

## Television Violence and Aggression: Setting the Record Straight

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The recent release of the Federal Communications Commission (FCC) report on violent television programming and its call for restricting children's access to such programming has once more brought this issue to public attention. There have been many statements about this issue by psychologists, politicians, and others.

By those who believe that television violence is harmful, we have been told that there is overwhelming evidence that exposure to violence on television causes aggression (what I will call the causal hypothesis for convenience), that there is no longer any legitimate debate about this, and that the effect is as strong as the effect of cigarette smoking on cancer. We have even been told that the press is biased because it gives more time to the opposing view than is warranted – that since there is no question, the press should not even mention the other view.

None of this is correct. The evidence is not overwhelming – indeed, it provides no good reason to believe that television violence causes aggression much less serious violence. The debate is certainly not over although some would like it to be. There is no comparison between the effect of smoking on cancer and the effect of television violence on aggression. And the press has, if anything, given far too much attention to the causal hypothesis than to those who disagree with it. The most ardent advocates of the causal hypothesis seem to object to any disagreement or criticism of their position. However, their position is wrong, it deserves to be criticized, and it is time once more to set the record straight

It should be clear from the government reports on this issue that the evidence is not overwhelming and that the debate is not over. In the short time that we have been in the 21st century, the government has produced three reports dealing with the effect of media violence. The Surgeon General's Report on Youth Violence concluded that exposure to television violence causes a short-term increase in aggression, but went on to say that television violence has little or no role in causing real violence. The Federal Trade Commission (FTC) report concluded that

exposure to media violence is correlated with aggression, but that the evidence is insufficient to know whether exposure to media violence *causes* the increase in aggression. The third report was released by the FCC on April 25, 2007.

### **The FCC Report on Television Violence**

The statement in the FCC report is somewhat harder to understand, since it says: “We agree with the views of the Surgeon General and find that, on balance, research provides strong evidence that exposure to violence in the media can increase aggressive behavior in children, at least in the short term.” How can one say that on balance (implying that the conclusion is far from clear-cut) there is strong evidence? If there really is strong evidence, then it’s not a question of “on balance.” One cannot have both “on balance” and “strong evidence” in the same sentence and have it make sense. Despite this somewhat odd wording in the FCC report, none of these reports considered the evidence overwhelming and none of them declared the debate over.

It is unfortunate that the FCC report does not explain how it reached its conclusion. The early part of the report mentions opinions that disagree with the idea that television violence causes aggression. It also describes some criticisms on both sides of the dispute – arguments against the causal hypothesis and against those who disagree with that hypothesis. Fair enough! Listening to these kinds of arguments and taking them seriously is one way for a commission to decide which side to favor. But if one is going to say that the causal hypothesis is right (as the FCC does at least for short-term effects), there is some obligation to justify that decision. What convinced them? What are the key weaknesses of the criticisms of the research that supposedly supports the causal hypothesis? What are the key studies or arguments in favor of the hypothesis?

It is not enough to say that “on balance” the Commission has decided that exposure to television violence causes aggression in the short run. As mild and moderate as that conclusion is, it is still a decision to pick one side of the debate over another. Judges in court have to give reasons for their decisions. That is the least we should expect from a multi-year study by the FCC. We did not get it.

This was a missed chance. The FCC could have paid serious attention to the actual evidence. It did not. Rather than analyzing the conflicting evidence and opinion, the report simply comes down on the side of those who believe that television violence is harmful. There is no careful analysis of the research, there is no careful explanation of their conclusions; there seems to be mainly an acceptance of that view because more of those they talked with favored it than favored the other view.

This is not the way science should work. It is not a popularity contest or a matter of consensus. It is or should be the research findings that matter, but alas, the FCC review does not seem to have spent the substantial time that would have been necessary actually to read the original research. Too bad. A careful, analytic review of all of the research by people with no preconceptions would have been valuable. A cursory sampling of scientific opinion and a fair amount of non-scientific opinion could not hope to provide anything of much use.

Taking all of the government reports together, they are surprisingly moderate. It is important to note the great pressure the agencies are under to find against media violence. Consider the question that the FCC was supposed to answer: “What are the negative effects on children caused by the cumulative viewing of excessively violent programming?” The question does not ask if there are negative effects – it presumes that there are; it does not ask if there are immediate effects – it focuses on cumulative viewing; it presupposes that some programming is excessive. To its credit (or perhaps the opposite from the point of view of Congress), the FCC essentially ignored the fine points of the question.

The FCC report does not discuss cumulative effects, but its conclusion (a short-term increase) implies a lack of a cumulative effect; it does not deal with whether some programs have excessive violence, but refers merely to violent programs. And, of course, the other reviews take even more moderate positions, either denying an effect on violent behavior or more generally questioning whether there is any causal effect of media violence. Nevertheless, this latest report does conclude that children need to be protected from television violence. This is what we must deal with, because presumably the U.S. Congress will now go into its usual debate about limiting violence in the media.

The question, then, is what the evidence indicates about the effect of the exposure to television violence on children. I will try to answer in several parts. First, I will discuss the difference between scientific and non-scientific evidence; second, I will analyze the actual research; and finally, I will present some relevant information from the real world. Some of this discussion repeats what I submitted to the FCC for their review.

## **Science vs. Opinion**

Let me start by distinguishing science from opinion. The FCC notes that several organizations that represent health care professionals and scientists have declared that television violence is harmful. If these organizations had undertaken careful reviews of the research, their statements might be given some weight because they would be based on scientific research. However, it is obvious that the statements are not based on any such reviews. This is clear from the fact that the statements contain serious errors of fact.

For example, the American Academy of Pediatrics (AAP) issued a statement in 2001 stating that exposure to television violence was harmful and urging parents to restrict children’s access to television violence and, indeed, to avoid any television exposure for children under 2 years of age. In making the statement that television violence was harmful, the AAP referred to more than 3,500 research studies with all but 18 showing a positive relationship (between exposure to violence and aggression). This is wildly inaccurate. As anyone familiar with the literature knows, there are about 250 studies on this topic and there are certainly many more than 18 that failed to find an effect.

The FCC report quotes Craig Anderson, who excuses these errors by saying they were due merely to confusion about what to count – that the AAP was counting all articles on the subject, not just independent research studies. While Anderson may be forgiven for defending every statement in favor of his position, his explanation makes no sense. In the first place, the

statement clearly says “research studies.” Moreover, the reference to the 18 that failed to get an effect cannot possibly be to all articles but must be to research. And note that the reference to the failed studies is not a round number. It does not say “about 20” or “about 30” – it says “18,” which is a precise number. One should be able to assume that they have the list of those 18 negative studies and could readily produce it.

But the AAP does not have such a list, because there is no such list. Their statement is obviously made without any detailed knowledge of the research. Other organizations, including the American Psychological Association, which surely should know better, have made similar though not quite as spectacular errors when referring to the number of studies. Therefore, we must assume that these statements are based not on science but on some combination of listening to what others tell these organizations, their own intuition, political expediency, and, I hope, real concern for our children. Since the statements are not based on science, they should be ignored. Yet the FCC report mentions that these organizations have supported the causal hypothesis and seems to give that some weight.

Everyone is entitled to an opinion on this or any other topic, but these opinions have no scientific standing. All that matters is what the research shows. There can be disagreements about how to interpret research findings, how much weight to give to particular studies, and so on. Nevertheless, in the end, informed decisions have to be based on the research and conclusions have to be justified in terms of the actual research findings.

### **What Does the Research Find?**

**Correlation.** First, most of the relevant work has shown that there is a correlation between exposure to violent television programming and aggression. That is, children who are exposed to more television violence tend to be more aggressive, and vice versa. The relationship is weak, but fairly consistent. However, as the FTC pointed out, the existence of this relationship does not indicate any causal relationship. It provides no evidence that exposure to television violence *causes* children to be aggressive. The most likely explanation of the relationship is that some children are more aggressive in general than others, and that the more aggressive children prefer violent television, watch and play more aggressive games, and act more aggressively themselves. To demonstrate that violent television *causes* aggressiveness, it is necessary to rule out this simple, intuitive explanation that is almost certainly at least partially true.

**Experimental research.** This is the only type of research that can provide clear evidence about causality. The great majority of studies on the effect of exposure to television violence have been experimental. In the typical experiment, some children are shown a program containing violence and others are shown a non-violent program. The children are then given an opportunity to act aggressively. If those who have seen the violent program act more aggressively than the others, this supports the hypothesis that violent television causes aggression.

As I will discuss below, there are many problems with much of this research. For now, however, let me summarize the results as well as I can. Although those who favor the causal hypothesis persist in describing these findings as overwhelmingly supportive of their position,

that is not correct. In fact, the majority of the studies do not provide such support.

When I reviewed all of the experimental research on both children and adults in 2002 (Freedman, 2002), just about half of the studies were consistent with the causal hypothesis and half were not. When the studies that used the most dubious measures of aggression were eliminated (more on this later), only 28 percent supported the hypothesis while 55 percent did not. The rest were either ambiguous or not directly relevant. When only studies involving children are considered, the findings are about the same but even weaker. There have been few relevant studies since 2002 so this summary is still accurate. Thus, the main point about the experimental research is that overall, ignoring the methodological problems, the results do not support the notion that exposure to violent television makes children more aggressive.

*Problems with the experiments.* It is a mistake to take research at face value if it contains serious methodological problems. I am a great believer in experimental work, having done it for most of my professional life. Nevertheless, it has to be acknowledged that the experimental research on the effects of television violence is fraught with difficulties. There are three major problems.

First, the “violent” and “non-violent” programs are difficult if not impossible to equate. A basic principle of an experiment is that the two conditions one is comparing must be identical except for the key element. If one wanted to test the effect of a diet pill on weight loss with two groups of subjects, for example, one would not have the first group take the pill and eat only salads while the other group did not take the pill and could eat anything they wanted. If the first group lost more weight it could be due to the pill, but it could also be due to the difference in diet. So the right way to test this would be to give both groups the same diet and a pill, with the only difference being that one group took pills containing the diet medicine and the other group took pills with no medicine. Then if there were a difference in weight loss, it would be due to the only difference between the conditions, namely the contents of the pills.

The same is true in the television violence research. The programs should ideally be identical except for the presence of violence in one program and the absence of it in the other. Unfortunately, this is very difficult to manage since the violence is typically such an integral part of the program that removing it leaves a program that makes no sense. There’s no question that this is a difficult problem to solve. (No one ever said that research was easy.)

What is surprising is that in most of the research there is no attempt to solve this problem. The researcher picks some program that contains violence and compares it to some entirely different program that does not contain violence, with little or no effort to make them comparable in other respects. As an extreme example, one study (Berkowitz, Corwin, and Heironimus, 1963) with adults used a clip of a vivid boxing sequence in which a boxer is horribly hurt as the violent program, and a travelogue about canal boats in England as the non-violent program. Other studies have tried harder to equate the subject matter, but most have not managed very well.

This is not a trivial issue because other differences between the programs can affect the results. The most obvious is that if one program is more exciting and arousing than the other, the

arousal itself can cause people to be more aggressive. Similarly, if one program is upsetting and the other is not, being upset can increase aggressiveness. Thus, any differences after watching the programs could be caused not by the presence of violence but by other, irrelevant factors.

Second, the measures of aggression are questionable. With a few exceptions, the studies do not measure the number of fights, amount of kicking or punching, or anything that most of us would consider aggressive behavior. Instead, they use analogues of real aggression that may or may not have much to do with the real thing. This is not easy to solve, because those who do the research cannot allow participants to harm each other and generally cannot even allow them to play freely.

Again, unfortunately, most of the researchers do not seem especially concerned about this. In the studies on children, one of the most common measures of aggression is how often the children punch a Bobo doll or some other toys (*e.g.*, Bandura, Ross, and Ross, 1961; Dubanoski and Parton, 1971). In case you never played with a Bobo doll, they are inflatable dolls that look like clowns and usually have a big nose. They are weighted at the bottom so that if one punches the doll, especially on the nose, the doll falls backward and then pops back up.

Why anyone would consider punching a Bobo doll an act of aggression is mysterious, since this is what the dolls are made for, just as soccer balls are made to be kicked. I don't think anyone would suggest that kicking a soccer ball is an act of aggression, so why think that punching a Bobo doll is aggressive? Combining this problem with the previous one, it is not difficult to imagine that children who are more excited and aroused (because they have watched an exciting and violent movie) would punch a Bobo more, thus compounding the problems and making it far less likely that the experiment has anything to do with aggression.

This is terribly important because it goes to the root of the issue. Even if every single laboratory study found that children scored higher on some measure that was called "aggression" after watching a violent program, wouldn't we want to be sure that they really were being more aggressive? Is punching an inflatable doll an aggressive act? Is choosing to play with a toy gun or a toy tank rather than a doll an act of aggression? Does any of this indicate anything about how aggressive the child is or will be in the future? I think not. The relatively few studies that observe children in free play (*e.g.*, Sanson and Di Muccio, 1993; Friedrich and Stein, 1973) and count aggressive acts such as hitting and kicking are much closer to studying real aggression. But these studies with a few exceptions failed to find any effect of watching a violent program.

The third problem is that the children may be doing what they think the experimenter wants them to do or has given them permission to do. Participants in research often respond to what they perceive the researcher's intentions and values to be. When a researcher shows children a violent program and then gives them the chance to be aggressive, the children may ask themselves why they were shown that program and what the experimenter has in mind. A reasonable conclusion they could draw is that the researcher approves of the violence in the program (otherwise why show it?) and is, in essence, giving permission to behave aggressively.

This so-called demand factor is very powerful and can explain many of the findings. Since the effects of this demand factor are very well known, careful research is designed to avoid

or at least to minimize it. If not, there is always the real possibility that any effects are due to demand. This is just standard research design. Yet those who have done the research on television violence do not seem to have been concerned by it and have largely ignored it.

Those who favor the causal hypothesis have criticized me for picking on (or I suppose they would say nit-picking) the studies. That is not what I am doing. These are serious problems that in other areas of research would disqualify the experiment from being published. In drug research, the experimental group is given a pill containing the medicine to be tested and the control group is given an identical pill without the medicine. If a drug trial did not do this, if one group got a pill and the other did not, or if one group got a pill plus a special diet and the other group did not get the special diet, it is doubtful that the research would be taken seriously. No one would suggest that it is nit-picking to insist on a double-blind procedure with drug trials and for all other conditions to be identical to the extent possible.

The research on the effects of violent television is admittedly much more complicated. Perfect controls and perfect measures may not be possible, but at least they should be attempted. To the extent that the controls are not perfect, and to the extent that the measure is only remotely related to aggression if at all, those doing the research should acknowledge the limitations rather than denying them. Moreover, researchers should take steps to avoid demand problems, although almost none of them have done so.

Taken together, the problems of equating the programs, measuring real aggression, and controlling for demand effects make it difficult if not impossible to conclude anything from the experimental research. These factors may well explain why some experiments do get positive effects. That is, the experiments that seem to find an effect of violent programs on aggression may not really be showing that, but rather showing the effect of arousal (because the violent program was more arousing than the non-violent one), or of demand (because the children may think the experimenter wants them to behave a particular way).

Because of the difficulty of eliminating these problems in the laboratory, overall the experimental research is of less use in dealing with television violence than it is with many other questions. In any case, let me repeat that even ignoring all of these problems and taking the research entirely at face value, the findings of the experimental research do not support the causal hypothesis.

**Field experiments.** Some of the research has been done in more natural settings than the experimental laboratory. These so-called field experiments have some real advantages. Although the violent and non-violent programs are no easier to equate in the field than in the experimental laboratory, the field experiments largely avoid problems of demand pressures and can sometimes measure real aggression. In addition, whereas the laboratory experiments are almost all very short-term, some of the field studies have looked at longer-term exposure and effects. Unfortunately, there are very few such studies so their impact is relatively slight.

None of the field experiments studied the effects on young children, but I will summarize the effects on older participants (mostly teenagers). I was amazed when one meta-analysis (Paik and Comstock, 1994) claimed that the effects of the field experiments strongly supported the

causal hypothesis. Despite trying, I have been unable to obtain the list of studies they included, so I cannot offer an explanation of their result.

All I can say is that by my count there are only 11 field experiments, so a meta-analysis could not possibly be very helpful. More important, of those 11, only three (Black and Bevan, 1992; and two studies included in a group of three by Parke and Leyens (Parke, Leyens, et al., 1977) offer the slightest support for the causal hypothesis. To make matters worse, the last two used clearly inappropriate statistics (as acknowledged by Leyens) and did not distinguish between real and playful aggression.

One of the most ambitious studies (Feshbach and Singer, 1971) actually found more aggression among those who did not watch violent television, the opposite of what the causal hypothesis predicts. Those who favor the causal hypothesis dismiss this study because it has some methodological problems, but these same people seem unconcerned by the errors in statistics in the two supportive studies.

The fact is that these field experiments are difficult to conduct. None of the studies is perfect, so I prefer to take them all at face value. That is what I have done. This leads to the straightforward summary given above – eight definitely non-supportive, three somewhat supportive. In other words, this body of research, limited as it is, not only does not support the notion that exposure to violent programs increases aggression but generally contradicts that idea.

**Longitudinal Studies.** Perhaps the most interesting research involves following children for several years and sometimes into adulthood to assess the effects of violent television. These so-called longitudinal studies obtain measures of children's exposure to television violence and their aggressiveness at one age and then obtain these measures again some years later.

In considering this research, it is important to understand that everyone agrees that aggressiveness is determined by many factors, including probably some genetic predisposition. This aggressive trait is quite consistent over time, so that children who are very aggressive when they are young tend to be highly aggressive when they are older. We also know that aggressive children tend to watch more television violence than less aggressive children. Therefore, even without any causal effect of television violence, we would expect that those who watch a lot of television violence when they are young (the more aggressive children) will be more aggressive when they are older.

The idea behind the longitudinal research is that the effect of violent television (if there is one) should show up in later years. Children who are equally aggressive at an early age and who watch a lot of violent television should become more aggressive over time compared to those who watch very little television violence. To restate this, if the causal hypothesis is correct, children who watch a lot of violent television should become more aggressive relative to those who watch a moderate amount, and they in turn should be more aggressive than those who watch very little. If the research found that pattern, it would not provide definitive proof of a causal effect because other factors might be producing the difference, but it would surely be an impressive finding.

As with the other kinds of research, the results of the longitudinal research have been overstated by those who favor the causal hypothesis. They have exaggerated the results that support their position, and criticized or ignored those that do not support it. There is no question that some of these studies have produced some results that are consistent with the causal hypothesis and support it. But other studies that are equally or more impressive have found results inconsistent with the hypothesis. Also, even the positive results are mostly from studies that have many more non-supportive than supportive results. Nevertheless, because these studies are so often cited and are given considerable weight, let me discuss some of those most often mentioned as supporting the causal hypothesis.

Perhaps the most widely mentioned study is what is now sometimes referred to as the 22-year study (Eron, et al., 1971; Huesmann, 1986), which first looked at children when they were 8, again 10 years later, and then again 22 years later when they were 30. This is a monumental piece of research that required vast effort and resources. Sadly, the findings have not been presented or discussed entirely objectively.

In terms of a causal effect of television violence on aggression, there was one result that provided good support for the causal hypothesis. From age 8 to age 18, there was a strong correlation between early exposure to violent television (actually preference for violent television but let's assume it is about the same thing) and later aggression, stronger than the reverse correlation between later exposure and early aggression. When I first read this, I was quite impressed. However, I noted also that this was true only for boys, not for girls, and on only one of three measures of aggression. Yes, it is a nice finding, but it occurs in only one of six possible comparisons, which weakens it considerably.

In Huesmann's paper on the same participants when they were 30, he stressed one finding: namely, that when early aggression is controlled statistically, early television viewing is related to the seriousness of criminal acts. That would indeed be impressive if it were not for the fact that there are many other results that are not consistent with this one or with the causal hypothesis. The study shows that early television viewing is not related to any measure of aggression in adult women, nor to any of six other measures of aggression in men. In other words, one out of 14 possible analyses supports the causal hypothesis. Since the authors do not correct their statistics as they should when doing multiple comparisons, the effect could well be due to chance.

So, it is a great study, but the findings contradict more than support the causal hypothesis. What this study actually shows, and the way it was first presented (Huesmann, et al., 1984), is the remarkable stability of aggressiveness from the age of 8 until the age of 30. Eight-year olds who scored high on aggressiveness were more likely to engage in almost all acts of aggression when they were 30. It is also true, as mentioned at the beginning of this discussion, that those who watched a lot of violent television when they were young were more likely to be aggressive when they were older. But, of course, that is simply another way of showing the stability of aggressiveness.

Another study by Huesmann, Eron, and several other authors (published as a book edited by Huesmann and Eron, 1986) involved looking at the relationship between violent television

and aggression in six countries. This so-called “cross-national study” has consistently been cited as providing strong support for the causal hypothesis, but that is incorrect. In the study there were (by coincidence) 14 possible effects – six countries, males and females in each, and two separate samples in one country. On the critical test of the hypothesis, there were four significant effects, one marginal effect, and nine no effects.

To make it worse, the effects were not consistent across the countries and were different from earlier findings. For example, in the United States part of the study there was an effect for girls but not boys, whereas the earlier study found one for boys but not girls. Moreover, with one exception, even when there were effects, they were extremely small. Thus, although the authors consider the overall finding supportive, I think most objective observers would conclude the opposite.

The most recent study by this group of researchers (Huesmann, et al., 2003) is a 15-year follow-up of the U.S. study that was part of this six-country project. It is a complex study with extremely complex analyses. As usual, early exposure to violent television is related to various measures of aggressiveness in adulthood. But also as usual, this is the expected stability-of-aggression effect.

The major finding is that even after controlling for earlier aggression, early exposure to violent television is related to a higher score on a composite measure of adult aggressiveness. While this seems strong support for the hypothesis, it has to be understood that the composite score includes measures of verbal aggression and indirect aggression that are quite different from the physical aggression that this research is usually concerned with. More troubling is that the composite score also includes a measure of aggressive personality from a standard test, which is obviously not a measure of behavior.

Thus, this study does get some results that support the causal hypothesis, but it is not clear just what has been shown. Certainly, given the content of the composite measure, the findings do not justify the FCC report’s conclusion that the study shows that the “effect of childhood exposure to media violence lasts into adulthood and increases aggressive behavior.”

I should repeat that I have discussed only the studies that produced any results supporting the causal hypothesis. Other studies of this kind (*e.g.*, Milavsky, et al., 1982) have failed to find support. Moreover, none of the studies shows a pattern of increasing correlations between television exposure and aggression that would be expected if the former causes the latter. I consider this a clear negative result. In summary, the longitudinal studies have produced mixed results – sometimes supporting and sometimes not supporting the causal hypothesis. Perhaps of greater importance is that when there are supportive results, with the possible exception of the last Huesmann study, there are more non-supportive results in the same study.

**With and without television.** Several studies have looked at what happens when television is introduced into a country or a community. If television causes aggression and violent behavior, assuming that the television programming contains some violence, there should be more and more violent crime after television is available. Centerwall (1989) compared the United States and Canada after the introduction of television with South Africa, which during the same period

did not have television. The argument was that because the crime rate went up in the United States and Canada but not in South Africa, this showed that television caused crime.

As many have pointed out, this makes little sense since one cannot compare the situation in the three countries. In addition, other countries that also got television at the same time as the United States and Canada did not have the same increase in crime. I think this study has been thoroughly discredited and I was surprised to see the FCC refer to it (although the report did include some of the criticisms).

Another study (Williams, 1986) identified three small communities in Canada. One of these had several television stations, one had only one station, and the third had no television. It was known that television was about to be introduced into the third community, so the researchers obtained measures of aggression in all three communities before and after the third community got television. The authors report that aggression went up in the third community compared to the others.

As with the comparison of countries, this study suffers from the insurmountable problem that the three communities differ in many ways that could explain the finding. Moreover, the actual results were far weaker than reported, failed to distinguish between real and play aggression, and were not consistent across measures. In any case, the television that was introduced into the third community was from the Canadian Broadcasting Company and had virtually no programs that contained any amount of violence. Thus, even if it could be shown that there was an effect, it could not have been due to violent programming.

A far better study (Hennigan, et al., 1982) looked at several hundred U.S. cities during a period when television licenses were temporarily frozen and then unfrozen three years later. The argument was that if television caused crime, there should be a relative increase in crime in those cities with television compared to those without, and that the difference should disappear once all cities had television. The findings provide no support for the causal hypothesis. There was no change in violent crimes, car theft, or burglary during the key period. Oddly, there was more larceny in communities with television than in the have-not communities. This difference disappeared when they all had television. The authors attribute this to feelings of envy because television showed mostly middle- or upper-class households. Whatever the explanation of this effect, it is clear that the study does not support the notion that television violence causes aggression or violent behavior.

**Other approaches.** Researchers have been quite ingenious in their attempts to assess the effect of television on aggression. None of the researchers using unique methods (*i.e.*, different from those described above) found results supporting the causal hypothesis. One such study is particularly interesting. Messner (1986) obtained measures of the popularity of the most violent regular prime-time programs on television. He looked at all 281 standard metropolitan areas and argued that the more popular these violent programs were in an area, the more people were exposed to violence, and that if such exposure caused aggression, then the more violent crime there should be.

The result was precisely the opposite of what the causal hypothesis would predict. The

more popular the programs were in an area, the lower the crime rate. There are many ambiguities in this study and in how to interpret it, and I will not bother discussing them. The point is not that this study by itself disproves the causal hypothesis, but that it could have provided data consistent with the hypothesis but instead provided data inconsistent with it.

### **Summary of the Research**

Those who favor the causal hypothesis often say that although one type of research may not be conclusive, there is a confluence of findings such that all of the methods produce support. The opposite is true. That is, there is a confluence of findings such that all of the methods *fail* to produce support for the causal hypothesis. Not one method has produced a clear majority of findings consistent with the idea that exposure to violent television makes people aggressive. It is not true for children and it is not true for adults.

Far from the overwhelming evidence that those who favor the hypothesis promote, the evidence overall and from each type of study is inconsistent, weak, and generally non-supportive. As I concluded in my earlier review of the research, a thorough reading of all of the research indicates that either there is no effect of violent television on aggression or, if there is any effect, it is vanishingly small.

### **Other Issues**

A number of other ideas about television violence and its possible effect on children have gained some popularity, even though there is little or no scientific evidence to support these assertions. Three such ideas are worth noting here briefly because the FCC mentions them in its report.

**Desensitization.** The FCC report repeats the idea that exposure to violent television desensitizes children to real violence. There is very little research on this, but it has produced inconsistent findings that I believe should be given little weight. There is no convincing evidence that children who watch a lot of television violence are any less concerned about real violence than children who watch little or no television violence.

**Depictions of violence.** Some have surmised that the effect of television violence depends on how it is depicted. It has been suggested that whether the violence is justified, whether it is punished, and so on determines its effect on aggression. In the first place, as noted at great length above, there is no reason to believe there is any effect. More to the point, the differential effect is pure speculation – there is no evidence to support it. I am surprised that the FCC mentions it as if it has been demonstrated.

**Mainly for certain children.** It has been suggested that the effect of violent television on aggression is stronger on more aggressive children or on children with particular characteristics. Again, given that there is no effect, this makes no sense. Although this idea is mentioned often, there is no consistent body of research to support it.

These two ideas – that the type of violence matters and that the effect depends on how

aggressive the child is to begin with – are just that, ideas. That the FCC mentions them indicates confusion between hypotheses that have been tested and proven by systematic research and intuitions and notions that have not been tested. There is nothing wrong with intuitions and notions of all kinds, but they should not be confused with the scientific findings.

### **What About the Real World?**

One of the most deceptive statements coming from those who favor the causal hypothesis is that the effect of television violence on aggression is as strong as the effect of smoking on lung cancer. This is hyperbole of the most egregious kind because it is not only wrong but might cause people to question the harmful effects of smoking. I am not an expert on the effects of smoking, but let me cite one figure: Someone who smokes regularly for 20 years is 10 to 20 times more likely to get lung cancer than someone who does not smoke.

There is nothing remotely comparable in any of the research on television violence. It is not true that someone who watches television violence for any length of time, no matter how long, is many times more likely to commit a violent crime than someone who does not watch. In fact, there is no reliable evidence that television violence causes any violent crimes. And even with minor aggression, there is no evidence that watching violent television causes people to be many times more likely to be aggressive. It does not even make sense to talk about it in those terms.

In addition, the relationship between smoking and cancer shows all of the effects that would be expected if smoking causes cancer. The more cigarettes people smoke, the more likely they are to get cancer; the more years they smoke, the more likely they are to get cancer; if they stop smoking, their risk of cancer decreases. In contrast, there is no evidence that the more years people are exposed to violent television, the more aggressive they are; or that if they stop watching violent television, they become less aggressive. These so called dose-response effects are crucial for the scientific case to be made, and they simply do not exist.

Having raised the issue of smoking and cancer, let me use it to contrast what the real world tells us about that and about television violence and aggression. If smoking causes lung cancer, there should be observable effects on real people living in the real world. It is not enough to show that smokers are more likely to get lung cancer, because there might conceivably be other explanations for that. However, we can make more precise predictions.

In the early part of the 20th century, most women in the United States did not smoke because it was not considered proper. We should therefore expect that during that period, the rate of lung cancer in men should have been much higher than in women. It was! Later in the century, attitudes changed and women began to smoke. We should expect that the rates of lung cancer in women would begin to go up and eventually become close to those in men. They did! In other words, what occurred in the real world was just what we would expect if smoking caused lung cancer. This is not definitive proof of the effect, but if we had not seen this relationship, it sure would have made us wonder and perhaps question our assumption about smoking and cancer.

How about television violence? If it causes people to be more aggressive, we should expect that to show up in rates of violent crime. About 10 years after television was introduced into the United States, the rate of violent crime began to go up and increased dramatically from 1965 to 1980. One possible explanation was that the increase was caused by exposure to television violence. The explanation of the time-lag for the increase was that the effect would be mainly on children, and that it would take several years until they got old enough to begin committing violent crimes. So when the crime rate began to increase, many people blamed it on television. Of course, many other factors could have caused the increase, but television was a convenient (though I would say implausible) target.

However, it is incumbent on those blaming television to follow through on their analysis. After about 1980 the rate of violent crime leveled off in the United States until about 1992. At that point, we still had lots of violent programming on television, we had vivid and more and more realistic violence in films, we had violent lyrics in rap music, and we had the rapidly growing popularity of video games, especially violent video games being played by young males. If violent television causes aggression, and if (as many of the same people believe) violent movies, violent lyrics in rap music, and violent video games cause aggression, the rate of violent crime should have gone through the roof.

That did not happen. Instead, there was a sharp decline in violent crime that started in 1992 and continued to the point that the rate is now below what it was before television became popular. And just to be clear, this was not due to a change in demographics, since the drop in violent crime rate was particularly sharp among young males who are the ones who commit a disproportionate number of such crimes.

This by itself, though clearly inconsistent with the causal hypothesis, does not disprove it. Many other explanations of the pattern are possible. But surely it must make those who favor the hypothesis, and who blamed television for the earlier increase, wonder why we did not see the pattern they should have predicted. It should also give pause to those legislators who are concerned about the harmful effects of television violence. They should ask themselves why, if television is so harmful, there is less violent crime now than there was when they were young.

## **Conclusion**

The FCC's conclusion that exposure to television violence makes children aggressive is not justified by the scientific evidence. Despite many statements to the contrary, the research does not support the hypothesis that television violence causes aggression. The findings from every methodological approach are mostly not supportive. There is no confluence of support from the various approaches, but exactly the opposite – a confluence of non-support from the various approaches. Moreover, the dramatic decline in violent crime that has occurred since 1992 contradicts the notion that exposure to violent media of all kinds causes people to behave violently.

In sum, there is no convincing scientific evidence that television violence causes children to be aggressive, or that any particular depiction of violence on television has this effect, or that it affects any particular type of children more than others. There has been a considerable amount

of research on this topic – enough so that if there were an effect, the research should have shown it. Therefore, my conclusion is that either there is no effect of television violence on aggression, or, if there is an effect, it is vanishingly small because otherwise the research would have found it.

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