972 and 2001) have warned the public that media violence is a risk factor for aggression. For example, in March 1972, then Surgeon General Jesse Steinfeld told Congress, it is clear to me that the causal relationship between [exposure to] televised violence and antisocial behavior is sufficient to warrant appropriate and immediate remedial action. . . . *there comes a time when the data are sufficient to justify action. That time has come.* (Steinfeld, 1972)

Surveys have consistently shown that over 80 percent of those doing research on the topic have concluded from the evidence that media violence is causing aggression (Murray, 1984).

So who are the vocal minority denying that there can be any effects? The best-known social scientists who deny there are any effects (e.g., Cumberbatch, Fowles, Freedman, Jenks) generally have never done any empirical research on the topic. However, they are glib and compelling writers, and their opinions cannot simply be dismissed. Furthermore, there is a large body of other intellectuals who deny that there are any effects. They range from the president of the Motion Pictures Producers Association (Jack Valenti) to the president of the Entertainment Software Association (Doug Lowenstein); from movie directors (e.g., Rob Reiner) to comic book producers (e.g., Gerard Jones); from science writers (e.g., Richard Rhodes) to booksellers (e.g., Chris Finan, president, American Booksellers Foundation).

Later in the paper we will deal with the individual criticisms of the most visible critics, but we first want to offer a psychological perspective on why well-intentioned, generally intelligent, and well-informed people can hold and promote attitudes on this topic that are so discrepant from what the majority of scientists, health care providers, and parents believe. We see three explanations for the discrepancy which are all grounded in psychological theory: (1) the need for cognitive consistency, (2) reactance against control, and (3) susceptibility to the "third person effect" of human behavior. However, all of these psychological processes depend on two underlying facts—one grounded in economics and one grounded in political principle, American history, and constitutional law. The economic fact is that violence in entertainment attracts audiences and makes large amounts of money for its purveyors (see Hamilton, 1998). The political principle is the sacredness of free expression in American society and law.

We propose that individuals involved in the production or marketing of violence will find it difficult to believe that viewing violence could be damaging to audiences because that belief would be cognitively inconsistent with their behaviors. Cognitive consistency is a remarkably powerful force that affects behaviors and beliefs (Abelson et al., 1968); so it would not be surprising that the behaviors shaped by subtle economic forces would shape beliefs. Only if the economic forces are blatant, or other beliefs relieve the inconsistency, can effects be admitted. For example, the director Oliver Stone says that "of course his [violent] movies [e.g., *Natural Born Killers*] are dangerous." His movies are intended to affect people, and that is one of the costs of free expression (BBC Panorama, 1997). Similarly, the cognitive consistency

process can lead to a denial of effects for those who believe strongly in free expression in the mass media. Many individuals with strong liberal beliefs about free expression in the mass media also have strong beliefs about society having a duty to protect children. If they accepted the fact that media violence harms children, they might have to rethink their beliefs about balancing freedom of expression and consider protecting children. It is easier for them to avoid this cognitive dissonance by denying that media violence has effects than it would be for them to resolve the dissonance. Furthermore, if the purveyors of violence accepted that violence has serious effects on children, they would have to categorize themselves with other purveyors of products that threaten health, for example, tobacco, which would produce even more dissonance.

The second psychological process we see as relevant applies only to the producers of violent media. Most humans at a young age develop an aversion to being controlled and respond to such attempts with reactance, or attempts to regain or increase their own control (Brehm & Brehm, 1981). We suggest that artists, writers, and producers are particularly susceptible to displaying such reactance when attempts are made to control their creative products, in which their egos are heavily involved. Artists often view as threats of control statements that their programs or films harm viewers. Suppose a researcher tells an artist that a program of theirs, which is a financial and critical success, is bad because it stimulates violence in the children watching it. The artist, rightly or wrongly, consciously or unconsciously, may interpret this statement as a threat of control. Therefore, a plausible response of the artist according to reactance theory would be to attack the researcher's thesis that the program has bad effects on the viewer.

The third psychological process we offer is intended more to explain a frequent opinion one hears from violence viewers who believe viewing violence cannot be bad. The opinion is that "media violence may affect some 'susceptible' people, but it will not affect 'me' or 'my children' because we are impervious to such influences." It is common in opinion surveys to find people reporting that a media message or personal communication might affect some people, but not the respondent. This phenomenon has been labeled the "third person effect" (Davison, 1983). Of course, as the research reported elsewhere shows, media violence can affect any child. The third person effect may be related to reactance theory. If viewers admit that they are being influenced by messages in the media, then they would have to admit they are being controlled to some extent by the media. Reactance would demand some action then. But if one denies that one is being controlled by the media, one does not need to act according to reactance theory.

We offer these three psychological processes only as suggestions that may help explain how many informed and intelligent people can read the reports of the studies done and still sincerely deny that media violence has serious effects on viewers. We certainly cannot offer any empirical evidence that these processes do operate among the noted critics. However, they are well-established psychological processes that are likely to operate among any of us. Of course, there are other plausible contributors to the dissents of specific critics. Some critics have invested a great deal in denying effects for a long time and have been paid by the violence purveyors to write dissenting books (e.g., Freedman). Obviously, it would be difficult for them to change. Others have alternative theories of violence that they are invested in and see media violence as a competitor (e.g., Rhodes). Probably, it is a combination of multiple processes that leads to the most vociferous dissents, which are obviously sincerely held.

Four Common Flaws in Critiques of Media Violence Research

Let us now turn to a discussion of a few of the most frequently repeated errors of reasoning that have been made by critics who challenge the conclusion that exposing children to media violence puts them more at risk to behave aggressively in the short run and in the long run.

1. *Assuming that the question is whether TV violence is the "only" cause of aggression, and arguing that TV violence can't matter because people who see the same TV shows differ in aggressiveness.* For example, critics say that "Detroit, Michigan and Windsor, Ontario see essentially the same TV shows. But the murder rate is much higher in Detroit. Consequently TV violence cannot be increasing serious aggression." This argument has been repeated over many years by intelligent people ranging from social scientists criticizing the research to politicians to network vice-presidents. Of course, this argument would only make sense if *nothing except TV violence* influenced murder—not guns, not poverty, not social support, not peer attitudes, not child rearing, not biological predispositions. The murder rate in Detroit has been higher for a lot of these reasons. It's puzzling how anyone could seriously offer this as an argument that TV violence has no effect. No reputable researcher of media violence has ever suggested that media violence is the only cause or even the most important cause of aggression. Serious aggressive behavior only occurs when there is a convergence of multiple predisposing and precipitating factors (Huesmann, 1998).

2. *Ignoring laboratory experiments.* It has been common among the naysayers (e.g., Freedman, 1984) to ignore completely the well-done laboratory experiments that have shown that exposure to violence stimulates aggressive behavior in the short run. Ignoring any of the different types of research (e.g., experiments, cross-sectional surveys, longitudinal studies) on media violence would be risky, but ignoring laboratory experiments is particularly inappropriate because it is the kind of study that most clearly tests causation. One typical rationale for ignoring laboratory experiments is the supposed artificiality of the aggression measures used in the laboratory, such as giving shocks to another person. The critics most often mention only the measures with the least face validity and never offer any empirical evidence that the lab measures are not valid indices of real-world aggression. The truth is that there is substantial empirical evidence that the measures of aggression used in laboratory studies are quite valid indices of how aggressively the person would behave outside the laboratory (Anderson & Bushman, 1997; Berkowitz & Donnerstein, 1982).

The other common complaint used against experiments without any justification offered is that "laboratory work suffers from strong experimenter demands" that bias the results in the direction of showing effects of media violence (Freedman, 1984). In fact, this criticism runs counter to the empirical evidence that suggests that participants in aggression experiments are likely to inhibit aggressive impulses because they fear being negatively evaluated by the experimenter (Turner & Simons, 1974). Finally, excluding laboratory experiments in favor of focusing only on field research reflects critics' misplaced confidence in such studies. While field studies may often (but not always—see Berkowitz & Donnerstein, 1982) have greater external validity, it is much harder to confirm the internal validity of the conclusions of field studies. And causation can never be tested as conclusively with field research as with a well-controlled laboratory experiment. The critics who ignore experiments conveniently overlook this fact.

3. *Selective reporting of negative results and changing criteria for accepting results.* Another common flaw in the critics' analyses of the research is their tendency to change the criteria for reporting a study or evaluating it depending on how the results came out. One study with positive results is discounted because of supposed demand characteristics on participants to behave aggressively, while a study with negative results is praised despite the fact that there were clear demands placed on the participants not to aggress. Another study with positive results is discounted because the stimulus films may have differed in attractiveness to the viewers, while a similar study with negative results is praised even though differential attractiveness of the films could have accounted for the negative effects. While meta-analyses that systematically combine all studies on a topic uniformly show positive and significant effect sizes for media violence on aggression (see Comstock & Scharrer, chapter 11, this volume), the reviews of the naysayers often convey the impression that most studies do not have positive effects simply because, for one flimsy reason or another, they exclude many studies with positive results and include every study with no results.

4. *Analyzing studies in a theoretical vacuum.* Perhaps the most egregious common error made by the naysayers is to evaluate the research on media violence as if it is completely disconnected from our existing knowledge about learning, social cognition, and aggression. We have outlined earlier in this chapter the established theory that explains how media violence influences aggression. The psychological processes that account for the effect were not invented to

account for the effect; they had been established independently. Given what we know about priming of social cognitions (Bargh, 1982), it would be incredibly surprising if media violence did not prime aggressive cognitions. Given what we know about arousal processes and excitation transfer (Zillmann, 1983a, 1983b), it would be startling if media violence did not produce such effects. Given what we know about the innate propensity of primates to imitate (Meltzoff & Moore, 1977, 2000) and the developmental course of observational learning in the real world (Bandura, 1977, 1986), it would be a shock if children did not acquire social scripts, world schemas, and normative beliefs from the mass media. Given the research on how hostile attributional bias (Dodge, 1980, 1985) and normative beliefs promoting aggression (Huesmann & Guerra, 1997) influence children's behavior, it would be surprising if such cognitions acquired from the mass media did not influence the children's behavior. And finally, given the established continuity of aggression from childhood to adulthood (Huesmann et al., 1984), it would be very surprising if the effects of media violence on children were not detectable when they were adults years later. Yet, the naysayers seem totally unaware of such psychological facts.

Given this background and overview of the most common errors in the critics' reasoning, let us now turn to a discussion of the specific views of several of the most prominent critics who challenge the conclusion that exposure to media violence increases the aggressive tendencies of the viewer.

The Freedman Chronicles

Over the past 20 years no critic has played a more prominent role in denying that media violence has any effect on behavior than psychologist Jonathan Freedman at the University of Toronto. Since 1984 (Freedman, 1984), he has written and published numerous articles disputing the fact that media violence has any significant effect on aggression and has culminated that work with a recent book (Freedman, 2002). Like most critics, he has never done any empirical research on the effects of media violence, and his critiques contain numerous examples of the four general flaws of thinking that we described above. Additionally, Freedman's objectivity, at least in his recent summative book, must be questioned as he was paid to write the book by the Motion Picture Association of America. Freedman accuses some of the scholars who have done empirical research on media violence of "basing their whole careers on showing that television violence is harmful" and therefore of being biased. In fact, a large amount of the important empirical work on media violence and aggression has been done by psychologists whose careers were devoted to *understanding social behavior or aggressive behavior* and who branched out to study media violence (e.g., Anderson, Bandura, Berkowitz, Bushman, Eron, Geen, Huesmann, Huston, Lagerspetz, Malamuth, and Parke, to name a few). Freedman, on the other hand, has based an entire 20-year career on trying to

show that media violence has no effect on aggression. Nevertheless, Freedman deserves some additional response because he is a social scientist and has attracted considerable attention. Because previous responses to his critiques (Huesmann, Eron, Berkowitz, & Chaffee, 1992; Huesmann & Moise, 1996) have focused on his early essays, we focus on his recent book here (2002).

The book is engagingly written, accessible to a nonscholarly audience, and presents glib arguments for not believing that media violence affects aggression based on some truths, many selective distortions and exaggerations, and a few outright untruths or misunderstandings. As always, Freedman points out some real flaws in research on the topic. Freedman also avoids some of the errors of his earlier reviews—he does now devote attention to laboratory experiments, for example. However, as before, his approach to the issue is a-theoretical, and he employs selective reporting and shifting criteria for evaluation of studies. Because he is an engaging and talented writer, probably sincere in his beliefs that there are no effects, and probably unaware of how selective his reporting is, he builds a bond with the reader. This allows him to shift from the third person to the first person and engage in "conversations" with the reader. When faced with a result inconsistent with his thesis that he cannot explain away, he relies on this bond to allow him to simply say things like, "It is a complicated study with very complicated results. I am confident that, overall, these results do not show that exposure to media violence increases aggression" (p. 29). Or in response to a published assertion he does not like, he may simply exclaim: "This is incredible," or "Scandalous," or "It is junk science." These are just some of the phrases Freedman uses to convey his feelings. His feelings are clear, but the justification for them is not made clear. What is worse, while he carefully qualifies every statement he makes concerning any kind of positive effect that might possibly be found between media violence viewing and aggression, he forgets about qualifying negative statements; as a result, he has a number of clearly false statements. To offer a few of his statements that are patently false unless qualified: "Virtually, no research shows that media violence desensitizes people to violence." "There is virtually no research on changes in attitudes due to viewing violence." "A majority of studies show no ill effects." "None of these reviews looked at hundreds of studies." (Paik and Comstock [1994] looked at 217, to give one counterexample.)

While his style contributes significantly to the dismissive message about media violence that he conveys, it is the more substantive review techniques he employs that really distort the truth. To begin with, Freedman completely dismisses meta-analysis as a viable review technique; for some time meta-analysis has been the accepted scientific mechanism for combining the results of many social science studies on a particular topic (see Comstock & Scharrer, chapter 11, this volume). In fact, in the early 1990s Paik and Comstock published an outstanding meta-analysis on this topic covering research through 1990. Essentially, Freedman ignores this analysis with the comment that "they do not provide any details for how they classified results; so it is not possible to comment" (p. 31). This statement in itself implies a lack of understanding of the nature of meta-analysis. In meta-analyses quantitative effect sizes are combined according to mathematical principles, and the outcome does not depend on an individual classifying any particular study as supportive or not supportive. The aggregate effect size speaks for itself. Freedman's rationale for dismissing existing meta-analyses are not only weak, but also self-serving, as Paik and Comstock's conclusions are quite at odds with Freedman's thesis. For example, Paik and Comstock report an overall effect size (as a correlation) of 0.37 for experiments and 0.19 for surveys (see Comstock & Scharrer, chapter 11, this volume, for details and an explanation of how to interpret how large these effect sizes are). Furthermore, while dismissing this fine meta-analysis of 217 studies without comment, Freedman spends some pages taking apart a less sophisticated meta-analysis of 23 studies by criticizing the author's conclusions about each study on the basis of nothing but his word that he agrees or disagrees. By dismissing meta-analyses and refusing to do one himself, Freedman greatly diminishes the value of his review. However, he could still have had an impact if he had presented consistent scholarly arguments in analyzing each study independently, but he did not.

Freedman's approach to analyzing a study is to mix together some quite appropriate methodological issues of concern with other unique value judgments of his own into a glib mix that might fool the lay reader but will not carry much weight with the informed reader. It is not that he is misleading the reader. He is quite straightforward. But the assertions he makes without supporting facts just do not hold up. Also, despite his statement that he attempted to overcome his own biases and review studies fairly, his biases remain readily apparent. He shifts his criteria for criticism depending on whether the study shows positive or negative effects. He emphasizes minor methodological errors and statistical errors in studies finding a positive effect but minimizes them in studies showing no effect. We can't go through every study he reviews, but let's consider two.

In his discussion of a study by Leyens, Camino, Parke, and Berkowitz (1975), Freedman dismisses the positive results, which fit perfectly with what theory would have predicted, by saying that "there are a number of serious problems . . . that make its result almost impossible to interpret" (p. 101). In this study Leyens reported that boys who watched a week of violent films in their cottages were observed behaving more physically aggressively afterward compared both to their behavior during a previous week and to boys who saw nonviolent films. What are the serious problems, according to Freedman? First, Freedman says, "it was a mistake" that the observers were instructed to code observed behaviors (hitting, etc.) without reference to what they imagined might be the intent of the act (p. 101). Freedman does not mention that this is the typical instruction used everywhere for behavior observations to reduce observer bias. No—instead he says that it is wrong because the boys

may be hitting each other without real intent to hurt the other person, and that should not count as aggression. But, of course, no one knows the boys' intentions except the boys, and, regardless of intent, if boys hit each other more after watching the films, one has demonstrated exactly the effect that observational learning theory predicts. Second, Freedman says that "the boys were more aroused by the violent movies than by the non-violent" (p. 102). This seems reasonable, though the authors collected data showing that this is not true (Leyens, Camino, Parke, & Berkowitz, 1975). If it is true, it simply suggests that the effect may be partially due to "excitation transfer" rather than observational learning. That is still an important result. Freedman would not see it as such, however, because he has taken a completely a-theoretical approach to the topic. He does not seem to realize that many researchers arguing publicly for concern about media violence believe that "excitation transfer" is an important component to the short-term effects of violence. Finally, Freedman says simply: "The statistical tests . . . are all inappropriate" (p. 101). Why? The children in each condition were divided between two cottages and cottages were the units assigned to condition. The analysis should have treated the subjects as nested within cottages but did not. Indeed this is a statistical error, but how serious? The key interaction is statistically significant at the 0.001 level in this study. It seems likely that, given that level of significance, the results would have held with the correct analysis. However, the point about all these criticisms is that Freedman ignores or minimizes similar flaws in a study when it suits his purpose.

As our second example, consider the Feshbach and Singer study (1971) that Freedman calls a landmark study and "the best field experiment on the issue" (p. 89). In this study boys in residential institutions were again randomly assigned to watch violent or nonviolent films. However, this time it was the boys who were assigned instead of the cottages. Freedman does not bother to discuss the serious potential methodological problems that such a within "home" design produces in terms of dependencies of each child's behaviors on what his peers are watching. Freedman does not discuss here the dependencies that this introduces in any statistical analysis of social behavior such as aggression. Freedman does recognize one major flaw in this study—that many boys were forbidden to watch the popular violent programs that other peers nearby were watching and such boys became angry. Thus, to most observers it is not surprising that this study showed more aggression among the boys who watched the nonviolent films. However, Freedman still sees this study as strong evidence of no effect for media violence. When in subsequent years even the senior author of this study has changed his interpretation of the results, one does not find Freedman's conclusion credible though, again, to a nonexpert the arguments may seem compelling.

The third major problem that reduces the scholarly value of the book is that it is just about completely a-theoretical. Freedman neither attends much to the psychological theory that has been advanced to explain why observation of violence engenders aggressive behavior nor does he attempt to place the research in any theoretical framework. As we outlined at the start of this essay, a substantial body of psychological theory has developed that explains social behavior in terms of priming effects, arousal effects, observational learning, and conditioning and that attends to the acquisition of such cognitive structures as hostile biases, mean world beliefs, and normative beliefs. Freedman ignores this theory. Of course, if he did not ignore it, he would need to explain certain contradictions. For example, how could he reconcile his conclusion of no effects of media violence on behavior with the accepted psychological laws that all primates have an innate tendency to imitate from a very early age? Does Freedman reject observational learning entirely? Or is his argument that children learn from observing those in the real world but not from observing those in the mass media? If so, why? Does it have to do with perceived realism? And how could one reconcile a finding of no effects on attitudes and beliefs with the theory of persuasive communication on which everything from commercials to public service announcements is based?

Freedman's overall approach in this book is to a great extent anecdotal. In other words, he attempts to prove assertions with a series of carefully chosen examples. The examples may be select studies; the examples may be select issues about studies; the examples may be select statements of others. He may indeed have read most of the studies on this topic that have been published in the English language, but he presents them with different emphases and different attention to detail, all designed to advance his thesis. This allows him to selectively attend to evidence that supports his view and ignore equally compelling evidence that does not. For example, in the introduction he presents three examples of youths' violent acts in which laymen jumped to the conclusion that the youth must be imitating a media presentation—for example, an English toddler killed by older boys shortly after a similar act was portrayed on TV; a boy who killed his sister with a fire shortly after a program about boys setting fires; and the martial arts fighting that seems to follow viewing *Power Rangers* on TV. Freedman is, of course, completely correct that such examples are terrible evidence to use in science. Yet, he uses the first two himself to argue against any media effect by showing that the perpetrators could not have seen the supposedly imitated show. Of course, few reputable scientists would have argued otherwise. On the other hand, he completely ignores the case of *Power Rangers* after just mentioning it, implying that any conclusion about it must also be false. If he had gone further, he would have had a much tougher time dismissing the proposition that *Power Rangers* indeed does increase aggressiveness (Boyatzis, Matillo, & Nesbitt, 1995).

Freedman once again offers the fallacious argument that if media violence is having an effect, all communities with TVs in all countries should be showing similar violence rates. When writing about the fact that crime increased in the United States at about the same time as the first generation of "TV-children" were reaching young adulthood, Freedman says, "Television was

also introduced to France, Germany, Italy, and Japan at around the same time as it came to the United States and Canada. . . . If television violence were causing the increase [in crime], surely it should have had the same effect elsewhere" (p. 7). There is no discussion here of how much violence children are being exposed to on the different television systems; there is no discussion of cultural differences in the other causes of aggression that we know must converge for aggression to become manifest; there is no discussion of cultural and social moderating factors for the effect. There is a listing of other social factors that Freedman asserts increase crime (without any critical discussion of the evidence), for example, the divorce rate doubling and the gap between rich and poor growing. He simply concludes, "These important social changes are certainly some of the causes of the increase in crime; television ownership may be irrelevant (p. 7)." Of course, this statement is almost certainly true, particularly with the word "may" in the sentence. What the statement illustrates is the different criteria that Freedman uses for evaluating media violence and for evaluating other possible causes of aggressive behavior.

What comes across clearly in all this is Freeman's anger. Now there is nothing wrong with an author being angry. Great essays and great exposés have often been motivated by anger. However, it is hard to understand the cause of his anger. While Freedman denigrates a lot of research and vociferously denies that media violence can be causing aggression, he does accept some of the most important empirical findings in the area and reaches some conclusions that are not very different from what the majority of researchers (whom he so harshly criticizes) have been saying. On page 46 he says, "the results of this review of the survey research seem to indicate . . . that exposure to or preference for media violence IS related to aggressiveness. The correlation is small, probably between .10 and .20. . . . It might conceivably be as large as .3." Now admittedly he uses many more qualifiers in his statements than most others, but this is exactly in the range concluded by others whom he criticizes, including Paik and Comstock (1994) as mentioned above. Similarly, in his discussion of the Leyens study, he says, "in general those shown the aggressive films became more aggressive immediately after the films were shown" (p. 100). And later on page 101, he says, "it is possible that the somewhat greater scores for aggression in the violent film group occurred because the boys were acting out what they had just seen in the movie." If he stepped back a little and looked at what he wrote, he perhaps would see that this is exactly what scholars and researchers in the area have been saying is happening.

In summary, we find, first, that buried in the negative rhetoric of Freedman's book is a grudging acceptance of some of the most important fundamental empirical facts about media violence viewing and aggression—that they are correlated and that exposure to media violence causes increases in aggressive behavior, at least in the short run. Second, we find that, while his writing is glib and compelling, the facts that back up his negative conclusions are lacking. He displays three of the general flaws of analysis we described earlier—lack of attention to psychological theory, selective presentation of results with shifting criteria for evaluation, and making implicit assumptions that scientists arguing for an effect believe that media violence is the only cause of aggression. At the same time he never tries to make the case that exposing children to media violence is beneficial.

Unfortunately, other critics of media violence research findings take a more radical position, arguing that media violence is not only harmless but also beneficial. Two such critics who have received attention recently are communications professor Jib Fowles and comic book producer and workshop organizer Gerard Jones.

JIB FOWLES AND *THE CASE FOR TELEVISION VIOLENCE*

Fowles's (1999) dismissal of the body of scientific research that supports a causal relationship between viewing media violence and real-world aggression rests on three premises. First, Fowles echoes Freedman's arguments about the quality and validity of such research. In fact, in the preface to his book Fowles justifies its writing by the recent emergence of "capable overviews of the empirical literature on television violence" that have "called the whole enterprise into question" (p. ix), a clear reference to Freedman's work. Having already addressed Freedman's arguments at some length, however, we will move on to Fowles's other, more unique arguments.

In addition to various methodological flaws, Fowles sees television violence research as flawed due to its political, public nature. After documenting rises and falls in public attention to media violence questions, he claims that television violence is a "whipping boy." Class, race, gender, ideology, and age conflicts, Fowles claims, are subverted and expressed as attacks on media violence. While Fowles never fully explains why this invalidates media violence research, the implication is that the findings of such research are a foregone conclusion due to political forces that motivate it.

In making this claim, Fowles makes a basic assumption, specifically that discourse about effects of televised violence determine scholarly attention to such effects as well as the conclusions that such attention will lead to. While public outcry may indeed spark academic interest in a topic, this assumption, in this case, seems to be fundamentally wrong. In an analysis that compared news reports about the effects of media violence on aggression to the cumulative findings of scientific studies of the same phenomenon, Bushman and Anderson (2001) found that while such effects have received progressively less news attention and have been described as less serious in that attention, research has continued to identify those effects. Scientific research on aggression effects of violent television has continued to provide consistent evidence of those effects in spite of declining support from the news media.

The balance of Fowles's attack on the conclusions of television violence research consists of a recitation of evidence, primarily historical and anthropological, that violence and violent entertainment did not begin with television. Fowles points out that interpersonal violence and warfare have been common among people belonging to what he refers to as primitive hunter-gatherer cultures such as the Bushmen of the Kalahari, American Plains Indians, Aborigines of Australia, and Inuit Eskimos. Violence, according to Fowles, is the natural state of human existence. Fowles also reminds us that cultures throughout history have had public displays of state-controlled violence, including the human sacrifices of the Aztecs and Incas, the gladiatorial games of ancient Rome, and the violent sporting contests that arose in nineteenth-century Europe and North America, and he presents media violence as their modern equivalent. This history of violence, both the spontaneous and personal and the staged and public, demonstrates a human violent streak that precedes the existence of television violence. People of all ages have needed an outlet for their violent impulses, and these needs are met quite nicely by violent television.

Fowles is making the first common error of the critics as we enumerated them above. He neglects to mention that *no* researcher studying the effects of violent television content pretends that television is the original, only, or even most important cause of human violence. In fact, most researchers emphasize the point that there are multiple causes for aggressive behavior, including biological predispositions (Gentile & Sesma, chapter 2, this volume; Huesmann, 1998; Huesmann & Miller, 1994; Huesmann, Moise, & Podolski, 1997; Paik & Comstock, 1994; Strasburger & Wilson, chapter 4, this volume). However, the existence of such factors certainly does not preclude the existence of environmental influences, including television content, on behavior.

Less tenable even than Fowles's criticism of the evidence demonstrating that television violence leads to an increase in aggression is the theory he proposes in its place. Fowles claims that television violence serves a prosocial function by providing an outlet for natural, inevitable violent impulses. Basically, according to Fowles, people who watch television violence do so because they need to. In support of this argument, he appeals to two different theoretical and empirical threads of research, each of which will be discussed here.

First, Fowles turns to the mood management literature developed by Dolf Zillmann and his colleagues. Mood management research has, among other things, documented that some people in some situations select media content that will help them return to a more comfortable or desirable mood state; in one study, for example, subjects who were made to feel bored subsequently selected mostly exciting programming (Zillmann, 1988). Under mood management theory, if one is uncomfortably aroused—whether stressed, angry, or frustrated—one should select calming media fare. If one is bored or lethargic, one should choose more exciting content, possibly that which contains violence. Doing so would increase generalized arousal.

Unfortunately for Fowles's argument for a fundamental need to view violence, however, mood management theory does not argue that viewing violence is good or even functional for everyone; in fact, any content that is arousing or exciting is sufficient to increase arousal, and violent content is unnecessary. Further, counter to Fowles's argument that viewing violence is largely an emotional release, the assumptions underlying mood management theory indicate that viewing violence may lead to increased aggression through a process of observational learning. One of the fundamental tenets of mood management, according to Zillmann (1988), is that it is based on learning, on a nonconscious level, the impact of various types of media content on one's own mood. In other words, after watching a variety of types of television content in a variety of mood states, the viewer learns that certain content makes her or him feel a certain way; thereafter, when she or he desires that feeling, she or he will seek out that type of content. Individuals who learn that violent content arouses them can be expected, when underaroused, to create violence themselves to reach a more desirable level of arousal.

More importantly, Fowles's explanation of violent television's place in mood management is flawed, as he believes that viewing the same violent content can lead to either increased or decreased levels of arousal depending largely on the immediate needs of the viewer. Apparently, one gets whatever one needs from violent television content.

Surprisingly, Fowles not only does not recognize these holes in his theorizing, he also does not seem to recognize the obvious contradiction between one view that media violence reduces aggression by arousing people, and another view advocating that watching violent television provides a catharsis effect, whereby the aggressive or stressed viewer is calmed. To support this catharsis hypothesis, he first cites research in which stressed men and aggressive teens were shown to select more violent television content than their less stressed or aggressive peers. To take this as evidence for a catharsis effect, of course, is not justified, since there is no indication that these stressed and angry folk were any less stressed or angry after viewing violence than they were before; it only demonstrates that stressed people are watching more violence. Claiming a causal relationship here seems unwarranted by Fowles's own standards for evaluating research, for, as he reminds us, "The great vexation about correlations is that they cannot in and of themselves specify causes" (p. 22).

A more important response to Fowles's claims for a catharsis effect, however, can be found in the aggregate body of scientific research into the phenomenon. Overall, research indicates that rehearsing or acting out aggression leads to an increase, rather than a decrease, in aggression. This is true when such rehearsal consists of watching violence (Doob & Wood, 1972) and when it is more actively carried out through physical behavior such as punching a

punching bag (Bushman, 2002). The research demonstrating the antithesis of a catharsis effect is also amply documented elsewhere (e.g., Strasburger & Wilson, chapter 4, this volume). Catharsis simply doesn't occur.

GERARD JONES AND KILLING MONSTERS: WHY CHILDREN NEED FANTASY, SUPERHEROES, AND MAKE-BELIEVE VIOLENCE

The absence of scientific support for a catharsis effect of media and fantasy violence, however, doesn't stop Gerard Jones from dedicating a book to its celebration (2002). Jones argues that children need violent media content in order to facilitate emotional and psychological self-regulation. He writes that children use whatever media content is appropriate to provide for whatever emotional or psychological needs they are experiencing at the moment—specifically, he claims that when children feel a need for nurturance, they may watch *Mister Rogers* or *Teletubbies;* if they need to feel strong and empowered, they will choose *Power Rangers* or *Pokémon* instead. Watching these shows and then emulating them in play, according to Jones, meets the child's needs.

In order to argue for this positive function of media violence, Jones first addresses—and dismisses—the scientific research on the effects of media violence. Like Fowles, Jones's first move is to echo and cite Freedman's arguments about the validity of laboratory experiments and survey research designs. Jones's next tactic is to quote a number of individuals he holds up as experts, each of which is made to seem to claim that media content does not make children violent. However, upon more careful consideration, these experts' statements do not contradict the conclusions of most media violence researchers. For instance, Stuart Fischoff is quoted as saying that "there is not a single research study which is even remotely predictive of [events like] the Columbine massacre" (p. 28). Jones interprets this to mean that there is no support for claims that media violence influences real violence, an interpretation that stretches far, far beyond the scope of Dr. Fischoff's words, which refer, not to all aggression, but to a horrific incidence of mass murder and suicide. Other psychiatrists are quoted as cautiously suggesting that for some violent or aggressive children, violent media may be seen as a tool for coping. None offers any evidence for this, at least none that Jones cites. Almost none of the supposed experts cited have ever done empirical research on media violence.

The third point in Jones's attack on media violence research strikes at the base of all social scientific research. Jones asserts that each individual child and each individual act of media use is unique, and that generalizations about them are inappropriate. As Jones puts it, "people rarely obey generalizations" (p. 8). Obviously, at one level of meaning for "generalization," this statement is true. But that is not the meaning of "generalization" that most scientists accept. For example, exactly the same argument could be offered about the generalization that smoking causes lung cancer. Without entering into a protracted discussion of the nature of truth and the social world, we can respond to Jones's criticism with a simple question: If generalizations about children's viewing patterns are inappropriate, why has Jones written a book in support of the generalization that media violence is good for children? Should we trust generalizations based on Jones's careful, one-sided collection of anecdotes or those based on hundreds of studies involving thousands of individuals according to the best practices of social science (Paik & Comstock, 1994)?

The sample that Jones draws on to support his thesis is a carefully selected one. For the most part, Jones recounts anecdotes about the children he has known best—himself as a child and his own son, as well as a few of his son's friends. He also spends a great deal of time recounting anecdotes shared with him by other parents in which they express relief after relenting to their children's demands for access to violent media and toys. Typical of these is the tale told by Emily's mother; Emily obsessed about guns for years, but her mother refused to buy her a toy gun or allow her to play with one. Finally, when Mom gave in and bought Emily a toy gun, Emily became much more well adjusted, much less gun-obsessed, and everything was wonderful. Similar stories are related by adults who reflect on how violent media content helped them to become healthy, functioning adults. Finally, the effects of continued restriction are presented when Jones describes the case of Kip Kinkel, the Oregon teenager who murdered his father before opening fire in a school cafeteria. Though Kip expressed an avid interest in guns from a very early age, his parents strictly forbade him from using violent toys or media until he was a teenager, when his father relented and bought him a real gun. If only his parents hadn't forbidden him access to violent television programming and toy guns, Jones muses, things would have been different; Kip clearly needed violent television.

Two flaws with this type of support are readily apparent. First, of course, is that since these anecdotes were selected for the sole purpose of supporting Jones's argument, they are biased and nonrepresentative at best. The second, possibly more damning flaw deals with the conclusions themselves, and is raised by Jones himself when he reminds us, in his discussion of research that demonstrates that media violence *does* cause aggression, that "a correlation is not a cause" (p. 30). Emily's change in mood, if it happened, may have been caused by any of a number of factors other than the opportunity to play with a toy gun. A likely alternative explanation is that when her mother relented to her demands, it marked an end to a long-standing conflict that had been a source of tension in the mother-daughter relationship. Rather than being swayed to do violence by a childhood deprivation of violent media content and toys, perhaps Kinkel was moved to violence by the implicit approval of violence his parents demonstrated as they bought him real guns. As for adults who remember violent media content as an important part of their maturation

process, how can they be sure that other elements of that media content, divorced from its violent elements, would not have been as effective?

Fortunately, we have ways of addressing questions like these. Through carefully designed laboratory research experiments, we can isolate individual elements of media content and measure its effects. Through carefully conducted survey studies, informed by developmental theory, we can identify patterns of relationships between specific considerations such as viewing violent television and mood in nonlaboratory settings. By adding parametric statistical techniques, we can even parse out the influence of other contributing factors. Finally, using longitudinal designs, we can see the effects of these factors across the life span. Of course, as is documented elsewhere in this book, such research has already been done, and when viewed collectively, it paints a picture far different than that presented by Jones's anecdote-based conclusions.

Not only is the supportive evidence Jones calls on suspect, but his overall model is flawed. First, it assumes that children are perfectly rational, active viewers of television content. Unable to conceive of an alternative, Jones, in reference to violent media content, raises the question, "If it's harmful, why do children love it so much?" (p. 144); the implication being that if it were harmful, it would be shunned. This is ridiculous; there is no shortage whatsoever of dangerous and harmful things that are dearly loved—junk food, tobacco, illicit drugs, and high-speed driving to name a few. One may as well ask, "If smoking is so harmful, why do people love it so much?" The answer is simple; the pleasure they derive from the immediate experience outweighs considerations of possible harm for the user. Does this make smoking safe or beneficial? Of course not.

The idea that children or even adults only watch television that is good for them is also contradicted by empirical evidence, which finds that viewing specific kinds of television content can lead to depression (Schweitzer, Zillmann, Weaver, & Luttrell, 1992), fear (Valkenburg, Cantor, & Peeters, 2000), and diminished sexual satisfaction (Baran, 1976), none of which seem to be particularly good outcomes.

Empirical evidence also belies the fundamental need for violent television content which Jones claims all children experience. Cantor and Nathanson (1997) report on a study in which parents were interviewed to reveal whether they restricted their children's television viewing, and whether violent shows in particular were forbidden. These parents were also asked to rate how interested their children were in various types of violent television fare. If Jones is correct and children have a fundamental need for violent television, then those children who are denied such content should be more interested in viewing it. In fact, parental restrictions on viewing violent programming did not predict interest in any kind of violent programming.

Finally, of course, we must recognize that the assumption underlying Jones's argument for violent television, specifically that play aggression is cathartic, is basically flawed. Physically acting out aggression on a neutral target simply does not decrease subsequent aggression (Bushman, 2002). This is true even for individuals who have been told (and believed) that aggressive action is effective at reducing stress and anger (Bushman, Baumeister, & Stack, 1999).

HOWITT AND CUMBERBATCH AND THE ENGLISH DISMISSAL OF EFFECTS RESEARCH

Another criticism of the conclusion that media aggression produces real-world aggression comes from England and is typified by the work of Dennis Howitt and Guy Cumberbatch (Howitt & Cumberbatch, 1975). Their analysis of research on the effects of violent media long ago led them to the conclusion that such effects, when observed, are probably spurious, and almost certainly irrelevant. To support this view, they rely on criticisms of specific studies as lacking in validity, apparent inconsistencies between the findings of various studies, and a criticism of the various mechanisms by which media effects have been proposed to operate.

These specific criticisms, however, are driven by an almost ideological belief that media do not have effects. Cumberbatch, writing in 1989 (Cumberbatch, 1989), asserts that since "evidence for direct influence is generally weak" and that such evidence, when found, consists of "trivial results" which are "controversial," "mass communication research has shifted from a search for effects to an attempt to understand how 'active viewing' operates" (p. 1). In short, according to Cumberbatch, media content has no effect whatsoever; this applies not only to violence, but also to sex roles, sexism, racism and racial stereotyping, ageism, sexuality, alcohol use, and prosocial behaviors and attitudes (Cumberbatch, 1989).

Howitt and Cumberbatch's emphasis on an active audience features several key components. First, according to Howitt and Cumberbatch, it means that media content is almost universally unproblematic. Cumberbatch (1989) argues that its nature as a market product assures that television content provides a reflection of audience values, beliefs, and concerns. It can't change attitudes or behavior because it is consistent with them already. The fact that people watch violent television content is held up as proof that audiences find it compatible with their beliefs and value systems (Howitt & Cumberbatch, 1975). The fact that most people report in surveys and polls that they find television excessively violent is dismissed as a social desirability effect (Howitt & Cumberbatch, 1975). An alternative explanation for this pair of findings (that people report that television is too violent yet watch it anyway), on the other hand, is that people aren't nearly as active in their television viewing as Howitt and Cumberbatch assume.

Regardless of content, however, the active television viewer will not be moved to violence by her or his viewing, according to Howitt and Cumberbatch (1975), in part because "we are taught that aggression . . . is wrong," and this teaching makes us "feel anxious at the prospect of acting aggressively" (p. 35). What the authors fail to include in this equation is that much of aggressive content on television does not teach that aggression is wrong. The question, then, becomes a simple one: can aggression be learned from the media?

In order to answer, Howitt and Cumberbatch limit their attention to a single, simplistic model of learning and another, equally simple model of attitude change. First, they hold up Bandura's early Bobo doll experiments, in which learning was conceived as direct imitation of specific acts, as examples of research on learning aggression. Howitt and Cumberbatch (1975) argue that due to the artificiality of the setting in which they occurred, the results are not generalizable to real children really watching television. Fortunately, scores of subsequent laboratory studies have used more realistic paradigms and shown the same effects (e.g., Bjorkqvist, 1985; Paik & Comstock, 1994). Second, they argue that for attitude change to result from viewing television, the attitude-relevant information must be deliberately attended to, and the attitude change would be specifically and narrowly limited to the content—they argue that images of spousal abuse could only have an impact on attitudes toward spousal abuse, and only if the viewer focuses and reflects on those images. Given that most significant findings don't fit these requirements, Howitt and Cumberbatch (1975) conclude there is no mechanism by which violence can be learned from television.

More recent theoretical developments, supported by and consistent with research findings, show such a conclusion to be untenable. Attitude change through "peripheral processing" is now accepted as a major mechanism for media influence (Petty & Cacioppo, 1986). In Bandura's (1994) more recent articulations of his theory, for example, he asserts that the social learning process can extend beyond mere imitation of specific behaviors to the creation of guidelines for the generation of future, innovative behaviors. Huesmann's (1998) model of social information processing provides for the learning or acquisition of scripts which are selectively applied across a variety of situations. The general aggression model articulated by Carnagey and Anderson (chapter 5, this volume) includes many sites where aggression might be learned, including normative beliefs and attitudes about aggression, perceptual schemata, expectation schemata, and behavior scripts. Clearly, learning from television can extend beyond direct imitation and narrow, highly specific attitude changes. Images of spousal abuse on television may be incorporated into a script for interpersonal conflict or one for interactions between men and women generally. They might lead to an expectation of compliance as a response to aggression. The images are certainly not limited in their effect to direct imitation or changing an attitude about spousal abuse specifically.

Finally, the notions that all media use is active and purposeful and that content that is not actively attended to cannot have an influence on viewers are simply unfounded. If children, for example, were already perfectly active television viewers, why would parental coviewing increase learning from educational television (Buerkel-Rothfuss & Buerkel, 2001)? Why would critical discussions with parents about television reduce the value-shifting effects of highly sexual television (Bryant & Rockwell, 1994)? Chaiken's (1980) heuristic-systematic processing model provides for effects even when audiences are unmotivated or unable to "actively" process information.

Ultimately, then, Howitt and Cumberbatch base their dismissal of a media violence effect on critiques and misinterpretations of old outmoded theories, an unjustified contempt for and myopic reading of research findings, and a dogged commitment to the unsupported, largely ideological belief in perfectly active audiences. Like their peers Freedman, Fowles, and Jones, they make their argument seem to hold water only through careful inattention and misrepresentation.

SUMMARY

If one does not want to believe a truth about human behavior, one can always focus on exceptions. There are flaws in some of the research on media violence, and some people do overstate the results. No single study is ever perfect, particularly in the social sciences. Some writers, including some of those we address in this chapter, have made a reputation for themselves by burying the body of studies indicting media violence under a pile of supposed flaws, and a myth has arisen that research on media violence does not present a compelling case that media violence stimulates aggression. We have tried to show in this chapter why that myth should remain a myth. We have tried to describe the psychology of the nonbelievers as far as we can understand it, and have tried to respond to their specific criticisms. Media violence is a contributor to aggressive behavior in the short run and, for children, at least, to aggressive behavior in the long run and even into adulthood. It is not the only factor accounting for individual differences in aggressiveness, nor even the most important factor. But as the mix of laboratory experiments, field experiments, cross-sectional survey studies, longitudinal survey studies, and meta-analyses show, it is a significant factor.