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Disinhibition Versus the Cathartic Effect: Artifact and Substance

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The present research was concerned with the effects of anger and physical aggression on subsequent physical aggression. Specifically, two experiments explored the discrepancy between previous findings that angered subjects' aggression may be reduced by aggression (the cathartic effect), and a recent finding of a disinhibitory effect of aggression by Geen, Stonner, and Shope (1975). Experiment 1 showed that the Geen et al. results may have been due to a "scale bias" inadvertently produced by their operationalization of the independent and dependent variables, both involving the Buss "aggression machine." Experiment 2 provided relatively strong evidence that the scale bias did not tap disinhibitory processes and that it was an artifact of modest substantive interest. Overall, very little support for various versions of the disinhibition hypothesis was obtained. In contrast, a strong cathartic effect was found after eliminating the scale bias, and with subjects exposed to a treatment involving anger-generating insults. The relationship between theoretical concepts and the internal/external validity of aggression experiments was discussed; some criteria were suggested for distinguishing between the substantive, procedurally biased, and artifactual findings.

Several recent studies have shown that an opportunity to "hurt" physically an insulting person tends to reduce the amount of subsequent aggression directed by angered people against the annoyer, in comparison to the appropriate control groups (e.g., Doob & Wood, 1972; Konečni, 1975a; Konečni & Doob, 1972). Some extensions of the range of applicability of the "cathartic effect" as defined above have also been accomplished (e.g., Konečni, Crozier, & Doob, in press). In addition, it has been suggested that this line of research should be distinguished from experiments in which anger is not induced in subjects, and/or subjects do not engage in physical aggression against the annoyer (i.e., studies that involve verbal aggression, nonaggressive physical activity, aggression against inanimate objects, watching aggressive models or violent/erotic films, etc.). Theoretical reasons for this distinction and reviews of the pertinent literature have been provided elsewhere (Doob & Wood, 1972; Ebbesen, Duncan, & Konečni, 1975; Konečni, 1973, 1975a).

In contrast to the previous demonstrations of the cathartic effect, Geen, Stonner, and Shope (1975) recently reported an *increase* in aggression under circumstances which, at first glance, seem to fulfill the requirements mentioned above for the occurrence of the effect. These authors' explanation for the discrepancy focused on the various ramifications of the concept of disinhibition. The specific issues raised by the Geen et al. (1975) methodology, results, and interpretations will be discussed in the context of the two experiments we report here. In addition to con-

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trasting the theoretical positions involved, our experiments made it possible to address some general issues concerning laboratory research on human aggression.

Experiment 1

The Geen et al. (1975) 2 × 3 design was structurally identical to that used by Doob and Wood (1972). Also, the critical 2 (Attack vs. Nonattack) × 2 (Opportunity to Aggress vs. No Opportunity) portion of the Geen et al. design (the four means are presented in the top part of Table 1) was conceptually equivalent to parts of the designs used by Konečni and Doob (1972) and Konečni (1975a). One of the findings was that subjects who had a prior aggression opportunity delivered more intense "shocks" than did the no-opportunity subjects.[1] Geen et al. concluded that this result represented strong evidence against the cathartic effect.

Alternatively, the Geen et al. finding may have been due to a "scale bias" induced by the procedure. Whereas few aspects of the Geen et al. experiment were conceptually novel, the details of the operationalization of the independent and dependent variables were somewhat different from those used in prior studies. The opportunity-to-aggress subjects were forced by the instructions to give the confederate 20 shocks by always pressing the *number 5* button on a 10-button Buss machine, even though the dependent measure also involved the Buss machine, and the no-opportunity-to-aggress subjects had no experience with the response scale.[2] Thus, as a function of their respective activities in the preceding period, the opportunity and no-opportunity subjects may have perceived the shock-intensity scale rather differently in the dependent-measure period. The opportunity subjects, having just pressed the number 5 button many times, may have concluded that this part of the scale was "demanded" by the experimenter/situation and/or "sanctioned" by the experimenter/situation. In either case, the opportunity subjects may have adopted a "set" for this part of the scale, which, in turn, may have had the following behavioral manifestations in the dependent-measure period (compared with the no-opportunity condition where such a scale bias clearly could not have been induced): (a) a higher average level of shocks on the first few trials and (b) a greater likelihood and frequency of venturing into the top part of the scale. Either of these factors could have inflated the mean in the opportunity condition compared with the no-opportunity cell.

If a part-of-scale bias was indeed induced, and was due to experimenter-demand factors, the Geen et al. results would properly be considered artifactual and would have relatively little to do with the cathartic effect or aggressive behavior in general. If, however, the bias reflected the perceived experimental sanction, the results would still be of theoretical interest from a disinhibition viewpoint. Whereas Geen et al. generally espoused the latter viewpoint in their paper (we shall return to it later), they did not apply the disinhibition arguments to the number-5-button issue.

Design, Rationale, and Predictions

Experiment 1 was designed primarily to investigate the possibility that a scale bias was induced by the Geen et al. (1975) procedure. However, some preliminary tests of the artifact versus disinhibition interpretations of the bias were also made possible by the design.

To the extent that the Geen et al. results were due to a scale bias (regardless of its

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[1] In addition, attacked subjects gave shocks of higher mean intensity than did the nonattacked ones. When the full 2 × 3 design is taken into account, there was also a significant interaction. However, the latter result seems to have been due to the two additional cells, which are irrelevant for the present discussion. Our calculations, based on the data and tests reported, and on the assumption of homogeneity of cell variances, led us to the conclusion that the interaction was entirely negligible for the 2 × 2 design under consideration (see the top part of Table 1).

[2] Prior to the dependent-measure period (which involved the Buss machine), the no-opportunity-to-aggress subjects must have either pressed none of the buttons on the Buss machine, or pressed the number 5 button without being told that it produced shocks (which of these alternatives was actually used is not clear from the Geen et al. report). Thus, the no-opportunity subjects had either had no experience with the scale, or no experience with it as a ***shock-intensity*** scale.

source), one should be able to increase or decrease the score of attacked-opportunity subjects simply by shifting the location of the bias up and down the scale. In Experiment 1, some attacked subjects pressed a high-level button (number 8) 20 times, whereas others pressed a low-level button (number 2) the same number of times. The addition of an attacked/no-opportunity-to-aggress group and three parallel nonattacked conditions completed the 2 (Attack vs. Nonattack) × 3 (Opportunity-to-Aggress/Button 8; Opportunity-to-Aggress/Button 2; No Opportunity) factorial design. The design was identical to the four critical conditions in the Geen et al. study, except that the button-8 and button-2 conditions replaced the button-5 treatment.

To demonstrate scale bias, it was necessary only to obtain the button-8 > button-2 pattern for attacked subjects. However, if scale bias is an artifact, the button-8 versus button-2 bias induction should affect both attacked and nonattacked subjects. In addition, since the button-2 subjects should be "anchored" to the bottom of the scale, the score of the no-opportunity subjects should fall somewhere *between* the scores of the button-8 and button-2 subjects in *both* the attack and nonattack conditions.

The vagueness of the disinhibition hypothesis makes the prediction process rather difficult. One possible interpretation of the hypothesis is that it applies only to attacked subjects, as there is nothing to be disinhibited in nonattacked subjects. According to this hypothesis, in *attacked subjects only,* a button-8 scale bias actually taps a disinhibitory process (these subjects presumably capitalize on the sanction to deliver high-intensity punishments). Thus, the button-8 > button-2 result should be obtained. In addition, the presumably large disinhibitory effect of having delivered 20 punishments (*regardless* of intensity) should cause even attacked/button-2 subjects to deliver punishments of higher intensity than would attacked/no-opportunity subjects.

In contrast to the above ("attack-only") version, some authors (e.g., Goldstein, Davis, & Herman, 1975, p. 166) suggest the possibility of the aggression-produced "disinhibition of an aggressive drive" in *nonattacked* subjects (cf. Bandura, 1973, and Konečni, 1975a, for a critique of such a view). Zimbardo (1969) also discusses the escalation of aggression that is independent of the target's behavior both before and during an aggressive "bout." Therefore, whereas the predictions based on the two versions of the disinhibition hypothesis are identical with regard to the behavior of attacked subjects (attack/button 8 > attack/button 2 > attack/no opportunity), the Goldstein et al./Zimbardo version predicts a similar pattern of results in conditions *not* involving attack.

In summary, the form of the opportunity (button 8, button 2)/no-opportunity main effect should distinguish the artifact interpretation of scale bias from interpretations based on *either* version of the disinhibition hypothesis. The pattern of results in the attack versus nonattack conditions should distinguish between the two versions of the latter hypothesis.

The further elaboration of predictions requires that the differences between Experiment 1 and the Geen et al. (1975) study be noted in detail. First, female experimenters, confederates, and subjects were used in Experiment 1, whereas Geen et al. used male confederates and subjects. In fact, one of their two (dis)inhibition arguments, which was used to explain the difference between their results and those of Doob and Wood (1972), focused on the sex-of-subjects issue. Doob and Wood used a female confederate and both male and female subjects. Citing Buss (1966b), Geen et al. stated that the Doob–Wood subjects may have been inhibited from attacking the female confederate (unlike their own study where males were supposedly disinhibited by attacking males). However, even if the Doob–Wood subjects were thus inhibited, this grand-mean argument is inadequate to explain either the Doob–Wood cell-means differences, or the Geen et al. cell-means differences, or the opposite direction of the differences in the two studies. Geen et al. did not discuss any higher-order interactions needed to make the argument tenable and also neglected to cite the Konečni–Doob (1972) findings, even though this study fully replicated the Doob–Wood results, used male and female subjects *and* male and female con-

federates, and cast serious doubt on both the inhibition and retaliation reinterpretations of the Doob–Wood data. (The introduction of the Geen et al. article consists almost entirely of such reinterpretations.) On the basis of previous work (e.g., Konečni, 1975a), which showed a main effect of sex (males punish more than females) and no interactions, we expected simply that the results of Experiment 1 would be depressed *overall* in comparison to the Geen et al. results.

Second, because of some restrictions imposed by the local human-subjects committee, we used noise wherever Geen et al. used shocks. Again, shock versus noise may result in a grand-mean difference across experiments, but it would be difficult to use this procedural difference to explain a different *pattern* of cell means. Moreover, Konečni (1975a) found that subjects perceived the punishing potential of "relatively painful shocks" and "very loud blasts of noise" as about equal.

Third, blood pressure was not recorded because the Geen et al. blood pressure data fully supported the cathartic effect by replicating the results of Hokanson and Shetler (1961), Hokanson, Burgess, and Cohen (1963), Baker and Schaie (1969), and Gambaro and Rabin (1969), among others.

Method

Subjects. Subjects were 48 experimentally naive female undergraduates who were reimbursed for their participation. They were randomly assigned to experimental conditions. Confederates were 10 female students who were all assigned to various conditions about equally often. Experimenters were 3 females in their early twenties. Each was responsible for one third of the subjects across experimental conditions.

Procedure. The rationale given to subjects, the seating arrangement, and the attack/nonattack manipulation were identical to those of Geen et al. (1975, pp. 722–723). The result of a rigged draw was said to "fully specify" what the two subjects would do throughout the experiment. The subject was given 12 cards, each bearing a statement on a controversial social issue, and said "I agree" or "I disagree" (depending on her views) after reading each statement aloud. The confederate, who was in auditory but not visual contact with the subject, either said "I agree" (meaning agreement with the subject's opinion) or delivered an aversive 97-dB(A) blast of 350-Hz square-wave noise for 4 seconds (meaning disagreement). The subject knew that the confederate had to deliver noise to express disagreement and that she had no option regarding its kind or loudness. Subjects in the attack conditions received 10 blasts of noise, whereas those in the nonattack cells received 2 blasts of noise. Except for the replacement of shocks by noise, these treatments were quite similar to those of Geen et al.

The manipulation of the opportunity-to-aggress variable was also very similar to the Geen et al. (1975, p. 723) procedure. Each of the confederate's 10 trials on a "maze-learning" task consisted of 3 "choice-points," and the confederate made 1–3 errors per trial, for a total of 20. On each of these 20 occasions (signaled by a light), subjects in the button-8 condition pressed the number 8 button on a 10-button Buss machine, as they had been instructed to do. Subjects in the button-2 condition pressed the number 2 button 20 times, following their instructions. In both the button-8 and button-2 conditions, subjects believed that the button they pressed delivered a "loud, but quite safe, blast of noise" to the confederate. Because of the previously mentioned ambiguity in the Geen et al. report (see Footnote 2), the no-opportunity subjects were divided into two subgroups. Half of the subjects simply monitored the confederate's progress over trials; neither noise nor the Buss machine was mentioned. The other half pressed the number 5 button on the Buss machine for each error, but there was no mention of noise. These subjects believed that pressing the button registered the confederate's errors "for subsequent computer analysis." The cables and electrical apparatuses surrounding the subject, and a PDP-12 computer next door, supported the cover story.

The dependent measure was obtained by means of a procedure highly similar to that of Geen et al. (1975, p. 723). The subject keyed in trigrams (on an apparatus not resembling the Buss machine), and the confederate made 10 "errors" over 16 "code-learning" trials. The subject had to press one of the 10 buttons on the Buss machine for each error, but she could freely choose the intensity of "loud blasts of noise" supposedly delivered to the confederate. The subject had been told that noise intensity increased with the button numbers, but that all levels were safe. Before the beginning of the code-learning task, subjects overheard the experimenter instruct the confederate to put on the earphones. In the case of the no-opportunity subjects who had previously pressed the number 5 button to register errors, the experimenter plugged in a different looking cable "explaining" that the machine was now plugged into the noise generator.

After the dependent-measure task, the subject responded on several scales described by Geen et al. (p. 723). A thorough debriefing session was then carried out.

Results and Discussion

Ratings. On a 100-mm scale prefaced by "How did you feel during the Opinion Simi-

larity Task?" (not angry–angry), attacked subjects rated themselves more angry ($M = 22.2$) than did nonattacked ones ($M = 8.9$), $F(1, 42) = 4.14$, $p < .05$. In addition, on a scale anchored by "I liked her very much" (0) and "I did not like her at all" (100), attacked subjects ($M = 46.4$) indicated that they liked the confederate significantly less during the opinion similarity task than did nonattacked subjects ($M = 29.6$), $F(1, 42) = 10.75$, $p < .01$. Finally, on a "happy" (0)–"not happy" (100) scale, attacked subjects ($M = 50.1$) were less happy than were nonattacked subjects ($M = 42.7$), but not significantly so, $F(1, 42) = 3.18$, $p < .10$. No other main effects or interactions on these three scales, or any effects on the other three scales used by Geen et al. (1975) even approached statistical significance. Overall, these results indicated that the attack manipulation was reasonably successful.

TABLE 1

MEAN INTENSITY OF PUNISHMENTS ON A 10-BUTTON BUSS MACHINE BY EXPERIMENTAL CONDITION

Mean intensity of shocks (Geen et al., 1975)

Subjects' interpolated activity	Initial treatment of subject by confederate	
	Attack	Nonattack
Shocks (button 5)	6.65_{a}	3.92_{bc}
No shocks	5.20_{b}	3.20_{c}

Mean intensity of noise blasts (Experiment 1)

Subjects' interpolated activity	Initial treatment of subject by confederate	
	Attack	Nonattack
Noise blasts (button 8)	6.09_{a} (8)	3.63_{bc} (8)
Noise blasts (button 2)	2.10_{cd} (8)	1.74_{cd} (8)
No noise[a]	4.49_{ab} (10)	1.45_{d} (6)

Note. Data in the upper section are from Geen, Stonner, and Shope (1975, Table 1). Cells having a common subscript, *within* the two experiments, are not different at the .05 level by the Duncan multiple-range test. In the upper section, these subscripts are provided for illustrative purposes only because they had been computed in the Geen et al. study on the basis of MS_e for the entire 2 × 3 design. In the lower panel, numbers in parentheses are cell *n*s.

[a] Means for the subgroups were as follows: attack/no pressing ($n = 5$), 4.64; attack/button 5 ($n = 5$), 4.34; nonattack/no pressing ($n = 3$); 1.40; nonattack/button 5 ($n = 3$), 1.50.

Intensity of noise. The means for the basic 2 × 3 design are shown in the lower part of Table 1. A footnote to this table provides the means for the subgroups in the no-opportunity-to-aggress conditions. Given the minute differences between the subgroups in both the attack and nonattack conditions, respectively, the subgroups were combined for the purpose of statistical analysis.

Both main effects were significant. Attacked subjects delivered noise blasts of greater intensity than did nonattacked ones, $F(1, 42) = 14.27$, $p < .01$. Subjects' interpolated activity (opportunity/no opportunity to aggress) also influenced their subsequent behavior, $F(2, 42) = 11.03$, $p < .01$. The interaction between the two variables was not significant, $F(2, 42) = 2.47$, $p = .097$. (MS_e for the above effects was 62.86.)

As is clear from Table 1, the means were generally depressed in comparison to those in the four critical conditions of the Geen et al. (1975) study, which are shown in the top part of the table. While comparisons across experiments must be made with caution, this finding may reflect the fact that females aggressed against females in the present study, whereas males aggressed against males in the Geen et al. experiment. Such considerations do not obscure the principal findings of the present experiment. The mean intensity of noise blasts administered by attacked subjects who had repeatedly delivered the button-8 noise in the preceding period was *greater* than that of attacked subjects who had had no prior opportunity to aggress, whereas the mean intensity of noise blasts delivered by the attacked/button-2 group was *smaller* than that of the attacked/no-opportunity subjects. In other words, by having subjects press one or another button in the first aggression period, either a cathartic *or* a disinhibitory effect may be readily obtained. By their particular operationalization of the opportunity to aggress (i.e., by requiring subjects to press repeatedly a button—number 5—well away from the bottom of the scale), which was followed by a dependent-measure task involving the same scale, Geen et al. may have inadvertently increased the odds of not obtaining the cathartic effect. Their data,

TABLE 2

PERCENTAGE OF RESPONSES IN DIFFERENT AREAS OF THE NOISE-INTENSITY SCALE BY EXPERIMENTAL CONDITION

Subjects' interpolated activity	Initial treatment of subject by confederate: Attack, Buttons 1–3	Attack, Buttons 4–6	Attack, Buttons 7–10	No attack, Buttons 1–3	No attack, Buttons 4–6	No attack, Buttons 7–10
Noise blasts (button 8)	12	44	44	56	23	21
Noise blasts (button 2)	81	18	1	91	9	0
No noise	29	55	16	95	5	0

Note. The percentages are based on the total number of responses in a given cell, $n \times 10$ (10 being the number of "error" trials on the dependent-measure task). Cell *n*s are given in Table 1. For example, in the attack/button-8 condition, 10 (12%) of the total of 80 (8×10) responses were on buttons number 1, 2, *or* 3.

therefore, are not convincing evidence against such an effect.

The pattern of the six means, the form of the opportunity (button 8, button 2)/no-opportunity main effect, and the absence of a significant interaction not only supported the notion of scale bias, but also suggested that the bias may be an artifact of only modest significance for aggression theory, rather than a finding that captures an underlying disinhibitory process. Both versions of the disinhibition hypothesis fared poorly. Neither version can account for the fact that the attacked/button-2 subjects delivered punishments of lower intensity than did the attacked/no-opportunity subjects. In addition, whereas the attack-only version cannot account for the nonsignificant overall interaction—as well as the nonsignificant 2×2 attack/nonattack $\times$ button-8/button-2 interaction, $F(1, 42) = 2.76$, $p = .104$—the Goldstein et al./Zimbardo version cannot explain the fact that the nonattacked/button-2 subjects delivered noise blasts of just as low intensity as did the nonattacked/no-opportunity subjects. While some isolated aspects of the results may be interpreted as supporting one or the other version of the disinhibition hypothesis, the overall pattern did not favor either version.

Distribution of responses. The way in which subjects in different conditions distributed their responses over various parts of the noise-intensity scale seems as important as the overall cell means. For this purpose, the scale was divided into three parts, such that responses in the two critical areas (around buttons number 2 and 8) could be distinguished from each other and from the middle area of the scale.

Several aspects of the data shown in Table 2 are of interest. In only three conditions did any subjects venture into the very top of the scale (buttons number 7–10), and two of these were the attacked/ and nonattacked/ button-8 groups. In the attack/button-8 condition, 44% of all responses were in this area, and of these, pressing the number 8 button was the most frequent response (13 out of 35, or 37%). In contrast, the attack/no-noise subjects pressed buttons in the top area relatively infrequently (less frequently than the nonattack/button-8 group) and concentrated instead on the middle area, especially, for some reason, on the number 4 button (53% of their responses were in the buttons number 4 through 6 range). In the nonattack/button-8 group, responses at the top of the scale were relatively frequent, in sharp contrast to the other two groups of nonattacked subjects and the attacked/button-2 subjects.

Thus, pressing the number 8 button 20 times in the preceding period appeared to lead *both* attacked and nonattacked subjects to spread their responses over the entire scale and to place a relatively large number of their responses at the top of the scale. On the other hand, pressing the number 2 button 20 times in the preceding period anchored subjects to the bottom of the scale. These findings further corroborated the conclusion that differential scale biases had been successfully

induced in Experiment 1 and suggested that a similar bias may have been induced in the Geen et al. (1975) study.

Effects of trials on intensity of noise. Subjects' behavior over the 10 dependent-measure trials—divided into 5 blocks of 2 trials each for the purpose of analysis—was also of considerable interest.[3] The main effect of trials was significant, $F(4, 168) = 11.33$, $p < .01$, revealing an increase in the intensity of noise over trials. The significant interaction of Attack/Nonattack × Blocks, $F(4, 168) = 6.18$, $p < .01$, indicated that the increase over trials was due largely to the behavior of attacked subjects. However, the significant triple interaction, $F(8, 168) = 2.75$, $p < .01$, $MS_e = 2.39$, modified the interpretation of both of the above effects. In Figure 1 are presented the overall Attack/Nonattack × Trials interaction and its profiles at the 3 levels of the opportunity/no-opportunity variable. Note that the form of the triple interaction did not dispute the conclusions reached earlier on the basis of the overall 2 × 3 means. In fact, from Figure 1 (Panels b, c, and d), it may be seen that the pattern of the means for Block 1 (first 2 trials) in the 6 basic conditions was quite similar to the pattern of the *overall* means (see Table 1). Thus, the intensity levels of *initial* noise blasts ("starting points") were affected by scale bias, just as were the overall means and the response distributions.

As can be seen from Panels b and c of Figure 1, the lines representing attacked and nonattacked subjects are essentially parallel. Thus, the button-2 and button-8 conditions did not contribute much either to the Attack/Nonattack × Blocks interaction or to the triple interaction. In the case of the button-2 groups, there was neither an effect of attack/nonattack nor of trials, $F(4, 168) < 1$, for the simple-interaction effect. For the button-8 groups, there was an effect of attack/nonattack, but no differential effect of trials, $F(4, 168) = 1.76$, $p = .139$, for the simple-interaction effect. Thus, both the Attack/Nonattack × Blocks and the triple interactions were largely produced by the no-noise groups, $F(4, 168) = 9.26$, $p < .01$, for the simple-interaction effect (see Panel d).

The absence of an increase over trials in both the attack/ and nonattack/button-2 conditions, and the parallel lines representing the behavior of the attacked/ and nonattacked/ button-8 subjects, were consistent with the artifact interpretation of scale bias. In contrast, the attack-only version of the disinhibition hypothesis could not account either for the lack of an increase over trials in the attack/button-2 condition or for the trials effect in the nonattack/button-8 cell. The Goldstein et al./Zimbardo version could not explain the lack of an increase over trials for both attacked/ and nonattacked/button-2 subjects, as well as a similar absence of a trials effect in the nonattack/no-opportunity cell.[4]

However, data for the no-opportunity conditions may be regarded as evidence for the attack-only version of the disinhibition hypothesis. No scale bias had been induced in these conditions; yet, the mean intensity of noise blasts increased over trials for attacked, but not for nonattacked, subjects. Nevertheless, it would seem unreasonable to interpret this differential behavior over trials as evidence against the cathartic effect. It could hardly be expected that the effect would emerge within the 10 trials of which the dependent-measure task consisted, that is, that a decrease would be observed after a "bout" containing only a few aggression opportunities. Of course, there is also the possibility that aggression escalates *within* a bout, whereas the cathartic effect occurs *across* consecutive bouts (the latter being a reliable finding in the Doob–Wood, 1972, Konečni, 1975a, and Konečni–Doob, 1972, studies).

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[3] These data were actually analyzed as forming a part of a 2 × 3 × 5 factorial design. Since Geen et al. (1975) had made no mention of the effects of trials, results for the basic 2 × 3 design of Experiment 1 were presented separately to facilitate a comparison with their study.

[4] It may seem that the absence of a trials effect in the nonattack/no-opportunity condition represents a failure to replicate some previous work, for example, a recent study by Buss, Booker, and Buss (1972). However, a close examination of their results (see Figures 1 and 2 of the Buss et al. report) shows that there was, in fact, *no* increase over the first *10* trials for the control subjects.

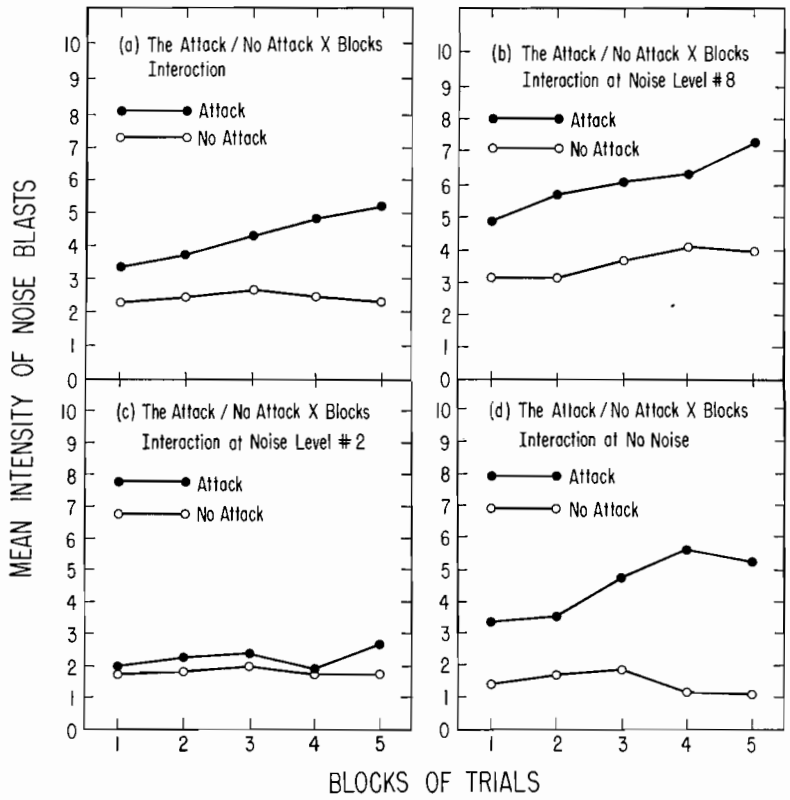

FIGURE 1. The overall (Panel a) and simple (Panels b, c, and d) Attack/No Attack × Blocks of Trials interactions (Experiment 1). The value given for a block is the mean of two consecutive trials.

An alternative explanation for the escalation over trials observed in the attack/no-opportunity cell is that these subjects had an implicit starting-point bias. Although this group had no prior experience with the scale, their response variability in the very *first* block of trials ($SD = 1.25$) was even somewhat *smaller* than that of the presumably biased attack/button-8 subjects ($SD = 2.00$). Given a starting-point bias located in the low part of the scale, and on the reasonable assumption that the attacked/no-opportunity subjects disliked the confederate and regarded low intensities as inadequate punishment, it is not surprising that an increase over trials was observed. To the extent that such low-starting-point biases exist, an increase over trials ("disinhibition") would be a built-in feature of the Buss aggression machine.[5]

An additional point concerning the Buss machine seems appropriate. The use of this machine in a learning-task context has recently been criticized on the grounds that

[5] Buss (1966a, p. 160) made a somewhat similar point: Subjects start low and then *explore* the scale, which yields an "upward drift." Goldstein et al. (1975) found, with a verbal analogue of the Buss machine, that nonangered subjects, forced to use the equivalent of the *number 10* button on the first trial, displayed an upward drift over trials. These authors concluded that Buss had been wrong, since only a downward drift would supposedly indicate scale exploration, given the button-10 start. Actually, the means for the successive blocks of 5 trials were 5.07, 5.11, 5.09, and 6.27: Not only was the effect of trials due solely to the last block, but the results could actually be interpreted as showing a *downward* drift from the number 10 button, implying exploration of the scale.

subjects' delivering shocks (noise) of higher intensity may sometimes be an index of their desire to help the confederate learn, rather than an index of aggression (e.g., Baron & Eggleston, 1972; Rule & Hewitt, 1971; Rule & Nesdale, 1974). Results for the no-opportunity groups did not support these criticisms, unless it is somewhat implausibly assumed that attacked subjects were more motivated to help the confederate than were the nonattacked ones.

Experiment 2

Experiment 1 offered more than enough support for a scale-bias interpretation of the Geen et al. (1975) results to suggest that such bias ought to be eliminated in order to examine the cathartic and disinhibitory effects adequately. In an attempt to attain this objective in Experiment 2, a dependent measure that did not involve the Buss machine was used, and a comparison was made between the Geen et al. attack procedure and a procedure that seemed more likely to lead to anger.

Rationale and Predictions

A simple way of eliminating scale bias seemed to be through the use of a dependent measure that did not involve either the Buss machine or intensity of punishments. Three conditions of Experiment 2 involved an exact replication of the attack/button-8, attack/button-2, and attack/no-opportunity conditions of Experiment 1, except that the *total number* of blasts of noise delivered by subjects in the context of a "creativity test" (e.g., Konečni & Doob, 1972) served as the dependent measure. This measure consists of many aggression opportunities, each of which the subject may choose to take or not take by pressing or not pressing a button.

Note that whereas the use of a different dependent measure may be expected to eliminate the effect of scale bias, *it should not prevent the emergence of a disinhibitory effect.* Neither version of the disinhibition hypothesis is presumably restricted to the Buss-machine operationalization of aggressive behavior or dependent on a procedural artifact (scale bias). As in Experiment 1, both versions predicted the following pattern of results: attack/button 8 $\geq$ attack/button 2 $>$ attack/no opportunity.[6]

A quite different pattern would support the cathartic effect: attack/no opportunity $>$ attack/button 2 $\geq$ attack/button 8. However, we were reluctant to predict that this effect would emerge in the three cells described so far, since the research to date has shown that anger—a pronounced negative *emotional* state—is necessary for its demonstration (Buss, 1961; Doob & Wood, 1972; Konečni, 1975a; Konečni & Doob, 1972). The attack manipulation used by Geen et al. may not be powerful enough to result in anger; its effects may be mainly cognitive, consisting of a negative evaluation of the confederate. One may disagree with people, especially concerning social issues known to be controversial, and even dislike them, without experiencing much emotion.[7]

An important feature lacking in the attack manipulation used by Geen et al. (1975) is *insult.* Buss (1961, 1966a) has suggested that insult may be a major antecedent of anger. Indeed, some form of severe, arbitrary insult has been used in a large number of studies successfully, judging by behavioral (aggression), physiological, verbal self-rating (anger), and rating-of-confederate criteria (e.g., Doob & Wood, 1972; Gambaro & Rabin, 1969; Hokanson & Shetler, 1961; Hokanson et al., 1963; Konečni, 1975a, 1975b; Konečni et al., in press; Konečni & Doob, 1972). In view of these studies, the second of the two disinhibition arguments offered by Geen et al. to explain the differ-

[6] It was not possible to predict whether the intensity of punishments delivered (button 2 vs. button 8) would have a differential effect in addition to the simple distinction of having versus not having delivered punishments.

[7] The diastolic-pressure increase following attack, observed by Geen et al. (1975), may have been due to the shocks subjects received and *attributed* by them to the shocks (subjects knew that the confederate had to administer shocks in order to show disagreement). Moreover, using a similar attack/nonattack procedure, Zillmann, Katcher, and Milavsky (1972) found, in contrast to Geen et al., that the physiological indices increased for *both* attacked and nonattacked subjects following the attack/nonattack manipulation, and about *equally* so.

ence between their own and the Doob–Wood results is surprising (the sex-of-subject/confederate argument has already been discussed). According to Geen et al., Doob and Wood were able to obtain the cathartic effect partly because the subjects had only been exposed to the confederate's "verbal badgering and insult" (p. 722). Subjects thus angered, who had had a prior aggression opportunity, supposedly obtained a low aggression score because they felt that the insulting confederate had already been "repaid." We concur that insult (and the resulting anger) was one of the key features of the Doob–Wood procedure. However, the available evidence suggests that the cathartic effect obtained by Doob and Wood was not due to inhibitory processes (Konečni & Doob, 1972). Instead, it may have been due to the anger-reducing properties of insulted subjects' aggression (in the first aggression period), leading to a decrease in the amount of their subsequent aggression. (See Konečni, 1975a, for an elaboration of this position, that is, a reinterpretation of the cathartic effect in terms of a relationship of bidirectional causality between the degree of anger and the amount of physical aggression expressed.)

Such considerations clearly warranted that the attack procedure used by Geen et al. be contrasted with a manipulation containing insult. Therefore, we included three additional conditions in the design of Experiment 2. These conditions in every way paralleled the three conditions already described, except that an insult procedure replaced the attack manipulation. We predicted that insult would prove to be more powerful than attack (in terms of various ratings), and that the cathartic effect, rather than a pattern consistent with either version of the disinhibition hypothesis, would be obtained in the cells involving insult. Control groups of subjects who experienced neither insult nor attack were also included in the design.

Method

Subjects. Subjects were 58 females drawn from the same population as those in Experiment 1. The 10 confederates and 3 experimenters were those used in Experiment 1.

Procedure. The design was a 2 (Attack vs. Insult) × 3 (Opportunity-to-Aggress/Button 8; Opportunity-to-Aggress/Button 2; No Opportunity) factorial, with two additional control conditions. Subjects assigned to the three attack conditions were treated exactly as the attack subjects had been treated in Experiment 1. The procedure used in the three insult conditions had been described in detail elsewhere (Konečni, 1975a; Konečni & Doob, 1972). A subject and a confederate, seated at the same table, worked independently for 7 minutes on identical, difficult anagrams. The confederate finished her anagrams quickly and proceeded to insult the subject in a standardized manner. She criticized the subject's slowness, alternately told her to hurry up and to give up, and repeatedly expressed doubts about the subject's intellectual ability. Ten different confederates were used to increase generality.

In the next period, the opportunity/no-opportunity variable was manipulated exactly as in Experiment 1. Both the attack/ and insult/no-opportunity groups were again subdivided as in Experiment 1. Since the differences were negligible, the respective subgroups were combined for statistical purposes.

The dependent-measure setting, task, and instructions have been described in detail elsewhere (Konečni, 1975a; Konečni & Doob, 1972). The task supposedly involved "creative imagination." The subject read words from a 30-word list, and the confederate provided "creative" responses. On a two-button box, the subject pressed the button labeled "good" whenever she thought a response was creative, and pressed a button labeled "noise" one or more times for each uncreative response. Confederates' responses were standardized. Subjects believed that the noise button delivered a "loud, but safe" blast of noise to the confederate. During this task, the subject and confederate were in auditory, but not visual, contact. The dependent measure was the total number of blasts of noise delivered by a subject.[8]

The two control groups differed from each other only in the initial treatment, which was either no attack (as in Experiment 1) or no insult (as in Konečni & Doob, 1972). In the latter case, the confederate treated the subject neutrally during the anagram task. Subjects in both conditions then monitored the confederate's work on the maze task without pressing any buttons. Because the differences were negligible, the two control groups were combined for statistical purposes. Finally, all subjects responded on the scales used in Experiment 1 and were debriefed.

Results and Discussion

Ratings. An analysis of variance (2 × 3, excluding the control group) revealed that

[8] In an attempt to replicate the Konečni–Doob (1972) results, the same stimulus words, responses, and order were used. Because the original list had not been divided into sublists equated for creativity, the effect of trials could not be examined.

insulted subjects had been considerably more angry in the first part of the experiment than attacked ones had been, $F(1, 46) = 8.06$, $p < .01$ (Ms = 23.3 for attack and 42.9 for insult, where 0 = "not angry" and 100 = "angry"). Attacked subjects, in turn, were more angry than controls (as revealed by a one-way analysis of variance), $F(3, 28) = 3.01$, $p < .05$ ($M = 9.2$ for the controls). In addition, insulted subjects disliked the confederate far more than attacked subjects did, $F(1, 46) = 20.48$, $p < .01$ (Ms = 41.0 for attack and 64.8 for insult, where 0 = "I liked her very much"), whereas attacked subjects disliked the confederate more than controls did, $F(3, 28) = 6.57$, $p < .01$ ($M = 20.8$ for the controls). Finally, whereas insulted subjects were not significantly less "happy" than attacked ones were, $F(1, 46) = 2.98$, $p < .10$ (Ms = 55.1 for attack and 66.0 for insult, where 0 = "happy"), they were less happy than controls, $F(3, 28) = 3.11$, $p < .05$ ($M = 35.0$ for the controls). In contrast, attacked people were not significantly less happy than controls, $F(3, 28) = 1.48$. No other effects on these three scales, or on other scales used by Geen et al., approached statistical significance.

In summary, whereas the attack versus no-attack differences found in Experiment 1 were replicated, the results indicated that the insult procedure was far more powerful than attack. This is important in view of the evidence suggesting that a successful experimental induction of anger is indispensable for the demonstration of the cathartic effect.

TABLE 3

MEAN NUMBER OF NOISE BLASTS ON THE "CREATIVITY TEST" BY EXPERIMENTAL CONDITION

Subjects' interpolated activity	Initial treatment of subject by confederate		
	Attack	Insult	No attack or insult
Noise blasts (button 8)	11.25_b (8)	9.88_b (8)	—
Noise blasts (button 2)	10.25_b (8)	10.38_b (8)	—
No noise	10.70_b (10)	14.90_a (10)	7.00 (6)

Note. Cells having a common subscript are not different at the .05 level by the Duncan multiple-range test. Because this test was based on MS_e from the 2 × 3 analysis of variance, no subscript is shown for the control condition. Numbers in parentheses are cell *n*s.

Number of noise blasts. A 2 × 3 analysis of variance (excluding the control condition; $MS_e = 7.71$) revealed a nonsignificant main effect of attack versus insult, $F(1, 46) = 1.62$, a significant main effect of the opportunity/no-opportunity variable, $F(2, 46) = 4.16$, $p < .05$, and a significant interaction, $F(2, 46) = 4.62$, $p < .05$. As can be seen from the pattern of the means in Table 3, both significant effects were due to the large number of noise blasts delivered by insulted subjects who had no prior opportunity to aggress. The one-way analysis on all 7 means revealed a significant effect of treatments, $F(6, 51) = 5.93$, $p < .01$, $MS_e = 7.50$. By Dunnett's test, the control group did not differ significantly (by two-tailed tests) from insult/button 8, attack/button 2, and insult/button 2, but it was significantly smaller than attack/no noise ($p < .05$), attack/button 8 ($p < .05$), and insult/no noise ($p < .01$).

Thus, a strong cathartic effect was obtained in the three insult conditions: Insulted subjects who had had a prior aggression opportunity subsequently delivered fewer punishments than did the equally insulted subjects without a prior opportunity. In fact, the opportunity conditions (button 8, button 2) did not significantly differ from the control condition. These results fully replicated the findings of Doob and Wood (1972), Konečni and Doob (1972), and Konečni (1975a); moreover, this was achieved with the Buss-machine operationalization of the opportunity-to-aggress variable, a procedure different from that used in earlier studies. In short, with subjects who had been insulted (and thus presumably made quite angry), and when scale bias had been removed by a change in the dependent measure, the cathartic effect was obtained, rather than a disinhibitory one. Both versions of the disinhibition hypothesis, of course, predicted an increase in aggression in the opportunity groups in comparison to the no-opportunity group.

The three attack conditions did not differ from each other. These results represent another failure of both versions of the disinhi-

bition hypothesis and stand in sharp contrast to the findings in the three attack cells of Experiment 1 and the two attack conditions of the Geen et al. study. The purpose of using a different dependent measure in Experiment 2 was to eliminate scale bias. However, this minor procedural change eliminated all effects of the opportunity-to-aggress variable on attacked subjects. These findings complemented the evidence gathered in Experiment 1: The scale bias effect seems to be an artifact of only minor substantive interest; it does not tap an underlying disinhibitory process.

General Discussion

The converging evidence from Experiments 1 and 2 suggests that the disinhibition-like effect obtained by Geen et al. (1975), and their failure to replicate the previous results, may have been due to a procedural artifact and a somewhat weak induction of anger. Predictions based on the various versions of the disinhibition hypothesis received no consistent support. In contrast, the cathartic effect was obtained after removing scale bias, and only with insulted subjects. The latter finding supported earlier suggestions that a relatively powerful method of inducing anger is necessary for the demonstration of the cathartic effect (e.g., Buss, 1961; Doob & Wood, 1972; Konečni, 1975a).

We are obviously not suggesting that disinhibition is an invalid concept. The disinhibitory (facilitating) effects on angered and nonangered subjects' aggression of watching aggressive models and films, as well as of other activities mentioned in the introduction, have been well documented. However, disinhibition apparently does not occur in situations where (a) the emotion of anger has been induced, (b) subjects had a prior opportunity to hurt (physically) the annoyer, and (c) the dependent measure involves physical aggression. Under such circumstances, a decrease in aggression, the cathartic effect, is obtained instead.

The theoretical reasons for the occurrence of the cathartic effect in experiments involving anger and physical aggression have been discussed in detail elsewhere (Konečni, 1975a), and need not be reiterated here. It also seems clear that the cathartic effect is not necessarily due to an inhibitory tendency such as "guilt" or "aggression anxiety," as Berkowitz (1962) and Geen et al. have suggested. Konečni and Doob (1972) and Frost and Holmes (Note 1) found that nonangered subjects who had a prior opportunity to hurt a confederate (which should have aroused guilt) were subsequently no less aggressive against a different confederate than were nonangered subjects without a prior aggression opportunity (who presumably felt less guilty). Konečni and Doob (1972) and Konečni (1975a) obtained the same result even when the two-opportunities nonangered group always had the same confederate as a target. Holmes (1972) found that angered subjects felt no more guilty after either direct or displaced aggression than did controls. In short, the cathartic effect obtained in Experiment 2 and previous studies seems to be relatively independent of the (dis)inhibition factors.

In exploring the Geen et al. (1975) results, the present experiments simultaneously addressed some more general methodological and substantive issues. Criticisms of aggression research have often focused on the "meaning" of laboratory measures of aggression (e.g., the "teaching artifact" mentioned earlier). Such concern is highly justifiable. However, the issue of procedural bias, which seems just as important, has received very little attention. The term *procedural bias*, as used here, denotes the effect of secondary components of an independent variable, namely those which have not been designated as the key features of the operationalization. (Thus, Geen et al. defined their conditions as "shocks vs. no shocks," not "shocks-button-5 vs. no shocks." Presumably, they did not regard the number 5 button as the critical feature of the opportunity-to-aggress variable.) This type of procedural bias is clearly due to the *relationship* between the independent and dependent variables, rather than to the latter alone. Although this problem is not confined to the research on the cathartic effect, it is particularly salient in this area, because subjects typically engage in aggression on two successive occasions. This requirement makes some catharsis

experiments open to alternative explanations based on procedural-bias issues (cf. Kahn, 1966; Konečni, 1975a).

Of course, a procedural bias may not necessarily be an artifact, by which we mean an effect irrelevant for aggression. Had we found that scale bias tapped a disinhibitory process, we would have considered it an indirect confirmation of an aggression-relevant concept, rather than an artifact. This point emphasizes the need for careful analyses of procedures used in aggression research. All too often, criticisms of a particular procedure or finding are not backed by experiments designed to separate artifact from substance.

However, the criterion for the accuracy of the type of analyses we are recommending must be the real-life patterns of aggressive behavior, rather than the current theoretical concepts. For example, whether or not the "many-punishments" standard (Konečni, 1975a) is an artifact depends on the likelihood that people continue to aggress in real-life interpersonal situations after their anger has subsided. Very little is presently known about the relationship between the degree of anger, and the duration and other characteristics of real-life bouts involving physical aggression. Similarly, the present conclusion that the scale bias effect is an artifact ultimately rests on the assumption that comparable biases do not govern aggressive behavior in real life; such an assumption may be incorrect.

Nevertheless, even when data collected in the field are in agreement with laboratory results, this does not preclude the possibility that the latter have been produced by procedural biases. For example, in a widely cited laboratory experiment, Kahn (1966) found that angered subjects' verbal aggression was increased by their prior verbal aggression, and suggested that this may have been due to cognitive consistency: Having once verbally expressed their dislike for the annoying confederate, subjects felt compelled to do so 5 minutes later irrespective of their true opinion at that time. If Kahn's explanation is correct, his results may be viewed as due to a type of procedural bias, because cognitive consistency was "imposed" by the procedure. Whether or not such a bias is an artifact depends on the degree to which real-life verbal aggression is governed by cognitive consistency. In a field experiment, Ebbesen et al. (1975) replicated Kahn's results, but the data offered little support for a cognitive consistency explanation. To the extent that one considers naturalistic data as standards against which laboratory data should be measured, the conclusion would be that in the laboratory "wrong" procedures are sometimes used to obtain "right" results. In other words, sound intuitions concerning external validity may sometimes, paradoxically, contaminate the internal validity of laboratory experiments and also lead to inadequate theoretical explanations.

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