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FIRING A WEAPON AND AGGRESSION [1]

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Five experiments explored the relationship of guns to aggression in the laboratory. The first four concerned the effect of target shooting on subsequent aggression (administration of electric shock). There was no consistent effect. The fifth experiment attempted to replicate a previously found effect of the presence of weapons on aggression. This time, there was no effect. Thus, there is no evidence that the presence, firing, or long-term use of guns enhances subsequent aggression.

A series of violent events during the past few years has sensitized this country to the use and misuse of weapons. The assassinations of the Kennedys and of Martin Luther King, the occurrence of mass murders by disturbed individuals, the flurry of ghetto rioting, and the shooting of college students at Kent State and Jackson State have left searing memories behind. The effect of possessing and firing weapons, as in hunting and target shooting, has been debated in newspapers and magazines, and on radio and television. In spite of the bright glare of publicity and congressional action on the licensing of guns, there has been little laboratory research on the effect of weapons and aggression.

There are two issues here. First, it is obvious that a nearby weapon is more likely to be used than one not immediately at hand, and we need no research to document this point. Thus, the *availability* of weapons, although of considerable social importance, is trivial psychologically: availability of weapons clearly enhances the probability that aggression will occur.

The second issue is important psychologically: Does the presence or use of weapons enhance the *general tendency* to aggress? The first part of this question, presence of weapons, has received a tentatively affirmative answer. The mere presence of a rifle and revolver is sufficient to intensify aggression (Berkowitz & LePage, 1967). The second part of the question has not been answered. There have been opinions from experts, surmises from knowledgeable persons, and statements made in books, magazines, newspapers, radio, and television—but there are no experimental data. The present research was directed to this point: Does firing a weapon enhance nonweapon aggression?

EXPERIMENT 1

Method

Measurement of aggression. The apparatus, an "aggression machine" described in previous publications (Buss, 1961, 1963, 1966a, 1966b), will be described very briefly. The subject is told, with an appropriate rationale, that he is to be the experimenter in a concept learning task. The learner in this bogus task is an experimental accomplice, who is shocked every time he makes an error. He makes 35 errors in 80 trials, that is, gives the subject 35 opportunities to deliver shock. There are 10 shock buttons, ranging from nonpainful (1) through painful (2 and 3) and very painful (4 and 5) to extremely painful (above 5). A shock from Button 1 would be sufficient to signal an error. Shock from higher buttons adds aversiveness and is therefore aggressive in the same sense as is the pain administered by a sadistic dentist or a vengeful parent.

Firing a weapon. The weapon was a small air-powered rifle which fired small, harmless pellets. Subjects fired this "BB gun" five times at a cardboard target placed 20 feet away.

Procedure. The subjects were told that the research involved several kinds of learning, both individual and group. To bolster this cover story, they were given an additional 1-minute task. On a narrow board were three pegs, one with nine disks on it; these varied in diameter, with the smallest on top. The goal was to move the disks to one of the other two pegs. Only the top disk could be moved each time, and it could be placed only on a larger disk or on an empty peg. The task, which quickly becomes complex, has been found to have considerable interest for students.

There were two groups of subjects. The sequence for the control subjects was (*a*) peg task and (*b*) aggression machine; they did not fire the gun. For

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half of the experimental subjects, the sequence was (*a*) peg task, (*b*) target shooting, (*c*) aggression machine; for the other half, it was (*a*) target shooting, (*b*) peg task, (*c*) aggression machine. The accomplice did not fire the weapon; he was told (in the subject's presence) that he would engage in the remaining tasks (peg board and target shooting) at the end of this experiment, after the real subject had left.

Subjects. The subjects were 22 men, recruited from an introductory psychology class at Rutgers University. Two subjects, one experimental and one control, were dropped from the experiment for failure to follow instructions, leaving an n of 10 per group. The accomplice was always a man, and four research assistants were rotated in this role.

Results

The dependent variable was the mean intensity of shock delivered over 35 shock trials. The two groups did not differ significantly, with the analysis of variance yielding an F of .31. This finding was open to several interpretations. The BB gun was light and might have been regarded as merely a toy, and individual differences in aggression level (shock intensity) might have obscured any effects of the independent variable. These issues suggested that another experiment was needed.

EXPERIMENT II

Method

This time the weapon was a heavy pistol with the heft of a bona fide deadly weapon. The pellets were powered by a carbon dioxide cartridge, and the target was again 20 feet away. Each subject ran *two* accomplices, both men. In the experimental group, the sequence was (*a*) aggression machine, (*b*) peg task, (*c*) firing the pistol, (*d*) aggression machine. In the control group, the sequence was (*a*) aggression machine, (*b*) peg task, (*c*) aggression machine. In this design each subject was in effect his own control, eliminating the effect of individual differences. Again there were 10 Rutgers men in each group.

Results

In both groups, there was a slight tendency to give higher shock to the second victim (accomplice), a trend that replicated a previous study (Buss, 1966b). Beyond this, there were no discernible trends in the data. Analysis of difference scores (shock to the second victim minus shock to the first victim) revealed a statistically nonsignificant difference between the experimental and the control group ($F = .04$).

These findings replicated those of the first experiment. Clearly, the unrestricted hypothesis that firing a weapon enhances aggression turned out to be incorrect. A more restricted hypothesis was therefore considered: firing a weapon might affect only those with a history of weapons use or only those who had never used weapons before. Previous experience with guns might determine whether firing one has any effect on subsequent aggression. The third experiment examined this possibility.

EXPERIMENT III

Method

This time the male subjects were drawn from an introductory psychology class at the University of Texas. The class was given a questionnaire which included four items on weapons: (*a*) I enjoy hunting birds and small game. (*b*) When I was younger, I liked target shooting. (*c*) I have been handling and firing weapons since I was a child. (*d*) I have little or no experience with guns. On the basis of their answers to these questions, two extreme groups were selected: those who answered the first three *true* and the last *false* (gun use) and those who answered precisely the opposite (no gun use).

Twenty subjects were selected from each extreme group, and Experiment II was repeated. Half of the gun-use subjects fired the pistol (experimental), and half did not (control). No-gun-use subjects were divided in the same way. This was a 2×2 design (gun use versus no gun use and experimental versus control) with an n of 10 subjects per cell.

TABLE 1

ANALYSIS OF VARIANCE: EXPERIMENT III

Variable	*df*	*MS*	*F*
Gun users versus nonusers (A)	1	41.20	1.8
Experimental versus control (B)	1	6.54	.3
A × B	1	39.91	1.7
Error	36	22.91	
First versus second victim (C)	1	12.45	18.6**
A × C	1	.08	.1
B × C	1	4.16	6.2*
A × B × C	36	.67	
Trials (D)	6	23.67	71.7***
A × D	6	1.07	3.2**
B × D	6	.53	1.6
A × B × D	6	2.16	6.5***
Error	216	.33	1.6
C × D	6	.78	1.6
A × C × D	6	.23	.5
B × C × D	6	33.58	70.0
A × B × C × D	6	.18	14
Error	216	.48	

* $p < .05$.
** $p < .01$.
*** $p < .001$.

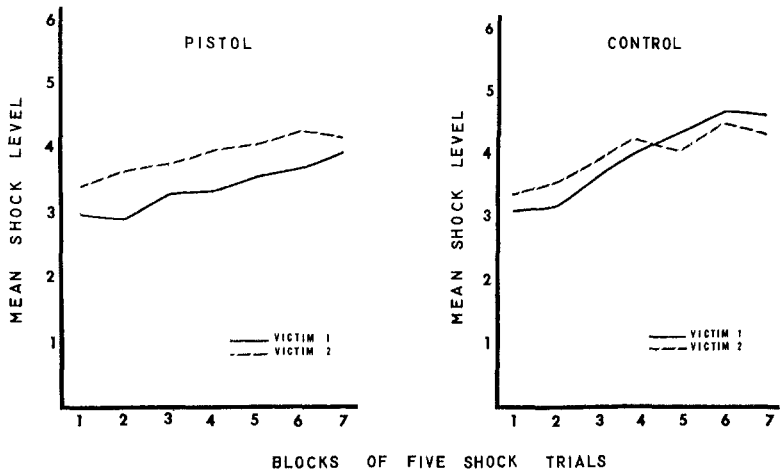

FIG. 1. Effect of gun users firing a pistol.

Results

The shock-intensity data were grouped into seven blocks of five trials each and plotted in Figures 1 (gun users) and 2 (nonusers). For the users, the subjects who fired the pistol gave more intense shock to the second victim; control subjects did not. The same pattern held for nonusers: firing the weapon led to more aggression.

The significance of these trends was evaluated by an analysis of variance, which is presented in Table 1. The trials effect was highly significant; the upward drift of shock intensity has occurred in virtually every study with the aggression machine. Thus, it appears to be a constant of research with this instrument, and it need not be discussed further. The five other significant Fs are discussed in sequence. (*a*) There was a main effect of first versus second victim ($F = 18.6$, $p < .01$). The second victim was shocked higher than the first, an effect due mainly to the performance of the experimental subjects. (*b*) There was a First versus Second Victim × Experimental versus Control interaction ($F = 6.2$, $p < .05$). This confirmed the point just made: experimental subjects shocked the second victim higher than the first, but control subjects did not. (*c*) There was a User versus Nonuser × Trials interaction ($F = 3.2$, $p < .01$). The

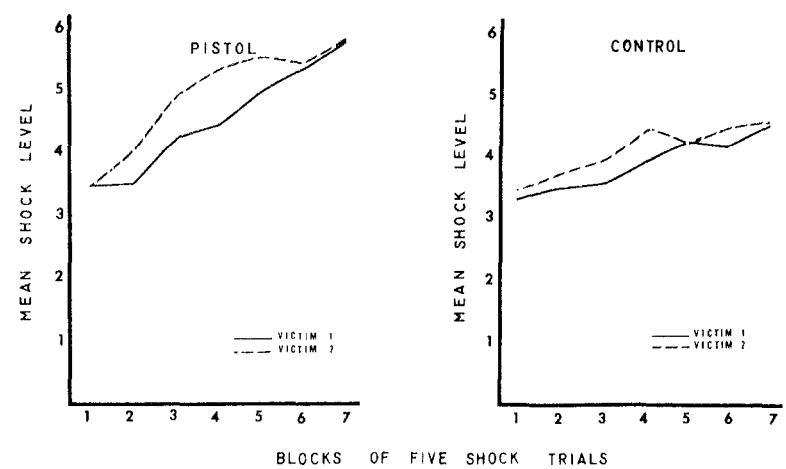

FIG. 2. Effect of nonusers firing a pistol.

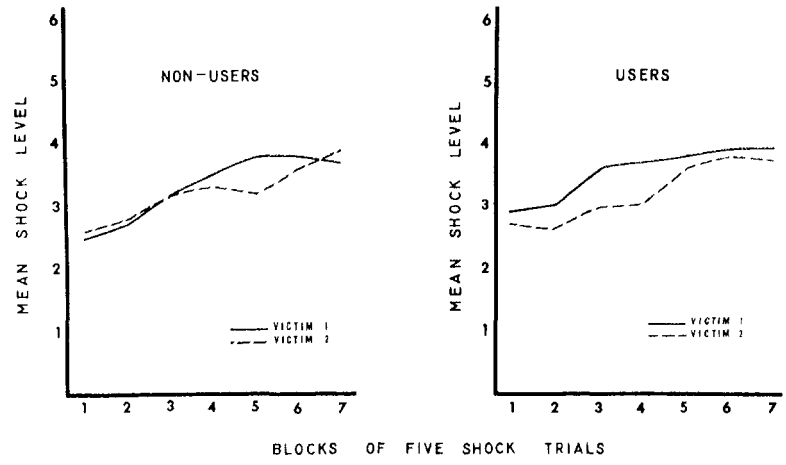

FIG. 3. Replication of Experiment III, experimental condition only.

users tended to increase shock over trials for both victims more than did the nonusers. (*d*) There was a User versus Nonuser × Experimental versus Control × Trials interaction ($F = 6.5$, $p < .001$). For gun users, the increase in shock over trials was greater for the second victim than for the first, but for nonusers, the increase over trials was greater for the first victim than for the second. (*e*) There was an Experimental versus Control × First versus Second Victim × Trials interaction ($F = 70.0$, $p < .001$). With respect to the difference in shock over trials from the first to the second victim, the experimental subjects showed a pattern different from that of the controls.

Three of these five significant effects involved an interaction between experimental versus control and other variables, which called for separate analysis of the data of these two groups. The analysis of variance of the control data yielded no significant effects (other than for trials). The analysis of variance of the experimental data yielded three significant effects. First, the users shocked higher than the nonusers ($F = 6.3$, $p < .03$). Second, the second victim was shocked higher than the first ($F = 8.6$, $p < .01$). Third, the nonusers increased shock over trials more than the users ($F = 3.0$, $p < .01$).

Among these complex findings, two facts stand out. First, firing the pistol did enhance aggression: the second victim received higher shock than the first only when the pistol was fired. Second, familiarity with guns was an important variable, with nonusers surprisingly increasing shock over trials more than did users.

A puzzling feature was the performance of the nonuser experimental group (see Figure 2). These subjects sharply increased the intensity of shock to the *first* victim, in contrast to the mild increase of the nonuser control group. This difference is difficult to understand because the experimental and control groups were treated identically until *after* the first victim was run; *subsequently*, the experimental subjects fired the pistol. In the absence of any explanation for the divergence between two groups treated alike, the possibility of sampling error was considered. The issue is important in that the performance of the nonuser experimental subjects contributed strongly to most of the significant effects. The only way to resolve the question was to replicate the experimental conditions.

EXPERIMENT IV

Method

A new sample of 20 male students was drawn, 10 users and 10 nonusers. They were run in the experimental condition, firing the pistol after running the first of two victims on the aggression machine.

Results

The data are presented in Figure 3. This time, shock intensity given to the first victim was more in line with that of previous experi-

TABLE 2

MEAN NUMBER OF SHOCKS GIVEN IN EACH CONDITION

Condition	Berkowitz & LePage (1967)		Present study	
	No. shocks received			
	1	7	1	7
No weapons	3.07	4.67	2.06	5.67
Weapons present	2.60	6.07	1.53	4.80

ments. The effect of firing the pistol was negligible: there was a slight *decrease* from the first to the second victim by the users. An analysis of variance confirmed these trends: there were no significant *F*s.

In Experiment III, firing a pistol enhanced aggression. In the present experiment, there was a slight and nonsignificant trend in the opposite direction. The most straightforward interpretation was that the findings of Experiment III were the result of sampling error and that firing a weapon did *not* enhance aggression.

This conclusion suggested a reexamination of the finding that the mere presence of a weapon increases aggression. The Berkowitz and LePage (1967) experiment needed to be replicated.

EXPERIMENT V

Method

The replication was exact, with one exception. In the experimental group, a shotgun and pistol lying on the table were associated with the confederate. Berkowitz and LePage told their subjects that the weapons belonged to the confederate, a student who had left them there while *conducting* another experiment. This instruction resulted in a large proportion of subjects being eliminated from the experiment as being too suspicious. As Berkowitz and LePage (1967) noted,

> This information evidently was the major source of suspicion; some of the subjects doubted that a student running an experiment would be used as a subject in another study, even if he were only an undergraduate [p. 204].

Accordingly, the cover story was changed. Subjects were told that the weapons were lying there because the confederate was going to lend them to a friend who was in another experiment. This story served to associate the weapons with the confederate without arousing excessive suspicion (4 subjects out of 34 were suspicious).

The experiment was the same in all other respects. Subjects were told that their ideas would first be criticized by the confederate, using electric shock. One shock signified a very good rating; 10 shocks, a very bad one. Half of the subjects received 1 shock, and half received 7 shocks. Then it was the subject's turn to evaluate the confederate's ideas, and the number of shocks he administered comprised the dependent variable.

Thus, there were two independent variables: associated weapons versus no weapons and one shock received versus seven shocks received. Fifteen men were run in each of these four groups, and another 9 were eliminated from the experiment: suspicion, 4; reporting an incorrect number of shocks, 3; and equipment malfunction, 2.

Results

The results of our experiment and those of Berkowitz and LePage (1967) are presented in Table 2. They found a significant weapons effect for subjects who received seven shocks, the presence of guns *increasing* the number of shocks. In the present experiment the presence of weapons significantly *decreased* the number of shocks ($F = 5.7$, $p < .05$).

In the face of these opposed findings, another replication was attempted. The shock level had been set at approximately that used by Berkowitz and LePage (1967), but it may have been too high. Therefore, shock intensity was reduced to the level of *barely painful.* Only the condition in which the subject received seven shocks was repeated, again with a weapons and a no-weapons group. This time the presence of weapons had no effect at all, the two means being identical (5.50). Thus, in three experiments (Berkowitz & LePage, 1967; and the two present replications), the presence of weapons increased or decreased shock frequency or had no effect. Such data do not generate confidence in any effect of weapons on aggression.

DISCUSSION

It may be helpful to review the findings. Experiment I showed that firing a weapon had no effect on subsequent aggression, and Experiment II confirmed this, using an own-control design and a different weapon. Experiment III compared gun users with nonusers and found (*a*) differences in initial aggres-

sion and that (*b*) firing a weapon does enhance subsequent aggression. Experiment IV repeated the experimental condition only (firing a weapon) and failed to replicate the findings of Experiment III. Finally Experiment V showed that the Berkowitz and LePage (1967) findings are evidently not reliable: two attempts to replicate, with minor differences in procedure, yielded opposing results and no results, respectively. The discussion will focus on two issues: problems of method and broader implications.

Methodological Issues

It might be argued that a human-shaped target might be more appropriate than the traditional bull's-eye. However, it is presumably the weapon itself (or firing it) that is important, not the particular target. When gun users practice shooting, they use a bull's-eye target, which suggests that this is the most appropriate target if one wishes to generalize findings.

It might be contended that shooting at a target might not enhance aggression, whereas hunting and killing animals does. There is no *direct* answer to this speculation, and a laboratory test of it seems unlikely. Nevertheless, there is *indirect* evidence. If hunting makes men more aggressive, then a long history of hunting should lead to a chronically higher level of aggression. If this were true, the gun users should have aggressed more than nonusers. However, in Experiment III, nonusers showed a *higher* level of aggression than users, and in Experiment IV there was no significant difference. These facts suggest that hunting, target shooting, and a fondness for guns do not enhance aggression.

A devil's advocate might also protest that the measures were not "valid," but the aggression machine and its variants have been widely used. A sampling includes aggression in relation to frustration (Buss, 1963, 1966a; Geen & Berkowitz, 1967; repression (Buss & Brock, 1963); alcohol (Bennett, Buss, & Carpenter, 1969); authoritarianism (Epstein, 1965); and dissonance (Brock & Buss, 1962). Berkowitz's procedure has also been used effectively in many different contexts (see Berkowitz, 1964, 1969). Thus, "validity" is not an issue here.

The last issue of method is sampling. It is well known that samples drawn from a college population do tend to vary, and such variability may yield unreliable data. One way of dealing with the problem is to use an own-control design, minimizing intersubject variability, but the best solution is to repeat experiments. Given the usual variations in sampling, it is not surprising that statistically significant findings fail to replicate. After all, tests of statistical significance are designed to *estimate* reliability of differences; repetitions of experiments *directly assess* the reliability of differences. Thus, after having seen that the differences found in Experiment III failed to appear in Experiment IV, no one should be surprised that the findings of Berkowitz and LePage (1967) could not be repeated. In fact, the three successive experiments, theirs and the two present ones, yielded just the kind of data found in statistics texts to illustrate a population mean of zero: weapons increase aggression, no weapons increase aggression, and no difference between weapons and no weapons.

As this article was being written, another attempt to replicate the Berkowitz and LePage (1967) study appeared. This one used students from a North Carolina university (Ellis, Wienir, & Miller, in press). Again, the presence of weapons failed to elicit additional aggression, and there was a trend toward *inhibition* of aggression with weapons present.[3]

Broader Implications

The present negative findings leave other facts about aggression untouched. That violence begets violence, as was suggested much earlier (Buss, 1961), has been amply demonstrated (Geen, 1968; Geen & Berkowitz, 1967); and the arousing effects of film violence are well known (Berkowitz, 1964). But Berkowitz (1968) has gone further, stating

[3] Parenthetically, when the researchers tried to associate weapons with the confederate the way Berkowitz and LePage (1967) did, they were unsuccessful. Their subjects simply refused to believe the cover story, which confirms our own speculations (see Experiment V).

that: "Guns not only permit violence, they can stimulate it as well. The finger pulls the trigger, but the trigger may also be pulling the finger [p. 22]." This conclusion appears to be based on only a single laboratory study (Berkowitz & LePage, 1967), which we and another group of researchers have failed to replicate.

In light of the apparent violence that is so much discussed today, researchers are likely to overemphasize the potency of mass media and weapons as *causes* of violence. Fashions change, and what is popular today as *the* cause of violence may tomorrow fade into oblivion. Such fickleness urges caution in extrapolating from isolated experiments and controlled laboratory research to the complex and confounded world of everyday life.

A final word of caution: none of this discussion touches on the *availability* issue. If guns are handy, they are likely to be used—a fact that suggests restricting the availability of weapons. But the effect of weapons on subsequent aggression is a separate issue. The present data suggest that neither the long-term use nor the transient firing of a weapon enhances subsequent aggression.

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