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*The Effects of Television Violence  
on Aggression: A Reply to a Skeptic*

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In his 1984 *Psychological Bulletin* article, "Effect of television violence on aggressiveness," Freedman reached several specific conclusions:

*It seems clear that . . . viewing violent material on television or film in the laboratory can increase aggressive responses in the laboratory. (p. 228)*

He concluded:

*Exposure to and preference for violent programming on television is correlated with aggressive behavior. (p. 243)*

However, he also concluded:

*There is little convincing evidence that viewing violence on television in natural settings causes an increase in subsequent aggressiveness. (p. 243)*

Finally, he qualified his conclusion by writing:

*My conclusion does not mean or imply that the causal hypothesis is incorrect. (p. 244)*

While these specific conclusions may seem judicious when examined in isolation, Freedman presented them in an overall context that was clearly intended to reject the causal hypothesis. Unfortunately, many who have read the article in the past six years missed the subtleties of the specific conclusions and retained only this general negative theme. The article has been cited frequently ( ) as showing that media violence does not stimulate aggressive behavior. Such a conclusion was not justified by the existing research at the time and is not justified now. In fact, it is at odds with the opinions of most developmental and social psychologists working in the field, who have come to believe that media violence increases the chance that individuals in the audience, adults as well as children, will subsequently behave aggressively themselves.

Contrary to Freedman's assertion on page 228, his article is not (and was not at the time) the only comprehensive review available. A number of equally thorough reviews of the literature had been published in the immediately preceding years (Chaffee, 1972; Comstock, 1980; Hearold, 1979; Huesmann, 1982; Lefkowitz &

In P. Suedfeld & P. E. Tetlock (1992). Psychology and Social Policy (pp. 191-200). New York: Hemisphere.

Huesmann, 1980). Since the publication of Freedman's article several other excellent reviews have been produced. The most recent are two meta-analyses, one by Comstock and Paik (in press) and one by Wood, Wong, and Chacheri (in press). Freedman's article is distinguished only by the fact that its conclusions were different from the others. Unfortunately, because of this characteristic it attracted sufficient attention that even six years later, a chapter such as the current one is necessary to set the record straight.

### NEGLECT OF LABORATORY RESEARCH

In his article Freedman agrees with most researchers that it has been proven beyond reasonable doubt that exposing children to media violence causes an increase in their aggressive behavior under the controlled laboratory conditions in which causation can be tested most convincingly. Freedman also agrees that in the real world, children who watch more violence on TV generally behave more aggressively. However, Freedman does not agree with most researchers on the implications to be drawn from longitudinal field studies. Unfortunately he then bases his general conclusions almost entirely on interpretations of this one subset of research. He simply disregards laboratory experiments, mentioning them only briefly. Such selective attention reveals a misplaced confidence in field studies, and although field studies may have greater external validity than laboratory studies and in some cases allow greater generalization of results, this is not always the case (Berkowitz & Donnerstein, 1982). Further, those who believe that a causal theory can be proved or disproved solely by field studies are bound to be disappointed. Yet this is just what Freedman has attempted to do by ignoring laboratory studies. A field study may suggest that one causal theory is more plausible than another, but it cannot prove a theory true or false. What is most impressive about the media violence research is the way in which the laboratory experiments, correlational single-wave field studies, and longitudinal developmental studies all complement each other in linking exposure to media violence with subsequent aggression. Freedman misses this convergence of evidence from different sources entirely by concentrating solely on field studies.

Freedman's reasons for rejecting laboratory evidence include the presumed artificiality of the laboratory experiments. First, noting that the aggression measures used in these studies are "only analogues of naturalistic aggression," he questions whether the findings obtained with these measures can be generalized to real-world aggression. However, Berkowitz and Donnerstein (1982) have surveyed the research bearing on just this matter and have shown that there is indeed an empirical basis for generalizing such laboratory results to the real world.

Freedman also asserts that the "laboratory work suffers from strong experimenter demands." That is, the subjects, on seeing the aggressive nature of the film shown to them, behave aggressively "because in a sense they have been told to or perhaps given permission by the experimenter." This criticism, however, runs counter to the empirical evidence. Turner and Simons (1974) have shown that participants in aggression experiments are much more likely to be affected by evaluation apprehension than by supposed demand characteristics. The "permission giving" posited by Freedman has never been demonstrated in aggression experiments. As a matter of fact, Turner and Simons (1974) note that if subjects suspect that the experimenter is

interested in their aggressive reactions, they are apt to suppress these responses. Apparently they believe they can make a better impression on the experimenter by acting cool, calm, and collected than they can by displaying open aggression. There is no evidence that subjects behave as if they believe the experimenter's interest in their aggression gives them permission to act aggressively. Still, experimenters in this field have always been aware of the possibility of demand effects, so from the earliest experiments on film violence they have taken great care to avoid obtrusive measurement (e.g., Bandura, Ross, & Ross, 1963).

### SELECTIVE REPORTING AND CHANGING CRITERIA

Freedman justified his exclusion of laboratory experiments by saying that he wanted to examine only the research conducted in natural settings. It is often claimed that investigations carried out in natural settings have an inherent advantage over laboratory experiments because their conditions are presumably more representative of the real world, and thus their findings supposedly can be better generalized to other natural situations. However, the degree of generalization depends more on similarity of meaning across different situations than on appearance. The situations established in laboratory studies can have much the same meaning as situations existing in many natural settings (Berkowitz & Donnerstein, 1982). Furthermore, even though in principle Freedman wrote that he favored highly representative research, he actually included some studies in his sample that were conducted in quite unusual situations. Examples include the experiments reported by Feshbach and Singer (1971); Parke, Berkowitz, Leyens, West, and Sebastian (1977); and Leyens, Parke, Camino, and Berkowitz (1975). Each of these studies involved boys incarcerated in special institutions for adolescents who have violated the law—subjects and settings that are far from representative of what most TV and movie viewers encounter in their homes and in theaters. The authorities in these reform institutions typically frown on any aggressive displays. Not only were the teenagers unlikely to think that movies shown to them gave them permission to assault each other, they were also apt to believe that they could be disciplined as troublemakers if they pushed, shoved, or fought with others around them. All this suggests that the aggressive actions recorded by the observers in the Parke and Leyens experiments occurred in spite of the restraining influences in the situation—certainly not because of any supposed "permission giving."

Freedman also disregarded at least one experiment that meets his major objections, a report of an experiment by Parke et al. on the second phase of their American research (1977). In this study, those who had been exposed to violent movies were verbally more aggressive to their partners than were the boys in the neutral movies conditions (regardless of whether they had been harassed). The chief difference between this study and others had to do with the relatively controlled nature of the interaction with the partner, a control that heightens the study's internal validity. In excluding the effects of past history (the other boy was a stranger) and by making sure that the boys' actions were not affected by other youngsters around them, the researchers could be more certain that the verbal aggression displayed by the subjects resulted from the movie treatment alone. This field experiment with positive results was ignored by Freedman, while he included less well controlled field experi-

ments that produced null or reverse results (e.g., Feshbach & Singer, 1971; Milgram & Shotland, 1973).

It is unwarranted to assume that every naturalistic study is more representative than any laboratory study. For example, Freedman refers to the experiment by Friedrich and Stein (1973) that deals with children in an unusual summer nursery school. Was this naturalistic enough that the findings from this investigation would be more generalizable to other populations and settings than, say, results from experiments conducted in university laboratories?

### Bias in Selective Reporting

Any survey of research must exclude some findings and focus on others. However, Freedman's review seems unduly slanted, often suggesting that positive results were more inconsistent than they were and negative results more clearcut. As one example, consider Freedman's comments about an investigation by Parke et al. (1977). Although the published article indicated a main effect for type of movie on the general aggression index, Freedman insisted there was no such effect (1984, p. 231). He correctly noted that the boys initially low in aggressiveness seemed to be somewhat more affected by the violent films than were those initially high in aggressiveness, but overlooked the finding (shown in Fig. 4 of Parke et al., 1977, p. 151) that both the highly aggressive and the less aggressive youngsters displayed more general aggression after the violent movies than after the neutral films.

There also seems to be bias in the way Freedman summarized the findings in Leyens et al.'s (1975) replication of the Parke et al. (1977) research. In reporting the adolescents' behavior during the evening (the time they saw the movies), Leyens and his colleagues noted that the residents of the two cottages shown the violent movies exhibited a significant increase in physical aggression from the baseline period to the week of movie viewing; the residents of the two cottages in the neutral film condition displayed virtually no physical aggression. Further, the posttests demonstrated that the boys in both aggressive movie cottages were physically more aggressive than their neutral movie counterparts during the movie week. Freedman, to the contrary, emphasizing what he said were differences between the cottages within a film category concluded, "One of the violent-film cottages showed a general increase in aggressiveness; the other violent-film cottage showed an increase only in physical aggression" (p. 230), implying some inconsistency. In other words, while most of the results of these experiments point in the same direction (with relatively minor variations), Freedman de-emphasized the converging findings, in part by failing to report some of the results, and instead emphasized the variations.

While some might regard Freedman's summary as showing that he is a hard-headed "naysayer," it is clear that Freedman was selective in his skepticism. He was rigorous in evaluating investigations reporting a connection between media violence and aggressive behavior, but remarkably gentle toward the few published studies that conclude there is no such influence. This is apparent in the way the anomalous field experiment by Feshbach and Singer is discussed (Freedman, 1984, pp. 229-230).

Critics of the Feshbach and Singer (1971) study have noted, among other things, that the nonviolent TV programs were significantly less enjoyable to the viewers than were the aggressive programs. The youngsters in the former condition clearly resented not being able to see their favorite programs. In fact, in several schools the boys in the nonviolent programs condition complained so much about not being able

to watch their favorite shows that the researchers relented and allowed them to see one popular violent program, *Batman*. Freedman acknowledged this as a reasonable criticism but thereafter ignored it entirely. What is important, he said, was that the boys who were limited to viewing the nonviolent programs were subsequently more aggressive than boys who were allowed to watch the violent programs in six of the seven replications involved in the study. However, the reasonable criticism cannot be ignored and does explain the aberrant results of this study parsimoniously as frustration effect: the youths in the nonaggressive TV diet condition were relatively frustrated in these replications, and predictably acted out as a result.

Because of this substantial threat to the internal validity of the Feshbach/Singer experiment, one really cannot draw any conclusions from the reported results. One should not even say, as Freedman did, that at least the difference between the violent and nonviolent TV diet conditions was in the wrong direction. The aggression scores may have been in the wrong direction because in this study the frustration-induced instigation to aggression was stronger than the aggression-enhancing effects of the violent programs.

It is also worth noting how Freedman treated essentially the same problem in his discussion of the Parke et al. (1977) experiments. In the first phase of their research, Parke and his associates found that the nonviolent movies were regarded as less interesting and less enjoyable than the violent films, and therefore they selected more interesting neutral movies for their second investigation. Freedman considered this a potentially serious problem. If this difference in the movies' interest value was a serious source of uncertainty in the Parke research, it must have been at least as serious a problem in the Feshbach/Singer experiment. But Freedman concluded instead that it was generous to say the Leyens et al. (1975) and Parke et al. (1977) studies had obtained some evidence for the notion that observed aggression could induce aggression. At the same time Freedman claimed that the highly aggressive boys in the Feshbach/Singer experiment had shown "a major decrease in aggression after viewing violent television." In fact, the boys in question were a control group whose viewing habits were left undisturbed by the investigators, and there is no before-after evidence of any decrease in their aggressive behavior.

Unfortunately, this is not the only case where Freedman's summary misrepresents the actual results obtained by investigators reporting a linkage between media depictions of violence and subsequent aggression. There are several important errors in his review of Phillips' (1983) study indicating that widely publicized prize fights tend to be followed by a rise in homicides under certain conditions. (See Phillips, 1985, for a further discussion of Freedman's errors in this regard.)

Another instance of Freedman's selectivity bias in favor of negative results is his inclusion of the Milgram and Shotland (1973) experiment testing the notion that the portrayal of stealing on TV would induce stealing in natural situations. This particular investigation has a number of serious methodological problems that were ignored by Freedman (see Liebert, Sprafkin, & Davidson, 1982, p. 115). For one thing, this experiment did not deal with aggression. Did Freedman bring in this study because he wanted to say there was no evidence that TV programs could induce any kind of illegal behavior? If so, he should have mentioned Phillips' studies of the contagion of mass media suicides as well as homicides (Phillips, 1974, 1979, 1982). These studies may also have flaws (Kessler & Stipp, 1984), but that does not distinguish them from many of the studies Freedman reviewed. Only the direction of the results

distinguishes them. The paper by Berkowitz and Macaulay (1971) reporting a mass media-induced contagion of criminal violence is also relevant but was ignored by Freedman.

Freedman also neglects the available data on real-world intervention studies that have attempted to reduce the effect of TV violence on aggressive behavior. Such planned interventions not only have social value if successful, but also serve as experiments to confirm or falsify the causal relation between TV violence and aggression. One such study was that by Huesmann, Eron, Klein, Brice and Fischer (1983). Using an attitude change procedure in a field experiment, they reduced both the aggressiveness of high violence viewers and the correlation between their violence viewing and their aggression. In a comparable control group the relation of violence viewing to aggression did not change, nor was the level of aggressive behavior diminished. The success of this intervention adds to the validity of the thesis that viewing violence stimulates aggression, yet Freedman did not mention the study.

Two other important articles published almost simultaneously with Freedman's were missed. Huesmann, Lagerspetz, and Eron (1984) presented cross-national data from Finland and the United States implicating media violence in the development of aggression. Freedman was aware of the study, as his comments on early partial reports reveal, but he never requested the complete report. Similarly, Freedman included a superficial, partial report of the Singer and Singer research but did not cite their readily available published complete results (Singer & Singer, 1980; Singer, Singer, & Rapaczynski, 1984). These papers contain several critical analyses that implicate TV violence as an instigator of aggression even when early aggressiveness is partialled out. Perhaps unaware of these results, Freedman concluded the opposite about the Singers' research.

### LACK OF DEVELOPMENTAL THEORY

Another major error Freedman makes is to ignore developmental theory. Freedman treats every study, whether its subjects were toddlers, children, teenagers, or adults, as equally important for examining the relation between media violence and aggression. Indeed, he seldom mentions the ages of subjects or survey respondents. But, within most models for the development of aggression, one would expect quite different effects at different ages. Freedman does recognize that a cumulative developmental process might be a plausible theory (even though he incorrectly asserts that no one has made this argument before), but he draws the wrong conclusions from such a theory. For example, he asserts that under a cumulative theory synchronous correlations between violence viewing and aggression must increase with age. This might happen, but it is not a necessary implication. As children mature, their viewing patterns change substantially; time spent with TV generally increases throughout childhood, then decreases through adolescence (Comstock et al., 1978). Viewing patterns are not very stable over time and their variability may change (Huesmann & Eron, 1986). It is perfectly plausible for heavy early violence viewing to have a cumulative effect in promoting later aggression, while the later aggression may not always correlate highly with the later violence viewing (Lefkowitz et al., 1977). However, adults are also vulnerable to the effects of violent displays on television. Transient effects of considerable magnitude have been demonstrated with adult sub-

jects. Although long-term learning may not be implicated in these studies with adults, other processes do contribute, for example, priming, response elaboration, and disinhibition (Berkowitz, 1984).

The development theory that Huesmann and Eron (Eron, 1982; Huesmann, 1982, 1986, 1988; Huesmann & Eron, 1984, 1986; Huesmann et al., 1983; Huesmann, Eron, Lefkowitz, & Walder, 1984) have hypothesized suggests that aggression, as a characteristic way of solving social problems, is learned at a young age and becomes more and more impervious to change as the child grows older. A substantial body of data support this view (e.g., Olweus, 1979; Huesmann, Eron, Lefkowitz, & Walder, 1984). Under this developmental model, one would expect exposure to media violence at a young age to be correlated with concurrent and later aggression; whether media violence viewing and aggression were correlated among older teenagers or adults would be irrelevant. Media violence affects aggression in adulthood by teaching young children lasting aggressive habits, not by changing adults' habits. Freedman has not approached the problem from a developmental perspective, and, therefore, he misses a number of such critical points. We note that even in Milavsky, Kessler, Stipp, and Ruben's (1982b) supposedly null data, the strength of the longitudinal effects from early violence viewing to later aggression is greatest for the longest lags.

### STATISTICAL MISEMPHASES

Freedman does not seem to appreciate recent advances in techniques for the analysis of longitudinal data. He emphasizes the results of cross-lagged correlational analyses, for example, while most researchers have agreed with Rogosa (1980) that this technique is suspect. Huesmann, Lagerspetz, and Eron (1984); Milavsky, Kessler, Stipp, and Rubens (1982a); and Singer, Singer, and Rapaczynski (1984) have turned to using a form of regression or path analysis in their recent research. One predicts as much of later aggression as possible from early aggression and then examines whether TV violence viewing adds significantly to the prediction. This is a very conservative approach, especially when dealing with a relatively stable characteristic such as aggression. Almost all the path coefficients obtained in this manner from early violence viewing to later aggression are positive in the recent Huesmann, Lagerspetz, and Eron (1984), Singer and Singer (1990), and Milavsky et al. (1982a, 1982b) studies. Most of Huesmann et al.'s and Singer and Singer's path coefficients are statistically significant. Still, Freedman treats these as weak results because the coefficients are not large relative to the coefficient for early aggression on later aggression. His mistake is to view the stability of aggression over time as a competing theory rather than as a contextual fact of human development and behavior. In terms of developmental theory, the results would be highly suspect if the coefficient from early viewing of TV violence to later aggression were higher than that from early aggression itself to later aggression.

Freedman, like many other skeptics, also falls into the trap of discounting correlations that seem small in absolute value, no matter how significant they are. Abelson (1985) and Rosenthal (1986) have both argued against this discounting especially when it comes to socially significant phenomena. Correlations that seem to explain relatively little variance may in fact indicate very socially significant effects.

## PREFERENCE FOR WEAK THEORIES

In his article Freedman reveals a limited conception of the potential universe of theories for explaining the relation between television violence viewing and aggression. His theories do not describe psychological processes but simply relations among variables. This neglect of process theory becomes a serious deficit when it leads to attempts to explain what must be a psychological phenomenon without any concern for psychological processes.

Freedman also finds imprecise, nonspecific theories plausible even if they are not falsifiable. For example, one reason why Freedman calls the evidence for TV violence causing aggression weak is the following conjecture:

*There is little difficulty in providing an alternative explanation of the relation between viewing television violence and aggressiveness: Those individuals who prefer violent television programs also tend to be aggressive. More simply, something in their personalities or behavior patterns, some predisposition, trait, combination of environmental pressures, learning history, or whatever, causes people to like aggressive programming and also to be aggressive. (Freedman, 1984)*

But this is not an explanation at all. What "something"? Many somethings have been tested empirically but have failed to explain the effect. What Freedman is really saying is, as long as one is not required to be precise, it is easy to speculate that there are alternative reasons for the correlation between TV violence and aggression.

Freedman sets an impossible criterion for studies he wants to reject, viz. that they "demonstrate a causal effect" (p. 228) or "definitive proof" (p. 242). This is a logically unreachable conclusion, according to Popper's (1959) falsificationist analysis. At the same time, Freedman sets no standards of evidence at all for his preferred hypothetical explanations, such as "something in their personalities or behavior patterns," which he concludes "receives especially strong support" (sic) because one study shows little time-lagged effect in a bivariate analysis. In short, results that are consistent with the effects hypothesis are never to be accepted as supportive because they are not conclusive in a positivist sense, while null findings are taken as strong evidence in favor of practically any other explanation one might invoke. His standard of evidence seems to be that of a criminal trial, with TV violence on trial; better let its crime go unpunished than to castigate it for a crime it might not have committed. This is a strange way to handle a review of a scientific literature.

There are at least two other errors in Freedman's article that, while not of critical importance, add to the generally negative tone. On page 236, Freedman includes a paragraph explaining the methodological danger of combining genders in correlating aggression with violence viewing. His implicit suggestion is that many important results are artifacts of such a procedure. Not only is this untrue, but this issue has in fact been addressed previously by the very researchers Freedman criticizes (Huesmann, Lagerspetz, & Eron, 1984). And Chaffee (1972), for example, found positive correlations in six of seven samples of boys and in five of six samples of girls. In the discussion of cross-lagged correlations in the Eron, Huesmann, Lefkowitz, and Walder (1972) 10-year study, Freedman is critical because of the cross-lagged pattern for TV violence viewing with self-reported measures of aggression (MMPI-F49 and Physical Aggression) were not presented while the lagged correlations with peer-nominated aggression were (Freedman, 1984, p. 241). He failed to note that self-report measures were not given to the children at age 8. Thus no cross-lags could possibly have been computed.

## SUMMARY

In his widely cited 1984 review article, Freedman concluded that while viewing of media violence was correlated with aggressive behavior, there was no compelling evidence that it stimulated aggressive behavior. In this chapter we have argued that Freedman's conclusion of no causal effect was not justified at that time and is not justified now. It was a result of his misunderstanding of some data, his dismissal of laboratory experiments as irrelevant, his selective disregard of data contrary to his view, his overreliance on atypical field experimental data, his fluctuating criteria for evaluating positive and negative evidence, and his failure to approach the problem from an adequate theoretical perspective.

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## *The Weapons Effect Revisited: The Effects of Firearms on Aggressive Behavior*

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### INTRODUCTION

Many popular accounts have suggested that the ready availability of firearms is a major factor contributing to criminal violence. This supposed effect has led some individuals to claim that sharp restrictions in gun ownership could reduce the availability of weapons for crime. Others have proposed that ownership of firearms is the result rather than the cause of violence. Still other accounts have argued that no direct relationship exists between ownership of firearms and violence. According to these latter two perspectives, efforts to restrict the availabilities of guns are not likely to reduce and might even increase the rates of criminal violence. The present chapter reviews scientific research that relates firearm availability to impulsive, aggressive behavior. We propose that the aggressive meanings attached to firearms are a major determinant of a weapon's influence on impulsive, aggressive responses. Policy recommendations to reduce criminal violence need to consider the aggressive meaning as well as the availability of firearms.

### FIREARMS AVAILABILITY AND CRIMINAL VIOLENCE

Compared to other modern industrialized countries, the United States has a high rate of private firearms ownership as well as firearms-related deaths. The total number of firearms deaths in the United States approached 30,000 in 1986. This number includes homicides (43%) and suicides (55%) as well as accidents (2%). In 1986, 7.5 out of 100,000 Americans killed themselves by firearms; this rate is the highest ratio in the world. There are approximately 400 deaths (Treanor & Bijlefeld, 1989) and more than 2,500 injuries each year to children who are under the age of 14 (Baker, O'Neill, & Karpf, 1984). Turner, Simons, Berkowitz, and Frodi (1977) estimated that 90% of all firearms deaths resulted from a gun that belonged to the victim's immediate social circle. This circle included the victim, a family member, a friend, or an acquaintance. We will focus on the high rates of ownership and death associated with firearms in the United States.

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