

THE UNIVERSITY OF MANITOBA

THE EFFECTS OF VIOLENT TELEVISION UPON CHILDREN'S AGGRESSION:
ELICITATION, DISINHIBITION, OR CATHARSIS?

BY

WENDY L. JOSEPHSON

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ABSTRACT

A review of the literature on general facilitative effects of televised violence on children's aggression revealed conflicting results and a pattern of overlapping methodological flaws. A field experiment was designed to test the predictions of three theories about television effects upon children's aggression (disinhibition, elicitation, and catharsis), while attempting to avoid the problems which typically threaten the conclusion validity of studies in this research area.

Subjects were 396 boys in grades two and three. Groups of six boys watched a 14 minute excerpt of either violent television action or equally exciting and popular nonviolent action. Half of the subjects were frustrated before watching TV, and half were frustrated afterwards. The subjects were then taken to the school gymnasium, where they were observed playing a 9 minute game of floor hockey. Two raters, blind to the subjects' condition, reported all aggressive actions into a taperecorder. Before the game began, half of the subjects were exposed to a cue which had been associated with violence in the violent television excerpt. The rest of the subjects were exposed to a neutral cue. When the game was over, subjects filled out a short questionnaire about their reality/fantasy orientation and identification during the television exposure. While subjects were out of the classroom, their teachers filled out a behavioural checklist about each boy, to assess his characteristic aggressiveness.

Effects of the television condition were significant only for boys whose teachers had rated them as being characteristically high in aggressiveness. These boys showed quite a dramatic elicitation effect, if they had been frustrated prior to TV viewing. A much more modest disinhibition effect was evident for this same subset of boys. The elicitation effect occurred in the first few minutes of the game, as predicted, and the disinhibition effect was significant only on the total aggression measure. The catharsis theory went unsupported. Contrary to prediction, reality orientation was associated with low levels of aggression in the game, as was identification with an aggressive hero among characteristically high-aggressive boys.

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THE EFFECTS OF VIOLENT TELEVISION UPON
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The more violence and aggression a youngster sees on television, regardless of his age, sex or social background, the more aggressive he is likely to be in his own attitudes and behavior. The effects are not limited to youngsters who are in some way abnormal, but rather were found for large numbers of perfectly normal American children.

This statement, attributed to Professor Robert Liebert of State University of New York, appeared in an article by Neil Hickey in the June 1975 issue of the TV Guide (p. 10). Hickey went on to state that Liebert's conclusion was based on an analysis of more than 50 studies, covering the behavior of 10,000 children between the ages of three and nineteen.

Hickey's presentation is similar to many articles which have appeared in the popular press since the U.S. Surgeon General declared that "the causal relationship between television violence and antisocial behavior is sufficient to warrant appropriate and immediate remedial action" (Steinfeld, 1972, quoted by Cline, 1974, pp. 177-178). (See for example, Anderson, 1976, in Chatelaine; Gottschalk, 1976, in Family Circle; Morgenstern, 1972, in Newsweek, and "What TV Does to Kids," 1977, in the Reader's Digest.)

The Surgeon General's conclusion, in 1972, was based upon two decades of research into the effects of TV violence. More studies have appeared in the scientific literature since 1972, albeit at a slower pace,

and Dr. Steinfeld's conclusion has frequently been supported in later reviews (Comstock, Chaffee, Katzman, McCombs & Roberts, 1978; Geen, 1978; Goranson, 1977; Howe, 1977; Huesmann, 1982; Lefkowitz & Huesmann, 1981; Liebert & Schwartzberg, 1977; Murray & Kippax, 1979; Rubinstein, 1981, 1982). Others, while taking a "sober second look" at the evidence and expressing some serious doubts about the literature, cautiously accept the Surgeon General's statement (Andison, 1977; Comstock, 1977, 1981; Wurtzel, 1977). However, several recent reviews of the literature by Halloran (1978), Howitt and Cumberbatch (1975), Kaplan (1982), Kaplan and Singer (1976), and Sohn (1981) have ended with the argument that the evidence at hand is insufficient to identify television violence as a significant contributor to children's aggression.

This last development is not such an abrupt turn-about as it might seem. Cater and Strickland (1975) have argued in their history of the Surgeon General's Inquiry that "press misinterpretations of the Report ... prompted the Surgeon General and members of his Scientific Advisory Committee to restate their conclusions more emphatically than they may have first intended" (p. 7). Robert Liebert, for example, wrote as part of a summing-up statement of the literature for the Surgeon General's Report:

At least under some circumstances, exposure to televised aggression can lead children to accept what they have seen as a partial guide for their own actions. As a result, the present entertainment offerings of the television medium may be contributing, in some measure, to the aggressive behavior of many normal children. Such an effect has now been shown in a wide variety of situations. (Liebert, 1972, pp. 29-30).

The substance of this earlier statement is virtually the same as the one made in 1975, quoted in Hickey's article. (That is what one would expect, since little new empirical evidence had been unearthed since

1972). What stands out is the tone: the cautious scientist of 1972 had become the strident advocator of 1975. This new advocacy of social scientists toward television violence is epitomized in a remark attributed to a researcher in this area. Comparing TV violence (unfavourably) to a snake, she advised: "Stomp on it, and then let's discuss it." (Goldsen, quoted by Steinbring, 1980, p. iii).

If the social scientists' conclusions were at first more cautious, it was with good reason. Research in this area had been intensely criticized for some time (e.g., Klapper, 1968; Lazarsfeld, 1955; Singer, 1971; Weiss, 1969). In fact, one critic proclaimed the entire literature "trash posturing as science" (Efron, 1975, p. 22).

Specific Imitation vs General Facilitation Effects

The major concern of the Surgeon General's advisory committee (Cisin, Coffin, Janis, Klapper, Mendelsohn, Omwake, Pinderhughes, Pool, Seigel, Wallace, Watson, & Weibe, 1972) and of Liebert and his colleagues (Liebert, Neale, & Davidson, 1973, p. 57) has been over the possibility that watching violent television makes children generally more likely to inflict harm on other people. It is important to distinguish general facilitation from specific imitative effects. In the latter, novel behaviours are learned which may or may not be performed in an interpersonal setting to inflict harm. One particularly horrifying example is the case of a young woman in Boston, Massachusetts, who was doused with gasoline and burnt to death two nights after a television program was shown that featured youths burning tramps alive in a similar manner. As Liebert and his colleagues (1973) have noted, however, there is widespread concern over TV violence not because of the (relatively infrequent) tragedies

which seem traceable to a specific show (although Bandura, 1973, has made an eloquent case for such concerns). A more frightening possibility is that the enormous carnage shown daily on television causes children to be, in general, more aggressive against many potential targets in a large number of situations.¹

The Validity of the Conclusion that TV Violence
Facilitates Children's Aggression

Table 1 lists the 37 sources this author has found in the scientific literature through September, 1982, which report investigations of the effects of media violence on children's nonimitative aggression. Any study which employed children under the age of 13 as subjects, used filmed stimuli, purported to be measuring nonimitative aggression (at least in part) and contained at least one condition in which children saw violent material and one in which they did not, is included in Table 1. Table 1, then, summarizes the empirical evidence upon which one could base the conclusion that television violence causes an increase or a decrease in children's general level of aggressive behavior. The validity of the conclusion naturally rests upon the quality of this evidence.

Cook and Campbell (1976) have identified four types of validity in testing causal relationships: internal validity, statistical conclusion validity, construct validity, and external validity. Cook and Campbell's excellent paper outlines many common "threats" to these four

¹Television violence has been accused of having numerous other undesirable effects on children, for example, desensitization to others' aggression (e.g., Thomas, Horton & Lippincott, 1977) and supporting a "mean world" philosophy (Gerbner, Gross, Eleey, Jackson-Beek, Jeffries-Fox, & Signorielli, 1977).

types of validity, providing a useful framework for organizing the numerous criticisms which have been levelled at research on television's effects on children's aggression.

I. Threats to Internal Validity

An internally valid study is one in which no plausible alternative explanation exists for the dependent variable effect other than the influence of the independent variable. Cook and Campbell have pointed out that the randomization which characterizes true experiments precludes most serious threats to internal validity. Since most studies in Table 1 are true experiments, there are relatively few with internal validity problems. Only five of the internal validity threats they outline seem applicable to this literature:

1. Correlational data: No causal inference possible. The most common threat to internal validity in this literature has been the use of purely correlational studies. Such studies can determine whether TV violence viewing and aggressive behaviour are reliably related to each other, but not whether one causes the other. Seven of the studies listed in Table 1 are subject to this limitation in at least one portion of their analysis (Dominick & Greenberg, 1972; Eron, 1963; Eron, 1982; Greenberg, 1975; Schramm, Lyle, & Parker, 1961; Singer & Singer, 1981; Stein, Friedrich, & Vondracek, 1972, second analysis). Of course, the limitation only affects conclusions about causal effects, not about "noneffects" (e.g., Furu, 1971), since reliable covariation of the two variables is necessary for a causal effect. It just is not sufficient.

Three of the seven studies listed have taken further measures to establish support for their conclusions about causality. Leonard Eron's

Table 1

Study	Reported effects on Non-imitative "Aggression"	Sample Characteristics	Setting	Critique of the Literature			Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
				Operationalization of TV violence	Operationalization of Nonimitative Aggression	Mediating Variables				
Bandura, Ross, & Ross, 1963 (a)	<u>positive</u>	Preschool boys and girls, aged 3-5, at Stanford Nursery School	experimental playroom attached to nursery school building	10-min. film of adult attacking Bobo doll; TV videotape of adult in animal costume doing same (vs. no film)	Pushing, kicking, etc. against a Bobo doll & other toys; "hostile verbalization" re: toys (20 min.)	All subjects frustrated after seeing film: shown neat toys but forbidden to play with them	<u>error rate problem:</u> 6 ANOVAs reported $\alpha_F < .351$ $\alpha_C > .05$ (although $< .10$)	1. <u>underrepresentation of aggression construct</u> 2. <u>surplus construct irrelevancies in TV violence construct:</u> Confounding of stimulus enhancement and "aggression" is possible. 3. <u>hypothesis guessing within experimental conditions:</u> possible "scuttlebutt effect" may have led to hypothesis formulation	<u>Generalizing across treatment construct</u> (i.e. to real TV programming)	
Bandura, Ross, & Ross, 1963 (b)	<u>none</u> (Boys more "aggressive" after rewarded aggression film than after no different non-aggressive film. Punished aggression for boys + all aggression films for girls showed no difference)	Preschool boys and girls, aged 3-5, at Stanford Nursery School	experimental playroom attached to nursery school building	5 min. videotape of adult attacking a Bobo doll and other toys, fighting with another adult over toys (vs vigorous individual and cooperative play by same characters with same toys vs no film)	Pushing, kicking, etc. against a Bobo doll and other toys (20 min.)		$[v_1 = 3, v_2 = 72$ $\phi = 2.24$ $(1-\beta) \geq .97$ for the main effect. $v_1=3, v_2=72$ $\phi^1 = 1.58$ $(1-\beta) \geq .76$ for the interaction]	1. <u>underrepresentation of aggression construct</u> 2. <u>hypothesis guessing within experimental conditions:</u> Possible "scuttlebutt effect" may have led to hypothesis formulation biasing results in a direction against the experimental hypothesis	<u>Generalizing across treatment construct</u>	

Study	Reported effects on Non-imitative "Aggression"	Sample Characteristics	Setting	Operational-ization of TV violence	Operational-ization of Nonimitative Aggression	Mediating Variables	Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
	from no-film group)								3. <u>confounding of levels of constructs and constructs</u> : 5 min. of attack on toys may have been insufficient to produce an otherwise measureable effect.	
Biblow, 1973	none (high fantasy ability Ss became equally less aggressive after TV viewing, regardless of the violence level of the content)	Middle-class grade 5 boys and girls (about aged 10) half high and half low in fantasy ability	school; run in small groups outside of normal classroom	Slides with sound track, especially for the experiment: "The Enemies" featuring physical + verbal fight between children (vs "Chitty Bang Bang" vs a set of math puzzles) -length unspecified	intentional delivery, during 10 min. free play, of harmful physical or verbal stimulus to another person or to personal property (human target got 1 extra point on a 0-4 scale)			[$v_1=2, v_2=54$, $\phi=2.24$ for the main effect (1-8)=.92 $\phi=1.58$ for the interaction of fantasy ability by TV condition (1-8)=.78] <u>reliability of measures</u> : interrater reliability not reported.		<u>Generalizing across treatment construct</u> (i.e. to real TV programming)
Cameron & Janky, 1971	<u>positive</u>	All kindergarteners with cooperating parents in a Michigan school district (73% had cooperative parents & usable data)	Home	All TV shows broadcast in the area which showed a person or person-like being hit, shove, strike, throw or shoot things at another person or person-like being; subjects' parents kept them on a	"behavior pathology" included parent reports of activeness, boldness and disturbed sleep as well as fighting and "acting aggressively"		<u>instrumentation</u> : parents were not blind to their children's experimental condition. However, retrospective reports of parents' expectations had low correspondence to reports of children's behavior changes.		<u>surplus construct irrel-evancies</u> in aggression construct and possibly in violent content construct (since excitement and content may have been confounded)	

Study	Reported effects on Non-imitative	Sample Characteristics	Setting	Operationalization of TV violence	Operationalization of Nonimitative Aggression	Mediating Variables	Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
					TV "diet" that contained only such programs (vs a diet that totally excluded such programs)					
Collins & Zimmerman, 1975	<u>positive</u> : on duration-of-hurt measure, if hero had some positive motives and consequences <u>none</u> : on frequency-of-hurt or response heirarchy measures for either violent show, or on duration-of-hurt, if the hero's consequences and motives were all bad	Second and sixth graders in two suburban public schools (ages 7-8, 11-13), both boys and girls	school; Fun in small groups outside of classrooms	Two 15 min. edited versions of a popular police-adventure show showing convergent or divergent motives and consequences for aggression (vs nature program about African wildlife)	1. frequency of hurt on help/hurt machine 2. duration of hurt on help/hurt machine 3. choice of aggressive solutions to hypothetical problems on Leifer & Roberts' response hierarchy.	presence of positive motives and consequences		<u>error rate problem</u> : 4 ANOVAS conducted; $\alpha_p \leq .20$	1. <u>underrepresentation</u> of response hierarchy measure 2. <u>surplus construct irrelevancies</u> : excitement/content confound 3(a) <u>hypothesis-guessing</u> -Interview re: "goodness/badness" of hero between film viewing and aggression measure may have sensitized subjects to experimental expectations, possibly contributed to by "scuttlebut effect" OR 3(b) <u>procedure x treatment interaction</u> - more reasonable sensitization result, given pattern of effects	<u>generalizing across effect construct</u>

Study	Reported effects on Non-imitative "Aggression"	Sample Characteristics	Setting	Operationalization of TV violence	Operationalization of Non-imitative Aggression	Mediating Variables	Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
Dominick & Greenberg, 1972	<p><u>positive</u>: on perceived effectiveness of aggression, for all subjects</p> <p>-for females on all measures except approval</p> <p>-also for middle class boys who reported that their parents didn't disapprove of violence</p> <p><u>none</u>: for lower class boys and for middle class boys who reported that their parents disapproved of violence</p>	Fourth, fifth and sixth grade boys and girls (ages approx. 10-12); varying levels of SES	In-class pencil + paper data gathering sessions	Self-reported watching of 20 programs judged by newspaper + magazine critics to contain violence	<p>1. Approval: 5 modified items from Sears Anti-social Aggression Scale;</p> <p>2. Willingness: 5 items from the Buss Durkee Hostility Inventory</p> <p>3. Perceived effectiveness: 5 newly constructed items</p> <p>4. Suggested solutions to conflict situations: 4 newly constructed open-ended questions. Items with solutions which could cause pain were given a score of 2, painless solutions a score of 1.</p>	gender, reported family attitudes, and social class	<p><u>correlational data</u>: no causal inference possible</p> <p><u>error rate problem</u>: 16 ANOVAs reported $\alpha_F < .67$</p>	<p>1. <u>underrepresentation of the construct of aggression</u></p> <p>2. <u>hypothesis guessing within experimental conditions</u>: procedure quite obvious in linking TV viewing, parents' approval, and subjects' aggressive attitudes in the set of questionnaires</p>		
Ellis & Sekyra, 1972	<u>positive</u>	First grade boys and girls (aged about 6) randomly selected from a Georgia school	School: more "relaxed" periods of the daily school routine	5 min. cartoon, of a football game which showed hitting, tackling, fighting, kicking, shouting, and shooting the referee (vs. animated musical variety show)	pushing, pulling, striking, kicking, etc. of classmates or objects; facial or body gestures at classmates, teachers, or observers; name calling + shouting (15 min. period)				<p><u>surplus construct irrelevant</u>: behavior toward inanimate objects and shouting both scored as aggression. Also, possible confounding of excitement and violent content.</p>	

Study	Reported Effects on Non-imitative "Aggression"	Sample Characteristics	Setting	Operationalization of TV violence	Operationalization of Nonimitative Aggression	Mediating Variables	Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
Eron, 1963	<u>positive</u> : for violent TV preference of boys <u>negative</u> : for mothers' report of hours of TV watched by boys <u>none</u> : for girls on either variable or for fathers' reports of boys' hours of TV watched.	Third graders (aged about 8) in rural and town schools in New York, both boys and girls	In-class paper-and-pencil data gathering sessions. Items referred to school and play situations	1. Number of children's parent-reported favourite TV programs (out of 3) which contained violence, as judged by raters familiar with the programs. 2. Mother's and fathers' reports of hours of TV watched	peer ratings of "Who pushes and shoves other children", etc.	gender	<u>correlational data</u> : causal inference reasonable but not certain	1. <u>error rate</u> problem: 8 ANOVAs, $\alpha_F \leq .36$ 2. <u>reliability of measures</u> : parents' reports of TV viewing amounts may have been unreliable	<u>surplus construct irrelevancies</u> : amt. of TV viewed and preference for violent TV both involve more than a high level of viewing; also, possible excitement and content confounding	
Eron, 1982 Correlational Study	<u>positive</u> : for 15 of 18 correlation coefficients <u>none</u> : for the other 3	Boys and girls in grades 1 through 5, from the U.S.A., Poland, Finland, and Australia	In-class paper-and-pencil data gathering sessions. Items referred to home, school and play situations	Violence ratings (as judged by two graduate students familiar with the programs) of 8 programs, each chosen as the most frequently watched on a list of 10 programs. The sum of the programs' violence ratings was calculated, first weighting each program according to the child's self report of how often he/she watched it, on a three point scale from "just once in awhile" to "every single time the program was on".	peer rating of "Who pushes and shoves other children", etc.	intelligence	<u>correlational data</u> : causal inference not certain	<u>error rate</u> problems: 18 significance tests were reported. $\alpha_F \leq .50$	<u>surplus construct irrelevancies</u> : possible confounding of excitement and content	

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Intervention Study	<p><u>positive:</u> among control groups <u>Ss</u></p> <p><u>none:</u> among subjects in the "attitude change" treatment condition</p>	American children identified in the correlational study as being heavy viewers of violent TV	same as for correlational study	same as for correlational study	same as for correlational study	<ol style="list-style-type: none"> writing and taping of anti-TV violence essay identification with aggressive TV characters belief in reality of TV violence 	<p><u>correlational data:</u> causal inference supported by results but still not assured</p>			
Feshbach, 1972 Expt. 1	<p><u>positive:</u> for riot-fantasy and "marginally" ($p < .10$) for war-reality films compared to circus control film</p> <p><u>none:</u> for any aggressive films compared to the no-film control, for war-fantasy and riot-reality compared to circus control</p> <p><u>negative or none:</u> although no signif. tests were reported, all aggressive film groups had lower aggression than the baseball control group</p>	Fifth & sixth graders (age 9-11) boys + girls, half middle class Caucasians + half low-income Blacks; all from a Los Angeles public school	School: S's taken individually from classrooms	6 min. excerpts from actual programing: -war movie, war news clipping, campus riot movie or news clipping (vs baseball game, circus action or no TV)	Intensity of noise administered to E for wrong answers in a colour-guessing game	Programs' individual differences	<ol style="list-style-type: none"> no significance tests reported for the negative effect <u>error rate problem:</u> 9 ANOVAs used to test TV treatment differences <p>$\alpha_F \leq .57$</p>	<ol style="list-style-type: none"> <u>surplus construct irrelevancies:</u> film condition differences may have been due to an excitement/content confound. <u>hypothesis guessing within experimental conditions:</u> administering a mood check list before and after film viewing may have sensitized subjects in some groups to experimental expectations, especially since there was opportunity for a "scuttlebutt effect" 	<u>Generalizing across effect constructs</u>	

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Expt. 2	<u>positive</u> : for Ss given a reality orientation <u>negative</u> : for Ss given a fantasy orientation	-same characteristics for Expt. 1 as for Expt. 1	-same as for Expt. 1	6 min. film composed of excerpts from the campus riot movie and news clip of Expt. 1 (Introduced by E as either a news report or as a Hollywood movie) vs no TV	-same as for Expt. 1	reality/fantasy orientation		<u>error rate problem</u> : 14 significance tests conducted. $\alpha_f \leq .331$	<u>hypothesis guessing</u> : same problem as for Expt. 1	<u>Generalizing across effect constructs</u>
Fouts, 1977	<u>none</u> : although 11 of 184 comparisons were significant, the author concluded that they would most appropriately be attributed to chance.	boys and girls aged 5-14, from Calgary, Alta.; recruited through schools and by means of newspaper ads.	group and individual viewing in a laboratory, followed by individual interviews	30 or 60 min. uncut episodes of "Adam 12", "Starsky and Hutch", "SWAT", "Streets of San Francisco", "Six Million Dollar Man", and "Bionic Woman" (vs "Emergency", "The Waltons", "Little House on the Prairie", "The Beach Combers", "All in the Family", "Excuse My French", "Laverne and Shirley", "Happy Days", "Bugs Bunny", "Road Runner"; and "The Flintstones")	self reports of whether child had hurt someone in the past week, interest in guns and martial arts, dangerousness of Calgary as a place to live, questions about what child would do in hypothetical conflict situations, attitudes toward aggression	<u>spurious equivalence of treatment and control</u>	1. <u>reliability of measures</u> : not reported 2. <u>random irrelevancies in experimental situation</u> : viewing conditions and prior exposure to interviewer varied from <u>S</u> to <u>S</u>	<u>underrepresentation of construct of aggression</u>		

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Furu, 1971 (also included an adolescent sample not discussed here)	none: anti-social aggression was unrelated to television viewing	4th grade boys and girls from Tokyo and nearby rural areas	Schools; paper-and-pencil data gathering sessions in class	level of TV viewing (above or below median number of programs watched for 4-day period) (a better measure, number of programs having aggressive hero action, was available for the adolescent sample, but no results on this measure were reported for 4th graders).	6 anti-social aggression items from Sears Aggression Scale			<u>reliability of measure not reported</u>	1. <u>construct underrepresentation of aggression measure</u> 2. <u>surplus construct irrelevancies of TV violence construct (amount of viewing measures more than violence exposure)</u>	

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Granzberg, 1982	<p><u>positive</u>: on retrospective parent and community member report measure for all communities studied</p> <p><u>positive</u>: on verbal prediction of own retaliation toward aggressor, among high exposure Ss in the less traditional community</p> <p><u>negative</u>: on verbal prediction measure, among low exposure Ss in the less traditional community</p> <p><u>none</u>: in the more traditional community on all measures except retrospective report</p>	<p><u>Retrospective reports measure</u>: All children and adolescents in three northern Canadian native Indian communities</p> <p><u>Verbal prediction measure</u>: All boys in grades 3-5 in the two more northerly communities studied.</p>	Interviews conducted in the home and school about all life settings	Access to television transmission (Method of determining high or low exposure not specified).	<p><u>Retrospective reports measure</u>: number of eyes lost in fights since TV became available (no comparison with pre-TV period available), subjective impressions of more fighting at home and at school, lists of physically damaging and deliberately thwarting acts</p> <p>"since TV" (no pre-TV comparisons available)</p> <p><u>Verbal prediction measure</u>: number of retaliatory responses given by child in response to 7 interview questions about what he would do if someone aggressed against him.</p>	<p>1. cultural traditionalism</p> <p>2. degree of exposure</p>	<p>1. <u>instrumentation</u> of the retrospective reports</p> <p>2. <u>local history</u></p>	<p>1. no numerical comparisons reported for retrospective reports measure</p> <p>2. <u>error rate problem</u> on self prediction measure</p> <p>$\alpha_c \leq .10$</p> <p>$\alpha_p \leq .40$</p>	<p>1. <u>construct underrepresentation</u> of self-prediction measure</p> <p>2. <u>surplus construct irrelevances</u> of TV violence construct -exposure includes much besides violence, confounds content and excitement</p>	

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Greenberg, 1975 (also included an adolescent sample not discussed here)	<p><u>positive:</u> on both measures for 12 year olds</p> <p><u>none:</u> on either measure for 9 year olds</p>	British (London) boys and girls about 2/3 working class; 1/3 middle class; ages 9 and 12	In-class paper-and-pencil data gathering situation	Number of programs regularly watched in which violent acts were common"	<p>1. Effectiveness of aggression scale from Dominick & Greenberg, 1972</p> <p>2. Willingness-to aggress scale from Dominick & Greenberg, 1972</p>		<p><u>correlational data:</u> no causal inference possible</p>	<p><u>reliability of measures:</u> used short (4 items each) indices of unknown reliability</p>	<p>1. <u>underrepresentation</u> of aggression construct</p> <p>2. <u>surplus construct irrelevancies:</u> Arousal and content may well have been confounded (use of television for arousal correlated .29 with "aggressive attitudes")</p> <p>3. <u>hypothesis-guessing within experimental conditions:</u> same obvious procedure as Dominick & Greenberg, 1972</p>	

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Hanratty, Liebert, Morris & Fernandez, 1969	<u>none</u>	4+5 year-old boys in a Nashville kindergarten	experimental trailer set up exactly like film situation	especially produced 2½ min. film of a 9 year old boy beating and shooting toy gun at an adult female dressed as a clown (vs no film)	Novel responses (including hitting, kicking, etc.) aimed at toy or human clowns in a 5 min. post-film play period			<u>statistical power</u> $V_1 = 1, V_2 = 18$ $\phi = 1.59$ $(1-\beta) \geq .57$	1. <u>surplus construct irrelevancies</u> : behavior directed at toy and human clowns both included in "aggression" score 2. <u>confounding levels of constructs and constructs</u> : 2½ minutes of child hitting toy may be insufficient to produce an effect that would appear at higher levels of TV violence 3. <u>generalizing across time</u> : effect may have appeared after the 5 min. during which observations took place, once S had adjusted to novel situation 4. <u>hypothesis guessing</u> : identical setting may have "demanded" imitation, excluding the possibility of any nonimitative behavior, including aggression	1. <u>setting x treatment interaction</u> : inhibition might have occurred in a setting other than one identical to the film, which might demand imitation 2. <u>generalizing across treatment construct</u> (i.e. to more typical programming)

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Hanratty O'Neal, & Sulzer, 1972	none (non-imitative "aggression" almost never occurred) regardless of whether or not they had been frustrated	First grade boys in a New Orleans parochial school (ages 6 + 7)	school: room set up like film situation	specially produced film of a man shaking his fist at, making hostile comments to, shooting with a toy gun and hitting (with a toy hammer) an adult female dressed as a clown (2½ min.)	"any aggressive acts not exhibited by model in the film" - only random shooting with a toy gun, not at the clown, was actually scored			<u>statistical power:</u> $V_1=1, V_2=24$ $\phi=1.88$ $(1-\beta) \geq .68$ for the main effect $\phi=1.33$ $(1-\beta) \geq .40$ for the treatment x frustration interaction	<u>1. confounding levels of constructs and constructs:</u> (see Hanratty et al., 1969) <u>2. generalizing across treatment time:</u> (see Hanratty et al., 1969) <u>3. hypothesis guessing</u>	<u>1. setting x treatment interaction</u> <u>2. generalizing across treatment construct</u> (see Hanratty et al., 1969)
Hapkiewicz & Roden, 1971	none	Second grade boys and girls, aged 6-8, from a New York City school	school: run in 2-person group outside of classrooms	2 cartoons similar to those shown on TV (total duration of 12 min.) "Fairweather Friends" and "Boxcar Bandit" (vs non-aggressive cartoon "Toot, Whistle, Plunk, and Boom" or no film)	pushing, grabbing, putting hand over peep hole to prevent others from viewing peep show (unspecified time period); scored by raters with reliability of .98			$ V_1=2, V_2=54$ $\phi=2.24$ $(1-\beta) \geq .94$ main effect $\phi=1.58$ $(1-\beta) \geq .67$ for the interaction <u>random irrelevancies in the experimental situation:</u> possible interference of social hierarchy or partners' aggressive reputations		

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Hapkiewicz, & Stone, 1974	<u>positive</u> : for males shown "realistic" violence <u>none</u> : for males shown cartoon violence, or for females shown either type of violence	suburban middle-class boys and girls (ages 6-10)	school: run in 2-person groups (SES and sex matched within pairings and across condition) of acquaintances outside of classrooms	15 min. "realistic" film of "Three Stooges" slapping, pushing + making verbal threats; cartoon: 2 Mighty Mouse cartoons (vs animated introduction to musical instruments)	pushing, hitting, grabbing or verbal demands in peep-show situation (Hapkiewicz & 1971) lasting 15 min.	gender, "realism" of content			<u>surplus construct irrelevanties</u> : excitement may be confounded with content	

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Himmelweit, Oppenheim & Vince, 1958 (also included an adolescent sample not discussed here)	none	boys and girls attending urban state schools in London and in Norwich, England. Aged 10-11; low, average + high IQ, working and middle class	Schools: In class paper-and-pencil data gathering sessions	Whether children were viewers or nonviewers of television	teacher's rating of whether S was "an aggressive type of child" and "children's answers to the personality inventories" of which only one question seems at all related: "not getting along with other children" on the worries inventory			1. <u>reliability of measures</u> : The low number of items tapping aggression and the unknown reliability of the very general teacher rating item suggest that any real effect would have been lost in error variance. 2. <u>reliability of treatment</u> : "viewers" likely had very different levels of exposure to violence on TV	1. <u>construct underrepresentation of aggression</u> . 2. <u>surplus construct irrelevantancies</u> : a) of aggression: Teacher rating may have tapped general assertive behavior; companion item was "Is a submissive type of child" "Worries" item would apply to nonaggressive victims as well as to aggressive children. b) of violence viewing: Total viewing includes far more than violence viewing; content/excitement confound	

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Huston-Stein, Fox, Greer, Watkins & Whitaker, 1981	<u>none</u>	pairs of nursery school boys and girls (aged 3-5)	observation room in the children's own (University) nursery school	8-12 min. excerpt from a Saturday morning children's TV program chosen from 22 such programs on the basis of a particularly high number of 15 sec. intervals in which judges scored an incidence of physical attack, physical threat, derogation or other verbal aggression, attack on objects, and deliberate attempts to frighten or intimidate someone.	physical or verbal attacks on each other or on objects during a 10 min. play period			<u>statistical power:</u> $V_1=3, V_2=25$ $\phi=1.41$ $1-\beta=.57$	<u>surplus construct irrelevancies:</u> of both TV violence construct and aggressive behavior measure	

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Kniveton & Stephen-son, 1975	<p><u>positive</u>: for boys without situational pre-experience, on nonviolent competitiveness measure</p> <p><u>none</u>: for boys with situational pre-experience, on non-violent competitiveness, and for all boys on violent fighting measure.</p>	boys from 5 primary schools in Nottingham, England; aged 5-6, working and middle class	Observation room in university social psych. department, identical set-up to film, one road-race set, one control panel, two boys	4½ min. film of two boys (aged 6 + 7) fighting over road-race set (before-after design)	<p>1. nonviolent competitiveness: all overt conflict, including asking to play with control panel, attempting to take it away, verbal disagreements, and "squabbling"</p> <p>2. violent fighting: physical attempts (beyond snatching away) to get cars or control panel (virtually no behavior in this classification)</p> <p>[4 min. play-time]</p> <p>both measures scored by a hypothesis blind observer (inter-rater reliability =.82)</p>	situational pre-experience, SES, acquaintance level of partners		<p><u>error rate problem</u>: 5 ANOVAs reported and a Mann-Whitney Z</p> <p>$\alpha_F \leq .287$</p>	<p>1. <u>surplus construct irrelevancies</u> in non-violent competitiveness measure; confounding of excitement and violent content.</p> <p>2. <u>hypothesis guessing within experimental conditions</u>: that observers were watching them from behind one-way mirror. Film followed by placement in identical situation may have communicated experimenters' expectations to those who had not previously (after previous experience) come up with personal explanation of why they were being observed.</p> <p>3. <u>evaluation apprehension</u> may have prevented behaviors of the violent fighting category from appearing</p> <p>4. <u>confounding levels of constructs and constructs</u>: more high-power violence might have produced stronger effects</p>	<p>1. <u>interaction of setting and treatment</u>: effect may not generalize outside of novel situation where children know they're being studied, and find themselves in identical surroundings to S's were shown film.</p> <p>2. <u>generalization of treatment construct</u></p>

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Kuhn, Madsen, & Becker, 1967	<u>positive</u> : for nonfrustrated subjects <u>none</u> : for frustrated subjects	nursery school boys and girls (mean age of 4)	School: individual testing out of regular class	Film of adult attacking Bobo doll (5 min.) vs. "film on a neutral subject"	"aggressive" (nondefined) behavior in a 5 minute play period alone with toys	frustration			1. <u>construct underrepresentation of aggressive behavior</u> 2. <u>surplus construct irrelevancies</u> : excitement confounded with content 3. <u>confounding levels of constructs and constructs</u> : more common, higher-level violence might have produced a measurable effect for all groups. 4. <u>interaction of procedure and treatment</u> : frustration manipulation may have produced inhibitions which masked any violent content effects	<u>generalizing across treatment construct</u>
Leifer & Roberts, 1972	<u>positive</u> : for both types of violent content and females on physical aggression score of response hierarchy; for females on behavioral measure only, for males viewing rewarded aggression on behavioral	nursery school boys and girls (aged 4)	School: individual testing out of normal classroom: interview situation for data collection	similar to Bandura, Ross, & Ross 1963(b), but with 12 year-old boys as models	1. behavioral measure: same as Bandura, Ross & Ross 1963(b) 2. response hierarchy: physical choice of hitting, pushing, slapping, or throwing things as a solution to hypothetical conflict situations;	gender, dependent measure used		<u>error rate problem</u> : 10 analyses reported (7 ANOVAs + 3 Mann-Whitney analyses) $\alpha_F \leq .273$	1. <u>underrepresentation of both aggression measures</u> 2. <u>hypothesis guessing within experimental conditions</u> : "Scuttlebutt effect" may have worked in this case in the direction of the experimental hypothesis	<u>generalizing across treatment construct</u>

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	ers only (other validation attempts used adolescents)	measure: for males and females viewing rewarded aggression on verbal score of response hierarchy none: for males viewing punished aggression, on behavioral measure and on verbal score of response hierarchy; for females, on verbal score of response hierarchy			verbal choice of name-calling in hypothetical situation					
Expt. 1	positive: the greater the amount of violence, the greater the "aggression"	students in kindergarten, third and sixth grade; low-middle to middle class neighborhood	School: informal out of class data gathering sessions (paper + pencil), children told E wanted to know what kids thought of different TV shows	full episodes of shows judged by a panel of adults (in various organizations) to be violent	choice of physical aggression to items on response hierarchy (analyses of verbal aggression scores done but not reported)			error rate problem: 37 regression analyses and 1 ANOVA reported $\alpha \leq .42$ F	1. underrepresentation of aggression construct 2. surplus construct irrelevancies: possible excitement and content confound. 3. hypothesis guessing within experimental conditions: filling out questionnaires about characters' intentions and motives may have sensitized Ss to experimental expectations	

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Expt. 2	<u>none:</u> for any program, for either grade level	pre-schoolers from Stanford Nursery School and 5th graders from nearby elementary school (both boys and girls)	same as Expt. 1	crime/adventure shows, shown in full without commercials; edited to control motivations and consequences (vs travelogue on Austria)	choice of physical aggression on response hierarchy		<u>Resentful demoralization of controls:</u> control Ss may well have known, via a "scuttlebutt effect" that other subjects had viewed more exciting content, and may have felt resentful at having to watch a travelogue	<u>Statistical power:</u> $V_1=1, V_2=49$ \emptyset ranges from 1.84 to 1.96 for main effect, depending on size of relevant cells. $(1-\beta) \geq .75-.79$ \emptyset ranges from 1.13 to 1.41 for interaction, depending on size of cells $(1-\beta) \geq .40-.50$	<u>underrepresentation of construct of aggression</u>	
Expt. 4	<u>positive:</u> for subjects initially low in aggression, on the response hierarchy measure only <u>negative:</u> for subjects initially high in aggression, on response hierarchy; for third graders in low-separation condition on both program-related items <u>none:</u> for sixth graders on program-related items or for third graders in high-separation condition for those items	third and sixth graders in parochial schools (boys and girls)	School setting, outside regular classrooms Expt's subjects told that their opinion re: video-tape recorder; Control Ss told to evaluate film techniques	"Silent Force", one program shown in Expt. 2; full program shown, with 2 commercials (vs travelogue on California)	choice of physical aggression 1. in response hierarchy 2. for situations identical to those in violent program 3. for situations similar to those in violent program	age, separation of motives + consequences, initial aggressiveness measure		<u>error rate problem:</u> 11 analyses reported $\alpha_F \leq .35$	<u>1. hypothesis guessing within experimental conditions pre-test and later inclusion of program-related items may have communicated expectation of change</u> <u>2. underrepresentation of construct of aggression</u>	

Study	Reported effects on Non-imitative "Aggression"	Sample Characteristics	Setting	Operationalization of TV violence	Operationalization of Nonimitative Aggression	Mediating Variables	Threats to Internal Validity	Threats to Statistical Conclusion Validity	Threats to Construct Validity	Threats to External Validity
Liebert & Baron, 1972	<p><u>positive</u>: for all groups on total + average duration-of-hurt measure; for all groups (but especially younger boys) on the "aggressive play" measure</p> <p><u>none</u>: for all Ss on the frequency-of-hurt measure; for 8 and 9 year olds on the latency measure</p>	boys + girls aged 5,6,8, and 9 from an Ohio college community; varying economic backgrounds	experimental laboratory in Fels Research Institute. Ss were brought to lab by parents answering a newspaper ad. or school-distributed letter asking for volunteers for a study of the effects of television on children	3½ minute sequence from "The Untouchables"	<ol style="list-style-type: none"> 1. frequency + 2. latency + 3. duration of pressing the "hurt" button on a help/hurt machine (total and average) 4. playing with toy gun + knife or assaulting Bobo dolls in a playroom for 5min. solitary play session 	dependent measure, age, gender		<p><u>error rate problem</u>: 7 ANOVAs reported $\alpha_p = .31$</p>	<ol style="list-style-type: none"> 1. <u>construct underrepresentation</u> of "aggressive play" measure and possibly also of help/hurt machine responses: children may not have believed or understood the rather abstract explanation of how the machine worked to help or hurt some child they never saw or heard 2. <u>surplus construct irrelevancies</u>: possible confounding of excitement and content (although control film was a fast-moving track meet and "help" measure was increased by violent film only for older girls) 	<ol style="list-style-type: none"> 1. <u>interaction of setting and treatment</u>: novel situation, knowledge of being studied 2. <u>generalizing across effect construct</u>: would effect general to more typical aggressive responses?
Lovaas, 1961 Expts. 1 + 2	<u>none</u> : in either experiment	<p><u>Expt. 1</u> 5 year olds at Gatzert Institute of Child Development; above average in I.Q. and SES;</p> <p><u>Expt. 2</u>;</p>	experimental room in nursery school building (Expt. 1) or experimental trailer (Expt. 2);	5 minute cartoon; Ss had to press a lever every 10 sec. to make cartoon reappear on the screen when it went off;	number of lever presses to operate a toy in which one doll hit another on the head, in the 2 minutes following viewing			<p><u>statistical power</u>: (for Expt. 1) $V_1=1, V_2=10$ $\beta=1.73 (1-\beta)$ $\geq .59$, for Expt. 1 $V_1=1, V_2=18$ $\beta=2.24 (1-\beta)$ $> .82$ for Expt. 2</p>	<ol style="list-style-type: none"> 1. <u>underrepresentation</u> of aggression construct 	<p><u>setting x treatment interaction</u>: effect might have shown up under normal viewing conditions or if other toys had been available for control Ss to play with</p>

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		"considerable experience" as Ss; both boys and girls <u>Expt. 2</u> : 4-6 year olds at Community Chest day care center, from low income families with working mothers; both boys and girls	Ss invited to "play some games and look at a movie"	"Rasseling Match" - almost continuous hitting, biting, etc. (vs. "Bear Facts" film of mother bear playing with cubs)					2. <u>generalizing across time</u> : effect might have shown up after 2 min. 3. <u>treatment x procedure interaction</u> : lever pressing task and constant program interruption may be responsible for noneffect	
Expt. 3	<u>positive</u>	same Ss (in reverse film conditions) as for Expt. 2	same as Expt. 2 (children likely now accustomed to trailer)	same as above	same as above, except an alternative toy was also available + session lasted 4 min. S told to begin by playing with the ("aggressive") doll toy, but then to play with whichever one (s)he liked. If child did not switch after 2 min., E reminded S that (s)he could.			<u>error rate</u> : $p < .06$ but $\neq .05$.	1. <u>underrepresentation</u> of aggression construct. 2. <u>surplus construct irrelevancies</u> of violence viewing 3. <u>experimenter expectancies</u> : E not blind to conditions and had ample opportunity to communicate expectancies to child since she could give instructions about choice of toy twice during the 4 min. session when S was responding	<u>setting x treatment interaction</u>

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4. interaction of procedure x treatment: compulsory "aggressive" play at beginning of session may be responsible for effect

Milavsky, Kessler, Stipp, & Rubens, 1982 (elementary school sample only)

none

boys and girls in grades 2-6, from 60 schools in Ft. Worth, Texas, and Minneapolis, Minn.

In-class paper-and-pencil data gathering sessions

violence ratings, by adults, of programs which the subject reported watching, weighted by the reported frequency of watching and the length of the program

4-6 item version of the Peer-_r Rated Aggression Index

{u=1, v ranges from 109 to 497
L ranges from 17 to 79 (1-β)=.98 to .99 (Cohen, 1977)}

reliability of measures: the violence exposure measure had average reliabilities of .68 for boys and .72 for girls

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Moriarty & McCabe, 1977 (also included an adolescent sample not discussed here)	none	male hockey, baseball, and lacrosse players, aged 6-13 ("Younger" and "Middle" age groups) in Windsor, Ontario	Group TV viewing in hockey school and other unspecified locations; behaviors observed next day on playing field or ice	edited videotapes of previously televised sports programs, varying from 12-40 minutes in length; judged by Es and by sports advisors to be antisocial	-verbal, nonverbal, or physical acts which appeared to raters to "demean, intimidate, threaten or harm a person"			1. <u>statistical power</u> : $V_1=2, V_2=30$ $\phi=0.68 (1-\beta)$ $=.28$ (equal n's assumed) 2. <u>reliability of measures</u> not reported; if ratings were not reliable, an effect might have been obscured	<u>generalizing across time</u>	
Mussen & Ruthford, 1961	positive for both frustrated and frustrated subjects	lower middle class first grade (aged 6 & 7) boys & girls	school setting	8 minute cartoon of plants + animals fighting (vs cartoon on "the fun of cooperative play" vs no film)	child answering affirmatively to five questions re: "popping" a balloon				1. <u>construct underrepresentation of aggression</u> 2. <u>surplus construct irrelevancies</u> : probable excitement/content confounding	
Savitsky, Rogers, Izard, & Liebert, 1971	none (novel aggressive responses negligible in all groups) for either frustrated or nonfrustrated Ss	First and second grade boys in a rural public school	School: individual treatment and testing outside regular classroom: setting for "aggression" exactly like that in film	same conditions as Hanratty et al., 1969	"any aggressive responses not modelled in film" directed toward human dressed as clown		[$V_1=1, V_2=42$ $\phi=2.49 (1-\beta) > .92$ for the main effect $\phi=1.7 (1-\beta) > .65$ for the frustration x treatment interaction]	1. <u>confounding levels of constructs and constructs</u> : TV violence may have been too "weak" in intensity + length 2. <u>generalizing across time</u> : 5 minutes may have been too short an observation period, given novel situation	1. <u>setting x treatment interaction</u> : id-entical setting may have "demanded" im-itation, leav-ing no time for novel responses of any kind 2. <u>generaliz-ation across treat-ment con-structs</u>	

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Schramm, Lyle, & Parker, 1961	<u>negative</u> : TV watchers, compared to non-watchers <u>none</u> : for high vs low levels of viewing	sixth graders in relatively remote towns with + without TV service; both boys + girls	Schools: paper-and-pencil in-class data gathering sessions	level of TV viewing: high, low, or none.	Sears Aggression Scale: 12 "aggression anxiety" items, 14 "projected" (mean world) items, 5 "self aggression", 8 "pro-social aggression" + 9 "anti-social aggression" items		<u>correlational data</u> ; no causal inference possible	<u>reliability of measure</u>	1. <u>surplus construct irrelevancies</u> of aggression measure and TV violence construct 2. <u>under-representation of construct of aggression</u>	
Siegel, 1956	<u>none</u>	nursery school boys + girls, aged 3-5	children taken "to an unfamiliar building for a new experience"; tested in pairs who were probably friends; children invited to "see a movie"; all Ss saw both films about 1 week apart in counter balanced order	10 min. cartoon featuring Woody Woodpecker engaged in "raw aggression and unremitting hostility in almost every scene" (vs "The Little Red Hen: Background for Reading Expression"; Matched for interest with Woody Woodpecker but read by a "calm-voiced" narrator)	number (weighted by judged intensity) of "hostile", "destructive", or "aggressive" acts in 14 minute observation periods (free-play in pairs with toys) directed at self, other child, or toys			1. <u>statistical power</u> $V_1 = V_2 = .22$ $\phi = 1.66$ $(1-\beta) > .57$ 2. <u>random irrelevancies in the experimental situation</u> : previous relationship and history of each S pair.	<u>surplus construct irrelevancies</u> : aggression includes actions toward toys as well as humans	

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Singer & Singer, 1981	positive: for both TV violence variables	boys and girls, aged 3-5, in 8 New Haven Nursery Schools (varied in race and SES)	free play periods in play-room and outdoor play area	1. parent reports of number of half-hour time periods the child spent watching TV, weighted by the parent's judgement of the intensity (scored 1-5) with which the child viewed the program. Scores were calculated for two-week samples four times over two years. 2. number of time periods spent watching action/dramas weighted and sampled exactly as above.	score from 1-5, based on the judgement of two raters (blind to TV condition) observing independently but simultaneously (reliability described as "high"); included knocking over child's own or others' toys and other objects, pushing, shoving, or physically attacking someone, disruption of others' play, threatening others, or taking others' things. Harming people scored higher than harming things. Scores averaged over 10-minute samples of free play behaviour, one in each week, that TV reports were being made by parents.			Correlational data: no causal inference possible. Some likely third variable explanations eliminated, but cross-lag correlations showed inconclusive pattern		surplus construct irrel-evancies: both TV violence indices are confounded with excitement; aggression measure includes some aspects other than interpersonal harm

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Stein, Fried- rich & Vondra- cek, 1972 1. Exp- eriment	positive: on interpersonal aggression of Ss high in initial aggression, compared to neutral TV group (did not increase level of personal aggression, but prevented it from decreasing as much as neutral TV group did); also on correlation between changes in aggression + frustration observations compared to prosocial group none: on inter- personal ag- gression of high-initial- aggression Ss compared to prosocial TV group; on in- terpersonal aggression of Ss initially low in aggression; on fantasy + object aggression for any groups, on real-life observed responses to frustration by peers; on cor- relation between changes in frus- tration and ag- gression compared to neutral group	children specially recruited for a 9- week nur- sery school pro- gram; wide SES range; controlled for SES, IQ, amt. of home viewing (aged 4+5)	9 week nursery school; 3 weeks baseline, then 4 wks when 20- 30 minutes viewing of TV was part of daily schedule, then 2 weeks post- viewing. Children observed in their own class- rooms immedia- tely after viewing, for 30- 60 min. per day.	Batman + Superman programs; 2 stories daily with only commercials removed (vs prosocial videotape "Mr. Rogers" vs neutral: miscellaneous films with no aggress- ion + no strong pro- social the- mes)	1. Interperso- nal: physical assault on another child, taking child's toys, verbal threats, jeer- ing etc., bossing other children, + tattling during free play 2. responses to frustration by peers (from observation) 3. correla- tion of frus- tration and aggression, from obser- vational sessions.	initial level of aggress- ion scored	spurious equivalence of treatment and control: treatment and control groups were later obser- ved while all playing to- gether	error rate problem: 24 ANOVAs reported $\alpha_F \leq 1.0$	surplus con- struct ir- relevancies: likely an excitement/ content confound	

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Stein, Freidrich, & Vondracek, 1972 (con'd)	<p>positive: for girls on correlation between "aggressive choice" + frequency of viewing TV, 2. Correlation between viewing Batman, and naming violent shows as favourite</p> <p>negative: for girls on correlation between observed interpersonal aggression and frequency of watching violent shows</p> <p>none: for boys on either measure</p>	same Ss as Experiment, described above	same as for Experiment, described above	<p>Frequency of viewing TV</p> <p>-of viewing "Batman" + "Superman" at home</p> <p>2. Favourite shows: -violent programs named -"Batman" -"Superman"</p>	<p>1. baseline interpersonal aggression score described above</p> <p>2. choice of an aggressive response (over a prosocial or avoidance response) to end a story about a frustration situation experienced by a same-sexed child</p>	gender	<p>correlational data; no causal inference possible</p>	<p>1. error rate problems: 60 correlations were tested for significance in the home viewing baseline aggression analysis; 63 were tested in the home viewing response to frustration story analysis</p> <p>$\alpha_F \leq 1.0$</p> <p>2. reliability of measures: unreliability of aggressive-choice measure</p>	<p>1. surplus construct irrelevanties: possible excitement/content confound</p> <p>2. underrepresentation of construct for "aggressive choices" measure</p>	
Steuer, Applefield, & Smith, 1971	positive	preschool boys and girls of mixed races attending Child Development center; all acquainted, matched on amount of home viewing (ages 3-5)	playrooms in Child Development center. Children played in a group with their viewing-gp cohorts (separate playrooms for experimental and control gps)	10 min. excerpts from Saturday morning shows containing 15 instances or more of behavior ($\bar{x}=22$) later scored as aggression in children (vs Saturday morning shows edited to contain no aggression)	Hitting, pushing, kicking, choking, + throwing things at another child. Scored during 10 min. free play situation right after TV viewing	individual differences: behavior of 2 of the 5 experimental subjects accounts for almost entire group difference	local history: since post-viewing sessions included either all control or all experimental Ss some local event affecting one group but not the other could explain the results.		<p>surplus construct irrelevanties: possible excitement/content confound</p>	

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Thomas, 1972	negative: for the "cognitively immature" none: for the "cognitively mature"	white middle class boys from Santa Monica public school (ages 5½-8½)	School: individual treatment + testing outside regular classrooms	6 min. "aggressive film" (vs "nonaggressive film" vs no film)	frequency of administering most aversive noise in a guessing game with <u>E</u>	cognitive style		error rate problem: 9 reported ANOVAs reported $\alpha_p \leq .45$	confounding levels of constructs and constructs -films not described	1. generalizing across effect constructs and perhaps also 2. generalizing across treatment constructs
Williams, 1980	positive: verbal aggression higher, for boys only, after TV became available in the experimental community -physical aggression higher for both boys and girls after TV became available in the experimental community none: for girls, in verbal aggression, comparing before and after TV became a variable in the experimental community -for communities which already had TV, compared to the experimental town before TV became available.	boys and girls in grades 1 to 5, in 3 Canadian logging towns	School playground	access to television transmission	number of physically harmful acts (hitting, pushing, tripping, interfering with or throwing something at another child); number of verbally harmful acts (derogating, arguing with or loudly commanding another child) Numbers were tallied simultaneously by two independent observers (reliability >.80) during 21 randomly selected minutes over a 7-10 day period	gender, on the verbal aggression-measure	local history: all subjects in the treatment condition were from a single community	surplus construct irrelevant: access to TV includes much more than exposure to violent content; excitement also confounded with this index	generalizing across time: although the post-TV follow-up was 2 years after TV became available, the author suggests that the effect may have been "boosted" by the novelty of TV to the children of the experimental community	

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Wotring & Greenberg, 1973, (Expt. 2 only; Expt. 1 used only adolescents)	positive: on physical aggression measure after viewing physical aggression none: on verbal aggression after viewing physical aggression, or on either type of aggression after viewing verbal aggression	Boys in grades 4,5, + 6 in 2 Detroit public schools, lower and middle class	School: in-class paper-and-pencil data gathering sessions (cover story not reported)	16 minute videotape of 16 physical aggression segments or 15 verbal aggression segments from different TV programs (vs 16 minute videotape of non-violent action from same programs)	1. verbal aggression: answers to open-ended items asking S what he would do in 3 provoking interpersonal situations 2. physical aggression: paper + pencil measure of recommending action in 7 potential conflict situations; Ss checked off one of 9 choices representing 9 levels of interpersonal harm	all Ss were frustrated (asked to do a senseless task over + over again + criticized for their performance)		reliability of measures: failure of verbal aggression to be affected by TV content may have been a result of unreliability; (reliability not reported)	1. underrepresentation of aggression of aggression 2. possible surplus construct irrelevant excitements: excitement may have been confounded with content 3. hypothesis guessing within experimental conditions: rather obvious manipulation and measures probably sensitized Ss to expectations	generalizing across treatment constructs

earlier (1963) study was part of an impressive research program carried out over a 10 year period by the Rip Van Winkle Foundation in New York State. Lefkowitz, Eron, Walder, and Huesmann (1972, p. 56) reported that although violent TV preference accounted for only 3% of boys' concurrent peer-rated aggression, it accounted for 9% of their aggression 10 years later. The longitudinal data they collected permitted these researchers to argue quite convincingly against all the plausible alternative hypotheses they (and this author) could think of for the TV preference-aggression relationship they found. A cross-lagged panel analysis (Lefkowitz et al., 1972) provided, according to Cook and Campbell, "some evidence, even if not totally compelling" (Cook & Campbell, 1976, p. 293) that preference for violent television caused aggression more than aggression caused preference for violent TV. Later partial-correlational analyses (Lefkowitz, Eron, Walder, & Huesmann, 1977) demonstrated that the relationship did not depend on any single one of a number of third variables having to do with the boy himself (aggressiveness, IQ, adolescent aspirations, or amount of TV watched) or having to do with his parents (occupational status, punishment, aggressiveness, or mobility orientation).

Eron's more recent report (1982) described preliminary results of two studies, one of which involves a replication of the earlier longitudinal study, in Chicago, U.S.A., in Australia, and in four European countries. The 1982 report included data from eight different samples in four countries. Follow-up data were available over two years for the Polish samples and over three years for four samples in Finland and the U.S.A. In only three of these 18 instances was there not a statistically significant relationship between peer rated aggressiveness and the amount of violence watched on TV. Although the chance is as

high as 50% that at least one of the 18 reported correlations was due to chance, the consistency of this relationship is impressive. Even if a few correlations are attributable to chance, one would have to conclude that the covariation of these two variables is a stable and widely spread phenomenon.

This tribute to external validity does nothing to improve confidence in the conclusion about causality, however. Unfortunately no cross-lagged analysis was presented in the 1982 publication, and only reading achievement (as a measure of intelligence) was eliminated as a possible third variable.

The hypothesis that TV violence causes aggression received some support from the second study of Eron's 1982 report, in which an intervention was introduced. Eron and his colleagues were able to reduce significantly the peer rated aggression score of high violence viewers by putting them through an attitude change treatment. Children were pre-tested on their "attitudes" toward television, including their beliefs about how realistic it was, and how similar they believed violent characters were to themselves (an identification measure). Then, in two sessions totalling three hours time, the experimental subjects wrote a paragraph on the topic of "why TV violence is unrealistic and why viewing too much of it is bad" (Eron, 1982, p. 208). They videotaped themselves reading their paragraphs, ostensibly for later showing in other schools. Then they were tested again on the "attitudes" measure. Four months later, their average peer rated aggression score was significantly lower than that of children who had been randomly assigned to the control group. The control group had initially been just as aggressive as the treatment group subjects, and had gone through

exactly the same procedure, except that their paragraphs were on the topic of "What I did last summer".

Had this reduction occurred because children began to watch less violent TV, a strong case would have been made for the "TV-causes-aggression" hypothesis. Unfortunately, no such clarifying pattern emerged. The other effect of the treatment procedure was to entirely wipe out the correlation between aggression and watching violent TV! Eron reasoned that the decrease in aggression had occurred because children were led to see violence as less realistic, and violent characters as less like themselves. The treatment subjects whose aggression dropped the most were those who showed the greatest changes on the "attitudes" measure. This is quite a plausible explanation. Certainly, this pattern of results improves the case for TV violence as the cause of aggression in Eron's subjects. The case is by no means closed, however.

Singer and Singer (1981) improved the case for causal inference in their correlational data by partialling out three potential third variables: SES, IQ, and ethnic group. They also presented cross-lag correlation patterns to support their causal hypothesis. However, these patterns actually support both causal hypotheses about equally: TV violence as the "cause" in two of the observation periods and aggressive behaviour as the "cause" in the other two.

In summary, seven studies in Table 1 are correlational. Eron's later research indicated that the positive relationship between TV violence viewing and aggression is widespread and quite consistent. Nevertheless, it is only a correlation. Cross-lagged analyses sometimes suggest causality in these studies, but not always.

2. Instrumentation. Internal validity is threatened if the instrument used to measure the dependent variable is different for different conditions. Instrumentation is a rare problem, listed as threatening only two studies in Table 1. In fact, the authors of one of these two studies, Cameron and Janky (1971), collected some supplementary data indicating that instrumentation very likely did not affect their results. Cameron and Janky asked parents to administer either a violent or non-violent weekly TV "diet" to their children by permitting the children to watch only programs from a list prepared in advance by the researchers. These same parents, well aware of what condition their children were in, also provided the observational reports on behavior which were Cameron and Janky's dependent variable. Potentially, parents of children in the different conditions had different expectations about their children's behaviour which might affect their observations. However, when asked at the end of the experiment about what their expectations had been, only 34 sets of parents (of the 254 who participated) reported even having expectations of whether or how their children's behavior would change. Of these 34, only four had children who changed in the parentally-expected direction. Although it seems remarkable that 220 sets of parents would monitor their children's television viewing for seven weeks without expecting some behavioral outcome, there seems to be no reason why parents would deceive the researchers. Certainly, Cameron and Janky "stacked the deck" against their finding of no expectations, by questioning parents at the end of the research project, when the behavioral changes had already occurred.

Granzberg (1982) based some of his conclusions on retrospective reports by parents and other community members. Their impressions

were generally that children had been less aggressive, in a number of ways, before the advent of television in the community. The problem with this procedure goes beyond the unreliability of such anecdotal and opinion data. It is certainly possible that reports about "the good old days" before TV were even hazier, and had a rosier bias, than the reports of more recent events, post-TV.

3. Spurious equivalence of treatment and control groups. Studies which fail to find a significant treatment effect may fail because their treatment manages to reach the control group as well. Fouts (1977), who found no violent TV effects, may actually have created equivalence, by his choice of TV material for the control group. Some control subjects saw uncut versions of "Bugs Bunny/The Roadrunner", which features a large number of highly aggressive acts. Some saw "Emergency", which Singer & Singer classified in their violent "Action Shows" category.

Treatment effects may have spread less directly into the control group in the experiment by Stein, Friedrich, and Vondracek (1972). After viewing their respective television programs, subjects from the treatment and control groups played together while observers scored their aggression. Any effect of the treatment could have spread to the control group, if the treatment group's aggression led to retaliation or imitation by the control group. Stein and her colleagues did not find increased aggression following violent TV, on five of their eight comparisons.

4. Resentful demoralization of controls. Internal validity of a "no effect" conclusion may be threatened if subjects in a low-desirability control condition become resentful and behave differently

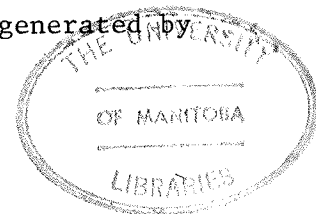
as a result. Leifer and Roberts' Experiment 2 (1972) may have been affected by this problem. Experimental subjects viewed a crime-adventure program, while control subjects saw a travelogue about Austria. No mention is made of any effort to keep experimental and control subjects from communicating, although they came from the same classes and appear to have viewed the televised programs at different times. Failure to keep the experimental "sheep" from the experimental "goats" could have resulted in comparisons among classmates about the treatments they received and subsequent resentment by those children subjected to the travelogue. They may have expressed their resentment as a general negativism which showed up as antisocial responses on the dependent variable. These researchers did manage to find an increase in aggression for low-aggressive subjects on one measure in Experiment 4, which also used a travelogue for the control group. However, they used a different travelogue (on California) for the latter experiment, and this may have been because of low subject satisfaction with the Austria travelogue.

5. Local history. When subjects from one treatment condition are all treated and tested in the same group, ideosyncratic events in the group session may be confounded with the experimental treatment and may account for the differences between groups. Cook and Campbell recommend instead (p. 229) that many smaller groups, or individual testing, be used to avert this danger. One study, Steuer, Applefield, and Smith, 1971, committed the error of treating and testing all experimental subjects in one group and all control subjects in another group. Observations were made while the five children in a particular treatment group played together, separated from the other group, for 10 minutes every nursery school day over four weeks. Members of the two groups were

matched on how much television they viewed at home. During the last two weeks, the play sessions were preceded by group television-watching sessions. Aggressive behaviour observed in play sessions for individual children in the violent-television group was compared with the aggressive behaviour observed in their matched controls from the non-violent-television group. Such a procedure makes the results potentially dependent on rather ideosyncratic events. For instance, daily graphs of subjects' behaviour indicate that virtually all differences between groups would have been accounted for by a fight breaking out (related to TV content or not) between Experimental subjects 1 and 4 on the second day of the treatment, establishing an ongoing vendetta between these two.

Two natural experiments, reported by Granzberg (1982) and Williams (1980), also fall prey to the danger of local history problems. Granzberg studied two communities before and after exposure to television. One community showed an increase in aggression after TV access, and the other did not. Although Granzberg interprets this pattern as cultural mediation of television influence, it may have been an artifact of local history. Some event unrelated to television may have increased aggression, quite independently, in one of the communities.

Williams' (1980) results were based on a single treatment community. Her subjects were initially no less aggressive than comparable communities that already had television access. The introduction of television was followed by increased aggression. Not only were children more aggressive than they had been before TV, they were also significantly more aggressive than children in neighbouring communities which had received television service for many years. Williams explained this as a novelty effect (an interaction between the excitement generated by



the new medium and the violence of its content). A local history explanation remains just as plausible, however, since many things happened in the community over those two years, besides the advent of television.

II. Threats to Statistical Conclusion Validity

Statistics allow the researcher to compare the observed numerical differences to those which one would expect by chance alone. In the absence of statistical tests, the research consumer cannot judge whether something "really happened" in a particular experiment, or if the results were simply a result of sampling luck. In Granzberg's (1982) report for example, no numerical comparisons are made for the retrospective reports, and although the modal report is that "more" aggression existed after TV became available, we have no way of comparing this to a chance level for such reports.

Feshbach's (1972) report of his first experiment included statistical comparisons of most of his groups. Unfortunately, it is impossible to tell whether or not TV violence reduced his subjects' aggression or simply failed to affect them. Subjects who saw violence were less aggressive than the controls who watched a baseball game, on the average, but no statistical test was reported for this comparison. Therefore, we can't tell if a real difference existed, for those subjects, or if the outcome was attributable to chance alone.

Just employing statistical tests is no guarantee that one's conclusions of "effect" or "no effect" are valid, of course. Cook and Campbell point out that statistics are "fallible gatekeepers" (p. 225) which may lead us to falsely conclude that treatment differences exist

when they do not (Type I error) or that no treatment differences exist when, in fact, they do (Type II error).

1. Error rate problems. When many statistical tests are conducted in an experiment, with the level of Type I error (α level) set per comparison, the probability that a Type I error has occurred in any one of these significance tests may actually be higher than α . In fact, the probability that a difference has been falsely declared significant somewhere in the experimntal analysis is as high as $C\alpha$ for C multiple comparisons of means which are completely independent of each other. One solution, recommended by Cook and Campbell, is to adopt a per experiment or experimentwise error rate (the expected number of errors per experiment, or probability that one or more erroneous conclusions will be drawn in a particular experiment). This is a "conservative" procedure, which dramatically reduces the power of individual significance tests if conventional α levels are to be maintained. Another solution uses an error rate per family of related tests. The per family error rate was used in assessing the studies of Table 1 for three reasons.

i) It represents a compromise between the highly liberal per comparison error rate and the highly conservative per experiment error rate.

ii) It does not penalize large, ambitious research projects investigating more than one hypothesis. Conversely, it does not give special weight and encouragement to simple one-shot studies, which would be the most powerful tests of individual hypotheses if per experiment error rates were used (cf. Wilson, 1962). Such a policy seems highly unproductive in advancing the science of behaviour. In fact, recent

reviewers (Murray & Kippax, 1979; McLeod & Reeves, 1980) have urged the importance of adopting more complex models of the relationship between television content and viewer behaviour.

iii) It is consistent with common usage (cf. Kirk, 1968, pp. 83, 85) to treat each F test of a main effect or interaction, in an analysis of variance (ANOVA) and its related a priori and post hoc tests as individual families meriting individually assigned α levels.

A family of comparisons is defined (after Kirk, 1968, p. 85) as all the comparisons among the means associated with a single treatment. For the purposes of Table 1, this includes tests of all dependent variables, for all comparisons between means of the television exposure treatment. In the case of correlational data, it includes all correlations of the measure of TV violence exposure with any other variables. When the per comparison error rates were not specified, they were presumed to have been set at the significance level referred to. If no a priori α level was specified and "ns" was used to denote nonsignificance, an alpha level of $\alpha = .05$ was assumed, except when only lower per comparison α levels (e.g., $\alpha = .01$) were treated as significant in other significance tests, in which case the lower level was used in place of "ns". (This procedure would be expected to underestimate the a priori per family error rate because of the practice of reporting the lowest α level at which a difference is significant, even if significance at a higher α level would have been acceptable.)

Thirteen studies in Table 1 were identified as having dangerously high error rates per family (α per family or α_F as high as .20 or greater): Bandura, Ross, and Ross, 1963(a); Collins and Zimmerman, 1975; Dominick and Greenberg, 1972; Eron, 1963; Eron, 1982, for the correlational study; Feshbach, 1972 (both experiments); Granzberg, 1982; Kniveton and

Stephenson, 1975; Leifer and Roberts, 1972 (pre-test, Experiment 1 and Experiment 4); Liebert and Baron, 1972; Lovaas, 1961 (Experiment 3); Stein, Friedrich, and Vondracek, 1972; Thomas, 1972. The study by Stein and her colleagues had α_F levels as high as 1.0 in both reported analyses. In three other studies (Bandura et al., 1963(b), Granzberg, 1982, and Lovaas, 1961) even the per comparison error probability (between .05 and .10) was higher than the conventional cut-off point. Although many would consider such results interpretable, a "6-in-100" or a "1-in-10" chance of making a false conclusion would not be acceptable to some research consumers.

2. Statistical power. Failure to demonstrate a treatment effect with too few subjects to permit a powerful statistical test is an unfair test of the treatment. Deciding whether a study has sufficient power is not a simple matter. Significance level, effect size to be detected, and number of treatment levels as well as sample size must be considered. Table 1 includes an estimation of the minimum probability of rejecting a false null hypothesis ($1-\beta$) for each of the 12 studies which failed to find any effects in a particular experiment. Of these, six had less than an 80% chance (Cohen's 1969 recommended power level) of detecting a television violence main effect as large as .5 standard deviations (Hanratty, Liebert, Morris, & Fernandez, 1969; Hanratty, O'Neal, & Sulzer, 1972; Huston-Stein, Fox, Greer, Watkins & Whitaker, 1981; Leifer & Roberts, 1972 (Experiment 2); Lovaas, 1961 (Experiments 1 and 2); Siegel, 1956). The chance of detecting an interaction effect of that size was even lower. The value of .5 standard deviation units was chosen because it accounts for 20% of the variance in the dependent measure (Cohen, 1969), a reasonable proportion of the variance to declare "meaningfully significant."

Besides lacking statistical power, studies reporting no treatment effect may suffer four deficiencies in the power of their designs to control error variance.

3. Unreliability of measures. Like a small sample, unreliable measures stack the deck against finding a significant effect, in this case by inflating the error variance. This problem threatened the conclusions of ten studies in Table 1. Eron's earlier study (1963) employed parents' reports of frequency of viewing and program preference to determine each child's exposure to television violence. Parents may well have been unreliable observers of these variables. We know, for instance, that their responses did not correlate very well with each other. The correlation between their ratings of how much TV their child watched was only .37 (Eron et al., 1971). The "noneffects" in the earlier Eron report may be attributable to such unreliability. Milavsky, Kessler, Stipp and Rubens (1982) used a self-report measure of violence viewing and estimated their reliability (using Werts & Linn's (1977) "simplex" version of test-retest reliability) to be only .68 for boys and .72 for girls. They, too, found no effect of TV violence upon subsequent aggression.

Moriarty and McCabe (1977) made no attempt to determine the inter-judge reliability of the observers who scored subjects' aggression. Biblow (1973) also failed to report the reliability of his observers. Neither study found a TV violence effect.

In their correlational analysis of background variables, Stein et al. (1972) used a measure whose reliability (using Winer's (1962) analysis of variance method) was only .45 for boys. It is not too surprising that their measures of TV viewing habits failed to correlate

with such a measure. Schramm, Lyle, and Parker (1961) measured aggression by means of the five subscales of the Sears Aggression Scale, which had split-half reliabilities ranging from .15 to .64 in Sears' (1961) original study. Schramm et al. did not report on the reliability of the measures in their study, but given Sears' findings, the failure of the subscales to relate to TV viewing habits is not too informative.

Other "noneffect" studies may also have suffered from unreliable dependent variables. Fouts (1977) used a grab bag of items that included self reports of whether the subjects had hurt someone in the last week, self-reported interest in guns and martial arts, estimation of how dangerous their home city was, multiple choice questions about solutions to hypothetical conflict situations, and their "attitudes" toward aggression. Furu (1971) assessed aggression by means of six items borrowed from the 48-item Sears Aggression Scale. Greenberg (1975) used only four items for each of his aggression measures, and found no effect on his younger (9 year old) sample, for which reliability may have been especially low. Himmelweit and her colleagues (1958) assessed aggression by means of a single item administered to teachers, asking if the subject was "an aggressive type of child." They also reported (p. 215) using the child's responses on personality inventories to assess aggression, but an examination of the inventories reveals only one item that is at all relevant to interpersonal harmdoing: an item on the "Worries" scale about not getting along with others.

In short, a number of studies which purported to find no relationship between televised violence viewing and aggression are less conclusive than they might appear. Because the measures used were so unreliable, the odds were stacked in favour of the null hypothesis.

4. Unreliability of the treatment implementation. If the treatment

is not the same for all individuals, error variance may be inflated to the point where it obscures a true effect. Himmelweit, Oppenheim, & Vince inflated their error variance by including in their "viewers" group anyone who watched any amount of television, from occasional exposures to constant heavy viewing. It is conceivable that some "viewers" never saw any violence at all!

5. Random irrelevancies in the experimental situation. Error variance is also inflated if the experimental situation is not standard, allowing irrelevant events to influence behaviour, and possibly mask the effects of the treatment. Fouts' TV viewing and interviewing situations varied considerably from subject to subject. Some watched their program alone, others with a sibling or two, still others with strangers who happened to be available in the same time slot. The aggression measure was later administered by an interviewer who, in some cases, had interviewed the child on a previous occasion at home. In other cases the interviewer was a total stranger to the child.

One important source of potential interference factors is the nature of the available target. A social dominance hierarchy may exist which prohibits or encourages aggression between two children from the same social environment. The established popularity or aggressive reputation of the other child may affect a subject's willingness to aggress, if there is no other target available. Two studies which measured aggression by observing pairs of acquainted children interact (Hapkiewicz & Roden, 1971; Siegal, 1956) may have overwhelmed any possible TV effects by introducing this competing social variable.

III. Threats to Construct Validity

Internal and statistical conclusion validity questions center around

whether the treatment, as operationalized, actually caused true changes in the outcome variable, as operationalized. Construct validity refers to whether or not this established causal relationship can reasonably be said to reflect the relationship between the intended cause and effect constructs. This is partly a definitional and partly a procedural problem. The researcher must operationally define both cause and effect so that they include the major components of the constructs in question. If they do not, they are largely irrelevant to the literature investigating those theoretical constructs. The operationalization of the constructs should also be as free as possible of features which would lead to a reinterpretation of the independent and/or dependent variable in terms of some other construct(s). The vast majority of criticisms directed at the TV violence-aggression literature have been about its construct validity.

1. Construct underrepresentation (Irrelevance). A number of critics (Bryan & Schwartz, 1971; Howitt & Cumberbatch, 1975; Kaplan & Singer, 1976; Klapper, 1968; Kniveton, 1974; Kniveton & Stephenson, 1970; Singer, 1971, Weiss, 1969; Wilson, 1974) have noted that the "aggressive" behaviour measured in many of the studies was of little social concern, as the "victim" was a balloon, a Bobo doll, or some other toy. If behavioral research is to address the issue of greatest social concern, dependent variables which are referred to as aggression measures ought to measure behavior intended to cause interpersonal harm (after Klapper, 1968), but often they do not. In three studies (Bandura, Ross & Ross, 1963(a), 1963(b); Kuhn, Madsen, & Becker, 1967) hitting a Bobo doll (whose only imaginable purpose is to serve as a punching bag!) and rough treatment of toys were termed "aggression". Frequency

of operating a toy which featured one doll hitting a second doll on the head with a stick was Lovaas's (1961) "aggression" measure. Merely replying "yes" when an adult asked if a balloon should be "popped" constituted "aggression" in Mussen and Rutherford's (1961) experiment. Rough treatment of toys and playing with toy weapons were supplementary to "interpersonal" measures for Liebert and Baron (1972), and provided them with a more consistent pattern of positive results than did the more relevant "help/hurt" machine variables.

Six studies (Dominick & Greenberg, 1972; Fouts, 1977; Furu, 1971; Greenberg, 1975, Himmelweit et al., 1958; Schramm et al., 1961) used attitude scales to assess aggressiveness. Given the often poor correspondence between the attitudes people express and their actual behaviour (cf. Fishbein, 1967, for example), this practice also seems inappropriate. The influence of televised violence on attitudes may be of interest in and of itself, but would not necessarily relate to the influence of televised violence on aggressive behaviour. Leifer and Roberts' (1972) response hierarchy and Granzberg's (1982) aggression measure both involved asking children what they would do in a number of provoking situations. Such measures appear to share the conceptual inadequacy of attitude scales as operationalizations of aggressive behaviour. Leifer and Roberts' repeated unsuccessful attempts to "validate" the response hierarchy bear ample witness to its lack of correspondence to behavioural measures of aggression. Collins and Zimmerman found no effects when they used the response hierarchy as a measure. Stein and her colleagues found no effect on that measure among their male subjects. On the other hand, the response hierarchy did show a positive relationship, for girls, with the amount of violence viewed at home.

In all, 12 studies in Table 1 rendered themselves completely irrelevant to the question of television violence on real aggression. Three others used irrelevant measures in addition to measures of interpersonal harm doing, and thus endangered the construct validity of only some of their conclusions.

2. Surplus construct irrelevancies ("Muddiness" of constructs).

Operationalizations of both the cause and effect constructs may include variables related to constructs other than the ones under investigation. This confounding leads to lowered construct validity. The most common irrelevancy confounded with the treatment construct is excitement. Violent TV shows are but one kind of television programming intended to be exciting. It is quite possible that effects attributed to TV violence might instead be partly, and even entirely, attributable to the arousal which follows exciting stimulation. Zillmann and his colleagues have provided considerable evidence (Tannenbaum & Zillmann, 1975; Zillmann, 1971; Zillmann, Hoyt & Day, 1974; Zillmann & Johnson, 1973; Zillmann, Johnson & Hanrahan, 1973), that this may be true of laboratory studies of media effects for adults. Furthermore, Huston-Stein and her collaborators (1981) found no effect due to violent content among children when the control condition depicted an equal number of movements of similar intensity.

Twenty-three of the studies in Table 1 could be reinterpreted as having arousal, rather than violent content, as their manipulated treatment, since both are manipulated or measured contemporaneously (Bandura et al., 1963(a); Cameron & Janky, 1971; Collins & Zimmerman, 1975, Ellis & Sekyra, 1972; Eron, 1963; Eron, 1982, correlational study; Feshbach, 1972 (Experiment 1); Granzberg, 1982; Greenberg, 1975; Hapkiewicz & Stone, 1974; Himmelweit et al., 1958; Kniveton & Stephenson, 1975; Kuhn, Madsen, & Becker, 1967; Leifer & Roberts (Experiment 1),

1972; Liebert & Baron, 1972; Lovaas, 1961 (Experiment 3); Mussen & Rutherford, 1961; Schramm et al., 1961; Singer & Singer, 1981; Stein et al., 1972; Steuer et al., 1972; Williams, 1980; Wotring & Greenberg, 1973). Only one of these (Liebert & Baron, 1972) provides any evidence against such an interpretation, and the authors' argument does not completely rule out the excitement hypothesis.²

A second source of treatment construct irrelevancies appears among the eight analyses (Eron, 1963; Furu, 1971; Granzberg, 1982; Himmelweit et al., 1958; Schramm et al., 1961; Singer & Singer, 1981; Stein et al., 1972 (Analysis 2); Williams, 1980) which use amount of TV viewing and/or preference for violent TV programs as the predictor variables representing exposure to televised violence. It is reasonable to expect that both high-frequency television watchers and those whose favourite programs are violent will end up watching more violence on television than those who watch little TV and prefer to watch nonviolent television. However, high-frequency viewers don't necessarily ever watch violent television, and may be exposed to a lot of television "treatment" that

²Liebert and Baron have rejected an arousal effect (pp. 189-190) as an explanation for their results because analyses of helping responses did not yield the same results as the analyses of the hurting responses. In addition, they found that helping and hurting responses had a correlation of $-.24$. However, their results do not entirely rule out the possibility that film-generated arousal "energized" whatever responses particular subjects would be predisposed to make in a situation where aggression and altruism were response alternatives. Slight support for this possibility is provided by the finding that girls' helping responses were affected by the television conditions. Older girls helped more after viewing violent content, and younger girls helped more after the nonviolent program, a fast-moving track meet. (An arousal hypothesis is supported only if we can assume that girls are predisposed to help and boys to hurt, and that the two programs were differentially arousing for children in the two age groups, or some other such unparsimonious set of assumptions).

is irrelevant to violence. Preferring violent television also carries with it some "excess meaning" as a measure of mere exposure to violent content.

Irrelevancies may confound the aggression measures as well. Eight of the studies (Cameron & Janky, 1971; Ellis & Sekyra, 1972; Hanratty et al., 1969; Himmelweit, et al., 1958; Huston-Stein et al., 1981; Kniveton & Stephenson, 1975, on the nonviolent competitiveness measure; Siegel, 1956; and Singer & Singer, 1981) included rough treatment of toys or nonaggressive assertive behaviour in the same score as inter-personal harm-doing, so that a purer aggression score was impossible to retrieve.

3. Hypothesis guessing. Howitt and Cumberbatch (1975) have argued that the laboratory procedures typically used to demonstrate the disinhibitory effect of TV violence on children's aggression are infested with demand characteristics which may account for the results. They suggest that children may increase their aggression in novel situations, where there are no established norms to guide their behaviour, because they are informed by the introduction of the violent television stimuli that aggressive behaviour is expected. Results of the experiment by Kniveton and Stephenson (1975) provided some support for this argument. They found that children were more "competitive" (a response class which included both aggressive and nonaggressive assertive behaviours, however) after viewing televised fighting between children only if they had not had a previous play session in the experimental room.

Cook and Campbell (1976, p. 243) caution against an overenthusiastic use of this criticism. They note that it is very difficult to determine when a demand characteristics explanation is plausible, or how subjects will behave in the presence of strong demand characteristics. They

concede that the danger is considerably greater when subjects have the opportunity to compare notes about the experimental treatment. Once dubbed the "scuttlebutt effect" (Taub & Farrow, 1973), the practice of returning "experienced" subjects to classrooms or play areas to mix with "naive" subjects has been shown to produce spurious results even with very young children, when subjects are individually debriefed after experimental participation (White & Le Huray, 1975). When immediate individual debriefing is not carried out, subjects may still arrive at a "consensually validated" hypothesis about what is expected of them, and this systematic bias could either produce a spurious effect or mask an effect that might otherwise have been measurable. A scuttlebutt effect may also pose a problem for statistical conclusion validity, as has been noted in Table 1. Conflicting rumours about the experimental procedures might lead to subjects entering the experiment in widely varying states of expectation, excitement, and apprehension. This would increase random heterogeneity of subjects and hence inflate experimental error.

If we know little about effects of demand characteristics in general, we know least about their effects on children. White and Le Huray's experiment, in which children actually knew what "right answer" the experimenter wanted, provides us with practically all the knowledge that we have. The issue of demand characteristics has been fought in the arena of weapons effects for the most part -- although one recent study (Perry, Roots, & Perry, 1978) has challenged film violence effects as well -- and always with adolescents and young adults. At that, the matter is far from settled.

Keeping in mind Cook and Campbell's cautionary advice, and the fact

that children are relatively unsophisticated about psychological experiments, this author has faulted only studies which seemed extremely obvious in their apparent demand characteristics, particularly if older children served as subjects and if the procedure appeared to encourage a scuttlebutt effect. Ten such studies appear in Table 1.

In both of the studies by Banduara, Ross, and Ross (1963a,b), and in Liefer and Roberts' (1972) validation pretest, children first saw a film of an adult treating a Bobo doll roughly, then were sent to play in a room that just happened to have a Bobo doll in it. A preschooler (even a Stanford university preschooler) might accept such a coincidence happening once, but if the local playroom scuttlebutt indicated that, at some time of the day (week?) or another, most kids would be taken through the same procedure, it might become very obvious that the grown-ups expected something to happen. The experimental results may have depended on just what the expectation was guessed to be. Both studies by Hanratty and her colleagues (Hanratty et al., 1969; Hanratty et al., 1972) and the experiment by Kniveton and Stephenson (1975) were even more obtrusive in their procedures: Children walked out of the TV room into a setting exactly like the one they had just seen in the film.

Lack of procedural subtlety also characterizes the studies by Collins and Zimmerman (1975), Liefer and Roberts (1972, Experiment 1), and Feshbach (1972). In the former two experiments children were questioned after viewing, but before "aggressing", about how good or bad the hero in the TV program was. It is possible that this procedure sensitized subjects to experimental expectations, especially since older children were included in the sample (ages ranged from 7 to 13) and no attempt was reported to avoid a scuttlebutt effect. However, since the

only effect demonstrated by Collins and Zimmerman was on the most subtle measure (duration of pressing a "hurt" button), cooperation with demand characteristics is a poor explanation for their results. It remains plausible as an explanation for Leifer and Roberts' Experiment 1 results, nevertheless. Leifer and Roberts may have sensitized their Experiment 4 subjects to expect changes as well, this time by pretesting them (about 18 days in advance) on the response hierarchy, then including six new TV program-related questions on the post test. Similarly, Feshbach's (1972) subjects may have been sensitized by the administration of a mood scale before and after TV viewing. His subjects, too, were older children (aged 9-11) with plenty of opportunity for classroom scuttlebutt. Feshbach's noise intensity measure was not unobtrusive like Collins and Zimmerman's duration measure, and only the most explicit procedure for communicating the reality/fantasy message (and therefore also for communicating mood-change expectations) yielded treatment differences.

Two correlational studies, Dominick and Greenberg (1972) and Greenberg (1975), seem to have left their hypotheses out in the open by administering a single paper-and-pencil measure with both the TV and aggression items on it. No explanatory cover story was reported in either study, and subjects were older children, aged 9 to 13.

4. Evaluation apprehension. When people know they are being studied, they may be less willing to respond in socially undesirable ways. Aggression is generally considered quite undesirable in children, so of course almost any study which failed to show a TV violence effect lies open to the interpretation that inhibitions against aggression were too high to be overcome by the disinhibitory influence of televised violence. That is a matter of external rather than construct validity,

unless the procedure includes features which might actually produce inhibitions, yielding artifactual results of "no effect." Only one experiment in Table 1 appears to have been conducted in such a way as to raise evaluation apprehension. The failure to find any effect (or indeed any response at all) on the "violent fighting" measure of Kniveton and Stephenson's (1975) experiment may be due to evaluation apprehension. Before the experiment began, the kindergarten boys who served as subjects for that investigation were shown the one-way mirror set-up and the observers who would be watching them play.

5. Experimenter expectancies. Only one study is seemingly vulnerable to the danger that the experimenter's expectancies were responsible for the treatment differences in subjects' behavior. Lovaas (1961) used an experimenter who was well aware of children's treatment condition, and who had ample opportunity to bias results. She gave the child instructions about operating both toys at the beginning of the "play" period, told him/her to begin playing with the doll ("aggressive") toy, then two minutes later could remind the child that (s)he could play with both toys. In this unusual social situation (e.g., lever pressing to operate TV, required order of playing with two toys), one could expect the child to be very sensitive to subtle voice cues from the experimenter. The positive results in Experiment 3 might be attributable to experimenter expectancies.

6. Confounding levels of constructs and constructs. The question of whether a demonstrated treatment effect occurs at various levels or intensities is a question of external validity. But when a "no effect" conclusion is made, Cook and Campbell treat it as a construct validity matter. When very low intensities or durations of TV violence fail to produce group differences in aggressive behavior, the "no effect"

conclusion should be qualified by the level of treatment employed in order to be construct-valid. Six of the studies which showed no facilitative effect of TV violence on aggression operationalized TV violence at very weak levels. All used very short ($2\frac{1}{2}$ to 5 minute) exposures, especially made for the experiment and not especially violent. Two featured an adult playing roughly with a Bobo doll (Bandura et al., 1963b; Kuhn et al., 1967). Three showed an adult (Hanratty et al., 1972) or a child (Hanratty et al., 1969; Savitsky, Rogers, Izard, & Liebert, 1971) hitting, gesturing at, and pretending to shoot at an adult dressed as a clown. Kniveton and Stephenson (1975) used a $4\frac{1}{2}$ minute film clip of 6 and 7 year old boys fighting over a road race set. A seventh study (Thomas, 1972) did not include any description of its 6 minute "aggressive TV" condition, so it is unknown how strong or weak the violence exposure was.

7. Generalizing across time. Three of the studies just mentioned (Hanratty et al., 1969; Hanratty et al., 1972; Savitsky et al., 1971) paired very short "violence" exposures to very short post-exposure observation periods. The remarkable similarity in all three studies of the film setting and the post-film play room may have "demanded" immediate imitation, to the exclusion of nonimitative responses. Had a longer observation period been used, a general disinhibition effect might have shown up as nonimitative responses re-emerged. On the other hand, the study by Moriarty & McCabe (1977) can be faulted for quite the opposite shortcoming. As the authors themselves pointed out (p. 144) they measured aggression 24 hours after program viewing, thus potentially missing any immediate effects of the television condition.

Williams (1980) has cautioned that the increase in aggression that

she measured in her experimental community may be a novelty affect, which will subside once television has been available for a longer period of time. This implies that television violence may not affect aggression most of the time, but only when viewing it is a special, exciting or disruptive event. Williams concluded this because children in communities with longstanding access to television were no more aggressive than children in the experimental community before it received television service. After TV, the experimental community was significantly higher in average aggressiveness than the other communities. It should be kept in mind, however, that two years had passed between the advent of television and Williams' post-TV data collection. One might expect the novelty to have "worn off" somewhat by that time.

8. Interaction of procedure and treatment. In three of Table 1's studies, the treatment effect may well have been due, not to the treatment alone, but to the interaction of the treatment with some element of the procedure. The author has already expressed her opinion that Collins and Zimmerman's results were not due to hypothesis guessing within experimental conditions. A more plausible explanation is that the questions about the aggressive hero's intentions and motivations carried with them an implied message that aggression can be good or is always bad (depending on the film condition) and that this is the reason for the positive effect in the mixed-motives condition. The film alone may have been insufficient to produce the effect.

Either the failure to find an effect or the later (Experiment 3) positive effects reported by Lovaas (1961) might have resulted from procedure x treatment interactions. The interruptions in the television stimulus, requiring subjects to press a lever every 10 seconds to continue

the cartoon, may have so distracted Lovaas' subjects that they failed to receive the full impact of the televised violence in Experiments 1 and 2. Subjects who served in Experiment 2 were "recycled" into the other television condition for Experiment 3, and it was with these experienced subjects that a positive effect was finally reported. Perhaps these subjects were less distracted by the now-familiar lever-pressing task. On the other hand, the positive effect may have been the artifactual one. Hapkiewicz and Roden (1971) cite an unpublished study which led them to attribute Lovaas' results to the instructions he used. It does seem quite possible that the instructions newly introduced in Experiment 3, requiring subjects to play with any toy they wanted but to start with the (aggressive) doll toy, might have interacted with the treatment to produce the reported results.

The failure to find a television effect among Kuhn, Madsen, and Becker's (1967) frustrated subjects may also be attributed to a treatment x procedure interaction. The frustration manipulation involved having the Experimenter scold children and take away their candy for "not paying attention". As the authors remarked, this might have raised such strong behavioural inhibitions that an otherwise detectable treatment effect was blotted out.

IV. Threats to External Validity

External validity is the generalizability of research findings "to or across times, settings, and persons" (Cook & Campbell, 1976, p. 234). In the case of media-aggression research it is also very important to know if results generalize to television material now available to the public and to typical aggressive responses. There is

definitely some overlap of the other three types of validity with external validity. If the treatment did not really cause the effects (internal invalidity) or if the researcher was mistaken about whether the effects occurred (statistical conclusion invalidity), it is unlikely that the treatment will cause that effect under different conditions. In most cases, threats to construct validity also threaten external validity. Effects on responses which are not aggressive might not be generalizable to the responses of interest, which are aggressive. Furthermore, effects which result from hypothesis guessing, evaluation apprehension, experimenters' expectancies, or treatment x procedure interactions would not likely happen again following television violence viewing, because the right combination of accompanying variables would be absent in other times and settings. By contrast, violent television material is almost invariably exciting, so the excitement/content confound poses no real threat to external validity.

In spite of the overlap of the four types of validity, some problems exist which pose a unique threat to external validity. These are the only ones listed in Table 1 as "Threats to External Validity".

1. Generalizing across treatment constructs. Some studies have been criticized (Klapper, 1968; Kaplan & Singer, 1976; Noble, 1970; Roberts, 1973; Singer, 1971) because they used as stimulus materials brief, specially prepared presentations of simple acts performed in a social vacuum. Eleven studies in Table 1 (Bandura et al., 1963(a), 1963(b); Biblow, 1973; Hanratty et al., 1969; Hanratty et al., 1972; Kniveton & Stephenson, 1975; Kuhn et al., 1967; Leifer & Roberts, 1972 (pretest); Savitsky et al., 1972; Thomas, 1972; Wotring & Greenberg, 1973) employed such unrepresentative materials and hence have questionable

relevance to the issue of television's actual effects. Moreover, it can always be argued that such investigations merely demonstrate that psychologists can develop harmful materials, not that actual television programming is dangerous. Studies which use real television programs are obviously more convincing and relevant to the issue.

2. Generalizing across effect constructs. There are four studies in Table 1 which have construct-valid measures of aggression (that is, they appear to be measures of intentional harm-doing behavior) but which are not at all representative of the means by which children usually aggress against others. Two of these used "help/hurt" machines (Collins & Zimmerman, 1975; Liebert & Baron, 1972) and two used modifications of the Buss Aggression Machine (BAM) which ostensibly delivers noise to a victim (Feshbach, 1972; Thomas, 1972). The external validity question for these studies is whether their results will generalize from mechanical aggression like this to the typical aggressive behaviours of actual social concern.

3. Setting x treatment interactions. A number of the studies in the literature can be faulted because they were conducted in settings so unique and novel to the children involved (e.g., psychology laboratories, trailers especially designed to match the TV program's setting) that one can seriously doubt whether behaviour found there would generalize to the "real world" beyond (Howitt & Cumberbatch, 1975; Howitt & Dembo, 1974; Kaplan & Singer, 1976; Klapper, 1968; Kniveton, 1974; Kniveton & Stephenson, 1970; Roberts, 1973; Singer, 1971). Four studies in Table 1 (Hanratty et al., 1969, Hanratty, et al., 1972; Kniveton & Stephenson, 1975; Savitsky et al., 1971) had children walk out of the television-viewing room straight into the same setting featured in the television

program. Beyond the previously discussed dangers this poses to construct validity, there is an additional external validity problem. As Liebert et al. (1973) have pointed out, chances of such a thing happening in real life are so small as to be negligible. Two studies (Kniveton & Stephenson, 1975; Liebert & Baron, 1972) transported children across town or even from different towns to psychology labs, where it was clear that their behaviour was being studied by interested adults. No cover story was reportedly used to distract the child's attention from this fact, or to make the situation more similar to typical day-to-day behaviour settings children find themselves in. Whether such "special occasion" behaviours would generalize to settings children are usually in is indeed questionable. Finally, the social situation employed by Lovaas (1961) defies generalization to normal settings. Children were taken to a special room or trailer "to play some games and look at a movie", found themselves lever-pressing every 10 seconds because the "movie" kept fading out, then engaged in heavily supervised lever-pressing with one or two toys.

Lack of generalizability to non-experimental times and settings is a major problem because it implies that television violence may not be harmful in the hands of the general public, but merely in the hands of psychologists, who can develop harmful materials and use them to direct the experiment-specific behaviours of their bewildered subjects.

Summing Up: Converging Scientific Evidence?

The reader will have noticed, by now, that we are dealing with a somewhat flawed scientific literature. What can one learn from more than a score of flawed studies? Liebert (1972, pp. 28-29) says that

while every study has its flaws, the many studies which have been done-- each with its own unique flaws--serve as replications or cross-validations of one another. That is, each demonstrates that their common results are not due to the particular methodological problems of the others.

This proposed strategy of "multiple operationism" (Webb, Campbell, Schwartz, & Seechrest, 1966) is sensible, in an area of research with many built-in difficulties. Granting this, however, does not automatically lead to Liebert's conclusion: The flawed studies do not necessarily have a common result. Of the 37 studies in Table 1, 22 reported at least one positive effect of television violence on aggression. Seven of the 37 studies reported at least one negative effect, and 31 of them reported at least one instance of no effect. Twelve of the studies failed to find any effects of TV violence on nonimitative aggression. Even this degree of convergence toward the conclusion that television violence facilitates aggression may be an overestimation. Kaplan and Singer (1976) as well as others (e.g., Greenwald, 1975; Roney & Zenisek, 1980) have pointed out that a strong bias exists against non-significant differences ever being reported in the literature. Faulty methodology may lead to both false confirmations and false disconfirmations of the null hypothesis, but this editorial double standard in what is deemed publishable may well produce a literature which is biased toward statistically significant findings even if the null hypothesis is, in general, true.

This long, critical look at the literature may have made it painfully obvious to the reader that Liebert's (1972) assessment of the literature is unduly optimistic. The pattern is of overlapping, not

unique, flaws. While individual studies do appear to have quite unique sources of internal invalidity, many studies share the problems which threaten statistical conclusion validity, external validity, and especially construct validity. If one chooses to consider only internally valid, statistically sound, construct valid and generalizable studies, following Campbell and Cook's (1976) analysis, reference to Table 1 will reveal that there simply aren't any. Furthermore, there are still none if we drop the requirement of generalizability.

One source of the confusion in the literature may be that, as Meyer and Anderson (1973) have suggested, "any given media effect depends on what type of content produces what kinds of effects on what kinds of people under what conditions" (p. 448). With this in mind, Table 1 was re-examined in an attempt to isolate the subject and situation variables which might mediate a possible television violence effect on aggression.

1. Gender. Eighteen studies in Table 1 reported a positive effect on at least one measure for both boys and girls, although in seven of these studies no gender comparisons were reported, so the effect may have been "carried" by subjects of one gender. Three studies had a positive effect on at least one measure for girls only, while seven had at least one positive effect for boys only (two of these had no girls in their study at all). Four studies found negative effects for both boys and girls (two of these included no gender comparisons). One study found a negative effect for girls only, and two found a negative effect only for boys (one of these studied only boys).

Thus, while it appears that in some instances boys and girls may respond differently to television conditions, accounting for subject

gender does little to clear up the confusion in the literature.

2. Age. Singer and Singer (1981) have suggested that preschool children may be particularly vulnerable to the effects of televised violence. Eron has said (1982) that the age of 8 may be an especially critical time for television to have its effects. Does the literature make more sense when we analyze it by age groups?

Nine studies in Table 1 reported at least one positive effect of TV violence on at least one aggression measure for preschoolers. Eleven studies also reported at least one positive effect for primary school age children (grades 1-3). Ten reported positive effects for elementary school age children (grades 4-6). But two studies also reported at least one negative effect on preschoolers, four for primary children, and three for elementary school children. Apparently, age (within the limited range defining "children") does not determine what effect television violence has on the viewer's general aggression level.

3. Socioeconomic status. Many studies used a mixture of social classes in selecting their samples, but unfortunately the effect of socioeconomic class is generally not documented. Of studies which provide the necessary information, four reported at least one positive effect for middle-class subjects while two reported at least one positive effect for working-class subjects. However, negative effects were reported for middle-class subjects in three studies and for working-class subjects in no studies. With such incomplete information one hesitates to draw any conclusions about socioeconomic status as a mediator of TV effects on aggression. So far, however, middle- and working-class subjects tend to differ only in that no negative effects have as yet been reported for working-class subjects.

4. Characteristic level of aggression. Only two studies in Table 1 have reported their results according to subjects' characteristic level of aggression: Leifer and Roberts' (1972) Experiment 4 and Stein, Friedrich, & Vondracek's experiment. Leifer and Roberts reported a positive effect for low-aggressive subjects and a negative effect for their highly aggressive subjects on the response hierarchy. Stein et al. reported a positive effect for highly aggressive subjects exposed to violent TV compared to neutral TV but none compared to prosocial TV shows. Low-aggressive subjects showed no effect, regardless of which control group comparison was being used. If initial aggressiveness does influence television effects it is presently impossible to tell just what that influence is.

5. Reality/fantasy orientation. Eron's correlational study found that peer rated aggressiveness was positively related to the belief that television violence reflected real life. The relationship was significant only in Poland, however, and must be treated as a "noneffect" in the other countries. In Eron's intervention study with American children, though, continued belief in the realism of television violence was one of the variables which predicted failure of the intervention procedure in reducing high violence-viewers' aggressiveness.

Feshbach (1972, Experiment 1) and Hapkiewicz and Stone (1974) manipulated reality/fantasy orientation. Both studies found increased aggression following TV violence for subjects who were in the reality condition. Feshbach's fantasy subjects were less aggressive after the TV violence. Hapkiewicz and Stone's subjects remained unaffected. It is difficult to know what interpretation to make of this, since Hapkiewicz and Stone's reality condition ("The Three Stooges") was considerably less

realistic than Feshbach's fantasy condition (a "Hollywood movie" about a campus riot). Additionally, many positive effects have been reported by researchers who used undeniably fantasy oriented television violence (e.g., Ellis & Sekyra, 1972). The best guess one could base on the information is probably that subjects who take a "reality" orientation to televised violence are more likely to demonstrate a positive effect than are subjects who take a "fantasy" orientation.

6. Frustration. Of the 22 positive effects reported in Table 1, four were associated with a procedure to deliberately frustrate the subjects (Bandura, Ross, & Ross, 1963(a); Hapkiewicz & Stone, 1974; Kniveton & Stephenson, 1975; Wotring & Greenberg, 1973). Mussen and Rutherford's frustrated subjects also showed a positive effect, but their unfrustrated subjects showed just as strong an effect.

Besides the positive effect found for Mussen and Rutherford's unfrustrated subjects, there were also positive effects found in six studies which did not include any attempt to frustrate subjects (Collins & Zimmerman, 1975; Dominick & Greenberg, 1972; Feshbach, 1972; Greenberg, 1975; Liebert & Baron, 1972; Lovaas, 1961, Expt. 3). Kuhn, Madsen and Becker (1967) found a positive effect only with their unfrustrated subjects.

None of the negative effects reported in Table 1 came from a study in which children were intentionally frustrated, and four studies which failed to show any effects used purposely frustrated subjects. Clearly, then, there is no good reason to suspect that it is frustration that separates the effects from the noneffects in the literature.

Theoretical Considerations

From the foregoing review of the empirical evidence, one could conclude that we still do not know what the effects of television violence are upon children's aggression. It may be premature, then, to pose the question of why television affects aggression. Nevertheless, a number of theoretical explanations have been offered to account for both positive and negative effects.

Disinhibition

The term "disinhibition" implies that aggression increases because the inhibitions which usually restrain a person from engaging in socially unacceptable behaviour are reduced when the person watches a model engage in such behaviour. One of the best known proponents of a disinhibition effect, Albert Bandura, describes the effect thus:

Given appropriate instigation, responses will be performed more rapidly, more strongly, or more often when behavioral inhibitions are weak than when they are strong, either because televised depiction of rewarded or unpunished aggression informs the viewer that aggression is expected in situations resembling the one depicted, or because fears and anxiety are vicariously extinguished (Bandura, 1973, pp. 129-130).

On the other hand, seeing punished aggression depicted on television would be expected to decrease aggression by raising inhibitions. Either inhibitory or disinhibitory effects would be enhanced if the viewer perceived his/her own outcome probabilities as being similar to those of the television aggressor. The frequency with which the "good guys" save the day on television by outaggressing the "bad guys" has led Bandura and others (Larson, Gray & Fortas, 1968; Liebert et al., 1973;

Wertham, 1974) to predict that television violence has, on balance, a general disinhibitory effect on children's aggressiveness.

According to Bandura (1973, p. 133) justified violence is particularly disinhibiting because it legitimizes aggressive behaviour as a response to instigation. Other conditions which are thought to enhance television's disinhibitory effect are frustration or other sources of emotional arousal (since these are disinhibitory themselves) and the presence of environmental cues which communicate permissive norms about aggression (Bandura, 1973, pp. 126, 137-138, 167).

Elicitation

Although he has not published any research on children's aggression, Leonard Berkowitz's first statement on the matter (1962, pp. 238-253) was that

depicted aggression was a cue stimulating hostile tendencies within children, and as a result, they were readily instigated to overt aggression in a subsequent situation ... In most instances, of course, there is only a very mild arousal [of hostile tendencies] resulting from the fantasy cues. But this moderate drive state can be strengthened so that overt behavior occurs if there are appropriate stimuli in the later situations. (p. 238)

Among the mediating factors Berkowitz identified as governing the probability of hostile actions were:

- 1) the strength of the subject's aggressive habits (positive effect)
- 2) the degree of association between the fantasy situation and
 - (a) the situations in which hostile habits were learned, and (b) the postfantasy setting (positive effects). Identification with the aggressive character on television ("putting oneself in the aggressor's place" (p. 246)) and special relevance of the television situation to the viewer's

needs were believed to influence the associational richness of cues between televised violence and the postfantasy situation, through improved attention and memory. Identification and relevance were also thought to increase the viewer's emotional arousal (see also Turner & Berkowitz, 1972). Perceived reality of the televised aggression was expected to influence the association between the depicted violence and both learning and postfantasy situations. Degree of discounting the event as "not true" was thought to weaken these associations.

3) intensity of the guilt and/or aggression anxiety also aroused by the fantasy violence (negative effect). Depiction of the aggression as justified was expected to reduce guilt and anxiety (see also, for example, Berkowitz & Geen, 1967). Berkowitz later (1970) cautioned that "The role of thinking must not be exaggerated, however. Impulsive behavior is not carried out with deliberation and forethought. It bursts forth, relatively free of control by intellect and cognitive processes" (pp. 132-133). It was Berkowitz's (1962) opinion that children had low guilt and anxiety about aggression, anyway (p. 239). While he agreed that disinhibition is important to the performance of aggression responses, Berkowitz has reinterpreted many so-called disinhibition effects as actually being due to elicitation of aggression by environmental cues (Berkowitz, 1962, 1970; Turner & Berkowitz, 1972).

4) **whether** the viewer is "set to aggress" by anger (positive effect). Berkowitz's revised frustration-aggression hypothesis (e.g., 1969) gave anger a central priming function as the emotional reaction to frustration. Viewing of violent television also had a priming function, requiring the presence of cues, however, for it to elicit the aggressive response. In 1969 (p. 18), Berkowitz suggested that the emotional state

resulting from frustration could be an internal eliciting cue. Berkowitz stated, initially, that some aggressive responses, such as the imitative aggression of young children, did not require anger. Later (1974), he described most aggressive behavior as having both instrumental and impulsive components. Only the impulsive components were considered to be affected by anger. Impulsive components were described as having the greatest contribution when responses were quick and not too deliberate. On the Buss Aggression Machine (Berkowitz's usual aggression measure) Berkowitz regarded the vigor and duration of button-presses as being largely determined by impulsive components, although frequency in a short time space and choice of intensity level, if made quickly, were also considered largely impulsive. However, Berkowitz's position in 1974 was that anger was only one of many types of physiological arousal which could energize impulsive aggressive responses.

In spite of the theoretical differences between disinhibition and elicitation, the post-viewing consequences of the two processes on observable behaviour would be virtually identical. In Berkowitz's words (1970, p. 104) "it is obviously difficult if not impossible to distinguish between elicited and disinhibited reactions". If in fact there is a meaningful distinction to be made, this is certainly a problem for the theoretically-oriented, but there does seem to be one way of distinguishing between the two behaviourally because of the different function of situational cues in the two approaches. Bandura has articulated this difference. Under a disinhibition effect, "[T]he mere presence of aggressively valenced cues ... will facilitate aggression if presented in ways that convey permissive or expected reactions toward such behavior" (Bandura, 1973, pp. 137-138)--in short, only if they are presented as informative

clues about normative expectations in the postviewing situation. Disinhibition would not be enhanced by the presence of cues associated with television violence unless these cues appeared to be signals from the experimenter that aggressive behavior was expected. Elicitation would occur even if the presence of the cue was presented as coincidental.

Catharsis

Feshbach (1976) has recently discussed five possible mechanisms by which a cathartic, or aggression-reducing, function might be served by televised violence:

The fantasy experience provided by some television programs with aggressive content can control or reduce aggressive acting-out behavior because the fantasy provides a substitute for aggression toward the actual target (unlikely), because it provides an opportunity for the expression of anger, because it functions as a cognitive control, because it is satisfying and enjoyable, and because it may facilitate new insights and cognitive reorganization (the latter unlikely given the current state of TV fare) (p. 84).

Several reviewers (Bryan & Schwartz, 1971; Cater and Strickland, 1975; Goranson, 1970, 1977; Krebs, 1973; Weiss, 1969) have argued that the "catharsis hypothesis", as it pertains to television violence, is almost entirely without support. The present author's review of the literature was consistent with this lack of support for a cathartic effect. However, since no position is very impressively supported by the available empirical evidence, it may be unwise to discard the catharsis hypothesis for the time being.

According to Feshbach, the dimension separating cathartic and disinhibitory effects of violent television is reality/fantasy. Realistic violence is disinhibitory, he argues, while fantasy violence is cathartic.

He describes the dimension as pertaining more to the person than to the violent material itself, so that a child who takes a reality orientation to television violence would be more aggressive afterwards and a fantasy-oriented viewer would be less aggressive, regardless of what orientation the violent program itself had.

Proponents of the three theories discussed here would argue that any or all of them may have effects in the real world outside of laboratory studies. Empirical evidence suggests that perhaps none of them do. Comparing the three hypotheses about television effects appears to require a design in which both cues and violent content are manipulated. In addition, the theoretical underpinnings of the catharsis effect (at least in most of its current versions) require that subjects be provoked prior to watching violent television. Children's reality/fantasy orientation to the programs might also be considered as well, to test Feshbach's contention about cathartic and facilitative effects. Children's characteristic level of aggression should be measured, as a potentially important mediator of Berkowitz's hypothesized elicitation effect. Identification with (in Berkowitz's sense of "putting oneself in place of") the aggressive television characters might also be measured, since it is expected to increase all of the hypothesized effects. Since the elicitation hypothesis applies to "impulsive" aggression, aggression might be profitably measured both a) immediately after the implementation of the treatment and b) over a longer period of time.

Hypotheses

The theoretical positions just outlined would lead to the following propositions about the relationship between televised violence and aggression:

1. Disinhibition. (a) Subjects who are exposed to violent television are expected to be more aggressive, afterwards, than those who are not, regardless of the presence or absence of cues. (b) Moreover, identification with the aggressive protagonist is expected to increase this effect, as is (c) the belief that the portrayed violence "really happened".

2. Elicitation. (a) The presence of cues related to the television violence is expected to produce significantly greater aggression, in subjects exposed to violence, than exposure to the same violence without cues. It is predicted that the effect will be stronger for those who (b) are characteristically high in aggressiveness, (c) identify with the aggressive television protagonist, and (d) believe that the televised violent events really happened. (e) It is thought that the effect will be stronger when measured immediately after the opportunity arises to behave aggressively (more "impulsive" aggression).

3. Catharsis. (a) It is predicted that, among violence viewers, reality oriented subjects will be more aggressive than fantasy oriented subjects. (b) Among fantasy oriented violence viewers, those frustrated before exposure to the television material are expected to be less aggressive than those frustrated after watching TV. (c) Viewers of nonviolent material are not expected to show any differences due to reality/fantasy orientation. (d) Identification with the aggressive

protagonist, it is thought, will lessen the aggression of fantasy viewers even further.

The remainder of this dissertation describes an attempt to test these hypotheses. Some care was taken, in the running of this study, to avoid the problems which --as noted above -- have compromised the validity of most of the previous "findings" in this controversial area. Specifically, an experiment was conducted, with random assignment of subjects to television and frustration conditions. This dealt with most threats to internal validity. The dependent variables were measured by trained raters, using a simple reporting procedure and a standard, explicit definition of aggression. These procedures minimized implementation problems. Groups of subjects from all conditions were interspersed randomly in the sequence of measurement, so that any change in the raters over time (e.g., fatigue or improved reliability) could not bias the results for any treatment. Similarly, no local history problem was likely, since 11 groups were run in each treatment condition, from different schools and on different days. There was no diffusion of treatment because subjects' exposure to the treatment was completely controlled by the experimenter. Careful pilot testing, to find equally popular treatment and control material, prevented resentful demoralization of controls.

Type I error was avoided by adopting a per family error rate of .05. Exact probabilities were reported for those who prefer to use a different level or type of error rate protection. A total of 396 subjects were run, in 66 groups, allowing a .81 probability of detecting the main effect of television condition, if it was .5 standard deviation units or larger. The interaction effects generally had greater power.

A number of steps were taken to maximize construct validity. Aggression was defined as an act which intentionally delivered interpersonal

harm. Accidental actions were not included. The television material was sampled from television programs which had actually been broadcast in the time period just prior to the experiment. Subjects were led to believe that the collection of data was part of a separate study, to reduce hypothesis guessing, and an involving procedure high in experimental realism for the subjects was used. The observers and the referee who ran the data collection phase of the study were blind to the hypothesis and to the subjects' condition. A reasonably long (14 minute) television exposure was employed, and subjects' aggression was measured in such a way that both immediate and somewhat delayed effects could be measured.

External validity was boosted in this study, not only by employing actual television programming (although in edited form). The aggression measures were also based on tallies of actions which are common among elementary school boys, actions such as pushing or kicking during a game of floor hockey.

PILOT STUDIES

Choosing the Television Materials

The first task of the pilot work was to find violent and nonviolent television programs that were equally exciting and well liked. The importance of equal excitement level has been discussed at length in reviewing the literature. It is also essential that the programs be equally well liked. Watching an uninteresting television program may be frustrating, and hence aggression enhancing. A relatively uninteresting program may also produce less attention and a lower tendency to identify with the protagonist. Any of these factors could affect aggression levels and confound the effects of the violent content.

Pilot Study I: Sampling the Range of Possibilities

Method. Letters were sent home with the second and third grade boys from a Winnipeg school, asking parents' permission for their sons' participation in a study of "what children get out of watching TV." (See Appendix A for a copy of the permission letter.) The parents of 65 boys gave their permission. The experimenter met with subjects in their classroom groups. Each received a two-page response booklet (See Appendix B), and the following instructions were given:

I want to ask you some questions about TV shows. There's a booklet on your desk in front of you. In a minute--but not yet--I'm going to ask you to turn it over and answer some questions for me. When you answer these questions I want you to be very quiet, and look only at your own page. I don't want any boys to look at anyone else's answers. Can you remember to do that for me? If you have any questions to ask, put your hand up and I'll come over to answer your question.

Are you ready to turn your booklets over? All right, then,

turn them over.

The first thing I want you to do is to mark down how old you are at the top of the page ... right here [demonstrate].

Now on this page, I would like you to print the names of the three TV shows that you like to watch the best. Print the name of your very favourite show--the one you like to watch more than any other shows--first, beside the number 1. When you have finished that, then print the name of the one you like next best beside the number 2. Then print the name of the show you like next best after that one, beside the number 3.

When you have finished printing the three names of the three shows you like the best, put your head down on the desk and take a little rest, so I'll know you're finished. Don't turn over your page when you're finished. Just leave it the way it is on your desk.

[Assist questioners, roam while scale is being completed. When heads are all on desks ...]

Now you may turn over the page.

On this page I want you to print the names of the three shows that are the most exciting shows you watch on T.V. Now, that's not quite the same thing as what shows you like the best. What I mean this time is what shows get you excited while you're watching them?

Print the name of the very most exciting show you watch beside the number 1. Print the name of the next most exciting show beside the number 2. Then print the name of the show that's next most exciting after that beside the number 3.

When you've finished, just put your head down on the desk for a little rest, like you did before. Leave your page just the way it is, on your desk.

[When all have finished, collect booklets.]

Results and Discussion. The top five nominees, in order of popularity, were as follows:

<u>Favourite Shows</u>	<u>Most Exciting Shows</u>
1. "Godzilla"	1. "Batman"
2. "Spiderman"	2. "The Incredible Hulk"
3. "The Incredible Hulk"	3. "Battlestar Galactica"

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|----------------|---|
| 4. "Batman" | 4. Elvis (a "special" which had been shown the previous week) |
| 5. "King Kong" | 5. Hockey |

These lists provided no basis for choosing the television materials, for two important reasons. For one thing, all but one of the programs in the two lists were violent. The one nonviolent nominee, a documentary about Elvis Presley, made the "Top Five" list for excitement, but received only one nomination for most favourite program. Even if this program had been equivalent to the violent programs on both excitement and popularity, there was a second consideration. To use a documentary as a control for any of the top five favourite shows would have been confounding violence with realism. The most favourite shows were not only violent, but highly unrealistic. Three were cartoons and the other two were cartoon-like, involving superheroes ("Batman") and monster-like transformations ("The Incredible Hulk"). Given the theoretical importance of the reality/fantasy dimension, none of these favourite TV programs was really suitable even for the violent television material.

Pilot Study II: A Change of Tactics

Since the pilot subjects' nominations provided no good candidates for television materials, a new starting-point was sought. Earlier research (Josephson, 1976) had employed the television program "S.W.A.T." as the violent program. Anecdotal reports indicated that the program had been very popular with boys. It had the additional advantage of being less obviously fantasy than the television programs nominated in Pilot Study I. In Pilot Study II, then, an excerpt of "S.W.A.T." was prepared and shown to subjects, along with excerpts of nonviolent programs, to be tested as candidates for the nonviolent control program.

Method. Subjects were once again recruited by sending home permission letters with second and third grade boys, from another Winnipeg school. Forty-three boys served as subjects. (The letter is included in Appendix C.)

The television program clips included a sniping and revenge sequence from "S.W.A.T.", a nonviolent sequence about motocross bike racing from "C.H.I.P.S.", and a nonviolent rodeo action scene from "Happy Days". All had been recorded in colour on videotape and edited down to about 14 minutes in length. Commercials and "filler" irrelevant to the plot were discarded in the editing process, as were any aspects of the nonviolent candidates which might be interpreted as aggression (such as joking threats, or gestures that appeared aggressive). Viewing was done in the classroom, from a Sony CVM-1200U 12-inch colour television receiver and a Sony AV8600 colour videotape recorder. Subjects watched in classroom groups and were shown all three program clips, in counter-balanced order.

Before the showing of the first program, the subjects were given the following explanation:

I am at the school today to find out what boys your age think of some TV shows. Just watch the program I am about to show you. Afterwards, I'll give you a booklet so that you can mark down what you think of the show.

Remember, now, just watch the show as though you were watching TV at home. I just want to know what you think of it.

After each program clip was shown, a two-page booklet was handed out to subjects, with the forms on which subjects were to rate the TV materials. (These forms can be found in Appendix D). The experimenter then explained the rating task:

First of all, I want to know how much you liked this show. On the first page of the booklet I gave you, you'll see 5 cartoons of children's faces. Under the first face ... [READ THROUGH THE CAPTIONS UNDER EACH FACE, POINTING AT THE CORRESPONDING FACE ON A SAMPLE BOOKLET].

Now if you thought the program I showed you was great, I want you to make a circle around the number 1, under the first face ... [ETC., TO THE LAST FACE]. Now turn to the second page. I'm going to have you mark down how excited you felt when you watched the TV program I just showed you. If you felt very excited, make a circle around the number 1, under the first face ... [ETC., TO THE LAST FACE].

Results and Discussion. The mean liking and excitement scores for each television segment are presented in Table 2. "C.H.I.P.S." did not differ significantly from "S.W.A.T." on either scale ($F(1,42) = 0.2054$ and 0.0979 , respectively, $p < .65$ and $.75$, respectively). The differences between "S.W.A.T." and "Happy Days" did not reach traditional levels of significance, either, but could be considered "marginal" ($F(1,42) = 2.7379$, for liking, and 3.6720 , for excitement; $p = .1055$ and $.0622$, respectively).

Table 2

Mean Liking and Excitement Scores for T.V. Programs in Pilot Study II

Program	Liking	Excitement
"S.W.A.T."	1.33	1.44
"C.H.I.P.S."	1.26	1.40
"Happy Days"	1.09	1.19

Note: N=43.

On the basis of these results, "C.H.I.P.S." appeared to be an acceptable choice for the nonviolent control program. The pattern of liking and excitement scores introduced a new question, however. The mean liking

scores for each program were very close in value to the mean excitement scores. In fact, the correlation between liking and excitement was .83 for "C.H.I.P.S.", .66 for "Happy Days" and .67 for "S.W.A.T." It seemed possible that liking and excitement scores were largely measures of the same thing. Perhaps children did not really understand what was meant by the term "excitement", and used the liking score (which they always assigned first) as the basis for their excitement rating. To explore this possibility a third pilot study was undertaken.

Pilot Study III: A Change of Procedure

Method. Subjects were 68 second and third grade boys from two other Winnipeg schools. Boys were recruited by means of the same permission letter used in Pilot Study II. The same program excerpts from "S.W.A.T." and "C.H.I.P.S." were used once again, but a different episode of "Happy Days" was used. In the hope of finding a well liked but unexciting television clip, the highly exciting rodeo episode was replaced by an episode in which two main characters, "the Fonz" and Richie, relived several comical events from their shared past, in an attempt to explain the concept of "friendship" to a visiting spaceman, Mork.²

The procedure was the same as for Pilot Study II, with two changes. In this study, the excitement scale was always administered before the liking scale, and the following explanation was added to the instructions for the excitement scale:

²The author was at the time searching for a nonviolent and non-exciting television excerpt that could be used to test directly the excitation hypothesis about TV violence effects on aggression. The history of this unsuccessful search is outlined in Appendix E.

The first thing that I want to ask you is how excited you felt when you were watching the TV program you just saw. Now, that's not the same thing as how much you liked the show; I'm just interested in how exciting it was.

When you're excited, you might feel sort of jumpy inside. Your heart may beat faster and your eyes get real big.

Results and Discussion. The mean excitement and liking scores are reported in Table 3. This time, neither of the two other programs differed from "S.W.A.T." on either dimension. "C.H.I.P.S." had exactly the same mean excitement score as "S.W.A.T.", and differed at the .77 level on the liking measure ($F(1,67)=0.0846$). The new version of "Happy Days" differed from "S.W.A.T." at the .81 level on excitement ($F(1,67)=0.0588$) and at the .37 level on liking ($F(1,67)=0.3933$). The procedural change did little to reduce the correlation between liking and excitement scores. It was .61 for "C.H.I.P.S.", .80 for the new version of "Happy Days", and .70 for "S.W.A.T."

Table 3

Mean Excitement and Liking Scores for T.V. Programs in Pilot Study III

Program	Excitement	Liking
"C.H.I.P.S."	-1.41	1.41
"Happy Days" (II)	1.38	1.38
"S.W.A.T."	1.41	1.46

Note: N=68.

Pilot Study IV: A Change in the Measure

One more attempt was made to reduce the correlation between the two scores. The excitement measure was reversed to reduce the contribution of "response bias" in the two scores.

Method. Ninety-eight second and third grade boys were recruited at two more Winnipeg schools, using the same procedure as before. The episodes from "S.W.A.T." and "C.H.I.P.S." were used, unchanged, from the two previous pilot studies. "Happy Days" was replaced with an excerpt, of equivalent length, from "The Brady Bunch". This excerpt dealt with a young boy's first attempt to smoke a cigarette, and the moral dilemma faced by his siblings, who accidentally witnessed the event.

There was only one other change from Pilot Study III. The order of the excitement scale was reversed, so that answer number one corresponded to the "not at all excited" face. The "very excited" face corresponded to answer number four. The revised excitement measure can be found in Appendix F.

Results and Discussion. The mean liking and excitement scores are presented in Table 4. Once again, "S.W.A.T." and "C.H.I.P.S." had identical mean excitement ratings. Their liking scores were also extremely close ($F(1,97)=0.0220$; $p<.88$). The "Brady Bunch" segment was rated as significantly less exciting ($F(1,97)=58.3730$, $p<.0001$) and less well liked ($F(1,97)=44.2073$, $p<.0001$) than "S.W.A.T.". The correlation between liking and excitement was quite high for the "Brady Bunch" episode ($r = -.78$), but considerably lower for both "S.W.A.T." ($r = -.40$) and "C.H.I.P.S." ($r = -.48$).

Table 4

Mean Excitement and Liking Scores for T.V. Programs in Pilot Study IV

Programs	Excitement	Liking
"C.H.I.P.S."	3.83	1.18
"BRADY BUNCH"	2.91	2.15
"S.W.A.T."	3.83	1.19

Note: N = 98; Higher scores indicate higher excitement, lower liking.

Summary

In Pilot Studies II, III, and IV, 209 boys from five different schools consistently rated "S.W.A.T." and "C.H.I.P.S." as equally exciting and well-liked. The average excitement score in all three studies was always close to "Very Exciting". The average liking score in all three studies was always close to "It was great!"

Although the consistency was welcome, there remained a question of the scales' ability to discriminate where they should. Pilot Study IV provided answers to that nagging question. Both scales showed significant differences between the "Brady Bunch" and "S.W.A.T.". In addition, by Pilot Study IV, procedural and measurement changes had reduced substantially the correlation between liking and excitement for C.H.I.P.S. and S.W.A.T., without noticeably affecting the average scores. This supported the conclusion that liking and excitement, while still quite strongly related, were not merely measures of the same thing, but discriminable concepts, even for 7-to-9 year olds.

The Resulting Television Materials

The violent program clip. The "S.W.A.T." segment began with two Los

Angeles police officers being ambushed while responding to a call about a domestic dispute. The two appeared to be close friends as well as partners. One of the officers was seriously wounded in the ambush. In response to the officers' radio call for help, the S.W.A.T. team arrived on the scene. They pursued the attackers, including a roof-top chase scene, but were unable to capture them. The next scene showed the wounded police officer being carried away on a stretcher, while his partner looked on sadly and helplessly. In the next scene, the wounded officer's partner volunteered to join a new S.W.A.T. team that was being established in his precinct.

On his first day with the new team, the S.W.A.T. leader offered the rationale behind the special force: to deter criminals with heavily-armed, well-trained paramilitary units. The commander also introduced a strong revenge motive, in stating, "Our present prime target is these blood-suckers who've declared war on cops. Yes, that's right. We're in a war, nothing less. And we intend to approach it just that way." Scenes featuring the unit's armament and armored truck followed. Target practice scenes came next, in which the new S.W.A.T. member distinguished himself as a very skillful shooter. He was given the position of marksman for the unit. In the final portion of the film segment, the S.W.A.T. team cleverly trapped the original three gunmen in their own ambush. The segment ended with one of the ambushers knocked unconscious, and the other two shot with rifles at close range by the new recruit and the S.W.A.T. commander. The S.W.A.T. leader turned to the new recruit, at the end of this first successful mission, and said "Welcome to the club."

This violent television segment was edited in such a way as to incorporate a number of features that have been identified as important in related research. The aggression of the S.W.A.T. team was portrayed as justified

revenge, a theme which Leonard Berkowitz and his colleagues have repeatedly found to be the most effective in producing aggression with their adult subjects (Berkowitz & Alioto, 1973; Berkowitz & Powers, 1979; Berkowitz & Rawlings, 1963). Aggression by the S.W.A.T. team was shown to be successful in ending the threat posed by the ambushers (c.f. Lando & Donnerstein, 1978). The success of the S.W.A.T. counterattack also attracted social rewards, as in the commander's comment to the new recruit, "Welcome to the club". The members of the S.W.A.T. unit were portrayed from start to finish as admirable: They were efficient, skillful, dedicated, and brave (c.f. Epstein & Rakosky, 1976).

The nonviolent program clip. The nonviolent program was a "laundered" episode from the series "C.H.I.P.S.". The main plot of the episode involved a boy named Danny, who joined a motocross bike racing team which was being coached by members of the California State Highway Patrol (C.H.I.P.S.) unit. The officers coached the team in their capacity as private citizens. In fact, they wore civilian clothes in all scenes except the first, in which they gave bicycle safety lessons to a group of children at Danny's school.

The protagonist, Danny, was introduced in the initial scenes as the class clown, who came from a poor family and was rather a loner at school. His teacher, observing his interest in the C.H.I.P.S. motorcycles, convinced him to try out for the motocross bike team, and convinced the coaches to let him try out, even though he didn't have a bicycle of his own. At the try-outs, Danny showed himself to be a fast and powerful bike rider, but somewhat lacking in skill. Despite falling in the mud hazzard, Danny performed well enough to make the team. Several scenes of practice sessions followed, which included exciting shots of fast, skillful bicycle riding by team members on a difficult obstacle course. Danny's skills improved

visibly. The final scenes showed Danny's first real competition with the team, in which he became the team's hero by winning a particularly close race, featuring some quite spectacular bicycle stunts. There was absolutely no violence in this television clip.

Selecting a Stimulus from the Violent TV Episode to Serve as a Cue

In order to test the elicitation hypothesis, some stimulus related to the televised violence had to be presented to some subjects just prior to measuring their aggressiveness. The stimulus chosen was a walkie talkie. The walkie talkie appeared in the "S.W.A.T." episode just before the S.W.A.T. unit attacked the three would-be assassins. These three villains spoke to each other over walkie talkies as they began to realize that their plan might be backfiring on them. The walkie talkie conversation was in progress when the S.W.A.T. unit's attack began.

This cue had a number of features to recommend it. It was associated closely with the onset of the heroes' justified aggression, and most particularly associated with the victims of this justified aggression (c.f. Geen & Berkowitz, 1966). Furthermore, walkie talkies do not have their own aggressive meaning. Their cuing properties would therefore depend upon their association with the televised violence. It was also expected that the placement of the walkie talkie scene, at a point of high tension in the episode, would ensure that most viewers would notice the cue. If it were not noticed, it would be useless as a violence-related cue in the main experiment.

As a check on the salience of the walkie talkie, all subjects who saw the "S.W.A.T." episode in Pilot Studies II, III, and IV were asked four additional questions after they finished filling out the liking and

excitement measures. They were given the following instructions:

Now on this last page I have a few more questions to ask you. I want you to be very quiet, still, when you answer these questions. Don't let anybody know what your answer is. O.K.? I would like everybody to turn thier booklet over, now. I want you to write the numbers 1, 2, 3, and 4 on the back of the booklet, like this [demonstrate]. The first question I'm going to ask you is: "Did any of the 'bad guys' in this show have a beard?"

If they did, I want you to print "YES", beside the number 1, like this. If they didn't, I want you to print "NO", like this beside the number 1.

[When all have answered question one ...]

My next question, number two, is this: "Did any of the bad guys have a walkie talkie?" Print "YES" beside the number two if they did, and print "NO" beside the number two if they didn't.

[When all have answered question two ...]. Question number three is this: "Did any of the bad guys wear a hat?" Print "YES" beside number three if they did, and "NO" beside number three if they didn't.

[When all have answered question three ...]. Question number four is, "Did any of the bad guys wear glasses?" If any of them did, print "YES" beside number four. If they didn't, print "NO".

Of the 209 subjects who viewed "S.W.A.T." in the pilot studies, 95% (199 subjects) reported noticing the walkie talkie. This was not simply a matter of being agreeable: Subjects did not typically say "yes" to questions about stimuli that were not associated with the victims of justified aggression. Only 7% reported a beard, 26% reported a hat, and 17% reported glasses. (Incidentally, the S.W.A.T. unit members did wear hats, although the "bad guys" did not).

METHOD-THE PRINCIPAL STUDY

Subjects

Subjects were recruited by sending permission request letters home with all the second and third grade boys in 13 Winnipeg schools. The letter (See Appendix G) introduced the study as an attempt to find out "the things that children 'get' from TV," and briefly described the main features of the experimental procedure. From the 835 letters sent home, 578 boys (69.2%) were granted permission to participate in the study. Boys from the pool of eligible subjects in each classroom were randomly assigned to six-person groups. Sixty-six groups were formed in this way, and randomly assigned to the six experimental conditions. The total sample size was thus (six boys per group times eleven groups per condition times six conditions=) 396.

Apparatus and Equipment

The television programs and "frustration stimulus" were shown in colour with a Sony AV8600 television recorder on a Sony CVM-1200U 12-inch colour receiver. The floor hockey game required two goal nets, six plastic hockey sticks, a plastic puck, three tape recorders, a whistle, a stop watch, and a "walkie talkie". In addition, two sets of team jerseys were used, one red and one green. Each set had three jerseys, each numbered "1", "2", or "3" on the front and back.

Design

The basic design involved four factors. The first factor was a subject classification variable, characteristic aggressiveness, which had three levels: high, intermediate, and low. The second factor was an experimental manipulation, order of frustration, which had two levels: Subjects were frustrated either before or after watching the

television materials just described. Television condition, another experimental manipulation, was the third variable. It had three levels: Violence plus cues, violence only, and nonviolence. Experimental group was treated as a fourth factor. Since groups of six boys watched television and played floor hockey together, group membership was expected to account for some portion of the total variance. Members of any particular experimental group experienced only one order of frustration and one television condition, but the six group members could have been (and usually were) from different levels of the characteristic aggressiveness factor. Thus the groups factor was nested within levels of the experimental manipulation factors, frustration order and television condition, but it was crossed with the characteristic aggressiveness factor. There were eleven six-person groups at each of the six combinations of frustration order and television condition.

Within each of the television programs employed, two additional subject variables were included as factors. One was reality/fantasy orientation of the television viewer (two levels: reality and fantasy). The other was the viewer's identification with the program's protagonist (two levels: identified and not identified). These factors were not considered truly crossed with the television condition factor: Subjects' reports of identification with a character, and believing in the reality of a program, are likely to depend upon the various details of the program and its characters. Therefore the reality/fantasy and identification conditions cannot be said to be identical across television programs.³

³ The author is grateful to Dr. Neil Malamuth for bringing this problem to her attention.

They were investigated separately within each of the television programs for that reason.

Procedure

Subjects who had been randomly selected to serve in six-person experimental groups were collected from their classrooms by the experimenter, who took that opportunity to leave six Teacher Rating Forms for the classroom teacher to complete, one for each subject.

Collecting information for the aggression classification. Teachers were asked to check off, on a nine-item list, any items which applied to the boy in question "often enough for it not to seem unusual." The items were adapted from the nine-item version of the Rip Van Winkle Peer Rating Index of Aggression (Lefkowitz et al., 1977). They included behaviours such as "starts fights over nothing" and "says mean things" (See Appendix H).

The experimenter led the boys to the "TV watching room", one of the dressing rooms beside the school gymnasium.⁴ Just after they left the classroom, the experimenter told the subjects:

By the way, you're going to be out of class for quite awhile this morning [or afternoon]. I have a TV program to show you, and some other people have a game for you to play, later. Also ... you guys are kind of lucky because I've managed to get some really neat cartoons for you today--you know, the kind they show at movies. I had some problems with the cartoon tape for some of the groups I ran before, but its fixed now, so you guys are going to get to see them.

Once inside the "TV watching room", the children were told by the experimenter, "I'm here with some other people today to do a study about

⁴In school #7, the "TV watching room" was a kitchen beside the auditorium; in school #8, it was an enclosed activities "pit".

what boys your age do in their spare time. I'm doing a study about TV watching, and the other people will be running a quick game of floor hockey with you. Why don't you all find yourselves a place to sit, so we can start."

Frustration manipulation. In the frustration before TV condition, the experimenter then announced, "I'm going to start with the cartoons." She started up the videotape player and played the beginning of the popular cartoon special "It's the Great Pumpkin, Charlie Brown!" Almost immediately, static and "snowy" reception interrupted the cartoon, becoming quickly worse until the screen showed nothing but "snow". At this point the experimenter, who had been hovering over the apparatus in apparent distress, pretending to try adjusting the picture, clicked off the tape player and announced, "Oh, no! The tape's all messed up. I won't be able to show you those cartoons, after all. That's really too bad. They're such neat cartoons." She then showed the subjects either the violent or the nonviolent television program. In the frustration after TV condition, the interrupted cartoon was introduced after subjects had watched the experimental TV programs.

Once the program had been shown, and the frustration delivered, the boys were sent to the gymnasium.⁵ Three adults, a male serving as referee and two observers (one male and one female) met them there. All three were blind to the subjects' television and frustration order conditions. The experimenter handed the referee a card with subjects' randomly determined team and number assignments on it, and left. The referee handed each boy his identifying jersey and a hockey stick. He explained the game to the boys.

⁵At school #8, the auditorium, and at school #2 the "indoor play room."

We're going to have you play a short game of floor hockey, the reds against the greens. You're going to play three periods, only three minutes each, so you each have a chance to play every position. Those people over there [pointing to the observers] will be doing a play-by-play recording of the game, like they do on the radio. This won't really be on the radio, of course, but we're going to start out with a pregame interview, so our announcer over there will know who you are.

Cue manipulation. At this point the referee interviewed each boy, asking for his name, his classroom and what position he liked best in hockey. For half the subjects, the referee used a tape recorder and microphone for the interview. For the other half, a "walkie talkie" was used and one of the observers listened in on the companion walkie talkie. The walkie talkie was a violent cue, for viewers of the violent program, because its use was featured just before the final shoot-out in that program. It was a neutral stimulus, of course, to viewers of the "C.H.I.P.S." program. The referee chose the walkie talkie or tape recorder on the basis of instructions at the top of the card which the experimenter had handed him (determined randomly, in advance). He was unaware of the significance of the interview method.

The referee started the game by sending team members who had the number "one" on their jerseys to the centre positions, "twos" to the left of centres, in the wing positions, and "threes" to play as goalies. After three minutes, players were rotated, faced off, and played for three more minutes. Another rotation followed, and a third three-minuted period of play.

From the moment the subjects donned their jerseys, until they took them off at the end of the game, they were carefully watched by the observers. One observer took each side of the gymnasium, and followed whatever action was going on. Meanwhile, they dictated into their tape recorders

every instance of aggressive action on the floor. For example: "red 3 shoves green 1," "green 2 hits red 2".

Observers had been instructed to report all aggressive actions, including the following:

1. pushing down, pushing over, or other pushing if victim protested
2. elbowing
3. hitting a person with a hockey stick
4. tripping a person
5. hitting a person with any part of one's body (except backing into him)
6. verbal aggression: insulting a person (insult to be reported) or calling a person an abusive name (name to be reported)

Raters were told to report all instances of behaviour which appeared to be deliberate actions causing harm to another person. If there was any question about intentionality they were to express their opinion about how intentional the action was, and say why. If either rater was quite sure an action was accidental, or if both thought it might be accidental, it was not included in the aggression score. "Body-checking" was not included unless it involved pushing or elbowing. Actions in all six of the specified aggression categories were reported by the raters. Other actions they reported (all of which were included in subjects' aggression scores), included shoving, slashing, poking, pinching, knocking down, sitting on, kneeing, grabbing, and hair pulling.⁶

⁶ To make sure that "aggression" scores were not just measures of hard, effective play, their correlation with the number of goals scored was calculated. Goals correlated .004 with total aggression and .097 with initial aggression.

Once the game was over, subjects were sent back to the "TV watching room". The experimenter met them there and instructed them to find a spot, all to themselves, on the floor. She handed each a copy of a questionnaire relating to whichever television program the group had watched earlier (See Appendix I). She explained,

I have some questions to ask you about the TV program I showed you earlier. First I have to ask you to be very quiet. I want you to answer these questions all on your own. They're kind of private, so don't tell anyone your answers, and don't let anyone look at your paper. First of all, please print your name on the top, then print the colour of the team you're on today, and the number you were wearing. I'm going to read the questions out to you, one at a time.

The first question is, "Did the things on this TV show really happen?" If you think they did, put an "X" in the box beside "yes". If you think they were just pretend, then put an "X" in the box beside "no".

Each question was read out in this manner. Discreet help was given by the experimenter if a subject asked for assistance in spelling a name. Subjects' answers to these questions were used later to determine whether each subject had taken a reality or a fantasy approach to the television content, and whether he had identified with the protagonist or some other character in the program. Once the questionnaires were filled out the experimenter returned the boys to their classroom and picked up the completed Teacher Rating Forms from the classroom teacher.

RESULTS

Tape recordings of aggression in the floor hockey game were transcribed onto group record sheets, which were divided into pregame and one-minute hockey game time slots. Transcription was done by the author, who had also served as the experimenter during the study. She transcribed the groups' tapes in chronological order, without knowing which condition the group had been in, while she coded it.

The observers had been stationed at opposite sides of the gymnasium, to better view all the action which took place. The use of verbal recording also enhanced observation opportunities, since the observers could keep their eyes on the subjects at all times. It was expected that some aggressive actions reported by one observer would be screened from the view of the other observer. Therefore, aggression reported by one observer was counted in a subject's score, even if not corroborated by the other observer's report. Of the 356 aggressive acts reported, 225 were reported simultaneously by both observers. Fourteen were recorded while only one tape recorder was functioning, due to equipment difficulties. One hundred and seventeen other acts were recorded by one of the observers, which meant the observers' reliability (total agreements/total aggression) was .66. Validity was the major concern in assigning observers to positions, so this rather low reliability was not too disconcerting. However, some reassurance was sought that the low reliability arose from the observers' somewhat different perspectives on the behaviour they were observing, and not from errors in reporting the events. To check on this, the observers were instructed on four occasions (two groups during the first week of the experiment, and two groups during the last week) to station themselves on the same side of the gymnasium, just out of earshot of each other.

They were not told why this instruction was being given. Of the 17 aggressive actions recorded on these occasions, 15 were reported simultaneously, for a reliability of .88. (In a sense it's much higher than that. There were thousands of behaviours which both raters agreed were nonviolent.)

Group and individual scores were determined for the entire sample using only experimental group number, team colour, and player number as identifiers. Two scores were recorded for each subject and each group. The first was called "initial aggression" and consisted of the aggressive actions counted during the pregame observation and the first three minutes of play. The second score, "total aggression", included all the aggressive behaviours counted in the entire observation period. Subjects were classified according to their reality/fantasy, orientation and identification with the TV characters, from their questionnaire responses. Finally, characteristic aggressiveness scores were assigned to each subject on the basis of the Teacher Rating Forms.⁷

Assigning Subjects to Levels of the Characteristic Aggressiveness Factor

Subjects' assignments to levels of the characteristic aggressiveness factor were based on the nine-item Teacher Rating Form. The number of items checked off for a particular subject constituted his characteristic aggressiveness score.

Assigning subjects to the three levels of this factor ought, ideally, to have been done by dividing the distribution into three roughly equal sized groups. The distribution of the scores made such a strategy impossible, however, since more than half the subjects (201 of them) had a characteristic aggressiveness score of zero (see Figure 1). Subjects with the score of zero

⁷Teacher Rating Forms and questionnaires had the subjects' names on them. Aggression scores for the floor hockey game could be matched with these sources of information by using the original lists given to the referee, which included each boy's name, jersey colour and number.

were assigned, then, to the level of low characteristic aggressiveness. The cutoff point between the second and third levels (intermediate and high) was placed between scores of three and four, since there was some indication of a minor mode at the score of four. This left 139 subjects in the intermediate aggressiveness category, and 56 in the high aggressive category.

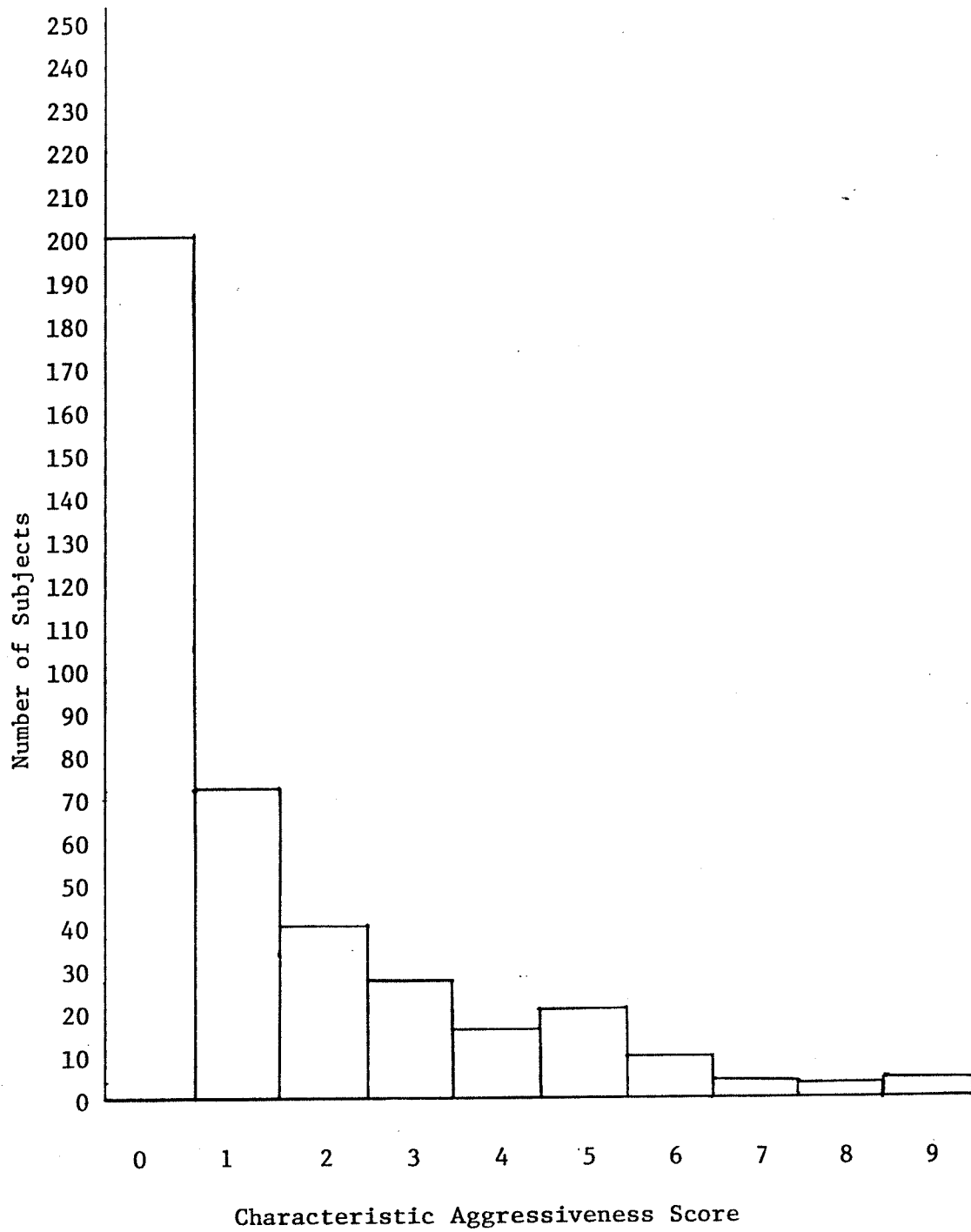


Figure 1: Distribution of Subjects' Characteristic Aggressiveness Scores

Method of Analysis

All analyses were conducted with Version Six of Finn's Multivariate computer package. Unequal cell sizes in this design ensured that the effects in the analysis of variance model would be correlated with each other. Variance shared by two such effects would be included in the statistical test of the earlier-tested effect, and partialled out of the later-tested effect. Since the factors in the present model follow a logical order of "history", effects were tested in the following order:

1. Characteristic aggressiveness, which is an attribute attached to the individual even before he became an experimental subject, was tested first.
2. Frustration order is the first experimental manipulation, so it was tested second.
3. Television condition was then tested, now including variance that belonged to it uniquely. Any variance it shared with earlier tested effects had been partialled out in the earlier steps of the analysis. First, second, and third order interactions followed in their logical orders.

In subsidiary analyses that included the questionnaire variables for each program, reality/fantasy orientation and identification were tested after characteristic aggressiveness and frustration order, since they were assumed to come into effect once the television program had begun. Since identification is more likely influenced by reality/fantasy orientation than vice-versa, it was tested after the reality/fantasy effect.

Tests of A Priori Hypotheses

Tests of the Disinhibition Hypotheses

Hypothesis 1(a). This hypothesis, which ignores the distinction between violence accompanied by cues and violence only, required that the groups of subjects receiving violent content be significantly more aggressive

than those receiving nonviolent content.

The univariate group means (presented in Table 5) were in the right direction to support this hypothesis, but the difference was not significant at an interpretable level (Multivariate $F^8(2,59)=1.2888, p=.2833$).

 Insert Table 5 about here

Hypothesis 1(b). Among boys who viewed violent television, it was expected that those who identified with the aggressive protagonist would be more aggressive than those who did not identify with him.

Of the 264 subjects who watched the violent television program, 155 reported pretending to be one of the members of the S.W.A.T. team (virtually always the leader). Mean aggression scores (See Table 6) for those who identified with an aggressive hero were actually a little lower than the mean aggression scores of those who did not, but the difference did not approach significance (Multivariate $F(2,29)=0.6089, p=.5508$).

 Insert Table 6 about here

Hypothesis 1(c). Among those who viewed violent television, subjects who believed that the content reflected real life were expected to be more aggressive than those who believed it to be fantasy. Of all the violence viewers, only 58 subjects thought the program they saw was real. The other 206 reported believing that the program was "just pretend". The mean

⁸Rao's F-approximation to the distribution of Wilkes' lambda criterion, an approximation that is exact in the present case, with two dependent variables (Tatsuoka, 1971, pp. 40-45).

Table 5

Mean Aggression Scores for Groups Viewing Violence and Nonviolence

Television Condition	Initial Aggression	Total Aggression
Violence Viewing (n=44 groups)	1.82	6.05
Nonviolence Viewing (n=22 groups)	1.04	4.63

Note: Comparisons between television viewing conditions and orders of frustration have been analyzed with groups as the unit of analysis. "Groups" in this case, refers to the groups of six subjects who watched television and played floor hockey together. Wherever the unit of analysis is the group, as in Table 2 above, the means reported are for the six-person group, not the individual subject.

Table 6

Mean Aggression Scores for Violence Viewing Subjects Identifying or Not Identifying With Aggressive Hero

Identification	Initial Aggression	Total Aggression
Yes (n=155)	0.30	0.99
No (n=109)	0.31	1.03

aggression scores for these groups, reported in Table 7, were in quite the opposite direction from the disinhibition model's prediction and the difference was significant (Multivariate $F(2,17)=7.3048$, $p=.0052$).

Table 7

Mean Aggression Scores for Subjects Reporting
Reality and Fantasy Orientations

Viewer's Orientation	n	Initial Aggression	Total Aggression
Reality	58	0.12	0.71
Fantasy	206	0.35	1.09

Tests of the Elicitation Hypotheses

Hypothesis 2(a). The elicitation model would predict that groups of subjects exposed to violent content plus cues would be more aggressive than groups of subjects exposed to violence without cues. Group means (see Table 8) were in the direction to support the hypothesis, but the difference between the conditions was far from significant (Multivariate $F(2,59)=0.6197$, $p=.5416$).

Table 8

Mean Group Aggression Scores for "Violence Only" and "Violence
Plus Cues" Conditions

Condition	Initial Aggression	Total Aggression
Violence & Cues	1.95	6.77
Violence Only	1.68	5.32

Note: Means are for entire six-person groups, not for individuals. Each condition has 22 groups.

Hypothesis 2(b). The elicitation effect was expected to be stronger for characteristically more aggressive boys than for those who were characteristically less aggressive. Mean aggression scores for subjects at each level of characteristic aggressiveness are reported in Table 9. The elicitation effect was not significant among boys low or intermediate in characteristic aggressiveness. (Multivariate $F_s(2,84)=0.3223$ and 0.5464 , respectively, $p_s=.7254$ and $.5811$, respectively). Among boys whose characteristic level of aggressiveness was high, the elicitation effect was significant (Multivariate $F(2,84)=4.2936$, $p=.0168$).

Table 9

Mean Aggression Scores for "Violence Only" and "Violence Plus Cues" Conditions at Each Level of Characteristic Aggressiveness

Characteristic Aggressiveness	Initial Aggression Measure	
	Television Condition	
	Violence Only	Violence & Cues
Low	0.21 (n=61)	0.15 (n=67)
Intermediate	0.32 (n=50)	0.27 (n=44)
High	0.38 (n=21)	1.00 (n=21)
Total Aggression Measure		
Low	0.67	0.73
Intermediate	0.80	1.00
High	1.71	2.67

Hypothesis 2(c). The elicitation effect was expected to be stronger among subjects who identified with the aggressive hero in the violent

television program. Mean aggression scores for these subjects are presented in Table 10. Contrary to prediction, there was no effect due to the presence of cues among violence-viewing boys who identified with the aggressive hero (Multivariate $F(2,29)=0.3420$, $p=.7132$). The cues effect also failed to show up among the nonidentifying subjects (Multivariate $F(2,29)=0.8303$, $p=.4461$).

Table 10

Mean Aggression Scores for "Violence Only" and "Violence Plus Cues" Conditions, for Subjects Identifying and Not Identifying with the Aggressive Hero

Identification	Television Condition	
	Violence Only	Violence & Cues
Identified	(n=76)	(n=79)
Initial Aggression	0.33	0.27
Total Aggression	1.03	0.96
Not Identified	(n=56)	(n=53)
Initial Aggression	0.21	0.41
Total Aggression	0.70	1.38

Hypothesis 2(d). The elicitation effect was also expected to be stronger for subjects who took a reality orientation to the televised violence than for subjects who took a fantasy orientation. Mean aggression scores relevant to this hypothesis are found in Table 11. The mean differences due to cues were not significant, for either reality-or fantasy-oriented boys (Multivariate $F_s(2,17)=0.1148$ and 2.770 , $p=.8925$ and $.0905$, respectively).

Table 11

Mean Aggression Scores for "Violence Only" and "Violence Plus Cues" Conditions for Subjects with Each Reality/Fantasy Orientation

Reality/Fantasy Orientation	Television Condition	
	Violence Only	Violence & Cues
Reality	(n=34)	(n=24)
Initial Aggression	0.15	0.08
Total Aggression	0.85	0.50
Fantasy	(n=98)	(n=108)
Initial Aggression	0.33	0.33
Total Aggression	0.90	1.27

Hypothesis 2(e). The elicitation effect was considered more likely to occur for the initial aggression measure than for the total aggression measure, because it was expected to affect the impulsive component of aggression. To test this hypothesis, univariate tests were performed for the one elicitation model prediction which had achieved a significant multivariate F , the test of elicitation among boys whose characteristic aggressiveness was very high. These boys had a mean initial aggression score of .38 in the violence only condition. Boys who were also exposed to cues had a mean initial aggression score of 1.00. The difference between the groups was significant at the .0089 level ($F(1,85)=7.1712$; error mean square=0.3350). The violence only and violence plus cues conditions had means of 1.71 and 2.57, respectively, on the total aggression measure. This difference was significant at the .0310 level ($F(1,85)=4.8161$; error mean square=1.9007). However, once initial aggression was partialled out of the total aggression, differences due to cues were no longer significant (step=down $F(1,85)=1.3837$, $p=.2428$).

As the elicitation model predicted, the increase in aggression due to cues occurred very soon after the presentation of the cues, and this "impulsive" component accounted for the total difference in aggression due to cues.

Tests of the Catharsis Hypotheses

Hypothesis 3(a). According to the catharsis model, violence-viewing subjects who took a reality orientation should have been more aggressive than those who took a fantasy approach. This same prediction was made in connection with the disinhibition hypothesis 1(c), and was disconfirmed. Indeed, those few subjects who believed the violent television program "really happened" were significantly less aggressive than subjects who realized that it did not.

Hypothesis 3(b). It was predicted that fantasy-oriented subjects frustrated before viewing violence would be significantly less aggressive than fantasy-oriented subjects frustrated after viewing violence (See Table 12 for mean scores). Those frustrated before viewing violence were actually more aggressive afterwards, but not significantly so (Multivariate $F(2,17)=2.5935, p=.1040$).

Table 12

Mean Aggression Scores of Fantasy-Oriented Subjects Frustrated Before and After Viewing Violence

Frustration Order	Initial Aggression	Total Aggression
Before TV (n=108)	0.44	1.17
After TV (n=98)	0.27	1.01

Hypothesis 3(c). It was predicted that viewers of the nonviolent television program would not show aggression differences due to reality/fantasy orientation. The mean scores for these subjects can be seen in Table 13. The differences were, indeed, nonsignificant (Multivariate $F(2,11)=0.1096$, $p=.8972$).

Table 13

Mean Aggression Scores for Reality- and Fantasy-Oriented Viewers of Nonviolent Television Content

Reality/Fantasy Orientation	Initial Aggression	Total Aggression
Reality (n=52)	0.15	0.69
Fantasy (n=80)	0.18	0.82

Hypothesis 3(d). Identification with the aggressive hero in the television program was expected to enhance a cathartic effect for the fantasy viewer. The pattern of mean scores (See Table 14) was in the opposite direction to the hypothesis, although neither identifying nor nonidentifying boys showed a significant effect due to frustration order (Multivariate $F_s(2,81)=0.2757$ and 0.5121 respectively, $p_s=.7598$, $.6012$, respectively).

Table 14

Mean Aggression Scores for Fantasy-Oriented Violence Viewers
by Frustration Order and Identification

Identification	Frustration Order	
	Before TV	After TV
Identified	(n=58)	(n=58)
Initial Aggression	0.41	0.26
Total Aggression	1.02	1.05
Not Identified	(n=50)	(n=40)
Initial Aggression	0.46	0.27
Total Aggression	1.34	0.95

Main Analysis

Thus far, we have been examining tests of the theoretical predictions made earlier. Table 15 presents the results of the overall multivariate analysis of variance including as factors: characteristic aggressiveness, frustration order, television condition, and groups within the frustration order x television condition interaction. For the reasons explained previously, the reality/fantasy orientation and identification variables were not included in the main analysis. They will be discussed separately for the two television programs, in subsequent analyses. Means relevant to the present analysis may be found in Table 16.

All of the interaction effects presented in Table 15 were significant at interpretable levels, as were the main effects for characteristic aggressiveness and frustration order.

Table 15

Multivariate Analysis of Variance Summary Table:
Aggression in a Game of Floor Hockey

Source of Variance	Error Term	df	Multivariate F	p
Characteristic				
Aggressiveness (A)	A x Gps/FT	4,168	8.6848	.0001*
Frustration Order (F)	Gps/FT	2,59	4.3522	.0173*
Television Condition (T)	Gps/FT			
	TESTED <u>A PRIORI</u> :			HYPOTHESES 1(a) AND 2(a)
A X F	A X Gps/FT	4,168	6.1006	.0002*
A X T	A X Gps/FT	8,168	2.2469	.0265*
F X T	Gps/FT	4,118	3.6939	.0072*
A X F X T	A X Gps/FT	8,168	2.2011	.0297*
Groups/FT	within cell	120,464	1.6044	.0003*
A X Groups/FT	within cell	170,464	1.3255	.0112*

* $\alpha = .05$

Table 16

Mean Aggression Scores in a Game of Floor Hockey

Treatment	Characteristic Level of Aggressiveness		
	Low	Intermediate	High
Initial Aggression (Mean=0.26)			
Frustration Before TV	(Overall Mean=0.33)		
Violence & Cues	0.18(34)	0.47(19)	1.62(13)
Violence Only	0.13(32)	0.24(25)	0.56(9)
Nonviolence	0.23(35)	0.25(24)	0.14(7)
Frustration After TV	(Overall Mean=0.19)		
Violence & Cues	0.12(33)	0.12(25)	0.00(8)
Violence Only	0.31(29)	0.40(25)	0.25(12)
Nonviolence	0.13(38)	0.10(21)	0.14(7)
Total Aggression (Mean=0.93)			
Frustration Before TV	(Overall Mean=0.93)		
Violence & Cues	0.68	1.53	2.77
Violence Only	0.53	0.88	2.00
Nonviolence	0.63	0.58	0.57
Frustration After TV	(Overall Mean=0.93)		
Violence & Cues	0.79	0.60	2.50
Violence Only	0.83	0.72	1.50
Nonviolence	1.05	0.62	1.57

Note: Numbers in parentheses indicate cell sizes.

Post Hoc Analyses of Significant Omnibus Tests

Significant multivariate F tests from Table 15 were followed up by conducting univariate F tests for each dependent variable. In cases where both initial and total aggression measures showed significant differences, a step-down procedure was used to determine whether differences in total aggression remained significant once variance due to initial aggression was partialled out. This procedure entailed conducting a large number of significance tests, and introduced the need to choose an error rate for reporting the results. As noted in the introduction, this author believes a per hypothesis error rate represents the most appropriate approach. Thus, for purposes of assigning error rate, a "family" of comparisons would be:

1. a single test of a main effect or interaction in a multivariate analysis of variance (MANOVA)
2. the set of multiple comparisons used to follow up a significant effect in the MANOVA. A Bonferroni procedure was employed for this type of hypothesis "family". The per hypothesis error rate ($\alpha=.05$) was divided by the number of comparisons in the family associated with the hypothesis. This determines the error rate per comparison (α_c).

A per hypothesis error rate can lead to very conservative per comparison Type I error rates. For instance, in following up the A X F X T interaction, 27 comparisons among means were required. To be considered significant, a given comparison had to exceed a chance probability level of .0018 ($\alpha_c = .05/27$). Such a stringent Type I error rate will seem unduly conservative to those whose preference is for a per comparison error rate.

The author's preference for a per hypothesis error rate is reflected in the tabular presentation of the post hoc results. The α_c is noted (derived by applying the Bonferroni procedure to each family of tests),

and an asterisk indicates which tests are significant at that level. However, since choice of error rate is a matter of taste rather than absolute "correctness" (e.g., Kirk, 1968, p. 86), individual comparisons will be reported in the text of this report if they reach the level of significance traditionally considered interpretable ($\alpha_c < .05$). They will be presented, in the final summing-up according to the degree of caution with which they should be interpreted.

Characteristic aggressiveness. Table 17 shows the mean initial and total aggression scores of subjects who had been pre-classified into the three levels of characteristic aggressiveness. High-aggressive subjects behaved true to form, showing much more aggression than their classmates from beginning to end of the game. (See Table 18 for reports of the univariate tests). Intermediate- and low-aggressive subjects did not differ from each other in aggression during the observation period.

Table 17

Mean Floor Hockey Aggression Scores for Subjects at Each Level of Characteristic Aggressiveness

Aggression Measure	Characteristic Aggressiveness		
	Low (n=201)	Intermediate (n=139)	High (n=56)
Initial Aggression	0.18	0.26	0.55
Total Aggression	0.76	0.80	1.91

Table 18

Univariate Tests of Mean Differences in Floor Hockey Aggression
Among Subjects Differing in Characteristic Aggressiveness

Contrast Among Levels of Characteristic Aggressiveness	<u>F</u>	<u>p</u>
High vs (Intermediate + Low)		
Initial Aggression	16.7706	.0001*
Total Aggression	32.7124	.0001*
Total Aggression (step-down)	16.4157	.0002*
Intermediate vs Low		
Initial Aggression	1.5657	.2143
Total Aggression	0.0775	.7814

Note: Degrees of freedom for each test =1,85. Error mean square for the initial aggression measure = 0.3350. For total aggression it is 1.9007.

*Bonferroni procedure: $\alpha_c = .05/5 = .01$

Frustration order. Mean initial and total aggression scores, for groups in each frustration order, are presented in Table 19. The difference in initial aggression was significant at the level of .0274 ($F(1,60) = 5.1143$, error mean square = 0.4009). The two frustration orders yielded exactly the same mean score on total aggression.

The characteristic aggressiveness X frustration order (AXF) interaction.

The mean aggression scores for subjects at each level of characteristic aggressiveness, in each order of frustration, are presented in Table 20. Univariate tests of the frustration order effect, at each level of characteristic aggressiveness, can be found in Table 21. Among subjects with a highly aggressive reputation, frustration before TV led to much more initial aggression than frustration following TV viewing. No other contrasts were significant.

Table 19

Mean Floor Hockey Aggression Scores for Groups Frustrated
Before and After TV Viewing

Aggression Measure	Frustration Order	
	Before TV	After TV
Initial Aggression	1.98	1.14
Total Aggression	5.58	5.58

Note: n=198, for each order of frustration. Means are for 6-person groups, not individuals.

Table 20

Mean Aggression Scores for Each Frustration Order, By Subjects'
Level of Characteristic Aggressiveness

Characteristic Aggressiveness	Frustration Order	
	Before TV	After TV
Low	(n=101)	(n=100)
Initial Aggression	0.18	0.18
Total Aggression	0.61	0.90
Intermediate	(n=68)	(n=71)
Initial Aggression	0.31	0.21
Total Aggression	0.96	0.65
High	(n=29)	(n=27)
Initial Aggression	0.93	0.15
Total Aggression	2.00	1.81

Table 21

Univariate Tests of Frustration Order at Each Level
of Characteristic Aggressiveness

Characteristic Aggressiveness	<u>F</u>	<u>p</u>
Low		
Initial Aggression	0.0006	.9522
Total Aggression	2.2092	.1409
Intermediate		
Initial Aggression	1.1660	.2833
Total Aggression	1.9713	.1640
High		
Initial Aggression	25.0890	.0001*
Total Aggression	0.1583	.6918

Note: Degrees of freedom for each test = 1,85. Error mean square = 0.3350 for initial aggression and 1.9007 for total aggression.

*Bonferroni procedure: $\alpha_c = .05/6 = .0083$.

The characteristic aggressiveness X television condition (AXT) interaction. The mean scores for boys characteristically high, intermediate and low in aggressiveness, for each television condition, are presented in Table 22. Means are presented graphically in Figure 2. Elicitation hypotheses connected with the interaction were tested a priori, yielding a strong effect on the initial measure of floor hockey aggression, among characteristically high aggressive boys only. Univariate tests of the disinhibition effect are reported in Table 23, for boys at each level of characteristic aggressiveness. As with the elicitation effect, only boys characteristically high in aggressiveness showed a disinhibition effect. The effect was significant on both measures at traditional

levels, but initial aggression was the important component of the total aggression measure. Once initial aggression was partialled out, total aggression no longer showed a significant effect.

The existence of both an elicitation and a disinhibition effect raises one further question. The disinhibition hypothesis assumes that cues are an irrelevant feature, and requires merely that the mean aggression scores across the two violence conditions be greater than the mean for nonviolence viewers. This may give an unwarranted advantage to the disinhibition hypothesis. Even if violent content alone had no effect on aggression, an elicitation effect (increased aggression among violence plus cues subjects only) might raise the mean of all violence viewers enough to show up as an artifactual disinhibition effect. To test this possibility, a further comparison was made, for characteristically high-aggressive subjects. Those viewing no violence were compared to only those who viewed violent TV without exposure to cues. The disinhibition effect was no longer significant for either initial aggression ($F(1,85)=2.0791, p=.1531$) or total aggression ($F(1,85)=1.8327, p=.1794$).

The frustration order X television condition (FXT) interaction.

Table 24 displays mean aggression scores for groups in each television condition, at each frustration order. Means are presented graphically in Figure 3. Univariate tests of the contrasts corresponding to disinhibition and elicitation hypotheses have been reported in Table 25.

Groups who were frustrated before TV viewing showed a disinhibition effect on the total aggression measure, which was significant at the .0329 level. The disinhibition effect for initial aggression did not reach the traditional level of significance. Groups frustrated before viewing also showed a substantial elicitation effect on the initial aggression.

Table 22

Mean Aggression Scores for Subjects in Each Television Condition
by Level of Characteristic Aggressiveness

Television Condition	Characteristic Aggressiveness		
	Low	Intermediate	High
Violence & Cues			
Initial Aggression	0.15	0.27	1.00
Total Aggression	0.73 (67)	1.00 (44)	2.67 (21)
Violence Only			
Initial Aggression	0.21	0.32	0.38
Total Aggression	0.67 (61)	0.80 (50)	1.71 (21)
Nonviolence			
Initial Aggression	0.18	0.18	0.14
Total Aggression	0.85 (73)	0.60 (45)	1.07 (14)

Note: Numbers in parentheses are cell sizes.

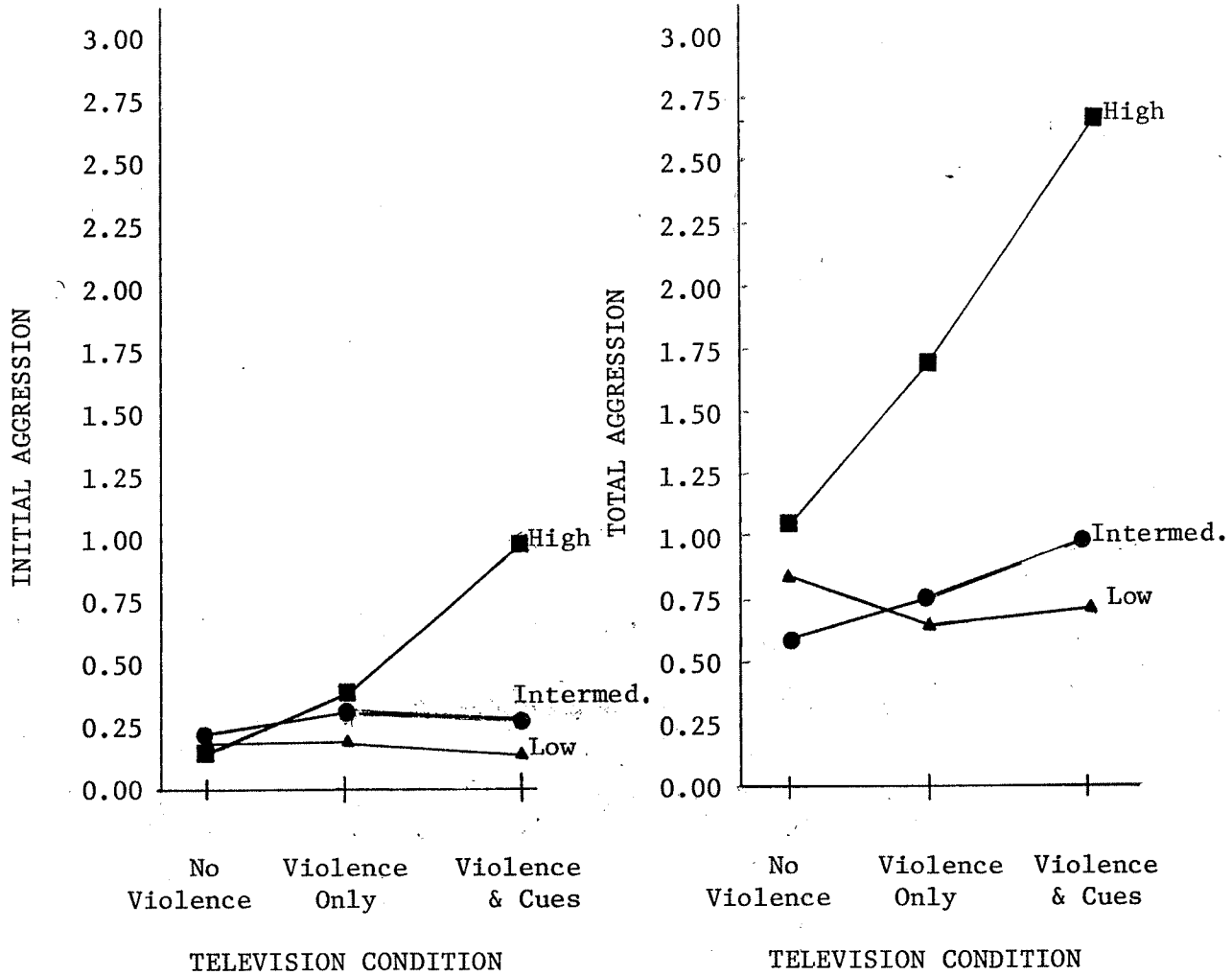


Figure 2. Mean Initial and Total Aggression in Each Television Condition for Subjects High, Intermediate, or Low in Characteristic Aggressiveness

Table 23

Univariate Tests of the Disinhibition Effect (Violence Only and
Violence Plus Cues vs Nonviolence Conditions) at Each Level
of Characteristic Aggressiveness

Characteristic Aggressiveness	<u>F</u>	<u>P</u>
Low		
Initial Aggression	0.0004	.9844
Total Aggression	0.4518	.5033
Intermediate		
Initial Aggression	1.4584	.2306
Total Aggression	1.5816	.2120
High		
Initial Aggression	8.7754	.0040*
Total Aggression	6.8663	.0105
Total Aggression (Step-down)	2.2165	.1403

Note: Degrees of freedom = 1,85 for each test. Error mean square = 0.3350 for initial aggression, 1.9007 for total aggression.

* Bonferroni procedure: $\alpha_c = .05/9 = .0056$.

Table 24

Mean Aggression Scores for Groups in Each Television Condition for
Each Frustration Order

Condition	Dependent Variable	
	Initial Aggression	Total Aggression
Frustration Before TV		
Violence & Cues	3.27	7.98
Violence Only	1.36	5.18
Nonviolence	1.36	3.64
Frustration After TV		
Violence & Cues	0.64	5.55
Violence Only	1.98	5.45
Nonviolence	0.73	5.82

Note: Means are group means. Each cell contained 11 groups. Each group included 6 subjects.

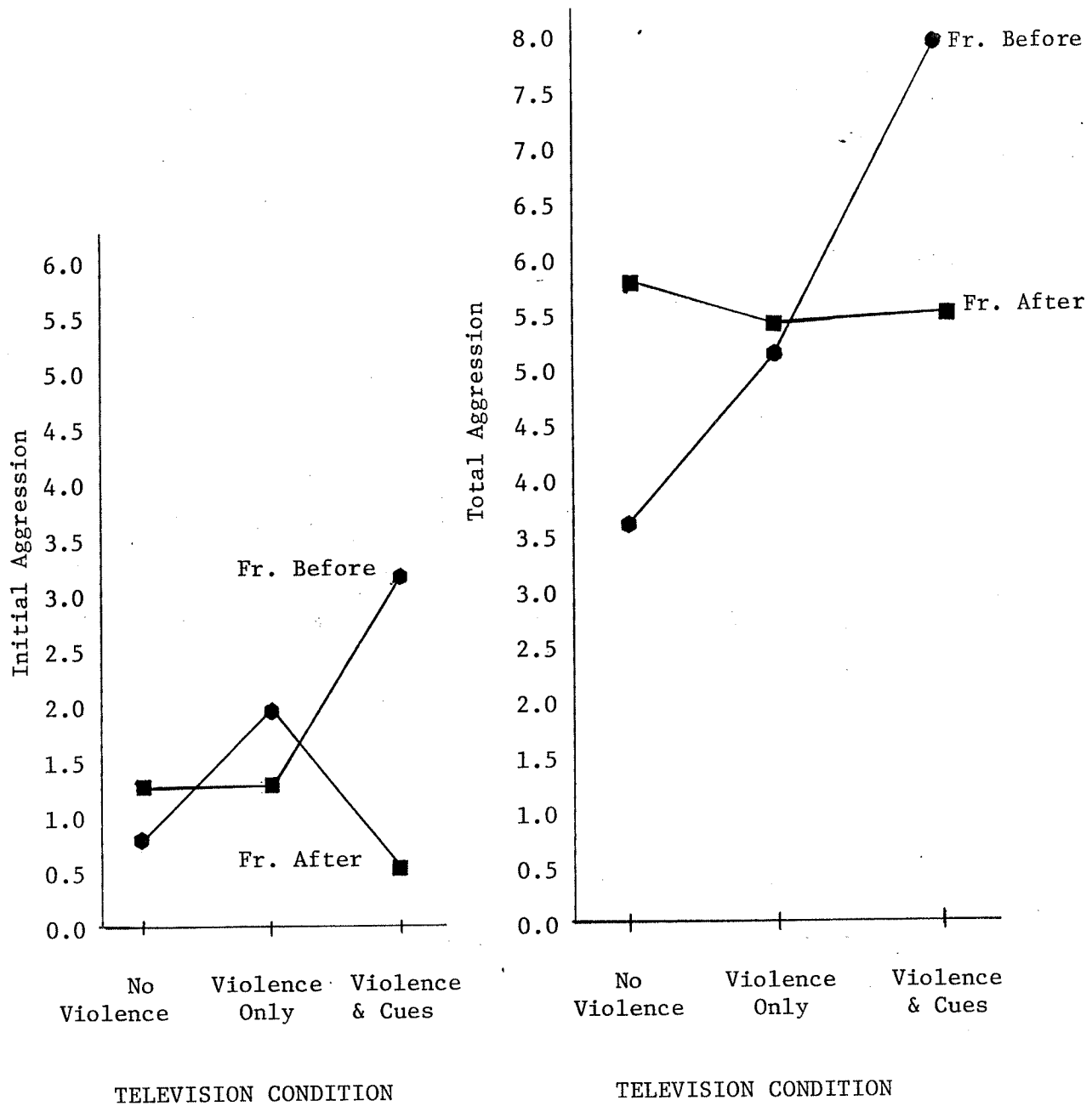


Figure 3. Mean Initial and Total Aggression for Subjects in Each Television Condition By Frustration Order

Table 25

Univariate Tests of Television Effects at Each Frustration Order

Order of Frustration	<u>F</u>	<u>P</u>
Disinhibition: (Violence Only and Violence & Cues) vs Nonviolence		
Frustration Before TV		
Initial Aggression	3.0621	.0853
Total Aggression	4.7727	.0329
Frustration After TV		
Initial Aggression	1.1103	.2963
Total Aggression	0.5470	.4625
Elicitation: Violence & Cues vs Violence Only		
Frustration Before TV		
Initial Aggression	11.1767	.0015*
Total Aggression	2.7044	.1053
Frustration After TV		
Initial Aggression	4.1084	.0472
Total Aggression	0.3963	.5314

Note: Degrees of freedom = 1,60. Error mean square = 0.4009 for initial aggression and 3.1508 for total aggression.

*Bonferroni procedure: $\alpha_c = .05/8 = .0063$

measure. Frustrating subjects after TV viewing produced only one interpretable effect. It is listed as an "elicitation" effect in Table 25, but is actually the reverse of an elicitation effect. When subjects were frustrated after TV viewing, those exposed to violence and cues were initially much less aggressive than those exposed to the violence only. They soon caught up, however, as can be seen from their total aggression scores, in Table 24.

The characteristic aggressiveness x frustration order X television condition (AXFXT) interaction. Table 26 displays the results of univariate tests corresponding to the disinhibition and elicitation effects within the AXFXT interaction. Mean aggression scores relating to these tests were presented in Table 16 and are graphed in Figure 4. As one would expect from looking at the earlier AXF and AXT interactions, the differences in aggression between experimental groups appeared for subjects who had a reputation for being characteristically aggressive.

 Insert Table 26 and Figure 4 about here

A pattern of aggression corresponding to a disinhibition effect appeared for characteristically high aggressive subjects, only if they had been frustrated before TV viewing. This contrast between sets of means was significant at the level of .0001 for initial aggression and at .0023 for total aggression. The effect for total aggression received rather a large "boost" from its initial aggression component since it was no longer significant ($p=.1409$) after initial aggression had been partialled out of it. Characteristically high-aggressive boys frustrated before TV viewing also showed an elicitation effect. This effect appeared only for the

Table 26

Univariate Tests of Television Effects at Each Combination of
Characteristic Aggressiveness and Frustration Order

Disinhibition: (Violence Only and Violence & Cues) vs Nonviolence

Condition	<u>F</u>	<u>p</u>
Low-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	0.4054	.5261
Total Aggression	0.0061	.9380
Frustrated After TV		
Initial Aggression	0.4290	.5143
Total Aggression	0.7512	.3886
Intermediate-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	0.3832	.5376
Total Aggression	2.7084	.1036
Frustrated After TV		
Initial Aggression	1.1986	.2768
Total Aggression	0.0130	.9094
High-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	17.1135	.0001*
Total Aggression	9.9073	.0023
Total Aggression (Step-down)	2.2102	.1409
Frustrated After TV		
Initial Aggression	0.0008	.9777
Total Aggression	0.2945	.5888

Elicitation: Violence & Cues vs Violence Only

Condition	<u>F</u>	<u>p</u>
Low-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	0.1304	.7190
Total Aggression	0.1829	.6700

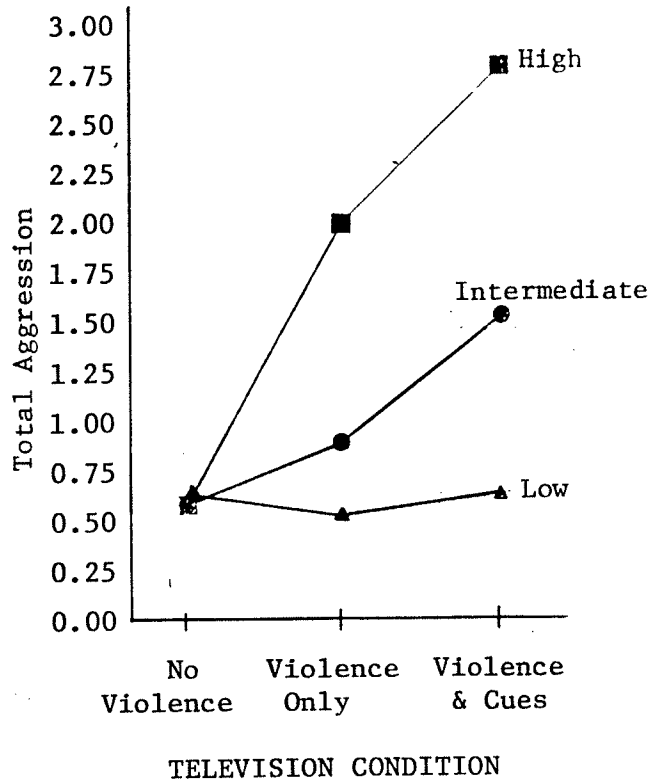
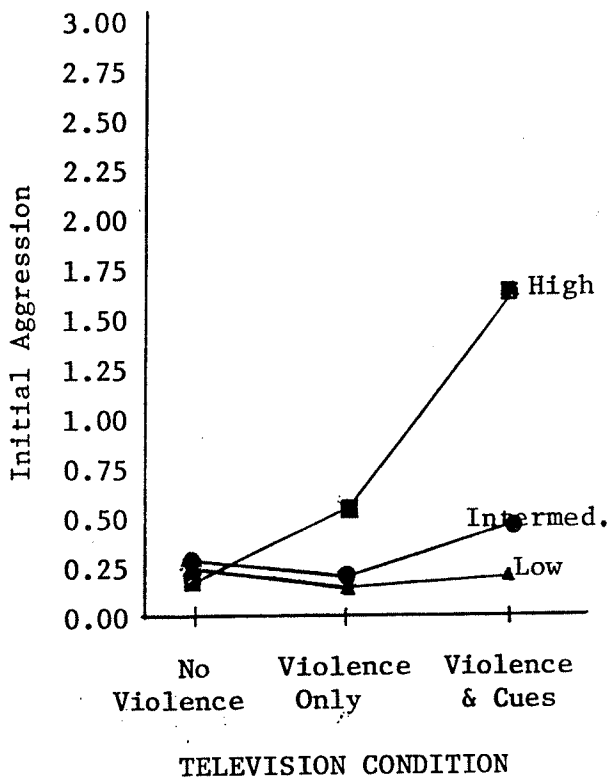
Table 26 continued

Condition	<u>F</u>	<u>P</u>
Frustrated After TV		
Initial Aggression	1.6484	.2027
Total Aggression	0.0128	.9102
Intermediate-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	1.7600	.1882
Total Aggression	2.3725	.1273
Frustrated After TV		
Initial Aggression	2.9258	.0909
Total Aggression	0.0947	.7591
High-Aggressive Subjects		
Frustrated Before TV		
Initial Aggression	17.8341	.0001*
Total Aggression	1.6556	.2017
Frustrated After TV		
Initial Aggression	0.8956	.3467
Total Aggression	2.5253	.1158

Note: Degrees of freedom for each test = 1,85. Error mean square = .3350, for initial aggression, 1.9007 for total aggression.

*Bonferroni procedure: $\alpha_c = (.05/27) = .0018$.

SUBJECTS FRUSTRATED BEFORE VIEWING TELEVISION



SUBJECTS FRUSTRATED AFTER VIEWING TELEVISION

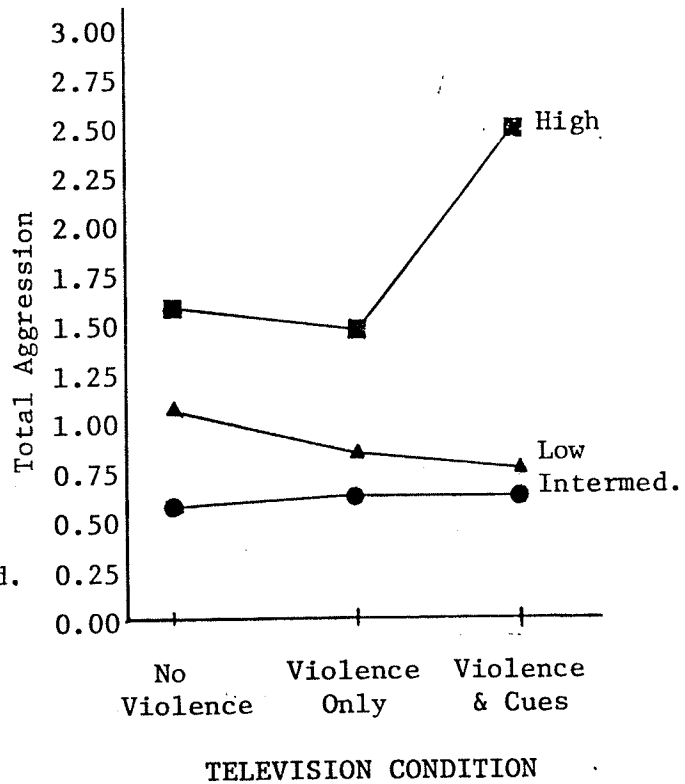
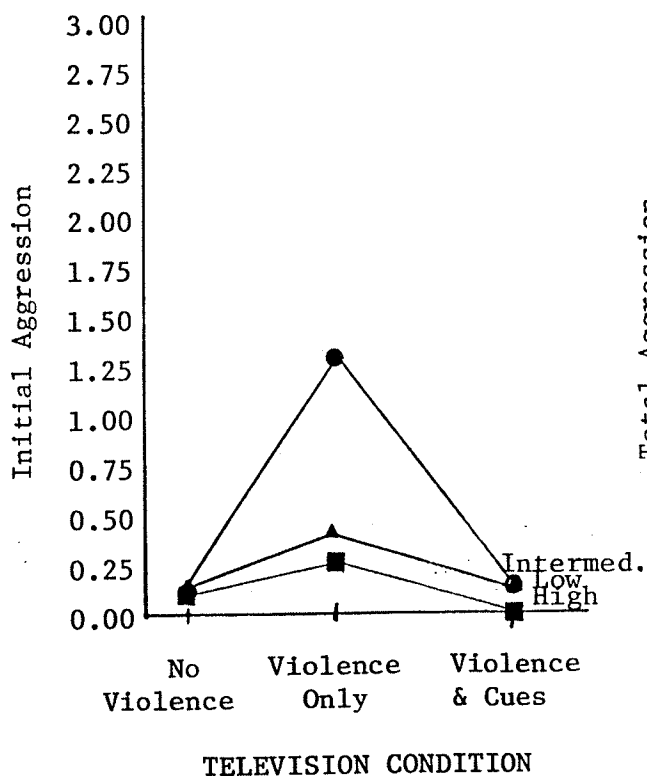


Figure 4. Mean Initial and Total Aggression Scores, by TV and Frustration Order Conditions, for Each Level of Characteristic Aggressiveness

initial aggression measure and was significant at the .001 level.

Since this group of subjects showed both an elicitation and a disinhibition effect, the same question arose that had been asked following the AXT interaction: Do both effects really exist, or is the disinhibition hypothesis being "carried" by the elicitation effect? A further comparison was done to answer this question, contrasting nonviolence viewers with viewers of violence only, among "high A" subjects frustrated before TV viewing. The disinhibition effect did not hold up for the initial aggression measure ($F(1,85)=2.0022$, $p=.1608$) but it maintained an interpretable probability level on the total aggression measure ($F(1,85)=4.2277$, $p=.0429$). This set of boys thus showed different effects on different measures: a dramatic elicitation effect on the initial aggression measure, and a more modest disinhibition effect on the total aggression measure.

Investigating the Effects of Reality/Fantasy Orientation and Identification with the Aggressive Hero Among Violence Viewers

Information about reality/fantasy orientation and identification with the program's hero were also collected from subjects who watched both types of television program. To determine the relationship of these two variables with violence viewers' subsequent aggression, a further analysis was conducted. Table 27 presents the results of a MANOVA including the reality/fantasy and identification effects, as well as their interactions with characteristic aggressiveness, frustration order and violence-associated cues. Means relevant to the analysis are found in Table 28. Two of the effects reached interpretable levels of significance: the characteristic aggressiveness by identification level (AXI) interaction, and the interaction of these two variables with the cues manipulation (AXIXC).

Table 27

Multivariate Analysis of Variance Table, Presenting Reality/Fantasy and Identification Effects Upon Violence Viewers

Source of Variance	Error Term	df	Multivariate <u>F</u>	<u>p</u>
Characteristic				
Aggressiveness (A)	TESTED IN MAIN ANALYSIS			
Frustration Order (F)	TESTED IN MAIN ANALYSIS			
Cues (C)	TESTED IN MAIN ANALYSIS			
AXF	TESTED IN MAIN ANALYSIS			
AXC	TESTED IN MAIN ANALYSIS			
FXC	TESTED IN MAIN ANALYSIS			
AXFXC	TESTED IN MAIN ANALYSIS			
Groups/FC	TESTED IN MAIN ANALYSIS			
AX Groups/FC	TESTED IN MAIN ANALYSIS			
Reality/Fantasy Orientation	TESTED <u>A PRIORI</u> : HYPOTHESIS 1(c)			
Identification (I)	IX Gps/FC	2,29	0.6089	.5508
RXI	residual ^a	2,81	0.1404	.8693
AXR	residual ^a	4,162	0.7473	.5612
AXI	residual ^a	4,162	2.9214	.0229*
FXR	RX Gps/FC	2,17	0.0938	.9110
FXI	IX Gps/FC	2,29	0.0953	.9095
RXC	TESTED <u>A PRIORI</u> : HYPOTHESIS 2(d)			
IXC	TESTED <u>A PRIORI</u> : HYPOTHESIS 1(b)			
AXFXR	residual ^a	4,162	0.9611	.4306
AXFXI	residual ^a	4,162	2.1377	.0785
AXRXI	residual ^a	4,162	0.3034	.8754
AXRXC	residual ^a	4,162	2.1584	.0761
AXIXC	residual ^a	4,162	2.6089	.0376*
FXRXI	residual ^a	2,81	1.0569	.3523
FXRXC	RX Gps/FC	2,17	1.5636	.2381
FXIXC	IX Gps/FC	2,29	0.6045	.5532
RXIXC	residual ^a	2,81	0.7170	.4914
AXFXRXI	residual ^b	2,81 ^b	0.7977	.4539
AXFXRXC	residual ^a	4,162	0.6743	.6108

Table 27 (Continued)

Source of Variance	Error Term	df	Multivariate F	p
AXFXIXC	residual ^a	4,162	0.9657	.4280
AXRXIXC	residual ^a	2,81 ^b	0.2600	.7717
FXRXIXC	residual ^a	2,81	0.0878	.9161
RXGps/FC	residual	36,162	0.5321	.9861
IXGps/FC	residual	60,162	0.8695	.7301

Note: Main effects for characteristic aggressiveness, frustration order, cues, and their interactions with each other, were included as sources of variance in the analysis, as indicated. This was done so that residual and other sources of variance would not be inappropriately inflated. These effects were not considered in Table 27 however, since they are redundant with tests conducted following the main analysis.

^aHad all the cells in this design been filled, interaction mean squares would have been used as the error terms for testing all the effects of interest. Since there were empty cells in some conditions, several interactions involving the "groups" factor had to be dropped from the model, and thus from the expected mean squares of other effects. Residual error was the appropriate error term for many effects for this reason.

^bBecause of empty cells in the design, degrees of freedom were reduced for some effects.

* α = .05

Table 28

Mean Aggression Scores for Violence Viewing Subjects in Each
Classification Level and Each Experimental Condition

Classification	Initial Aggression			
	<u>Experimental Condition</u>			
	<u>Frustration Before TV</u>		<u>Frustration After TV</u>	
	Violence & Cues	Violence Only	Violence & Cues	Violence Only
Low-Aggressive				
Reality Oriented				
Identified	0.00(6)	0.00(1)	0.00(3)	0.00(5)
Not Identified	0.00(1)	0.00(5)	0.00(5)	0.00(1)
Fantasy Oriented				
Identified	0.24(17)	0.18(11)	0.19(16)	0.40(15)
Not Identified	0.20(10)	0.13(15)	0.11(9)	0.37(8)
Intermediate-Aggressive				
Reality Oriented				
Identified	0.00(1)	0.00(2)	0.00(3)	0.20(5)
Not Identified	---(0)	0.00(1)	0.00(1)	0.00(4)
Fantasy Oriented				
Identified	0.63(8)	0.33(12)	0.00(10)	0.75(8)
Not Identified	0.40(10)	0.20(10)	0.27(11)	0.38(8)
High-Aggressive				
Reality Oriented				
Identified	0.67(3)	0.50(4)	0.00(1)	0.40(5)
Not Identified	---(0)	---(0)	---(0)	0.00(1)
Fantasy Oriented				
Identified	1.17(6)	0.50(4)	0.00(5)	0.00(4)
Not Identified	3.00(4)	1.00(1)	0.00(2)	0.50(2)

Classification	Total Aggression			
	Experimental Condition			
	Frustration Before TV		Frustration After TV	
	Violence & Cues	Violence Only	Violence & Cues	Violence Only
Low-Aggressive				
Reality Oriented				
Identified	0.50(6)	0.00(1)	0.00(3)	0.40(5)
Not Identified	0.00(1)	0.40(5)	0.40(5)	1.00(1)
Fantasy Oriented				
Identified	0.59(17)	0.55(11)	0.62(16)	1.00(15)
Not Identified	1.00(10)	0.60(15)	1.56(9)	0.75(8)
Intermediate-Aggressive				
Reality Oriented				
Identified	0.00(1)	0.50(2)	0.67(3)	0.40(5)
Not Identified	---(0)	2.00(1)	0.00(1)	0.00(4)
Fantasy Oriented				
Identified	1.75(8)	0.83(12)	0.30(10)	1.25(8)
Not Identified	1.50(10)	0.90(10)	0.91(11)	0.75(8)
High-Aggressive				
Reality Oriented				
Identified	1.33(3)	1.75(4)	1.00(1)	2.40(5)
Not Identified	---(0)	---(0)	---(0)	0.00(1)
Fantasy Oriented				
Identified	1.83(6)	2.00(4)	3.60(5)	1.25(4)
Not Identified	5.25(4)	3.00(1)	0.50(2)	0.50(2)

Note: Numbers in parentheses indicate cell sizes.

Post Hoc Analyses

The AXI interaction. Mean aggression scores, for boys at each level of characteristic aggressiveness and identification, are in Table 29. Univariate tests of the identification effect at each level of characteristic aggressiveness are reported in Table 30. The only significant difference between the two identification levels was among the characteristically high-aggressive boys: Those who did not identify with the aggressive hero were more aggressive than those who did, on the initial aggression measure. By the end of the game, however, the difference between identification levels was no longer significant.

Insert Tables 29 and 30 about here

The AXIXC interaction. Means and univariate tests corresponding to this effect are presented in Tables 31 and 32, respectively. Only one test reached a traditional level of significance, the identification effect among "high A" boys who were exposed to violence-related cues. "High A" boys who identified with the aggressive hero were less aggressive than nonidentifying "high A" boys only when they were exposed to the cues. This was true only on the initial aggression measure.

Insert Tables 31 and 32 about here

Table 29

Mean Aggression Scores for Boys Identifying and Not Identifying
With the Aggressive Hero, by Characteristic Level of Aggressiveness

Characteristic Aggressiveness	Identification	
	Identified	Not Identified
Low	(n=74)	(n=54)
Initial Aggression	0.20	0.15
Total Aggression	0.62	0.81
Intermediate	(n=49)	(n=45)
Initial Aggression	0.33	0.27
Total Aggression	0.86	0.93
High	(n=32)	(n=10)
Initial Aggression	0.47	1.40
Total Aggression	2.06	2.60

Table 30

Univariate Tests of the Identification Effect Among Violence Viewers, for Boys at Each Level of Characteristic Aggressiveness

Level of Characteristic Aggressiveness	<u>F</u>	<u>p</u>
Low		
Initial Aggression	0.0693	.7930
Total Aggression	1.5591	.2154
Intermediate		
Initial Aggression	1.3882	.2422
Total Aggression	0.9993	.3205
High		
Initial Aggression	7.6430	.0071*
Total Aggression	1.9337	.1682

Note: Degrees of freedom for each test = 1,82. Error mean square = 0.5846, for initial aggression, and 1.9577, for total aggression.

*Bonferroni procedure: $\alpha_c = .05/6 = .0083$

Table 31

Mean Aggression Scores for Boys Identifying and Not Identifying With
the Aggressive Hero, for Each Cues Condition and Each Level of
Characteristic Aggressiveness

Characteristic Aggressiveness	Cues Condition	Identification	
		Identified	Not Identified
Low			
	Cues	(n=42)	(n=25)
	Initial Aggression	0.17	0.12
	Total Aggression	0.55	1.04
	No Cues	(n=32)	(n=29)
	Initial Aggression	0.25	0.17
	Total Aggression	0.72	0.62
Intermediate			
	Cues	(n=22)	(n=22)
	Initial Aggression	0.23	0.32
	Total Aggression	0.86	1.14
	No Cues	(n=27)	(n=23)
	Initial Aggression	0.41	0.22
	Total Aggression	0.85	0.74
High			
	Cues	(n=15)	(n=6)
	Initial Aggression	0.60	2.00
	Total Aggression	2.27	3.67
	No Cues	(n=17)	(n=4)
	Initial Aggression	0.35	0.50
	Total Aggression	1.88	1.00

Table 32

Univariate Tests of the Identification Effect for Violence Viewing Subjects at Each Level of Characteristic Aggressiveness and Exposure to Violence-Associated Cues

Cues Condition and Level of Characteristic Aggressiveness	<u>F</u>	<u>p</u>
Low-Aggressive		
Cues		
Initial Aggression	0.0341	.8541
Total Aggression	0.7589	.3863
No Cues		
Initial Aggression	0.6079	.4379
Total Aggression	0.1613	.6890
Intermediate-Aggressive		
Cues		
Initial Aggression	0.0073	.9323
Total Aggression	0.9282	.3382
No Cues		
Initial Aggression	1.4378	.2340
Total Aggression	0.0479	.8273
High-Aggressive		
Cues		
Initial Aggression	5.8940	.0174
Total Aggression	1.0193	.3157
No Cues		
Initial Aggression	0.4414	.5084
Total Aggression	0.0556	.8142

Note: Degrees of freedom for each test = 1,82. Error mean square = 0.5846, for initial aggression, and 1.9577 for total aggression.

Bonferroni procedure: $\alpha_c = .05/12 = .0042$.

Investigation the Effect of Reality/Fantasy
Orientation and Identifying with the Hero Among Nonviolence
Viewers

Table 33 presents the results of a MANOVA including reality/fantasy and identification effects for viewers of nonviolence. Mean scores are in Table 34. A factor called "cues" was included in this analysis to compare the subjects interviewed with the walkie talkie to those interviewed with a taperecorder. Neither stimulus corresponded to anything in the nonviolent program, so their effect upon the nonviolence viewers was expected to be neutral. The "cues" factor was included in the present analysis to test that assumption.⁹ None of the effects in Table 33 reached significant probability levels.

⁹The author would like to thank Dr. Leonard Berkowitz for drawing to her attention the importance of testing the initial neutrality of the stimulus used in such a design.

Table 33

Multivariate Analysis of Variance Table, Presenting Cues, Reality/
Fantasy and Identification Effects Among Nonviolence Viewers

Source of Variance	Error Term	df	Multivariate <u>F</u>	<u>p</u>
Characteristic				
Aggressiveness (A)	TESTED IN MAIN ANALYSIS			
Frustration Order (F)	TESTED IN MAIN ANALYSIS			
"Cues" ("C")	Gps/F"C"	2,17	0.5257	.6005
AXF	TESTED IN MAIN ANALYSIS			
AX"C"	AX Gps/F"C"	4,34	0.8874	.4820
FX"C"	Gps/F"C"	2,17	0.1773	.8391
AXFX"C"	Ax Gps/F"C"	4,34	0.7266	.5800
Groups/F"C"	TESTED IN MAIN ANALYSIS			
AX Groups/F"C"	TESTED IN MAIN ANALYSIS			
Reality/Fantasy Orientation (R)	TESTED <u>A PRIORI</u> : HYPOTHESIS 3(c)			
Identification (I)	IX Gps/F"C"	2,7	1.6448	.2597
RXI	residual ^a	2,35	0.7239	.4920
AXR	residual ^a	4,70	0.5531	.6974
AXI	residual ^a	4,70	1.0429	.3915
FXR	RX Gps/F"C"	2,11	0.2300	.7983
FXI	IX Gps/F"C:	2,7	1.9923	.2066
RX"C"	RX Gps/F"C:	2,11	0.1156	.8919
IX"C"	IX Gps/F"C"	2,7	1.8800	.2221
AXFXR	residual ^a	4,70	0.3152	.8669
AXFXI	residual ^a	4,70	0.1467	.9639
AXRXI	residual ^a	2,35 ^b	0.9559	.3943
AXRX"C"	residual ^a	4,70	0.1932	.9412
AXIX"C"	residual ^a	2,35 ^b	0.6659	.5203
FXRXI	residual ^a	2,35	1.2120	.3098
FXRX"C"	RX Gps/F"C"	2,11	0.0347	.9661
FXIX"C"	IX Gps/F"C"	2,7	0.1681	.8486
RXIX"C"	residual ^a	2,35	0.2785	.7586
AXFXRXI	residual ^a	2,35 ^b	0.8650	.4299
AXFXRX"C"	residual ^a	2,35 ^b	0.0437	.9573

Table 33 (Continued)

Source of Variance	Error Term	df	Multivariate <u>F</u>	<u>p</u>
AXFXIX"C"	residual ^a	2,35 ^b	0.1478	.8632
AXRXIX"C"	residual ^a	2,35 ^b	0.0361	.9647
FXRXIX"C"	residual ^a	2,35	0.0247	.9757
RXGps/F"C"	residual	24,70	0.6467	.8836
IXGps/F"C"	residual	16,70	0.8842	.5892

^aHad all the cells in this design been filled, interaction mean squares would have been used as the error terms for testing all the effects of interest. Since there were empty cells in some conditions, several interactions with the "groups" factor had to be dropped from the model and from the expected mean squares of lower order interactions. Therefore, the appropriate error term for some effects was residual variance, although interaction terms would have been used if there had not been empty cells.

^bBecause of empty cells in the design, degrees of freedom were reduced for some effects.

Table 34

Mean Aggression Scores for Nonviolence Viewing Subjects in Each
Classification Level and Each Experimental Condition

Viewer Classification	Initial Aggression			
	Experimental Condition			
	Frustration Before TV		Frustration After TV	
	"Cues"	No. "Cues"	"Cues"	No "Cues"
Low-Aggressive				
Reality Oriented				
Identified	0.50(4)	0.00(3)	0.29(7)	0.00(2)
Not Identified	0.11(1)	0.00(1)	0.17(6)	0.00(2)
Fantasy Oriented				
Identified	0.33(6)	0.33(6)	0.14(7)	0.00(7)
Not Identified	0.00(2)	0.25(4)	0.33(3)	0.00(4)
Intermediate-Aggressive				
Reality Oriented				
Identified	0.50(2)	0.00(1)	0.00(3)	0.00(3)
Not Identified	0.00(2)	0.00(3)	0.00(2)	---(0)
Fantasy Oriented				
Identified	0.14(7)	0.33(3)	0.00(3)	0.33(6)
Not Identified	0.00(2)	0.75(4)	0.00(3)	0.00(1)
High-Aggressive				
Reality Oriented				
Identified	0.00(1)	0.00(1)	---(0)	0.00(1)
Not Identified	---(0)	1.00(1)	0.00(1)	---(0)
Fantasy Oriented				
Identified	0.00(3)	0.00(1)	0.00(1)	0.25(4)
Not Identified	---(0)	---(0)	---(0)	---(0)

Table 34 (continued)

Viewer Classification	Total Aggression			
	Experimental Condition			
	Frustration Before TV		Frustration After TV	
	"Cues"	No "Cues"	"Cues"	No "Cues"
Low-Aggressive				
Reality Oriented				
Identified	1.00(4)	0.00(3)	1.00(7)	0.00(2)
Not Identified	0.33(9)	0.00(1)	1.50(6)	1.50(2)
Fantasy Oriented				
Identified	1.00(6)	0.83(6)	1.00(7)	1.43(7)
Not Identified	0.00(2)	0.50(4)	0.67(3)	0.50(4)
Intermediate-Aggressive				
Reality Oriented				
Identified	1.00(2)	0.00(1)	0.33(3)	0.00(3)
Not Identified	0.00(2)	0.33(3)	1.00(2)	---(0)
Fantasy Oriented				
Identified	0.43(7)	0.67(3)	0.67(3)	1.00(6)
Not Identified	1.00(2)	1.00(4)	0.67(3)	0.00(1)
High-Aggressive				
Reality Oriented				
Identified	1.00(1)	1.00(1)	---(0)	0.00(1)
Not Identified	---(0)	1.00(1)	2.00(1)	---(0)
Fantasy Oriented				
Identified	0.00(3)	0.50(1)	1.00(1)	2.00(4)
Not Identified	---(0)	---(0)	---(0)	---(0)

Note: Numbers in parentheses are cell sizes.

DISCUSSION

This paper was begun with the question of whether TV violence produces generalized aggression in children. When the research findings to date were examined, no conclusive answer emerged. Many of the reviewed studies suffered from serious methodological flaws. When one looked at the "best" studies done to date, the lingering impression was that, if violent television affects children's behaviour, it is no simple effect to be found among children in general.

The present study sought to improve upon the methodologies of previous investigations, while pitting against one another three major theories of how filmed violence might affect aggression. With regard to the first point, television condition and frustration order were experimentally manipulated to randomly assigned groups of subjects, so causal inferences could be made safely about these variables. Characteristic aggressiveness was not, of course, manipulated but it could be placed in the causal sequence on the basis of temporal precedence. Variability due to instrumentation was reduced by having trained raters measure the dependent variable, using a simple reporting procedure and a standard, explicit definition of aggression. Any instrumentation variability that did arise could not have biased the results for any treatment, since groups of subjects from all conditions were interspersed randomly in the sequence of measurement. Similarly, local history artifacts were unlikely, since eleven groups were run in each condition, from different schools and on different days. Moreover, variance due to group membership was separated out

from treatment variance, by including "group" as a factor in the analysis design. Any hint of aggressiveness was edited from the non-violent television condition, and subjects' television exposure was completely controlled by the experimenter, so there was no problem of spurious equivalence between TV conditions. Resentful demoralization of controls was prevented by ensuring in advance that violent and nonviolent programs were equally well liked by boys of this age.

Statistical conclusion validity was guarded in a number of ways. The per family error rate reduced the dangers of inflated Type I error. There were enough subjects to minimize Type II error. Even when effects had relatively few degrees of freedom, power was quite high. For instance, the FXR interaction had only 2 and 17 degrees of freedom but its power to detect an effect of .5 standard deviations or more was .96. While "opposite side" estimates of interjudge reliability were rather low, "same side" estimates indicate that aggression scores were acceptably free from measurement error. Treatment implementation was the same for all subjects in a particular condition, since it was part of a standardized procedure controlled by the experimenter.

For the sake of construct validity, the dependent variable was chosen carefully to meet the criterion of interpersonal harm-doing behaviour. Actions likely to have been accidental were not included. At least one irrelevant consideration was specifically ruled out: both aggression measures were unrelated to effective play, as measured by goal scoring. Also for the sake of construct validity, much effort was put into standardizing the TV materials. Both were the same length, involving admirable males characters who successfully met some challenge. What is even more

important, though, is the fact that they were equally exciting, so it was possible to distinguish between the effects of violence and the effects of excitement.

Hypothesis guessing was minimized in three ways. Although the children's parents knew in advance what procedures their sons would be going through, they were not informed of what, exactly, the experimenter expected them to "get" from TV, and therefore could not have passed the actual research hypothesis on to their children. Secondly, the television exposure and the dependent variable collection were presented to the children as two separate studies, focussing on two distinct spare-time behaviours of interest. This was intended to prevent hypothesis guessing during the study itself. If two groups were run from the same classroom, they were run immediately after each other, so that there was little opportunity for experienced subjects to discuss the study with naive subjects. Even if subjects from different classes had exchanged information, for instance during lunch hour or recess, it would not likely have made children suspicious of the procedures. Knowing what had happened in other groups would not likely have "tipped off" the subjects about anything except the probability of actually getting to see the cartoons. (Just in case any information had leaked out, groups were always told that the experimenter had had trouble with the cartoons in the past but had now fixed the problem.)

Experimenter expectancy effects were minimized by having the referee and raters blind to the subjects' experimental condition and to the hypotheses. Although the experimenter was aware of the subjects' treatment condition, she was absent during the data collection portion of the procedure. The experimenter later coded the audiotaped data

by group number alone, blind to condition.

External validity was enhanced by the use of actual TV network programs chosen to be popular and, therefore, likely often watched by boys in this age group. Moreover, children were studied on their own turf, with a measure of aggression that reflected some of the most common aggressive acts of boys that age--pushing, kicking, hitting and so forth in a "play" situation.

Although a great deal of effort went into designing and conducting a methodologically sound study, a number of limitations must be noted. Some of them arose from the need for trade-offs, among the four types of validity. Cook and Campbell have suggested (1976, p. 245) that the order for priorities, in the event of trade-offs, be as follows: internal validity, construct validity, statistical conclusion validity, and external validity.

Particular attention was given to internal validity. The one internal validity problem had to do with the secondary analysis of violence viewers, which included reality/fantasy orientation and identification with the aggressive hero. These variables were measured by means of retrospective self reports at the end of the experiment. They were not manipulated because of the very high costs of such manipulation in terms of construct validity and external validity. Instructions to take a particular identification or reality/fantasy orientation might have sensitized subjects to the treatments, bringing about a procedure X treatment confound, and would not have answered the question of most concern, about which children might be "at risk" for television violence effects because of the way they approach the medium. The problem of sensitization also led to the decision to measure these variables after

the dependent variable had been measured, rather than before. Because of this decision, what we know about the television and frustration order effects is not endangered by procedural sensitization. However, what we know about the reality/fantasy and identification variables may be questioned on the basis of causal direction. For theoretical reasons, these variables were treated as causes--either direct or in interaction with the manipulated variables--of floor hockey aggression. Unlike characteristic aggressiveness, however, they cannot be fixed in time with complete assurance. It is possible, for instance, that it was the level of his own aggression in the game which caused a subject to assess the television material as being only "pretend", or to say he did not pretend to be the aggressive hero.

Construct validity may have been threatened by evaluation apprehension. Subjects were aware of being observed during the floor hockey game, and this might possibly have raised the inhibitions of the players to the point where the television effects could only show up on the characteristically most aggressive (most uninhibited) boys. To minimize this possibility, the observers were introduced as interested in the fun and recreational aspects of the game. They were ostensibly there to "make the game more fun" by doing a play-by-play recording of it. The explanation was intended to focus the children's attention away from the adults' power and potential evaluativeness. The entire session was run in an atmosphere of permissiveness. Both the referee and the observers were instructed not to intervene at all during the game. Unfortunately, there is no way of being sure that the intended permissiveness was conveyed to all the subjects. Perhaps the television effects would have been more dramatic and widespread if

the subjects had not been aware of being observed.

The brevity of the television exposure may have resulted in an underestimation of effects, particularly of the disinhibition effect. One exposure to violence and cue theoretically ought to be sufficient to pull out an elicitation effect, but it seems unreasonable to expect that one program--however violence-packed--would undo all the careful efforts of home, school, and other socialization agents over at least seven years to build inner restraints against aggressiveness. It seems necessary to look at long and short term effects as separate social learning processes. When adults deliberately offer a child violent material for entertainment purposes, that may signal to the child that he is in a situation where aggression is not disapproved of, and may even be expected. Any inhibitions which are based on fear of disapproval or punishment would be suspended, for that particular situation.

This sort of informational function of violence presentation has been treated as an artifact, under the topic of hypothesis guessing. To prevent hypothesis guessing, the present study reduced the probability of such a short term effect by presenting the data collection procedure as a separate procedure. Although it has been treated as an artifact of the experimental situation, this sort of disinhibition effect would be quite generalizable to real world situations. Families that permit or encourage their children to watch violent television may be offering those children not only violent models, portrayals of reinforced aggression, and the opportunity to associate neutral stimuli with violence. They may also be informing their children that aggression is O.K. in their books.

On the other hand, the expected long term effect of repeated exposures is that the lessons of television violence will peel off children's hard-won inner restraints against aggression as fast as they are gained through the socialization attempts of parents and others. Any single exposure would have a trivial effect on the child's general aggressiveness, in the context of this ongoing struggle. The present study probably handicapped both versions of the disinhibition model, by using only a single, short exposure and by focussing on the effects of violent content per se rather than on the potentially disinhibiting message implied by showing such content.

This is both a construct validity problem and an external validity problem for tests of the disinhibition hypothesis. Another problem of external validity is the use of only boys as subjects. Furthermore, most of the subjects in this study were from lower-middle or middle class neighbourhoods, and all were in the second or third grade.

Evaluation of the Theories

With regard to the theories tested, one can say at the outset that none of them was well supported by the data, but they failed to different extents. The catharsis theory fared the worst. There was no support for its major prediction (3b), that fantasy-oriented subjects frustrated before watching a violent program would be less aggressive than those frustrated after viewing. In fact, the data tended in the opposite direction, and approached significance ($p \leq .10$). The theory's other predictions similarly went unconfirmed. Nor did any other findings in the analysis lend even qualified support for catharsis theory.

The predictions of the disinhibition theory similarly suffered at

the hands of the data. There was no overall increase in aggression among subjects who saw a violent program, and subjects who said they identified with the hero or who believed the "S.W.A.T." story was a slice of real life were not more aggressive. (In fact in the last case they were significantly less so.) Evidence of a disinhibition effect did occur in one particular case, however. Characteristically aggressive boys (not everyone) who were frustrated first and then shown a violent program (but not the other way round) were significantly more aggressive over the nine or so minutes of the hockey game, above and beyond any "cue" effects due to the walkie-talkies. While not the general result expected, this limited finding may have serious implications, to which we shall return.

Elicitation theory had predicted that boys who saw the "S.W.A.T." show would be more aggressive in their later hockey game if they were first exposed to the walkie-talkie "cue", rather than the neutral tape recorder. For the overall sample, this went unsupported. Similarly, the overall "cues effect" did not interact with either the identification or reality/fantasy factors as expected. But the theory did successfully predict that characteristically aggressive boys would show a stronger effect for cues, and it also focused attention on initial, "impulsive" aggression, which was indeed the measure upon which the "cue effect" occurred for the highly aggressive subjects. Furthermore, the predicted "cue effect" shown by characteristically aggressive subjects was the strongest, most dramatic finding of this study.

All three theoretical models predicted that reality-oriented subjects would demonstrate the strongest increase in aggression after seeing violent television, but quite the opposite pattern emerged. As one

would expect from other studies (e.g., Eron, 1982, the Polish sample) characteristically aggressive boys were overrepresented among reality viewers. They accounted for just over 14% of the sample as a whole, but more than 24% of the reality-oriented violence viewers. Even though they are a disproportionately aggressive segment of the sample, in their usual school behaviour, these reality-oriented boys were particularly nonaggressive in the floor hockey game. Eron's (1982) intervention study has suggested a mediating role for reality orientation, concluding that it enhances the effects of television violence upon children's overall level of aggressiveness. In this study, at least, there is evidence that a reality orientation may be associated with a short term decrease in aggressive behaviour following exposure to televised violence, whatever its long term effects might be. This might happen if, as Snow's (1974) subjects indicated, fantasy violence is considered "neat" and "cool", whereas real-life violence is frightening and "sickening". However, two other studies from Table 1 (Hapkiewicz & Stone, 1974; Feshbach, 1972, Expt. 2) did find that a reality orientation increased aggression in the short run. Both of these studies manipulated reality/fantasy orientation, and fell prey to some of construct validity problems that such manipulations can produce. Hapkiewicz and Stone's "real" violence program, "The Three Stooges", was probably both more violent and more exciting than the "Mighty Mouse" cartoons in their fantasy condition. Feshbach's reality/fantasy instructions have been criticized earlier as a possible wide-open invitation to hypothesis guessing. The reality/fantasy variable in the present study is not without its problems, particularly that of questionable basis for causal inference, and therefore the present study must be seen

as a weak challenger.

Another surprising finding was that characteristically aggressive boys who said they identified with the aggressive hero in the "S.W.A.T." program were significantly lower on the initial aggressiveness measure than boys who did not identify with the hero. At first glance, this would seem to offer a glimmer of hope to the catharsis theory, but the glimmer is quickly extinguished. For one thing, identifying subjects were only significantly less aggressive than nonidentifiers during the pre-game interview and the first three minutes of play. By the end of the game, they had not quite "caught up" to their nonidentifying classmates, but the difference was no longer significant, even at a per comparison error rate. If, indeed, television violence had "drained off" the need to aggress, one would expect the effect to have lasted longer. Secondly, only the subjects frustrated prior to TV viewing stood to benefit from the catharsis opportunities theoretically offered by fantasy violence. In fact, fantasy viewers were a little more aggressive if they had been frustrated prior to violence viewing than if they had been frustrated afterwards (the effect was scarcely "marginal" at even per comparison levels, however, at $p < .07$). Finally, the A x I x C interaction argues against a catharsis explanation. There is no reason why violence-related cues ought to affect catharsis.

The A x I x C interaction leads one to look to the elicitation theory for an explanation of this unexpected identification effect. Certainly the original elicitation hypotheses would have predicted just the opposite to what happened. Multiple comparisons following the three-way interaction indicated that characteristically aggressive boys who identified with the aggressive hero were less aggressive than

nonidentifiers only if the aggressive cue was presented. This difference was significant only at per comparison α levels ($p < .02$) and must therefore be considered with some caution. It does suggest the possibility of cue-elicited aggression anxiety.

The main problem with such an explanation is that characteristically high-aggressive boys seem like an unlikely group to fall prey to aggression anxiety. In fact, as a group they have been identified as particularly unlikely to show signs of guilt or remorse over aggression (Eron, et al., 1971; Perry & Bussey, 1977). However, aggression anxiety may also result from a history of being punished for aggression (Berkowitz & Frodi, 1977). Boys whose teachers had identified them as being aggressive may well have been heavily punished for aggression. One of the items on the measure of characteristic aggressiveness describes the child as "always getting into trouble." Eron's research has indicated that characteristically aggressive boys are more likely than their classmates to receive physical punishment from parents (Eron et al., 1971) and to be rejected by their peers (Eron, 1982). It seems plausible, then, that characteristically aggressive children may have learned an association between punishment and their own aggressive behaviour. Pretending to be an aggressive hero might have led such a subject to experience aggression anxiety while watching the televised violence. Later exposure to a TV-violence-related cue might have elicited aggression anxiety, and a temporary reduction of aggressiveness.

If these boys can be made so anxious in the presence of aggression-related stimuli, one might wonder how they manage to remain so highly aggressive in their day-to-day lives. For one thing, the rewards of aggressive behaviour may well outweigh the associated punishments, in

the long run (c.f. Buss, 1971; Bandura, 1978). A second answer may lie in the very short duration of this effect. By the end of the game, these subjects were not detectably less aggressive than their nonidentifying (and presumably not anxious) high-aggressive cohorts.

This explanation requires a great deal of bending over backwards to fit the data, and flies in the face of both theory (e.g., Hypothesis 2(c)) and empirical research with children (Eron, 1982) and adults (e.g., Leyens & Picus, 1973; Turner & Berkowitz, 1972). Like the reality/fantasy measure, the measure of identification is rather a weak spot in the present study and thus permits a timid question rather than a strong challenge to research and theory on the question of identification.

Possible Theoretical Refinements

Some theoretical retooling will be in order if these results prove general. For one thing, catharsis theory has received yet another vote of nonconfidence. There seems to be little likelihood that further effort in search of this phenomenon would be rewarded.

Neither elicitation nor disinhibition theory presently predict their effects only when subjects are frustrated prior to watching violent television ... though that is the procedure Berkowitz has most commonly followed in his elicitation research. It seems that subjects may need to be "set to aggress" while viewing the violence as well as later on, when the violence related cue presents itself, if elicitation is going to occur following a single exposure to televised violence and its associated cue. Perhaps there must be both internal and external cues common to both the viewing situation and the potential aggression situation, before aggression will actually be elicited. Presenting subjects with the television program when they were not frustrated may have been

equivalent to presenting Pavlov's dogs with the bell and meat powder when they were not hungry. Berkowitz's "weapons effect" presumably does not need this "double cueing", since there is no reason to believe that we learn the "aggressive meaning" of weapons only on occasions when we are angry. Of course, the connection between weapons and aggression is an association we are presented with over and over again, in our lives. "Double cueing" may only be necessary when the connection has not yet been well learned.

The disinhibition model will also need some reworking if future studies continue to find disinhibition only among subjects frustrated before TV viewing. Since the effect does not appear to rely upon identification, there is nothing presently in the model to explain this outcome. In fact, Bandura generally introduces frustration into his procedure after the viewing condition, if at all (c.f. Bandura, Ross, & Ross, 1963(a); 1963(b)).

In addition, some explanation is required to explain why only the characteristically high-aggressive boys experienced disinhibition. It seems likely that the inhibitions of these boys are already so weak that they are very easily removed--even by one television program. Other boys may have such well-established inhibitions that any single program has a negligible effect upon them. Characteristically aggressive boys may also be more susceptible to the situational "demands" communicated by offering children violence-laden entertainment. What inhibitions they do have may depend on fear of punishment and disapproval by others. Less characteristically aggressive boys may be further inhibited by their self-imposed punishments of guilt and self-disapproval.

Clearly, the nature of short and long term disinhibition needs to

be made more specific, before either can be given a fair experimental test. If the link between the two is to be established, some specific predictions will need to be made about how these effects accumulate. Otherwise, studies using brief exposures to televised violence will be of limited theoretical usefulness. Finally, it seems safe to conclude now that one can have both kinds of TV violence effects from the same exposure: a short term elicitation effect and a longer term disinhibition effect.

General Implications of the Study

Theoretical interests aside, what do the results tell us about the dangers of TV violence for boys' behaviour? The most powerful effects in the study all involved characteristic aggression. Using Tatsuoka's (1970) multivariate adaptation of Hays' (1963) ω^2 , it was estimated that this factor accounted for 28.3% of the variance in aggression during the hockey game. It is not surprising that boys who seemed to their teachers to be quite aggressive acted so in this particular situation. (It does validate the floor hockey measures as indices of aggression.) But all of the "action" in this study occurred among those 56 "highly aggressive" boys.

The 201 "low" and 139 "intermediate" subjects showed no interpretable TV or cue effects. One supposes that the cumulative effects of their reinforcement histories, role models, etc. have given them stronger "inner controls" over their aggressive impulses. Perhaps a larger or more sustained dosage of TV violence in some future study would evoke generalized aggression from a larger proportion of the boys. But the present data show effects only among 14% of the subjects.

That is not necessarily a socially insignificant finding. Stimuli which make boys with poor "inner controls" aggress may be the sparks that start fires, and if several million boys are watching a violent TV show that can be a lot of fires. Furthermore, there were two effects among these subjects: the rather powerful cue-triggered elicitation effect which appeared quickly among the boys frustrated prior to TV viewing, and a much weaker but longer-lasting disinhibition effect, also among subjects frustrated prior to viewing.

Re-examining the Literature, with Hindsight

One might ask how often the present findings have appeared in past research. The answer is, not very often, but then there are many hesitations about most of the studies. For example, only three of the studies in Table 1 deliberately frustrated subjects prior to the TV exposure: Biblow (1973), Mussen & Rutherford (1961), and Wotring and Greenberg (1973). The first study found no effect at all, but this could have been due to unreliability of those who rated children's aggression. Furthermore, Biblow did not use typical television material, but a slide show called "The Enemies". Mussen & Rutherford found a positive effect regardless of frustration condition in their study. However, it is questionable whether their balloon-popping questions measured anything to do with aggression and their "television" effects may have been largely "excitement" effects. Neither of these studies looked at the characteristic aggressiveness of their subjects. For that matter, Wotring and Greenberg (1973) both underrepresented the aggression construct (with their questionnaire about potential conflict situations) and included construct irrelevancies in the television

treatment by confounding violence and excitement. They risked sensitizing their subjects to the treatment, and encouraged hypothesis guessing, by having a rather obvious procedure and adopting no cover story. They jeopardized generalizability by constructing their TV conditions as a hodgepodge of 15 or 16 unrelated violent or non-violent episodes.

Only two studies in Table 1 considered the pre-treatment aggressiveness of the subjects studied. Stein, Friedrich, and Vondracek (1972) found in their experiment that TV violence increased aggression on one "interpersonal" measure of aggression only for their characteristically aggressive subjects. These subjects did not become more aggressive; their aggression simply did not subside as much as that of children in the neutral TV conditions. Children who watched prosocial TV were as unchanging as the violence viewers in their level of aggressiveness, however. To add to the confusion, Stein et al. did find TV effects that cut across characteristic aggressiveness levels on the other two measures, both of which dealt with aggressive responses following frustration in the nursery classroom. Leifer and Roberts (1972, Experiment 4) found that only their characteristically low-aggressive subjects increased their aggression following exposure to TV violence.

Both of these studies, too, were riddled with problems. Stein, Friedrich and Vondracek, suffered a per family error rate of 1.0, and probably confounded violence and excitement in their television manipulation. In Experiment 4, Leifer and Roberts had an error rate of .35, used a muddy operationalization of aggression (another response hierarchy!) and encouraged hypothesis guessing by using a very transparent pre-post measurement design.

These two examples illustrate how difficult it will be to untangle the past findings in this literature. This and other studies have varied so much on so many procedural dimensions that it is impossible to say exactly why they obtained different results. For every study that fits a particular pattern, and for every study that does not fit, there is at least one explanation that lies solely in faulty methodology of some kind. Those determined to see a pattern can disregard the "misfits" as methodologically flawed. Those determined to see no pattern can find a host of methodological reasons to disregard findings of positive and negative associations.

Directions for Future Research

Our only recourse is to future experimentation. The spurt of activity in the early 1970s may have led to the erroneous conclusion that this topic is "researched out." Nevertheless, little is settled with any finality. Until much more research is done, with a high level of conclusion validity, most of our questions will remain unanswered.

Thus far, television violence has been expected to have, or not have, an effect on children's aggression. It actually may have several different effects. One can separate specific modelling effects from more general ones, and elicitation effects from general disinhibitory ones among the latter. Perhaps some of the confusion would be lessened if an effort was made to investigate these effects separately.

One study seems obvious at this point. Although there was only a weak and limited disinhibition effect in the short time period used in this study, the possibility remains that daily violence viewing is gradually stripping away the inhibitions which keep most boys in control of their aggressive impulses. At the moment, a

cumulative disinhibition effect seems to be the best explanation for the long term effects that are sometimes found. Would there be a stronger disinhibition effect in the present study if boys had seen violent TV programming for two or three days instead of once? The present study could be extended to find out.

This author would like to continue work in several directions. I would like to further explore the elicitation effect, particularly to examine how long a stimulus remains a cue to aggression once it has been paired with televised violence. Perhaps new cues only "work" when they are presented rather soon after pairing, or perhaps children who watch a great deal of violent TV eventually acquire a host of violence-associated cues which have the power to elicit aggression from them. Perhaps this "repertoire" is made up only of cues which are perpetually paired with violence (such as guns). In future research, I would like to vary the intervals between television exposure and cue presentation, including an intervening "extinction" trial for some subjects, in which the cue was re-presented in a nonviolent television context.

A second important question is whether "high-risk" subjects are likely to expose themselves to violent TV material when they are most at risk--i.e., when they are frustrated or angry. In future research, frustrated high-aggressive boys could be given the choice of watching violent TV or engaging in alternative activities (including watching nonviolent television, and engaging in some quiet, solitary activity). The effects of self-chosen viewing upon subsequent interpersonal aggression might be quite different from being arbitrarily exposed to violent or nonviolent television material.

I continue to be puzzled and intrigued by the present study's findings about the relationship of identification and reality/fantasy orientation with TV violence. In future, it might be necessary to measure these variables by exposing subjects on another occasion to similar material under similar circumstances to those in the experiment itself. Measuring the child's fantasy style might well be useful in understanding the effect of these subject variables in moderating the effects of experimentally manipulated variables. In future, information about other background variables, such as home television viewing, could be used to help trace the relationship between the short term aggression effects and characteristic aggressiveness.

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APPENDIX A

LETTER SENT HOME TO PARENTS FOR PILOT STUDY I



THE UNIVERSITY OF MANITOBA

DEPARTMENT OF PSYCHOLOGY

WINNIPEG, CANADA
R3T 2N2

Dear Parent:

During the next few weeks we will be conducting a study at a number of Winnipeg schools on what children get out of TV. This letter is being sent home with all the boys in your son's room. The part of the study to be conducted at your child's school is a "pilot study" designed to find out what kinds of television programs are exciting and interesting for children in grades two and three. Children will be asked simply to tell us what their favorite TV programs are, and what programs they find most exciting. They will also listen to a description of the major study we will be doing later, and we will ask them to indicate what their reactions would be if they were in that situation.

If you are willing to have your child participate in this study please sign the permission slip at the bottom of the page and have it returned to your child's teacher today or tomorrow. If you have any questions you would like to ask us about the study, please call Wendy Josephson in the evening at 452-1706. We will send home a report of the study's results once they are available.

Yours sincerely,

Wendy Josephson
Ph.D. Student

Robert A. Altemeyer
Associate Professor

=====

My child, _____ has my permission to
serve in the TV study. My child is _____ years old.

(Signed) _____
(Parent or Guardian)

Please return in envelope.

APPENDIX B

ANSWER BOOKLET USED IN PILOT STUDY I

How old are you? _____

What TV shows do you like to watch the best?

1. _____

2. _____

3. _____

What are the most exciting shows on TV?

1. _____

2. _____

3. _____

APPENDIX C

LETTER SENT HOME TO PARENTS IN PILOT STUDIES II, III, AND IV



THE UNIVERSITY OF MANITOBA

DEPARTMENT OF PSYCHOLOGY

WINNIPEG, CANADA
R3T 2N2

Dear Parent:

During the next few weeks we will be conducting a study at a number of Winnipeg Schools on what children get out of watching TV. This letter is being sent home with all the boys in your son's room. The study to be conducted at your child's school is designed to find out what kinds of television programs are exciting and interesting for children in grades two and three. Children will be shown part of an ordinary television program (for example, "KOJAK" "CHIPS", or "HAPPY DAYS") and will be asked how much they liked it and how exciting they found it.

If you are willing to have your child participate in this study, please sign the permission slip at the bottom of the page and have it returned to your child's teacher today or tomorrow. If you have any questions you would like to ask us about the study, please call Wendy Josephson in the evening between 7:00 and 9:00 at 452-1706. A copy of the study's results, once completed, will be available at the school.

Yours Sincerely,

Wendy Josephson
Ph.D. StudentRobert A. Altemeyer
Associate Professor

-
-
- () My child does have my permission to serve in the TV study.
() My child does not have my permission to serve in the TV study.

(Signed) _____

Parent or Guardian

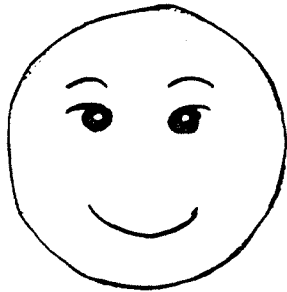
APPENDIX D

LIKING AND EXCITEMENT SCALES INCLUDED IN RESPONSE BOOKLETS FOR PILOT STUDY II

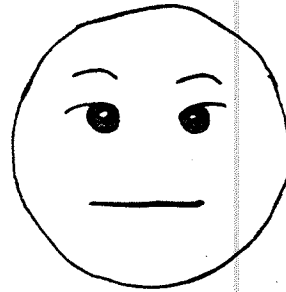
What did you think of the TV show you just saw?



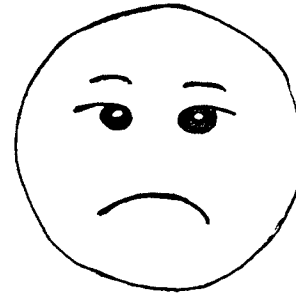
1. It was great!



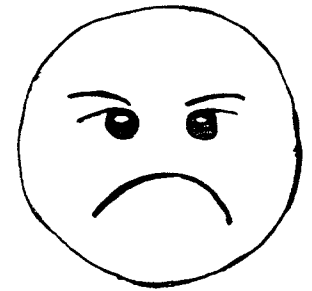
2. It was good.



3. It was O.K.

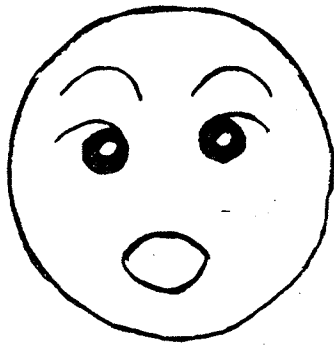


4. It wasn't very good.

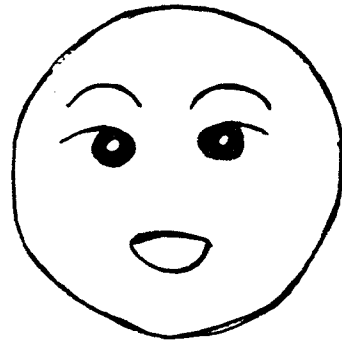


5. It was awful!

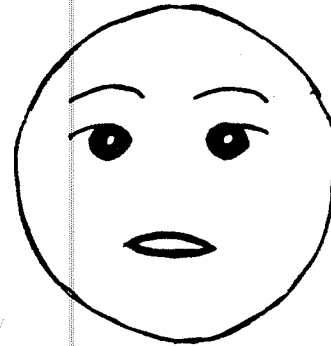
How excited did you feel while you were watching the TV show?



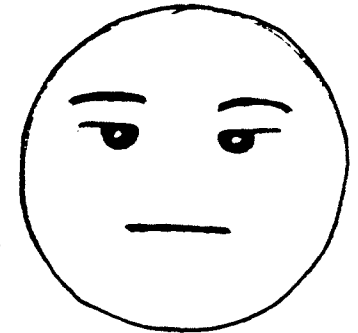
1. Very Excited



2. Pretty Excited



3. A Little Bit Excited



4. Not At All Excited

APPENDIX E

AN ATTEMPT TO FIND A NONEXCITING BUT WELL LIKED TELEVISION PROGRAM

As mentioned in the introduction, a general excitement explanation is a reasonable alternative hypothesis in many studies of television violence effects on children's aggression. To eliminate this alternative hypothesis requires only that the violent and nonviolent programs be equally exciting. Finding two such programs was the purpose of Pilot Studies I to IV. Directly testing that alternative hypothesis would require another control condition: television material that is both nonviolent and nonexciting. It is essential that this third program be as well liked as the other two, however, so that frustration, inattention, or other byproducts of a dull experience are not confounded with the excitement manipulation. The results of the pilot studies suggested that it would be difficult to find such a program. High average excitement scores were invariably paired with high liking of the TV programs. The one T.V. clip that was rated as substantially less exciting was also significantly less well liked. On the other hand, the reduced correlation between liking and excitement for "S.W.A.T." and C.H.I.P.S." in Pilot Study IV suggested that liking was not entirely dependent on excitement.

Study I

Method

Continuing the search for a nonexciting but well-liked program, 145 second and third grade boys were recruited from four Winnipeg schools. The recruitment of subjects and the procedure were the same as for Pilot Study IV, except that subjects filled out either the liking scale or the excitement scale, not both. This was done to remove the effect of filling out one scale on subjects' responses to the other scale. There were six programs shown, in randomly selected subsets of three, to the subjects. The programs were: two more episodes from "Happy Days" and a segment

each from "Big Blue Marble", "Different Strokes", "Animals, Animals, Animals", and "Untamed World."

Results and Discussion

The mean liking and excitement ratings of these programs are presented in Table 35, along with the mean scores of the programs tested previously in pilot studies. The correlation between the mean liking and mean excitement scores for these 11 programs was $-.88$. Excluding the programs from the earlier pilot studies, which differed in procedure, measurement instrument, and "purity" of the scores, increased the correlation to $-.96$.

Table 35

Mean Liking and Excitement Scores for All T.V. Programs Rated

Program	Excitement	N	Liking	N
"C.H.I.P.S." ^a	3.81*	(26)	1.30	(27)
"S.W.A.T." ^a	3.69*	(26)	1.25	(18)
"Happy Days" I ^a	3.85*	(14)	1.19	(14)
"Happy Days" II ^a	3.38*	(8)	1.25	(8)
"Happy Days" III	3.80	(20)	1.42	(19)
"Happy Days" IV	3.58	(26)	1.22	(27)
"Brady Bunch" ^a	2.92	(15)	2.07	(15)
"Big Blue Marble"	2.60	(30)	3.57	(35)
"Different Strokes"	3.71	(41)	1.10	(30)
"Animals, Animals, Animals"	3.27	(15)	2.53	(19)
"Untamed World"	3.43	(14)	1.77	(18)

Note: Higher scores mean higher excitement, lower liking.

*Scores reversed from original scale, for comparability.

^aThe means for liking and excitement for "S.W.A.T." and "C.H.I.P.S." are "pure". That is, they are based on those subjects who filled out that particular scale on that particular program first. They had not, as yet,

been exposed to the other scale at all. For "Happy Days I" and "Brady Bunch", the sample exposed to that particular program first was halved. Liking scores were calculated on one half of the sample and excitement scores were calculated on the other half. However, the excitement scale was always administered first in "Happy Days" I, and second for "Brady Bunch" and "Happy Days" II. Therefore, the liking mean score is "pure" for "Happy Days" I, and not for "Brady Bunch" and "Happy Days" II. The excitement mean score is pure for "Happy Days" II and "Brady Bunch", but not for "Happy Days" I.

Such results made it seem very unlikely that a nonexciting program could be found that would be comparable in popularity to the two exciting programs. Although individuals' ratings of liking and excitement had been as low as $-.40$ in Pilot Study IV, this was true only for the highly popular and highly exciting "C.H.I.P.S." and "S.W.A.T.". The lower correlation was not merely the result of insufficient variation associated with the higher mean scores of "C.H.I.P.S." and "S.W.A.T.". The variances in Pilot Study IV were $.46$ for liking and $.41$ for excitement. The variances of the mean scores in the present study were $.58$ for liking and $.16$ for excitement. Perhaps the degree of independence between scores that was achieved in Pilot Study IV is possible only once an adequately high level of one or both scores is achieved. Past this "critical" level, other factors may exert a stronger influence on each score. For instance, a fairly high excitement value may be necessary for a high liking score, but it may not be sufficient.

Study II

Two hypotheses were considered to account for the results of Study I. One hypothesis was that only exciting TV fare is popular with second and third grade boys. A second hypothesis was that the distinction between liking and excitement was too difficult for second and third graders to make. Liking of a program might be, for them, a cue for the emotional

label of "excitement". If so, subjects might experience differential states of arousal, under two T.V. conditions, but report themselves differentially excited only if they had different degrees of liking for the programs. A second study was conducted, employing physiological measures, to test this second hypothesis.

Method

Subjects. Permission letters were sent home with second and third grade boys in yet another Winnipeg school. A copy of the letter can be found at the end of this Appendix. From among those boys with parental permission, 33 were chosen at random to be in the study.

Apparatus and materials. Heart rate was assessed by means of a Whittaker Pulse Watch. Readings were taken every 15 seconds, and recorded by the experimenter. The galvanic skin response (GSR) was recorded by means of the Lafayette 7609A Psychogalvanometer and 77010 Single Channel Recorder. The GSR recordings of resistance were made in the A. C. automatic centering mode. Lafayette electrodes (#76602), curved to conform to finger shape, were applied to the volar pads of the thumb and third finger of the left hand, with a zinc-sulphate gel to improve contact between skin and electrode. The electrodes were held in place by a velcro wrap.

Three television excerpts were used, all approximately 14 minutes long: "S.W.A.T." and "C.H.I.P.S.", from the original pilot studies, and a documentary about the cat family from "Untamed World". Of all the programs rated as significantly less exciting than "C.H.I.P.S." and "S.W.A.T." in Study I of this Appendix, "Untamed World" had been liked the best. (It was, nevertheless, still significantly less popular than the other two programs.) The television excerpts were shown via a Sony AV8600 colour videotape recorder and a Sony CVM-1200U 12-inch colour television receiver.

Procedure. Subjects were taken from their classrooms, individually, to a large, quiet room (the school's "Special Uses" room). On the way to the room, the experimenter introduced herself to the subject and explained that she was doing a study about "the kinds of T.V. programs that boys of your age like." She told the subject that she had brought some T.V. programs to the school for boys to watch, and some equipment that would show how excited a boy was while he watched the T.V. show. She then invited the subject in to see the equipment for himself.

The experimenter showed all the equipment to the subject, and let him see another subject's response record. She seated the subject in front of the T.V. and put the finger cuffs and the Pulse Watch clip on her own fingers. She allowed the subject to try them on, as well. The experimenter's and subject's fingers were cleaned with a cotton ball soaked in alcohol, before applying the finger cuffs, and the cuffs were carefully cleaned before the subject put them on.

The experimenter then asked the subject if he would like to be in her study. She explained that she would be asking him to sit very quietly, with the clip and cuffs on his fingers, while she got her equipment ready. Then she's show him a T.V. program, and ask him a couple of questions about it. All of the subjects agreed to be in the study, although one boy appeared to be somewhat nervous about the electrodes. He was returned to class and replaced by another subject, randomly selected from among those subjects who had parental permission.

The experimenter made sure that the subject was seated comfortably, with his left hand and arm on the table. She reminded him that it would be a few minutes before she turned the T.V. on, since she had to "balance the recorder" first. She reminded him:

Remember, I need to have you sit very quietly and still from now on, and even for a few minutes after the T.V. program is over. Just sit there quietly after it is over until I come over and help you take the cuffs off your fingers. The reason for that is that if you move your hand around, it can make the recorder jump, so it looks as though you were extra-excited about the TV show, when really you just moved your hand. Are you ready? Would you like to ask any questions before we start?

Once the recording pen was centered, and the psychogalvanometer switched to the A.C. mode of operation, a one-minute base line was recorded. Then one of the three television programs (determined randomly) was shown to the subject. After the program was over, the experimenter recorded for a further two minutes, then removed the subject's electrodes and Pulse Watch clip.

The experimenter then administered the excitement scale, and the liking scale in the same manner as her Pilot Study IV.

Results and Discussion

Two physiological scores were calculated for each subject, using Zillman's method (1971, p. 425). Δ Heart rate was calculated in the following manner:

Heart rate readings one minute before the end of the T.V. presentation, 30 seconds before the end, at the end, and 30 seconds after the end, were averaged. This average was subtracted from the reading taken thirty seconds before the T.V. program was turned on.

Δ GSR was calculated in a similar manner. The maximum GSR readings, in each ten-second interval from 1 minute before the program's end until 30 seconds after its end, were averaged. This mean was subtracted from the average of the maximum readings in the three 10-second periods just before the T.V. material was switched on. GSR readings were expressed in terms of Arousal Interval Scale scores, as recommended by Brodsky &

Brodsky (1978).

Subjects' mean scores on these physiological measures, and on the liking and excitement scales, are presented in Table 36. The intercorrelations between the measures are reported in Table 37. "Untamed World" was rated as significantly less exciting ($F(1,30)=9.3103$, $p=.0048$) and less well liked ($F(1,30)=9.5339$, $p=.0044$) than the other two programs. This comparison was not significant for the physiological measures ($F(1,30)=0.0287$, $p=.8667$, for ΔHR ; ($F(1,30)=0.7919$, $p=.3807$, for ΔGSR). The difference between "S.W.A.T." and "C.H.I.P.S." was nonsignificant for all four dependent variables (all $F_s < 1.0$, all $p_s > .70$).

Table 36

Mean ΔGSR , Δ Heart Rate, Excitement, and Liking Scores for Viewers of "S.W.A.T.", "C.H.I.P.S." and "Untamed World"

Program	Dependent Measure			
	ΔHR	ΔGSR	Excitement	Liking
"S.W.A.T."	-1.26	0.30	3.18	1.27
"C.H.I.P.S."	-1.03	0.39	3.18	1.18
"Untamed World"	-1.69	0.86	2.36	1.91

Note: $n = 11$ subjects in each program viewing condition. Higher scores indicate higher excitement, lower liking.

Table 37

Intercorrelations of Dependent Variables Among Viewers of All Three Programs

	Δ HR	Δ GSR	Excitement	Liking
Δ HR	1.0000			
Δ GSR	-.3858*	1.0000		
Excitement	.2891	-.0797	1.0000	
Liking	-.2258	.0524	-.3489*	1.0000

* r (crit. $p < .05$; $df = 31$) = .2917, for a one-tailed test.

The failure of the physiological measures to distinguish between programs was discouraging, although it was reassuring to find no differences in physiological arousal between "C.H.I.P.S." and "S.W.A.T.". This pattern of results suggested that the experimenter may have been completely mistaken about the excitement value of a documentary about cats. Subjects may have been just as excited by it as they were by "S.W.A.T." and "C.H.I.P.S.", but might have been rating it as less exciting because they liked it less. The correlation between the two measures argued against this interpretation however. The excitement and liking scores were obviously not just two measures of the same thing: Although significantly correlated, they shared only 12% of their variance in common.

At this point, the search for a nonexciting but well liked control program was abandoned. Hopefully, it will be possible to find such a program for use with elementary school boys. Until it is found, no direct test of the arousal effects of television on aggression will be possible with this age group.



THE UNIVERSITY OF MANITOBA

DEPARTMENT OF PSYCHOLOGY

WINNIPEG, CANADA
R3T 2N2

Dear Parent:

During the next few weeks we will be conducting a study at a number of Winnipeg schools on what children get out of watching TV. This letter is being sent home with all the boys in your son's room, although it is possible that there will not be time to include all the boys in the study. We are trying to find out how children react to different television programs. We will measure how excited children become (for instance, how fast their hearts beat) when they watch part of an ordinary television program such as "Untamed World" or "S.W.A.T.". We will record this information from little cuffs which the boys will wear around their fingers. We will also ask them how much they liked the program, how exciting they thought it was, and a few questions to see how much of the program they remember.

If you are willing to have your child participate in this study, please sign the permission slip at the bottom of the page and have it returned to your child's teacher today or tomorrow. If there are any questions you would like to ask us about this study, please call Wendy Josephson between 7:00 and 9:00 in the evening at 452-1706. A copy of the study's results, once completed, will be available at the school.

Yours sincerely,

Wendy Josephson
Ph.D. Student

Robert A. Altemeyer
Associate Professor

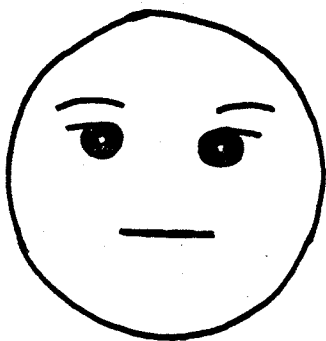
My child has my permission to serve in the TV study. My child's name is _____

(Signed) _____

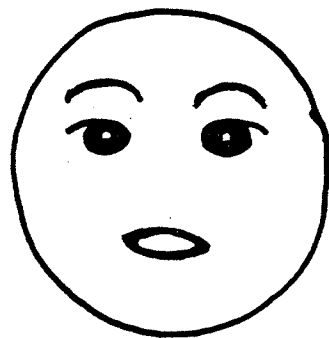
Parent or Guardian

APPENDIX F**REVERSED EXCITEMENT SCALE USED IN PILOT STUDIES IV, V AND VI**

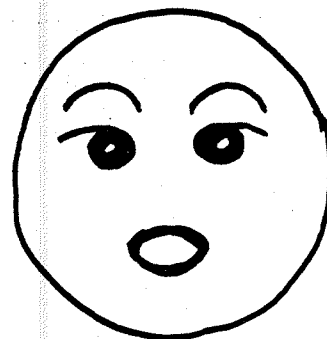
How excited did you feel while you were watching the TV show?



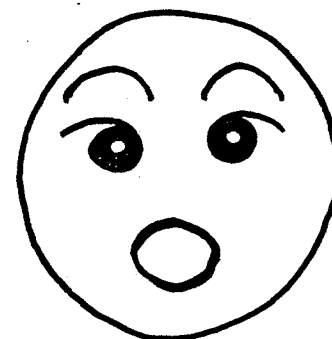
1. Not At All Excited



2. A Little Bit Excited



3. Pretty Excited



4. Very Excited

APPENDIX G

LETTER SENT HOME TO PARENTS FOR THE MAIN EXPERIMENT



THE UNIVERSITY OF MANITOBA

DEPARTMENT OF PSYCHOLOGY

WINNIPEG, CANADA
R3T 2N2

Dear Parent:

In a few days we will be conducting a little study at your son's school on the things that children get from watching television. We would like to ask your permission for your son to be one of the subjects in this study.

In our study, children will be shown part of an ordinary television program, such as "C.H.I.P.S." or "S.W.A.T.". We will then find out, by asking a few questions and playing a short game, what they "got" from it. The whole study will take about 30 minutes, much of which will be spent playing floor hockey.

If you are willing to have your son participate in this study just sign the permission slip at the bottom of the page and have it returned to your son's teacher today or tomorrow. If you have any questions you would like to ask about the study, please call Wendy Josephson at 474-8255. We will send a report of the results of this study to the school once they are available.

Yours sincerely,

Wendy Josephson
Lecturer

Robert A. Altemeyer
Associate Professor

My child _____ has my permission to serve
in the "TV study".

(Signed) _____

(Parent or Guardian)

APPENDIX H

CHARACTERISTIC AGGRESSIVENESS MEASURE

TEACHER RATING FORM

Please check off on this list any behavior which _____
shows at school:

- _____ Does not listen to the teacher.
- _____ Gives dirty looks or makes unfriendly gestures to other students.
- _____ Makes up stories and lies to get other students into trouble.
- _____ Does things that bother others.
- _____ Starts fights over nothing.
- _____ Pushes or shoves other students.
- _____ Is always getting into trouble.
- _____ Says mean things.
- _____ Takes other students' things without asking.

APPENDIX I

QUESTIONNAIRES FOR OBTAINING INFORMATION ABOUT REALITY/FANTASY ORIENTATION
AND IDENTIFICATION, FOR THE VIOLENT AND NONVIOLENT TELEVISION PROGRAMS

1. Did the things on this show really happen?

_____ Yes

_____ No

2. Is the S.W.A.T. leader a real person?

_____ Yes

_____ No

3. Is the S.W.A.T. leader like a real person?

_____ Yes

_____ No

4. While you were watching this show, did you sometimes pretend that you were one of the people in the show?

_____ Yes

_____ No

5. Who did you pretend you were?

1. Did the things on this TV show really happen?

Yes

No

2. Is the boy Danny a real person?

Yes

No

3. Is Danny like a real person?

Yes

No

4. While you were watching this show, did you sometimes pretend that you were one of the people in the show?

Yes

No

5. Who did you pretend you were?

APPENDIX J

RAW DATA FOR THE MAIN EXPERIMENT

Key for Data Coding

- column 1: Characteristic Aggressiveness
1 = high, 2 = intermediate, 3 = low
- column 2: Frustration Order
1 = before TV, 2 = after TV
- column 3: Reality/Fantasy Orientation
1 = reality, 2 = fantasy
- column 4: Identification with Protagonist
1 = identified, 2 = not identified
- column 5: Television Condition
1 = nonviolent content
2 = violent content only
3 = violent content and cues
- column 6: Cues
1 = cues, 2 = no cues
- columns 7-8: Group number (within each level of television condition
and frustration order)
- column 9: Initial Aggression score
- columns 10-11: Total Aggression Score

co.1 1 2 3 4 5 6 7 8 9 10 11

2	1	2	2	3	1	0	1	0	0	0
1	1	1	1	3	1	0	1	1	0	1
3	1	2	2	3	1	0	1	1	0	4
1	1	2	2	3	1	0	1	2	0	4
1	1	2	2	3	1	0	1	9	1	4
1	1	2	1	3	1	0	1	2	0	4

2	1	2	2	3	1	0	2	1	0	2
2	1	2	1	3	1	0	2	0	0	2
3	1	2	2	3	1	0	2	0	0	2
3	1	2	1	3	1	0	2	0	0	1
2	1	2	2	3	1	0	2	0	0	0
1	1	2	2	3	1	0	2	0	0	2

3	1	2	1	3	1	0	3	0	0	0
3	1	2	2	3	1	0	3	0	0	0
1	1	2	1	3	1	0	3	0	0	0
2	1	1	1	3	1	0	3	0	0	0
3	1	2	1	3	1	0	3	0	0	0
3	1	2	1	3	1	0	3	0	0	0

2	1	2	2	3	1	0	4	0	0	1
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col. 1 2 3 4 5 6 7 8 9 10 11

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col.	1	2	3	4	5	6	7	8	9	10	11
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col.	1	2	3	4	5	6	7	8	9	10	11
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col.	1	2	3	4	5	6	7	8	9	10	11
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col.	1	2	3	4	5	6	7	8	9	10	11
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col. 1 2 3 4 5 6 7 8 9 10 11

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col.	1	2	3	4	5	6	7	8	9	19	11
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col. 1 2 3 4 5 6 7 8 9 10 11

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col.	1	2	3	4	5	6	7	8	9	10	11
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col.	1	2	3	4	5	6	7	8	9	10	11
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3	2	2	1	1	2	0	3	0	0	0	1
2	2	2	2	1	2	0	3	0	0	0	0
3	2	2	1	1	2	0	4	0	0	0	5
2	2	2	1	1	2	0	4	1	0	0	2
3	2	1	2	1	2	0	4	0	0	0	1
1	2	2	1	1	2	0	4	0	0	0	5
3	2	2	1	1	2	0	4	0	0	0	1
3	2	2	1	1	2	0	4	0	0	0	0
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3	2	1	1	1	1	0	1	0	0	0	1
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3	2	1	1	1	1	0	1	1	0	0	1
2	2	1	2	1	1	0	1	0	0	0	0
3	2	1	2	1	1	0	2	0	0	0	5
3	2	2	1	1	1	0	2	1	0	0	2
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col.	1	2	3	4	5	6	7	8	9	10	11
3	2	1	2	1	1	0	3	0	0	0	1
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3	2	2	2	1	2	0	5	0	0	0	2
2	2	2	1	1	2	0	5	1	0	0	1
3	2	2	1	1	2	0	5	0	0	0	1