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The Impact of Mass Media Violence on U.S. Homicides

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The impact of mass media violence on aggression has almost always been studied in the laboratory; this paper examines the effect of mass media violence in the real world. The paper presents the first systematic evidence indicating that a type of mass media violence triggers a brief, sharp increase in U.S. homicides. Immediately after heavyweight championship prize fights, 1973–1978, U.S. homicides increased by 12.46 percent. The increase is greatest after heavily publicized prize fights. The findings persist after one corrects for secular trends, seasonal, and other extraneous variables. Four alternative explanations for the findings are tested. The evidence suggests that heavyweight prize fights stimulate fatal, aggressive behavior in some Americans.

Since 1950 more than 2500 studies have attempted to discover whether mass media violence triggers additional aggressive behavior (Comstock et al., 1978; Murray and Kippax, 1979; Roberts and Bachen, 1981; National Institutes of Mental Health, 1982). With few exceptions (reviewed in Phillips, 1982b), researchers have studied aggression *in the laboratory,* and there is consensus that media violence can trigger additional aggression in the laboratory setting. However, policy makers, unlike researchers, have been primarily concerned with violence *outside* the laboratory, particularly with serious, fatal violence like homicide. Studies of media effects on homicide have been extremely rare and there is no systematic evidence to date indicating that mass media violence elicits additional murders.[1] As Andison has noted (1980:564), we do not know whether "there are deaths and violence occurring in society today because of what is being shown on the TV screen."

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This paper presents what may be the first systematic evidence suggesting that some homicides are indeed triggered by a type of mass media violence. The current study builds on earlier research (Phillips, 1974, 1977, 1978, 1979, 1980, 1982a) which showed that: (1) U.S. suicides increase after publicized suicide stories. This finding has been replicated with American (Bollen and Phillips, 1982) and Dutch (Ganzeboom and de Haan, 1982) data. (2) The more publicity given to the suicide story, the more suicides rise thereafter. (3) The rise occurs mainly in the geographic area where the suicide story is publicized. (4) California (Phillips, 1979), Dutch (Ganzeboom and de Haan, 1982), and Detroit (Bollen and Phillips, 1981) auto fatalities all increase just after publicized suicide stories. (5) The more publicity given to the stories, the greater the increase, and (6) the increase occurs mainly in the area where the story is publicized. (7) Single-car crash fatalities increase more than other types, and (8) the driver in these crashes is significantly similar to the person described in the suicide story, while the passengers are not. These results are statistically significant and persist after correction for day-of-the-week and seasonal fluctuations, holiday weekends, and linear trends. After testing alternative explanations, Phillips concluded that suicide stories appear to elicit additional suicides, some of which are disguised as auto accidents.

It would be interesting to discover whether *homicide* stories elicit additional homicides. But it is difficult to conduct such a study because, unlike suicide stories, homicide stories occur so often that it is very difficult to separate the effect of one story from the effect of the others. However, some other types of violent stories occur much less often, and it is possible to discover whether these types of stories trigger a rise in U.S. homicides.

MASS MEDIA VIOLENCE AND U.S. HOMICIDES

In reviewing the literature on media effects, Comstock (1977) concluded that violent stories with the following characteristics were most likely to elicit aggression: When the violence in the story is presented as (1) rewarded, (2) exciting, (3) real, and (4) justified; when the perpetrator of the violence is (5) not criticized for his behavior and is presented as (6) intending to injure his victim.[2]

One type of story that meets all of these criteria is the heavyweight prize fight, which is almost universally presented as highly rewarded, exciting, real, and

[1] Some anecdotal data link a particular murder with subsequent murders or murder attempts (e.g., the Tylenol "copycat" crimes). But I know of only one *systematic* study of the topic (Berkowitz and Macaulay, 1971). This study found no increase in homicides after three publicized murder stories.

[2] Comstock also notes that a story is more likely to be imitated if the aggressor in the story is like the person exposed to the story, and if the victim in the story is like the imitator's victim. These points will be taken up later in this paper.

justified. Furthermore, the participants are not criticized for their aggressive behavior and are presented as trying to injure each other.

In a well-known series of studies, Berkowitz and various associates (1963, 1966, 1967, 1973) examined the impact of a filmed prize fight in the laboratory. They found that angered laboratory subjects behaved more aggressively after seeing a filmed prize fight scene. In contrast, angered laboratory subjects exposed to a track meet film displayed a significantly lower level of aggression.

In sum, the heavyweight prize match is a promising research site because (1) it meets Comstock's criteria for stories most likely to elicit aggression, and (2) it is known to elicit aggression in the laboratory.

DATA SOURCES

An exhaustive list of championship heavyweight prize fights and their dates was obtained from *The Ring Book Boxing Encyclopedia,* which is the standard reference on the topic. The period 1973–1978 has been chosen for analysis because, for this period, daily counts of all U.S. homicides are publicly available from the National Center for Health Statistics.[3]

METHOD OF ANALYSIS

A standard time-series regression analysis is used.[4] Homicides are known to fluctuate significantly by day of the week, by month, and by year (Conklin, 1981). In addition, as we will see, homicides rise markedly on public holidays. All these "seasonal" effects must be corrected before one can assess the effect of prize fights on homicides.

A 0–1 dummy variable was constructed for all days that were Mondays, another dummy variable was coded for Tuesdays, and in general a different dummy variable was assigned to each day of the week, with Sunday being the omitted variable. Similarly, a 0–1 variable was coded for each month of the year (with January being the omitted variable), and for each year (with 1978 being the omitted variable). In

[3]Data for 1973–1977 consist of computerized death certificates generated by the National Center for Health Statistics and made available by the Inter-University Consortium for Political Science Research. As of this writing, 1978 computerized death certificates are not yet publicly available. Consequently, for 1978, a published table (National Center for Health Statistics, 1978: Table I-30) has been used instead. A 50 percent sample of 1972 deaths is also available but will not be analyzed, because its inclusion with the complete, 100 percent sample data for 1973–1978 would violate the assumption of homoscedasticity required in the analysis that follows. It is theoretically possible to correct for this type of heteroscedasticity and then include the 1972 data in the analysis. But it was judged unnecessary to do so, because the data set is already very large even without the 1972 information. In all, there are 2192 data points for the daily data, 1973–1978.

[4]For the application of this approach to daily mortality data, see Bollen and Phillips (1981, 1982). For general introductions to time-series regression techniques, see Ostrom (1978), Rao and Miller (1971) and Johnston (1972).

addition, a dummy variable was assigned to each of the public holidays (New Year's Day, Memorial Day, Independence Day, Labor Day, Thanksgiving, and Christmas). Finally, a dummy variable, PFIGHT(X), was used to indicate the presence of a championship prize fight. The regression coefficient of PFIGHT(X) gives the effect of a prize fight on homicides X days later (i.e., the effect of a prize fight lagged X days). Initially, the effect of the prize fight is examined for the 10-day period following it; later, a longer period is studied.

RESULTS

Table 10.1 gives the size and statistical significance of each coefficient.[5] This table shows that, after the average championship prize fight, homicides increase markedly on the third day (by 7.47) and on the fourth day (by 4.15), for a total increase of 11.62.[6] The rise in homicides after the prize fight is statistically significant.[7]

[5]These statistical significances are biased if there is serial correlation among the regression residuals. The conventional test for serial correlation, the Durbin-Watson test, is appropriate when a lagged dependent variable is included in the regression model (Nerlove and Wallis, 1966), as is the case in Table 10.1. A common alternative test, using Durbin's h statistic, cannot be used here for reasons described in Bollen and Phillips (1982: fn. 7). Consequently, another test for serial correlation proposed by Durbin (1970) was used instead. This test reveals no evidence of first-order autocorrelation. Autocorrelation of higher orders was sought by the methods described in Bollen and Phillips (1982), with no evidence of serial correlation being uncovered. One other feature of Table 10.1 should be mentioned briefly. The table shows that homicides increase markedly on all U.S. public holidays, except Memorial Day. To my knowledge, this finding has not been previously demonstrated with U.S. daily homicide data.

[6]The coefficient of .12 for HOMICIDE(1) indicates that there is a small, lagged endogenous effect. This implies, for example, that each of the lagged prize fight dummies has its impact distributed over more than one day. Therefore, the effect of prize fights on homicides one day later (for example) does not take place one day later, but is realized over a longer period. The small coefficient for the endogenous variable (.12) means that the long-run effects of PFIGHT(X) and other variables decay very rapidly and aren't much more than their immediate ones, but the distributed effects do exist. Thus, the pattern of lags is more complicated than is immediately apparent from Table 10.1. In sum, because of small, lagged endogenous effects, the impact of one PFIGHT(X) variable overlaps to a small extent with the impact of another. However, the presence of the lagged endogenous variable does not affect the validity of the statistical tests of the hypotheses (see also footnote 7).

[7]In Table 10.1 we are examining the series of 11 coefficients, PFIGHT(0), PFIGHT(1), . . . PFIGHT(10). Under the null hypothesis, none of these 11 prize fight coefficients is likely to be very large. On the other hand, under the alternative hypothesis that prize fights trigger homicides, one or more of these 11 coefficients is likely to be large and positive. If one or more of the PFIGHT(X) coefficients is sufficiently large, we can reject the null hypothesis in favor of the alternative. One way to discover whether H_0 can be rejected is to proceed as follows. Because the covariance matrix indicates that the estimates of the coefficients for PFIGHT(X) are uncorrelated, and because of the asymptotic normality of the coefficient estimates, it follows that these coefficient estimates are in fact independent. This in turn implies that the *t*-statistics for each of these coefficients are independent. Under these circumstances, one can use the binomial test to evaluate the probability of finding that r or more of the PFIGHT(X) coefficients are statistically significant at a given level. Table 10.1 indicates that there are two PFIGHT(X) coefficients — PFIGHT(3), PFIGHT(4) — which are statistically significant at .025 or better. For $n = 11$, $P = 0.25$, $r \geq 2$, the binomial test indicates that the probability of finding two or more significance levels of .025 in 11 independent trials is .0296. Thus, we can reject the null hypothesis on the joint evidence provided by the 11 PFIGHT(X) coefficients.

TABLE 10.1
U.S. homicides regressed on heavyweight prize fight, controlling for daily, monthly, yearly, and holiday effects, 1973–1978

Regressand HOMICIDES	R^2 .671	$\bar{R}^2$ .665	D.F. 2148	N 2190
Regressor	**Regression coefficient**	***t*-statistic**		
Intercept	55.34*	30.16		
HOMICIDE(1)	.12*	5.64		
PFIGHT(−1)	1.97	.94		
PFIGHT(0)	1.95	.93		
PFIGHT(1)	−.26	−.13		
PFIGHT(2)	1.32	.63		
PFIGHT(3)	7.47***	3.54		
PFIGHT(4)	4.15†	1.97		
PFIGHT(5)	−.60	−.29		
PFIGHT(6)	3.28	1.57		
PFIGHT(7)	.35	.17		
PFIGHT(8)	.99	.47		
PFIGHT(9)	3.10	1.48		
PFIGHT(10)	2.28	1.09		
Monday	−16.46*	−21.74		
Tuesday	−16.71*	−17.97		
Wednesday	−18.42*	−19.13		
Thursday	−15.81*	−15.88		
Friday	−8.02*	−8.41		
Saturday	14.54*	16.95		
February	1.88**	1.99		
March	1.13	1.23		
April	.43	.46		
May	−.69	−.73		
June	1.61	1.74		
July	4.16*	4.46		
August	4.46*	4.83		
September	3.91*	4.16		
October	2.79*	3.02		
November	3.04*	3.25		
December	5.86*	6.30		
1973	−1.11	−1.70		
1974	1.71*	2.62		
1975	1.28	1.96		
1976	−3.01*	−4.60		
1977	−1.73*	−2.62		
New Year's Day	41.08*	10.29		
Memorial Day	−1.05	−.28		
Independence Day	21.61*	5.89		
Labor Day	16.92*	4.56		
Thanksgiving	18.34*	4.98		
Christmas	10.25*	2.79		

Note: The variable HOMICIDE(1) indicates homicides lagged one day. Two-tailed *t*-tests are used for all seasonal variables; one-tailed *t*-tests for prize fight variables.
*Significant at .01 or better.
**Significant at .05 or better.
***Significant at .0002.
†Significant at .025.

Table 10.1 shows that the third day displays by far the largest peak in homicides. It is interesting to note that this "third-day peak" appears not only in the present study but also, repeatedly, in several earlier investigations: California auto fatalities peak on the third day after publicized suicide stories (Phillips, 1979), as do Detroit auto fatalities (Bollen and Phillips, 1981) and U.S. noncommercial airplane crashes (Phillips, 1978, 1980). At present we do not know the precise psychosocial mechanisms producing the third day lag, but this phenomenon has now been replicated so often in different data sets that it seems to be a relatively stable effect which will repay future investigation.

The observed peak in homicides after a prize fight cannot be ascribed to day-of-the-week, monthly, yearly, or holiday effects, because all of these "seasonal" variables were corrected for in the regression analysis. In addition, one cannot plausibly ascribe the homicide peak to random fluctuations, because the peak is statistically significant.

SOME ALTERNATIVE EXPLANATIONS FOR THE PEAK IN HOMICIDES

Two different explanations can be tested with the data in Table 10.2. For each fight, this table indicates: (1) The number of homicides observed three days after the prize fight. (2) The number of homicides expected on the third day, under the null hypothesis that prize fights have no effect on homicides.[8] (3) The difference between the observed and expected number of homicides. (A positive difference indicates that homicides are higher than expected just after the prize fight.) (4) Whether the fight was held outside the United States. (5) Whether the fight was discussed on the network evening news.

TABLE 10.2
Fluctuation of U.S. homicides three days after each heavyweight prize fight, 1973–1978

Name of fight	Observed no. of homicides	Expected no. of homicides	Observed minus expected	Fight held outside U.S.?	On network evening news?
Foreman/Frazier	55	42.10	12.90	yes	yes
Foreman/Roman	46	49.43	−3.43	yes	no
Foreman/Norton	55	54.33	.67	yes	no
Ali/Foreman	102	82.01	19.99	yes	yes
Ali/Wepner	44	46.78	−2.78	no	yes
Ali/Lyle	54	47.03	6.97	no	yes
Ali/Bugner	106	82.93	23.07	yes	no
Ali/Frazier	108	81.69	26.31	yes	yes
Ali/Coopman	54	45.02	8.98	yes	no
Ali/Young	41	43.62	−2.62	no	no
Ali/Dunn	50	41.47	8.53	yes	yes
Ali/Norton	64	52.57	11.43	no	yes
Ali/Evangelista	36	42.11	−6.11	no	no
Ali/Shavers	66	66.86	−.86	no	no
Spinks/Ali	89	78.96	10.04	no	yes
Holmes/Norton*	53	48.97	4.03	no	no
Ali/Spinks	59	52.25	6.75	no	yes
Holmes/Evangelista*	52	50.24	1.76	no	no

*Sponsored by World Boxing Council; all other fights sponsored by the World Boxing Association.

[8]Under the null hypothesis, PFIGHT(X) has no impact on the number of homicides; thus, for Table 10.2 the expected number of homicides under H_0 is calculated by omitting PFIGHT(X) from the regression variables and rerunning the regression equation.

"Personal experience" hypothesis. Perhaps the prize fight affects only those actually attending the fight, not those experiencing it through the mass media. If this is so, one cannot claim that *mass media* violence is triggering a rise in homicides.

If one must personally experience the prize fight in order to be affected by it, then prize fights occurring outside the United States should trigger few if any U.S. homicides. In contrast, prize fights held inside the United States should elicit much larger rises in homicides. The evidence in Table 10.2 contradicts these predictions. After the average "foreign" fight, U.S. homicides rise by 12.128, while a much *smaller* rise, 2.862, occurs after the average U.S. fight.[9] Thus, the "personal experience" hypothesis does not seem plausible.

"Modeling" hypothesis—first test. A different hypothesis can also be tested with the data in Table 10.2. Prize fights may trigger some homicides through some type of modeling of aggression. If this is so, then prize fights receiving much publicity should have a greater effect than prize fights receiving less publicity.

One way to test this hypothesis is to see whether prize fights discussed on the network evening news are followed by relatively large increases in homicides, while relatively small increases occur after the remaining, less-publicized prize fights.[10] The evidence in Table 10.2 is consistent with this "modeling" explanation. Homicides rise by 11.127 after the average "publicized" fight, and by only 2.833 after the average unpublicized one. The difference between these two figures is statistically significant at .0286 (two-sample *t*-test, one-tailed).[11]

It is perhaps worth noting that the most touted of all the prize fights in this period, the so-called "Thrilla in Manilla" between Ali and Frazier, displays the largest third-day peak in homicides.

"Modeling" hypothesis—second test. The modeling hypothesis can also be tested in another way. The laboratory literature on the modeling of mass media aggression (see footnote 2) repeatedly suggests that (1) a person is more likely to imitate an aggressor on the screen if he is similar to that aggressor; and (2) a person is more likely to aggress against a target victim if his target is similar to the victim on the screen.[12] In sum, the laboratory literature suggests that there is modeling of both the *aggressor* and of the aggressor's *victim.*

[9]At present, we do not know why U.S. homicides rise so much more after foreign than domestic fights. Perhaps a detailed study of the characteristics of these fights would help to resolve this question.

[10]A thorough analysis of this topic is desirable but would be extremely laborious. Future studies might attempt to measure the additional publicity derived from advertisements in all the media, not only at the time of the fight, but also in the weeks and months preceding it. In addition, one might wish to measure closed circuit television receipts, corrected for inflation.

[11]The formula used for this particular *t*-test does not require that the two compared populations have equal variances. For a description of this test, see Brownlee (1965:299–303). One might prefer to substitute the Mann-Whitney for the *t*-test. When this is done, $P = .0211$.

[12]Berkowitz and associates (1963, 1966, 1967, 1973) have shown this in a series of ingenious studies particularly relevant to this paper. They showed that laboratory subjects were most likely to inflict shocks on a target if that target had the same name as the losing boxer on the screen.

If *aggressor* modeling exists after a prize fight, then after a young, black male wins a boxing match, murders by young, black males should increase (but murders by young, *white* males should not). Conversely, after a young, white male wins a boxing match, the opposite findings should occur. Unfortunately, *aggressor* modeling cannot be studied with the death certificates examined in this paper, because these certificates do not reveal the identity of the murderer, only of the victim.

However, it is possible to use these death certificates to discover whether *victim* modeling exists after a heavyweight prize fight. If such modeling occurs, then, just after a prize fight, homicide victims should be unusually similar to the losing boxer. Specifically, after a young, *white* male is beaten in a prize fight, the homicide deaths of young, white male victims should increase; no such increase should appear for young, *black* male victims. Conversely, after a young, *black* male is beaten in a prize fight, the homicide deaths of young, *black* male victims should increase, while the homicide deaths of young, *white* males should not.

These predictions can be tested with the information in Tables 10.3 and 10.4, which distinguish between the impact of "black loser" prize fights (in which a black is beaten) and "white loser" prize fights (in which a white is beaten).[13] The detailed mortality data necessary to generate these tables can be found only in the computerized death certificates cited in footnote 3. These are available only for 1973–1977. Thus, it should be stressed that the period to be examined in the remainder of this paper is 1973–1977, not 1973–1978, as in Tables 10.1 and 10.2.

Table 10.3 examines the impact of "white loser" and "black loser" prize fights on the homicides of young, *white* male victims. The evidence supports the hypothesis of victim modeling. *White* loser prize fights are followed by significant increases in young, white male homicide deaths; in contrast, *black* loser prize fights do not seem to trigger young, white male homicide deaths.[14]

White homicides increase significantly on the day of the prize fight (by 3.86 per fight), two days thereafter (by 3.14 per fight), and eight days after the fight (by 2.97 per fight). Thus young, white male homicides rise by a total of 9.97 (= 3.86 + 3.14 + 2.97)

[13]In the period under study (1973–1977) nearly all the losing boxers were 20–34.9 years of age; consequently I have defined "young males" as men in this age range. Nearly all the losing boxers were white (Wepner, Bugner, Coopman, Dunn) or black (Frazier, Norton, Foreman, Lyle, Young, Shavers). However, two of the losing boxers were Hispanic Americans (one Uruguayan and one Puerto Rican). There is no separate classification for Hispanic Americans on the computerized death certificate, and it is unclear whether one can treat these fighters as either white or black. Consequently, these two fights have been excluded from the analysis that follows.

[14]The analysis described in footnote 7 (and applied to Table 10.1) can be reapplied to the results in Table 10.3. Examining the coefficients, WL(0), . . . WL(10), we see that three are statistically significant at .0251 or better. The covariance analysis indicates that the estimates of the coefficients WL(X) are uncorrelated. Because of this and the asymptotic normality of the coefficient estimates, we can treat as independent the *t*-statistics for WL(0), . . . WL(10). Using the binomial test, with $n = 11$, $P = .0251$, $r \geq 3$, one finds that the probability of finding three or more coefficients significant at .0251 in 11 independent trials is .0022. Hence, the homicides of young, white males increase significantly just after "White Loser" prize fights.

TABLE 10.3
Impact of "White Loser" (WL) and "Black Loser" (BL) prize fights on the homicides of young, white male victims, U.S., 1973–1977[a]

Regressand	R^2	$\bar{R}^2$	D.F.	N
HOMICIDES	.3781	.360	1772	1825

Regressor	Regression coefficient	*t*-statistic
Intercept	10.43*	23.78
HOMICIDE(1)	.01	.23
WL(−1)	.70	.46
WL(0)	3.86**	2.54
WL(1)	.30	.20
WL(2)	3.14**	2.07
WL(3)	.48	.31
WL(4)	.57	.37
WL(5)	−.29	−.19
WL(6)	.93	.61
WL(7)	.35	.23
WL(8)	2.97†	1.96
WL(9)	.53	.35
WL(10)	.58	.39
BL(−1)	1.36	1.27
BL(0)	.04	.03
BL(1)	−1.44	−1.34
BL(2)	−.59	−.55
BL(3)	1.20	1.11
BL(4)	.69	.64
BL(5)	−1.06	−.99
BL(6)	1.61	1.50
BL(7)	−.14	−.13
BL(8)	.32	.30
BL(9)	.28	.26
BL(10)	−.53	−.49

[a]As in Table 10.1 the effect of prize fight variables is calculated, controlling for seasonal variables. For reasons of clarity, the coefficients for these seasonal variables have not been displayed in Table 10.2 since the prime purpose of this table is to contrast the impact of "White Loser" and "Black Loser" prize fights. One-tailed *t*-tests are used for the prize fight variables; two-tailed *t*-tests for all other variables.

*Significant at less than .001. **Significant at .006. ***Significant at .019. †Significant at .0251.

per white loser prize fight. Interestingly, the typical white loser prize fight has a larger total impact (9.97) than almost any other variable in the table. Of the 27 "seasonal" variables examined, only one (New Year's Day) has a larger impact on young, white male homicides.[15] This suggests that the impact of a white loser prize fight is not only statistically significant, but practically significant as well. At present, it is not known why this type of prize fight seems to exert so large an effect.

[15]The coefficient for this holiday is 15.75. Although the effect of any given prize fight is large compared with the effect of seasonal variables, the cumulative effect of all prize fights combined is not large compared with the cumulative effect of all seasonal variables combined. This is because there are relatively few prize fights.

TABLE 10.4
Impact of "White Loser" (WL) and "Black Loser" (BL) prize fights on the homicides of young, black male victims, U.S., 1973–1977

Regressand	R^2	$\bar{R}^2$	D.F.	N
HOMICIDES	.452	.436	1772	1825

Regressor	Regression coefficient	*t*-statistic
Intercept	10.59*	20.79
HOMICIDE(1)	.04	1.63
WL(−1)	.83	.48
WL(0)	−1.30	−.75
WL(1)	−1.60	−.93
WL(2)	.19	.11
WL(3)	2.82	1.59
WL(4)	−.82	−.47
WL(5)	−1.19	−.69
WL(6)	−1.66	−.96
WL(7)	2.80	1.62
WL(8)	−.78	−.45
WL(9)	1.62	.94
WL(10)	.59	.34
BL(−1)	−.25	−.21
BL(0)	1.19	.98
BL(1)	−.60	−.49
BL(2)	.18	.15
BL(3)	.67	.54
BL(4)	2.68**	2.19
BL(5)	2.28***	1.86
BL(6)	−.22	−.18
BL(7)	.04	.03
BL(8)	−.76	−.62
BL(9)	1.50	1.23
BL(10)	.30	.25

*Significant at less than .001. **Significant at .014. ***Significant at .032.

See also footnotes to Table 10.3.

Table 10.4 examines the impact of "white loser" and "black loser" prize fights on the homicides of young, *black* male victims. Once again, the evidence supports the hypothesis of victim modeling. Black loser prize fights are followed by significant increases in young, black male homicide deaths. In contrast, white loser prize fights do not trigger significant increases in black male homicides.

Black homicides rise significantly on the fourth and fifth days after black loser fights by a total of 4.96 (= 2.68 + 2.28) per fight.[16] The total impact of the black loser prize fight exceeds the impact of almost all seasonal variables. Only New

[16]The analysis of Table 10.4 is parallel to that of Table 10.3. Once again, statistical theory and the covariance analysis justify treating as independent the 11 *t*-statistics for BL(0), . . . BL(10). We observe two BL(X) coefficients significant at .032 or better. The probability of finding two or more BL(X) coefficients significant at this level in 11 independent trials is .0465. Hence, young, black male homicide deaths increase significantly just after "Black Loser" prize fights.

Year's Day and Thanksgiving trigger larger increases in homicides (the coefficients for these holidays being 8.88 and 8.00, respectively). Evidently, a black loser prize fight has a significant, substantive effect on young, black male homicides.

Precipitation hypothesis. The above evidence is consistent with the notion that prize fights sometimes serve as aggressive models and trigger some U.S. homicides. But perhaps the prize fight merely precipitates a murder that would have occurred anyway, even in the absence of the prize fight.

If a prize fight merely "moves up" a murder so that it occurs a little sooner than it otherwise would have, then the peak in homicides after a prize fight should be followed by a *dip* in homicides soon after. An examination of the three-week period following the prize fight reveals no significant dip in homicides. None of the negative coefficients for PFIGHT(1), PFIGHT(2), . . . , PFIGHT(21) is significant, even at the .10 level. Hence, the precipitation hypothesis seems to be implausible.

Gambling hypothesis. Perhaps the prize fight provokes no aggressive modeling whatsoever. It merely triggers an increase in gambling, which in turn provokes anger, fighting, and murder. If this explanation is correct, then homicides should rise not only after prize fights but also after other occasions that provoke a great deal of gambling. In the United States, the Super Bowl probably provokes more gambling than any other single event. Yet homicides do *not* rise significantly after these occasions.

One can construct a variable, SUPERBOWL(X), to assess the impact of the Super Bowl on homicides X days later, and one can include this variable in the regression model specified in Table 10.1. The coefficients for SUPERBOWL(X) are listed in Table 10.5. There is some weak evidence that homicides actually *decrease* on the day of the Super Bowl and one day later, and then rise above the expected rate on the third day. Even if one considers these coefficients to be statistically significant (which they are not), it is evident that the Super Bowl is associated with a net drop in homicides rather than a rise. This is not what one would expect if the gambling hypothesis were correct. This hypothesis is also rendered implausible by some of the other evidence presented above: If the gambling hypothesis were true, then it is difficult to see why the traits of the homicide victims should be similar to the traits of the losing boxer.[17]

[17]The evidence presented does not support the notion that the gambling hypothesis is a necessary and sufficient explanation for the rise in homicides after a prize fight. But it remains possible that gambling *in combination with* aggressive modeling is helping to provoke the increase in homicides. One way to test this hypothesis is to examine police case histories of murders occurring three and four days after a prize fight. These case histories would have to be compared with case histories taken from control periods.

TABLE 10.5
Impact of the Superbowl on U.S. homicides, controlling for the effect of seasonal and prize fight variables, U.S., 1973–1977

Regressor	Regression coefficient	*t*-statistic
SUPERBOWL(−1)	2.78	.68
SUPERBOWL(0)	−5.03	−1.22
SUPERBOWL(1)	−6.36	−1.55
SUPERBOWL(2)	2.26	.55
SUPERBOWL(3)	6.00	1.46
SUPERBOWL(4)	−1.41	−.34
SUPERBOWL(5)	1.10	.27
SUPERBOWL(6)	−1.67	−.41
SUPERBOWL(7)	−2.24	−.54
SUPERBOWL(8)	3.61	.87
SUPERBOWL(9)	−4.82	−1.17
SUPERBOWL(10)	2.16	.52

Note: The coefficients for the other regressor variables (i.e., prize fight and seasonal variables) are not displayed.

In sum, we have now assessed four possible explanations for the rise in homicides after a heavyweight prize fight. At present, the best available explanation is that the prize fight provokes some imitative, aggressive behavior, which results in an increase in homicides. The size of this increase will be considered in the next section.

Size of the increase in homicides after prize fights. Column 3 of Table 10.3 gives the amount by which homicides increase on the third day after each prize fight. The sum of the numbers in this column is 125.64, indicating that U.S. homicides rose by this amount on the third day after championship heavyweight prize fights, 1973–1978. The sum of the numbers in column 2 gives the total number of homicides expected on the third day—1008.36. Dividing 125.64 by 1008.36 gives the percentage increase in homicides on the third day—12.46 percent. Thus, whether one considers the percentage increase or the absolute increase, it appears that homicides rise by a nontrivial amount on the third day after a championship heavyweight prize fight.

The rise in homicides on the fourth day is smaller but still not negligible. Employing calculations similar to those in Table 10.2, one can determine that homicides increase by 67.97 on the fourth day. The percentage increase is 6.58 percent (= 67.97/1033.03). For the third and fourth days combined, homicides increase by a little less than 200 (193.61 = 125.64 + 67.97). The percentage increase for the two-day period is 9.48 percent (= 193.61/[1008.36 + 1033.03]).

This paper has presented evidence which suggests that heavyweight prize fights provoke a brief, sharp increase in homicides. Some implications of this evidence will be briefly considered in the final section of this paper.

SUMMARY

Many researchers have claimed that one cannot generalize with confidence from the impact of mass media violence *in the laboratory* to the impact of mass media violence *in the real world.*[18] These critics point out that laboratory experiments have been set in highly artificial contexts. Typically, the sorts of aggression studied in a laboratory (like hitting plastic dolls or inflicting electric shocks) have not been representative of serious, real-life violence, such as murder or rape. In almost all studies, the laboratory subjects have been nursery school children or college students and thus not representative of the U.S. television audience. Typically, the laboratory subject is presented with a brief, violent excerpt of a television program. In contrast, the "real-life" viewer may watch several hours of television at a sitting, and the violence may be interspersed with humor, commercials, and trips to the bathroom. In contrast to the laboratory subject, who watches television alone, the real-life viewer may well be surrounded by family or friends. Their comments may distract from the television or shape the perception of its many messages. For these reasons, it is inappropriate to generalize from the laboratory to the real world.

The above argument appears to be seriously challenged by the evidence provided in this paper. The data presented in this paper indicate that mass media violence *does* provoke aggression in the real world as well as in the laboratory. In contrast to laboratory studies, the present investigation assesses the effect of mass media violence in a natural context. Unlike laboratory studies, the present study examines a type of violence which is of serious concern to policy makers. Finally, the present investigation does not focus exclusively on a mass media audience consisting of college students and nursery school children. The laboratory study, with its great potential for rigor, has always been capable of establishing the internal validity of findings. The present study has helped to establish that these findings have external validity as well.

[18]Comstock (1975:30–40) provides a valuable summary of the debate on this topic. In addition, see Phillips ("Behavioral impact," 1982), who also indicates why it is difficult to generalize from the few field experiments that exist.

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11

Contrast Effects and Judgments of Physical Attractiveness: When Beauty Becomes a Social Problem

Douglas T. Kenrick and Sara E. Gutierres

Three studies were conducted to test the hypothesis that judgments of average females' attractiveness or dating desirability will be adversely affected by exposing judges to extremely attractive prior stimuli (i.e., judgments will show a "contrast effect"). Study 1 was a field study in which male dormitory residents watching a popular television show, whose main characters are three strikingly attractive females, were asked to rate a photo of an average female (described as a potential blind date for another dorm resident). These subjects rated the target female as significantly less attractive than did a comparable control group. Two other studies demonstrated analogous effects in a more controlled laboratory setting. In addition, the third study indicated a direct effect of informational social influence on physical attractiveness judgments. Implications are discussed, with particular attention to mass media impact.

Within the past several years, social psychologists have gathered a wealth of data attesting to the central importance of physical attractiveness in interpersonal interaction (see Berscheid and Walster, 1974, for a review). This variable has been found to have a particularly profound effect in dating situations (e.g., Berscheid, Dion, Walster, and Walster, 1971; Brislin and Lewis, 1968; Byrne, Ervin, and Lamberth, 1970; Stroebe, Insko, Thompson, and Layton, 1971; Walster, Aronson, Abrahams, and Rottman, 1966). Brislin and Lewis (1968), for instance, found a correlation of .89 between perceived physical attractiveness and the "desire to date."

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It has been noted that physical attractiveness has generally been used as an independent variable in social psychological research (Berscheid and Walster, 1974; Gross and Crofton, 1977). As Gross and Crofton (1977) put it, "beauty has been conceptualized as an invariant 'cause' in previous studies" (p. 86). Nevertheless, a number of studies have shown that judgments of attractiveness can be influenced by other information likely to affect interpersonal attraction in general, such as knowledge of a target person's attitudinal similarity (Walster, cited in Berscheid and Walster, 1974), information that she/he possesses positively valued traits (Gross and Crofton, 1977), actual acquaintance with the target person (Cavior, 1970), or association of the target person with a highly attractive other (Meiners and Sheposh, 1977). In addition to these situational variations, judgments of physical attractiveness have been found to vary across cultural and racial groups (e.g., Cross and Cross, 1971; Marshall and Suggs, 1971), although Berscheid and Walster (1974) suggest that modern mass media may soon obscure any such differences in favor of Western standards. These authors also suggest that socialization of romantic preference is accomplished through the mass media and that "few advertisements or popular movies and novels depict mundane levels of physical attractiveness" (p. 167). If the media do influence one's standards of attractiveness, while at the same time suggesting that only highly beautiful or handsome others are appropriate as love objects, one might expect an inverse relationship between exposure to mass media and the extent to which an individual's standards for the attractiveness of a romantic partner are "realistic."

In fact, if one can extrapolate the findings from research into other areas of perceptual judgment, there is reason to be concerned about even the short-term impact of mass media on our judgments of the attractiveness of the more mundane potential romantic partners around us. One consistently reported finding in perceptual judgments is a "contrast" effect; that is, judgments of moderate stimuli in a series are found to be displaced away from extreme or distant stimuli. This effect has been found for judgments of physical dimensions such as weight (e.g., Heintz, 1950; Sherif, Taub, and Hovland, 1958), length of lines (e.g., Krantz and Campbell, 1961); and shape (Helson and Kozaki, 1968); as well as social stimuli such as attitudes (Hovland, Harvey, and Sherif, 1957), pleasantness of facial expressions (Manis, 1971), criminal acts (Pepitone and DiNubile, 1976), and personality impressions (Simpson and Ostrom, 1976). If such effects can be presumed to generalize to judgments of physical attractiveness, prior exposure to highly unattractive individuals would result in an enhanced perception of the attractiveness of an "average" person, with the reverse being true of exposure to very attractive persons. Some additional implications would follow from the existence of such an effect in this realm. In our mass-media–oriented culture, where we are bombarded with highly attractive females, such exposure should produce a rather high "adaptation level" (Helson, 1964), resulting in lowered assessments of the beauty of average "real world" females. Given the particularly high relationship between such judgments and dating desirability, such exposure might also lead to an analogous decrement along this latter dimension as well.

It should be noted that in this case, there are at least two bodies of literature that might lead one to expect that such contrast effects would *not* hold in this realm. First, there is evidence that perceptual contrast will not be induced by stimuli that, although sufficiently distant from the stimulus along the relevant dimension, possess other characteristics that lead subjects to consider them to belong to different "universes of discourse" (Helson, 1971). Brown (1953), for instance, had subjects lift a tray between judgments of a series of weights. Although a similarly heavy anchor weight produced a contrast effect, lifting the tray had no effect on judgments. Similarly, Bevan and Pritchard (1963) found that shape judgments were not affected by grossly deviant or oversized stimuli. If media females are not considered to belong to the same category or "universe of discourse" as real-life females, their beauty might be discounted and fail to influence judgments of nonmedia females. Second, since physical attractiveness has been shown to have the qualities of a reinforcer (e.g., Byrne, Ervin, and Lamberth, 1970; Dion, 1977), and since contextual association with reinforcing events has been shown to enhance the attractiveness of a target person (Clore and Byrne, 1974; Lott and Lott, 1974), exposure to an average female in a context of highly attractive females might be expected to lead to a classical conditioning effect such that the average female would actually come to be judged more positively. Similarly, in a context of highly unattractive females, she should come to be judged more negatively if such a process applied here. Three studies were therefore performed to test the hypothesis that exposure to extreme attractiveness stimuli would produce contrasted judgments of a target person of average attractiveness.

Study 1 was a field study in which subjects were asked to judge a potential blind date for a fellow dormitory resident. One group of subjects was students who were watching the television program "Charlie's Angels" (whose main characters are three beautiful women), whereas controls consisted of residents of the same dormitory (sampled during the same night) who were not watching this show at the time.

STUDY 1

Method

Subjects. Subjects were 81 male dormitory residents at Montana State University. They participated in groups of 1 to 6 on the evening of February 1, 1977.

Procedure. Two male confederates entered a dormitory room during one of two time periods (during the hour preceding "Charlie's Angels," or during the hour at which "Charlie's Angels" was aired).[1] They explained to the students in the room:

[1] All subjects who were watching television during this time slot were tuned to this program, which was quite popular at the time of this study (sixth in the Nielsen ratings).

Confederate A: Listen, could I just interrupt you guys for 30 seconds? We're having a major philosophical dispute here and we need to do an informal survey to resolve the question. You see, we have a friend coming to town this week and we want to fix him up with a date, but we can't decide whether to fix him up with her or not, so we decided to conduct a survey.
Confederate B: You see, I don't think she looks very good.
Confederate A: But I think she looks pretty good. At any rate, we want you to give us your vote on how attractive you think she is. (Confederate A begins to hold up the picture, but it is faced away from subjects so they can't see it).
Confederate B: Right, on a scale of 1 to 7, with 1 being very unattractive, 4 being exactly average, and 7 being beautiful.
Confederate A: (turns over photo) Now, nobody say anything until everyone makes up his own mind, and be honest—give your honest opinion.
Confederate B: Remember, 1 is very unattractive, 4 is right in the middle, and 7 is very attractive.

Confederate A held up the picture, allowed each subject to make a silent judgment, and then had the subjects give their responses. They thanked the subjects, left the room, and immediately recorded the time, condition, and each subject's response independently. Agreement between the two confederates was 100 percent in all instances.

Stimulus photo. The stimulus photo was an 8 cm × 5 cm. black-and-white yearbook snapshot taken from a series of slides presented in earlier research (Kenrick and Gutierres, Note 1). A group of 11 undergraduate males from the population used in the present research had given this photo a mean rating of 4.11 on a scale analogous to that used by the subjects in our field study.

Results

Subjects' data were broken down into four groups. Those watching "Charlie's Angels" constituted the "experimental" group, whereas those watching another television program earlier in the night were designated as control subjects. To control for the possibility that ratings by experimental subjects may have been due simply to their having been made at a later hour, two additional control groups were included. Subjects in these groups were not watching television but were sampled either during the "Charlie's Angels" time slot or during the preceding hour. The data from the group watching "Charlie's Angels" were plotted against the data from all control groups, using a planned orthogonal contrast. It was expected that the "Charlie's Angels" group would show the lowest mean ratings of the target's attractiveness. Although subjects were cooperative in making silent independent judgments, the group mean was used as the unit of analysis in the contrast presented below.

In line with predictions, results indicated relatively lowest ratings by viewers of "Charlie's Angels" ($M = 3.43$, versus 4.00 combined M for the controls),[2] $F(1, 24) = 5.03$, $p < .03$. This contrast accounted for 84 percent of the between-groups variance. A similar test using the individual subject as the unit of analysis resulted in $F(1, 77) = 7.39$, $p < .01$.

Discussion

Results of Study 1 were directly in line with predictions, indicating relatively lowest ratings of an average female by subjects who were observing highly attractive media females. Nevertheless, since subjects were not randomly assigned to conditions, our results are open to several interpretations. First, "Charlie's Angels" viewers may have been more negative in their rating of the target female because of the immediate influence of the beautiful media stimuli. Second, the effect may have been due to the fact that these viewers were more negative because of a chronic tendency to expose themselves to highly attractive females depicted in the media. Both of these possibilities would be consistent with our hypotheses. A third possibility, however, is that some other difference may have existed to make "Charlie's Angels" viewers more generally negative in their judgments of females. Although we can offer no intuitively compelling reasons to assume this to be the case, it seems best to consider our hypothesis confirmation in Study 1 as suggestive evidence only at this point. Study 2 was designed to offer a more direct test of our hypothesis by manipulating exposure to media beauty in subjects randomly assigned to conditions.

STUDY 2

Method

Subjects. Subjects were 48 male undergraduates enrolled in introductory psychology at Montana State University, who participated in small groups of 3 to 5.

Procedure. Subjects arrived for an experiment entitled "personality" and were told

> This is a study of first impression formation. We are interested in determining how much we can tell about a person from only a brief encounter or glance. Many people assume that we can tell quite a bit from a person's face alone. It is assumed that we can tell whether someone is honest or dishonest, sociable or unsociable, et cetera

[2]Five groups of subjects (19 total) were watching "Charlie's Angels." Respective means and number of groups were 4.31 ($n = 3$) for nontelevision watchers sampled during the "Charlie's Angels" time slot and 4.08 ($n = 8$) and 3.82 ($n = 12$) for television watchers and nontelevision watchers, respectively, who were sampled during the previous time slot. A test of the residual effects was not significant.

from their eyes or mouth, for instance. Advertisements, books, and magazines often include a certain type of face in an attempt to present a certain image to the public—of an intelligent scientist, an overworked housewife, a dedicated businessman, a vivacious and happy young model, and so on. As part of the present investigation we've given intensive psychological tests and interviews to a group of students and interviewed several of their friends and acquaintances as well. We've developed a personality profile for each of these persons. You will be seeing only a yearbook photo of one of these people, and your task will be to simply give your honest first impression of what that person is really like. You may not feel that you have enough information to respond to each dimension, but simply take your best guess, since accuracy or inaccuracy of impression formation is what we're interested in.

Following this, subjects were instructed not to communicate with one another verbally or nonverbally, since the experiment necessitated completely independent judgments. Seating was arranged so that observation of other subjects' written responses was not possible. For experimental subjects, a black-and-white slide of an attractive female in a magazine advertisement[3] was turned on during the verbal instructions, and the experimenter pointed to it (as if to give an example) as he mentioned the "young model." Control subjects heard the same instructions but were not exposed to the magazine ad. Following the instructions, all subjects were shown a slide of a female of average attractiveness (the same one used in the first study) and were given a "personality rating" sheet to fill out on her. The rating form contained several bipolar scales (likable–unlikable, reasonable–unreasonable, courteous–rude, selfish–unselfish, warm–cold, sincere–insincere, responsible–irresponsible, beautiful–ugly, kind–cruel). The rating of the target person along the dimension beautiful–ugly constituted the main dependent variable. It was predicted that she would be rated as significantly less beautiful following exposure to the attractive female advertisement.

Results

In line with predictions, ratings of the target person indicated that she was seen as significantly less beautiful by subjects exposed to the advertisement, $F(1, 46) = 7.10$, $p < .01$. Mean ratings were 4.41 for the experimental group and 3.52 for the controls (higher ratings indicate that the target person was seen as relatively *less* beautiful).[4] These results parallel those of the first study and cannot be explained as due to self-selection of subjects.

[3]This slide was an advertisement for Wella Balsam, depicting the popular model Farrah Fawcett-Majors. In pretesting, a group of 21 undergraduates (11 males, 10 females) from the same population used in the study proper gave this slide a mean rating of 6.67 on the 7-point scale analogous to that used in Study 1.

[4]None of the other adjectives showed any effect of the manipulation except "responsible–irresponsible." Subjects exposed to the advertisement saw the target as significantly more responsible. Although these results were not predicted, they may fit with Stephan, Berscheid, and Walster's (1971) finding that under some circumstances, males who judge a female as sexually attractive may also see her as relatively "careless" and "uninhibited."

STUDY 3

The first two studies were concerned with the indirect influence of contextual stimuli on judgments of attractiveness. They demonstrated that exposure to beautiful media females could result in lowered assessments of a female of average attractiveness. A third study was conducted to provide an additional test of the question addressed in the first two studies while also examining the more direct impact of informational social influence (Deutsch and Gerard, 1955) on judgments of attractiveness. We are often exposed to our peers' assessments of members of the opposite sex, and a number of classical social psychological findings suggest that peers' judgments can influence assessments even of "objective" reality (e.g., Asch, 1951; Crutchfield, 1955; Sherif, 1935). Given the interpersonal importance of attractiveness judgments, it is of some interest to determine the applicability of this classical effect in the present realm.

Also of some interest was the question of whether exposure to peer evaluations would influence judgments of persons not directly commented on. That is, would exposure to peer evaluations produce an alteration of *standards* that would generalize to judgments of other persons (not directly evaluated)?

Finally, the third study included female subjects as well as males. Since physical attractiveness judgments have been found to influence interpersonal behaviors between same-sex as well as opposite-sex persons (see Berscheid and Walster, 1974, for a review), it is of interest to determine the applicability of our findings to same-sex assessments.

Method

Subjects. Subjects were 98 undergraduates (49 males, 49 females) enrolled in introductory psychology at Montana State University who participated in groups of 2 to 7. One subject was deleted because of suspicion regarding the male confederates.

Procedure. Subjects arrived for an experiment entitled "pretesting stimuli" and were led to a small room containing several chairs and a slide projector. In half the conditions, two male confederates posed as subjects and always sat together in the back row of two rows of five chairs. The experimenter explained

> As the sign-up sheets indicated, we're interested in having you help us pretest some stimuli for an experiment we'll be running at the start of next semester. What that experiment will involve is seeing how well people judge personality from only a small amount of information. We'll be showing people a photograph including only a person's face and asking them to judge the person's overall personality. We've found in the past, however, that these judgments are often influenced by other irrelevant factors, so we'll be asking you to make an objective judgment of several photographs so we can control for these irrelevant factors next semester. All right, for the first 6 photographs you'll be rating the physical attractiveness of each face.

Prior stimuli. Subjects were then given a sheet containing six 9-point scales labeled 1 (extremely unattractive) and 9 (extremely attractive). They were further instructed to observe each slide carefully for 40 seconds, at which point a blank screen would appear that would signal them to make their judgment. For all conditions Slides 2, 3, and 5 were held constant. These slides showed black-and-white yearbook photographs of females previously judged to be "average" in attractiveness (Kenrick and Gutierres, Note 1). Half the subjects saw highly attractive slides in Slots 1, 4, and 6, whereas the other half saw unattractive slides in these positions. Attractive and unattractive slides were also yearbook photos selected in the same manner as the "average" photos.

Confederate comments. Half of each group heard the confederates make comments about the third and fifth (average) slides. These comments were negative in the groups exposed to the high-attractive series (e.g., "What a dog," nonverbal utterances of displeasure) and positive in the groups exposed to the low-attractive series (e.g., "You can set me up with her," nonverbal utterances of attraction). After Slide 5 had been rated, the experimenter mentioned that any comments might influence the others in the room and asked that the subjects refrain from giving any public responses to the stimuli. (For controls, this request was made before Slide 1 was shown.)

Target person. After Slide 6 had been rated, subjects were told that the next slide would be "evaluated on a completely different dimension" and were handed a sheet that asked them to check one of seven sentences ranging from "I would find this person extremely desirable as a date" to "I would find this person extremely undesirable as a date." Female subjects were instructed to evaluate the female as a potential date for a male friend. This final photo was selected in the same manner as the other "average" slides (2, 3, and 5) and was, like them, held constant for all subjects.[5]

Subjects were then fully debriefed and were probed for suspicion. All reported having clearly heard the confederates' comments (when they were made), and most generally reported in informal discussion that they found them obnoxious and did not feel they were influenced. Nevertheless, as might be expected, other males had in several cases spontaneously joined in verbal agreement with the confederates' comments during the experimental session. Data were therefore treated using the group as the unit of analysis.

[5]Although this photo was not rated on physical attractiveness within the context of the present study (as the other average photos used were) we did have it rated on a 7-point scale like that used in the first two studies (1 = extremely unattractive, 4 = average, 7 = extremely attractive) by 66 undergraduates (34 males, 32 females) from the same subject pool during the following academic year. The mean rating obtained was 4.42 for this group. Note that the polarity here is reversed from that of the 7-point "dating desirability" scale used in Study 3, and this should be read as 3.58 if one wishes to make direct comparison. We would suggest that the reader who chooses to do so should keep in mind that there may well be some slippage in making such a conversion.

Predictions

1. It was expected that exposure to attractive prior stimuli would lead to relatively decreased ratings of the target person's dating desirability.
2. It was further expected that subjects' ratings of the physical attractiveness of Slides 3 and 5 (the "average" slides for which comments were made) would be affected in the direction of the confederates' comments. Since this would have resulted in decreased ratings of these two slides in the attractive condition (since negative comments were made here) and increased ratings in the unattractive condition (since positive comments were made here), this would have shown up as an interaction between the confederate comment factor and the attractiveness factor.
3. In addition, we were interested in seeing whether confederates' comments would indirectly enhance the influence of the prior stimuli on ratings of the final target person (by further heightening standards in the attractive condition, and vice versa). This would also have shown up as an interaction effect, as indicated in Prediction 2 above.

Results

Manipulation check. Comparisons between attractive and unattractive slides in Positions 1, 4, and 6 yielded differences significant beyond the .001 level in each instance. Mean ratings of the three attractive slides were 7.00, 7.74, and 7.82 for the attractive and 4.24, 3.26 and 2.36 for the unattractive series, respectively. Discounting the effects of the independent variables (discussed below), the overall mean ratings for Slides 2, 3, and 5 (constant slides in series) were 5.18, 5.00, and 5.51, respectively.

Sex of subject. Prior to the analysis using the whole group mean as the unit of analysis, an analysis dividing each group into male and female subjects was performed. This analysis indicated that sex of subject yielded no main effects or interactions on any of the dependent variables, except for ratings of Slide 5, for which a main effect of subject sex was obtained, $F\,(1, 29) = 8.26$, $p < .01$. Females rated this slide as more attractive ($M = 5.91$) than did males ($M = 5.19$).

Main analyses. Ratings of the target person's "dating desirability" showed the predicted main effect of prior stimuli (P), $F\,(1, 14) = 15.01$, $p < .002$. As expected from our earlier results, subjects exposed to the attractive prior slides gave significantly lowered ratings of the target person (see Table 11.1). As indicated in the Predictions section above, a significant interaction of prior stimuli and confederates' comments (C) would have indicated that the comments further enhanced the standards set by the prior stimuli. The F for the interaction term was less than 1, thus failing to support this suggestion. The C "main effect" was also nonsignificant, $F\,(1, 14) = 1.51$.

Prior stimuli and confederate comments. On the slides for which confederates made direct comments, ratings were, as predicted, lowered in the attractive

TABLE 11.1
Influence of stimulus attractiveness and confederate comments on ratings of prior stimuli and target person, Study 3

	Attractiveness of prior stimuli			
	Attractive		Unattractive	
Item rated	Confederate comment	Control	Confederate comment	Control
Slide 3[a]	4.63	5.08	6.04	5.00
Slide 5[a]	4.30	5.02	6.96	6.08
Target person[b]	4.33	4.16	3.34	2.78

[a]Higher ratings indicate *more* positive ratings of female's beauty on a 9-point scale.
[b]Higher ratings indicate *less* desirability as a date on a 7-point scale.

condition (when negative comments were made) and enhanced in the unattractive condition (when positive comments were made). This showed up as a significant P × C interaction on Slide 5, $F(1, 14) = 4.66$, $p < .05$, and a marginally significant effect on Slide 3, $F(1, 14) = 4.41$, $p < .06$ (Table 11.1). In addition, there was a significant main effect of prior stimuli on Slide 5, $F(1, 14) = 25.50$, $p < .001$, as well as a similar trend on Slide 3, $F(1, 14) = 3.91$, $p < .07$, indicating relatively lower ratings in the context of highly attractive stimuli.[6] Since confederates' comments should have canceled out for the C effect, *F*s were, not surprisingly, less than 1 for ratings of both Slide 3 and Slide 5.

GENERAL DISCUSSION

The results of Study 3 were consistent with those of the first two studies in supporting the existence of a contrast effect phenomenon for judgments of physical attractiveness. This effect occurred despite the fact that the attractive stimuli in the first two studies were drawn from a different "universe of discourse" than that from which the target person was drawn. To the extent that the beauty of media females may have been "discounted" due to this factor, it was not sufficient to remove the adverse contrast effect. Similarly, the contrary prediction based on a simple application of classical conditioning principles (i.e., that reinforcement value of attractive photos would generalize to an average target photo in the same series) was not borne out. This result is consistent with other findings suggesting that simple generalization of affective reactions to attraction objects is often overruled by other (e.g., cognitive) factors (Kenrick and Johnson, 1979).

[6]Ratings of Slide 2 were also significantly more negative when it followed an attractive slide in Position 1, $F(1, 14) = 9.23$, $p < .01$.

Possible Implications

Media impact (the "Farrah factor").[7] The present results support the suggestion that our initial impressions of potential romantic partners will be adversely affected if we happen to have been recently exposed to posters, magazines, television, or movies showing highly attractive individuals (or if such stimuli are concurrently present). Kenrick and Gutierres (Note 1) found analogous results to those obtained here, using stimuli randomly chosen from advertisements in best-selling magazines. Their results indicated, not surprisingly, that media females are indeed selected from a highly skewed distribution with regard to physical attractiveness.

Whether our obtained effects are long lasting cannot be determined from the present series of studies. Even if such effects are very short-lived, however, they could still be of some consequence, influencing the desirability of females who happen to meet a male immediately following or during exposure to such media. Of some interest in this regard is a recent study by Snyder, Tanke, and Berscheid (1977), which suggests that initial judgments of a target's attractiveness may function as a self-fulfilling prophecy. In the Snyder et al. study, targets who were perceived to be unattractive actually came to behave in a less friendly and likable manner than targets who were regarded as attractive. Further, there is other research showing that the judged physical attractiveness of a computer date actually determines the likelihood of seeking further interaction with that person (Walster et al., 1966).

Thus, let us imagine a scenario involving a college-age male who, like the subjects in our first study, is engrossed in an episode of a television show containing unusually beautiful females in the central roles (the examples are not hard to come by, especially given the recent conscious and concerted effort of TV network producers to place very highly attractive women in starring roles). He is briefly introduced to a neighbor who happens to be a female of average physical attractiveness. Our data suggest that his immediate assessment of her attractiveness and dating desirability will be lower than might otherwise be the case. Based on the findings of Dion (1977), he might be expected to subsequently reduce his visual attention to her (thus retarding any return of his "adaptation level" to mundane levels). He might also act in such a manner as to inhibit demonstrations of friendliness on her part, following Snyder et al. (1977), and also be less likely to seek to interact with her in the future, in line with the findings of Walster et al. (1966).

Individual differences in history of exposure. Research on perceptual judgment in other areas suggests that the judgment of a stimulus is determined by both the immediate stimulus context and by a "pooled" estimate of the judge's past experience with stimuli in the same realm of discourse (Helson, 1964, 1971).

[7]This term was coined by the editors of *Human Behavior* magazine (February 1979 issue) for the effect we have been investigating in this series of studies.

Given the male college student's vast history of exposure to female facial stimuli, the manipulations in Studies 2 and 3 could only be seen as having a transitory impact, likely to be erased by relatively short exposure to real-world females. Nevertheless, given a tendency to selectively attend to and actively seek visual exposure to highly beautiful females (Dion, 1977), our results suggest the possibility that "chronic" standards for physical attractiveness may be somewhat inflated, particularly among individuals who are exposed to relatively more mass media (whose pooled estimate of a facial stimulus is based on a highly skewed and "nonrepresentative" sample). Note in this regard that there is evidence that the average adolescent in this society has spent more time watching television than in school (Gerbner and Gross, 1976).

We have focused our discussion thus far on the effects of media beauty, but it should also be pointed out that our results have potential implications for other realms as well. For instance, some individuals may be chronically exposed to unusually high levels of attractiveness by virtue of their occupation (e.g., airline pilots, bartenders in Playboy clubs, professors at UCLA, and so on). If these individuals are themselves unattractive, the effects of such exposure may be particularly adverse, leading to the adoption of unrealistically high standards and consequent dissatisfaction with those females actually available to them and likely to be interested in them (at least according to the "matching" hypothesis; Berscheid and Walster, 1974).[8]

Cognitive influences. Social psychologists have recently shown renewed interest in studying subjects' phenomenological reconstructions of the social situations they are faced with. Social behavior seems to be influenced not simply by "objective" environmental stimuli but also by the subjects' tendency to interpret these stimuli, selectively attend to them, and selectively recall them (e.g., Berkowitz, 1978; Snyder and Uranowitz, 1978). When the present research is considered in the light of such findings, some additional researchable implications unfold. Given the rewarding nature of attractiveness, individuals may well selectively notice the atypically attractive persons in their environments and selectively recall them (perhaps even actively generating images of such persons in their absence). Thus, an individual whose everyday activities expose him or her to a "representative" sample of opposite-sex persons may nevertheless *construct* a "biased" adaptation level.

It should be noted at this point that we have not elucidated the cognitive mediators underlying our obtained effect. Although such a question is not relevant for the social implications we have touched on, it would be of some theoretical interest to investigate the cognitive processes responsible for our effect. It seems unlikely, for instance, that contrast effects obtained in this realm (and in other realms of social judgment) are due to "receptor fatigue," as the analogous effects obtained with purely sensory phenomena might be (Helson, 1964), unless one posits a relatively

[8]Our thanks to an anonymous reviewer for this suggestion.

central and higher order mechanism for such judgments. On the other hand, an explanation in terms of "scale usage" effects (e.g., Anderson, 1975; Parducci, 1965) cannot deal with the results of Studies 1 and 2 and must be stretched a great deal to account for the results of Study 3.

Influence of Peer Evaluations on Attractiveness Judgments

In addition to demonstrating indirect contextual influences on judgments of attractiveness, the results of the third study indicated that information regarding peer judgments will influence evaluations of physical beauty. Subjects' judgments tended to conform to evaluative comments expressed by confederates in this study. Since subjects' judgments were private in this case, these results would seem to be an instance of what Deutsch and Gerard (1955) have referred to as "informational" as opposed to "normative" social influence. This latter effect would also seem to have clear "real-life" analogs. It has been noted that initial preinteraction encounters are strongly influenced by visual characteristics (Levinger, 1974), and very often such first visual encounters are accompanied by friends' explicit evaluations of the target individual's attractiveness. In fact, students can often be observed actively seeking peer evaluations of the attractiveness of potential romantic partners with whom relationships have progressed to what Levinger (1974) has termed the level of "surface contact."

The results of Study 3 showed only a direct effect of confederates' comments on physical attractiveness judgments, whereas general "standards" for dating desirability were not influenced by these comments. It is possible that long-term exposure to peers with either very high or very low "standards" would result in a more general effect, but the present methodology (in which only two females were evaluated by the peer models) does not allow for any such determination.

Influence of Media Depictions of Males on Females' Judgment

The present series of studies used only female target persons. Although physical attractiveness has generally been found to be more important for females than for males (e.g., Berscheid et al., 1971; Efran, 1974; Stroebe et al., 1971; Walster et al., 1966), attractiveness has not been found to be insignificant for men, by any means. These same studies, for instance, have shown physical attractiveness to be significant in importance for males as well, and Berscheid and Walster (1974) point out that although females consistently *report* physical attractiveness to be less important in their judgments of males, the findings with regard to *behavioral* measures are sometimes contradictory (e.g., Byrne et al., 1970). It seems likely that the present findings would have similar implications for judgments of males, although this remains to be empirically verified.

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