

Violence, Aggression, and Ethics: The Link Between Exposure to Human Violence and Unethical Behavior

Joshua R. Gubler¹ · Skye Herrick² · Richard A. Price³ · David A. Wood⁴

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Abstract Can exposure to media portrayals of human violence impact an individual’s ethical decision making at work? Ethical business failures can result in enormous financial losses to individuals, businesses, and society. We study how exposure to human violence—especially through media—can cause individuals to make less ethical decisions. We present three experiments, each showing a causal link between exposure to human violence and unethical business behavior, and show this relationship is mediated by an increase in individual hostility levels as a result of exposure to violence. Using observational data, we then provide evidence suggesting that this relationship extends beyond the context of our experiments, showing that companies headquartered in locations marked by greater human violence are more likely to fraudulently misstate their financial statements and exhibit more aggressive financial reporting. Combined, our results suggest that exposure to human violence has significant and real effects on an individual’s ethical decision making.

Keywords Violence · Aggression · Ethics · Hostility · Dehumanization

In the Association of Certified Fraud Examiners (ACFE) (2014) *Report to the Nations*, respondents estimate that the typical organization loses five percent of its revenues to fraud annually. When applied to the gross world product, this translates to annual losses of nearly \$3.7 trillion (ACFE 2014). Clearly, understanding ways to reduce fraud would be immensely beneficial to society.

Prior research abounds both on factors that motivate employees to engage in unethical behavior and how such behavior might best be combated (Kish-Gephart et al. 2010; Martin and Cullen 2006). However, this research does not extend to the larger social environment in which employees function. Motivated in part by prior research demonstrating that individuals living in contexts marked by high levels of violence (e.g., war-torn countries) are less averse to “questionable market practices,” as well as work showing employees are less ethical when they hear violent rhetoric from supervisors (Rawwas et al. 1994; Gubler et al. 2015), we explore the impact of exposure to human violence on individual propensity to engage in unethical behavior at work. We ask: can exposure to human violence impact an individual’s ethical decision making at work?

Research suggests that exposure to human violence, particularly through media, raises individual aggression levels, leading to negative outcomes for its viewers in a host of areas (Anderson and Bushman 2001, 2002). However, to our knowledge, no research has explored the effect of exposure to human violence on an individual’s willingness to be unethical in a business setting. This is surprising, as two key outcomes of violent media identified in the literature, (1) dehumanization of others and (2)

✉ David A. Wood
davidwood@byu.edu

¹ Department of Political Science, Brigham Young University, 764 Kimball Tower, Provo, UT 84602, USA

² Google Inc., 1600 Amphitheatre Parkway, Mountain View, CA 94043, USA

³ School of Accountancy, Huntsman School of Business, Utah State University, 3540 Old Main Hill, Logan, UT 84322, USA

⁴ School of Accountancy, Marriott School of Management, Brigham Young University, 529 TNRB, Provo, UT 84602, USA

increased hostility levels towards others, have both been shown in other contexts as motivators for unethical behavior towards others (Bandura 1999; Gabbiadini et al. 2014).

As recent meta-analytic research indicates, a growing body of research suggests that exposure to human violence encourages those so exposed to dehumanize others with whom they interact (Greitemeyer and Mügge 2014). For example, research by Greitemeyer and McLatchie (2011) shows that playing violent video games leads to the dehumanization of others, which in turn leads to an increase in aggression towards others. Building on research by Bandura (1999), we hypothesize that the “moral disengagement” from others that results after they are dehumanized should facilitate not only increased physical aggression towards others, as has been shown, but also a willingness to lie and cheat others, i.e., to engage in unethical behavior. Inhibitions preventing unethical behavior towards others naturally crumble when the “other” is seen as little more than an object, or animal, instead of fully human.

Another body of research provides strong evidence that exposure to human violence increases physical and verbal hostility towards others. This is perhaps most apparent in research on the effects of violent video games on hostility towards others (Anderson et al. 2010; Huesmann 2010). However, increased hostility towards others has also been shown to be a result of exposure to violent movies (Bushman and Huesmann 2006), and a large literature highlights the effects of exposure to such violence in a military setting on hostility levels once military combatants return home (Galloway et al. 2012). Although the context of these studies differs, the proposed mechanism linking exposure to human violence and hostility towards others is the same: exposure to violence increases aggressive thoughts towards and expectations of aggression from others (Bushman and Anderson 2002). We hypothesize that increased hostility levels as a result of exposure to human violence should be manifest not only in increased willingness to physically or verbally harm others, but also in a willingness to economically harm others: to lie and cheat to get what one wants.

Thus, we expect that exposure to human violence will cause individuals to be less ethical through increased dehumanization of others and/or increased hostility towards others. In what follows, we present results from four studies that confirm this expectation—three experimental studies using online labor markets and an observational study on violence and corporate fraud. Using multiple experiments and different methods triangulates our findings, providing strong evidence for the patterns that emerge.

Experiment 1: Method

Participants

Given expected effect sizes from previous research, we recruited 250 participants for the first experiment; 13 of these submitted their work but did not respond to any questions and thus were excluded from analysis. All participants were from the United States and over the age of 18. In this and the following experiments, we did not analyze the data until the entire sample was collected.

We solicited participants from Amazon’s online labor market Mechanical Turk (MTurk). MTurk was an ideal platform for this, and our subsequent experiments, for several reasons. First, it is a real-world setting for the experiments: like other employers on MTurk, we hired participants to complete a task. As we will describe below, this task was quite similar to common tasks on MTurk. Thus participants did not know they were participating in an experiment. Second, research suggests that MTurk participants respond similarly to large random samples of U.S. participants in traditional experiments, and replication studies using MTurk have shown similar results to experiments using other types of populations (Berinsky et al. 2012; Paolacci and Chandler 2014). Especially important for our studies is prior research that shows that MTurk samples are not more likely to cheat than college samples and are truthful in self-report information, although they still do respond to incentives to cheat (Suri et al. 2011; Goodman et al. 2013).

Task

Participants reviewed 10 sentences as part of a grammar-editing task. The task was described as editing individual sentences in a random order from an author. If the sentences were correct, they were to simply mark a box labeled “correct.” If they were incorrect, they were to type the sentence correctly and then describe the error. They were informed that they would be paid for each submitted sentence, regardless of whether their edits were correct or not. The incentive to cheat was clear: mark all sentences “correct,” and you earned money more quickly.

Using a 1×2 , between subjects design, we manipulated seven of the ten sentences: some participants viewed seven violently worded sentences with relatively simple mistakes, while others viewed seven equivalent sentences that did not contain violence. Two of the seven sentences are included below as illustrations (these sentences contain the grammatical errors).

1. On Tuesday I’m leaving for afghanistan to kill members of the opposition. [or] On Tuesday I’m

leaving for Washington to climb Mt. Rainier with my friends.

2. She picked up her gun, shot the civilians, and walks out the door. [or] She picked up her purse, winked at the waiter, and walks out the door.

After completing the editing task, participants were asked to evaluate the author on several criteria including general questions about writing quality and also a measure of dehumanization. Participants then submitted the task and were paid.

The dependent variable was whether participants checked the “this sentence is correct” box for a sentence that contained a grammatical error. Given random assignment between conditions, we expected similar editing ability so that if one condition showed more individuals marking incorrect sentences as correct, this would suggest individuals exerted less effort in the task for the same pay. Thus, this measure was intended to capture shirking behavior—agreeing to perform a task and then neglecting the task or performing it below a reasonable expectation—which is often considered unethical (Mitnick 1992; Gunn 2005).

Building on the theory described earlier, we tested whether dehumanization mediates the effect of these sentences on unethical behavior. To test this, we employed the commonly used inhumanization scale developed by Leyens et al. (2001), shown in previous studies to have a high degree of construct validity (Leyens et al. 2003; Maria-Paola et al. 2002). This measure captured the degree to which an individual ascribed secondary emotions to another, based on the assumption that humans, on average, ascribe complex secondary emotions (e.g., “remorse”, “guilt”, “euphoria”, etc.) to humans alone, assuming that animals can feel just primary emotions (e.g., “rage”, “fear”, etc.).¹

Results

Figure 1 shows that we observed the hypothesized main effect: 24 % more of the participants in the violent condition ($M = 0.87$, 95 % CI 0.82, 0.93) than in the control condition ($M = 0.63$, CI 0.54, 0.72) marked at least one incorrect sentence as correct rather than taking time to carefully complete the task ($\chi^2(1) = 18.65$, $p < 0.001$). Additionally, participants in the violent condition marked a greater number of the seven incorrect sentences as correct

¹ Our 8-item measure included a list of four “secondary emotions” (2 positive and 2 negative) and four “primary emotions” (2 positive and 2 negative) and asked subjects to rate on a 7-point scale (7 = very much, 1 = not at all) the capacity of people in general to feel each emotion. We created an index of “secondary emotions” by averaging individual responses to the four emotions. Individuals with low scores on this scale are viewed as seeing others as less human.

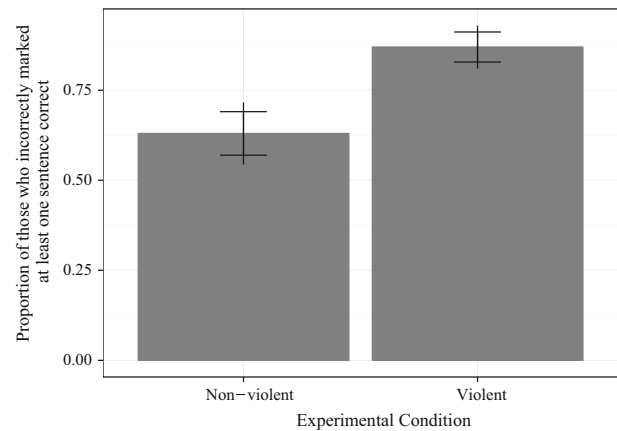


Fig. 1 Proportion of those who incorrectly marked at least one sentence incorrect: Experiment 1. Bars represent 95 % confidence intervals. Lines on bars represent 83 % confidence intervals, which show the point at which bars cannot overlap if the proportion difference is significant at the 95 % level

($M = 1.82$, 95 % CI 1.57, 2.07) than in the control condition ($M = 1.21$, 95 % CI 0.99, 1.43, $t(235) = -3.61$, $p < 0.001$). There was no difference between conditions in editing quality for the three sentences that contained no violence and no errors between conditions (violent condition $M = 2.44$, 95 % CI 2.30, 2.58; nonviolent condition $M = 2.47$, 95 % CI 2.35, 2.59; $t(235) = 0.32$, $p > 0.25$). A formal test using the Hayes (2013) PROCESS macro to explore whether dehumanization mediated the effect suggests no mediation (indirect effect = 0.03, $z = 0.64$, $p > 0.25$). We did not measure the potential mediating effect of hostility in this experiment.

Experiment 2: Method

Participants

Given the larger-than-expected effect sizes from study 1, we solicited just 150 participants from MTurk for experiment 2. Seven of these participants submitted their work but did not respond to any questions and thus are excluded from analysis. Participants in the first experiment were excluded from being able to participate, and as in Experiment 1, all participants were from the United States and over the age of 18.

Task

The task was identical to experiment 1 with one exception: we provided more information about the author of the sentences they were asked to edit. We hypothesized that one possible reason for the lack of evidence for the mediating role of dehumanization in the first experiment

was that participants may not have viewed the sentence author as “human.” To humanize the author of the sentences for participants before performing the editing task, participants were told, “*You have received sentences from: Casey Long. He is using this service because: I recently received my GED and am using this service to help me learn how to write better so I can attend our community college*” (emphasis as in the experiment). Other than this change, the two experiments were the same.

Results

Figure 2 shows a replication of experiment 1; we again observed the hypothesized main effect, where 17 % more of the participants in the violent condition ($M = 0.93$, 95 % CI 0.87, 0.99) than in the control condition ($M = 0.76$, 95 % CI 0.65, 0.86) marked at least one incorrect sentence as correct rather than taking time to carefully complete the task ($\chi^2(1) = 8.51$, $p = 0.002$). Additionally, participants in the violent condition marked a greater number of incorrect sentences as correct ($M = 1.80$, 95 % CI 1.55, 2.05) than in the control condition ($M = 1.52$, 95 % CI 1.16, 1.89, $t(141) = -1.30$, $p = 0.099$). Again, there was no difference between conditions in editing quality for the nonviolently worded sentences (violent condition $M = 2.30$, 95 % CI 2.12, 2.49; control condition $M = 2.37$, 95 % CI 2.18, 2.56; $t(141) = 0.53$, $p > 0.25$). Unexpectedly, a formal test following the same mediation methodology again showed that dehumanization did not mediate the relationship between violence and shirking (indirect effect = -0.11 , $z = -0.97$, $p > 0.25$). This second null finding motivated us to measure state hostility in the next experiment.

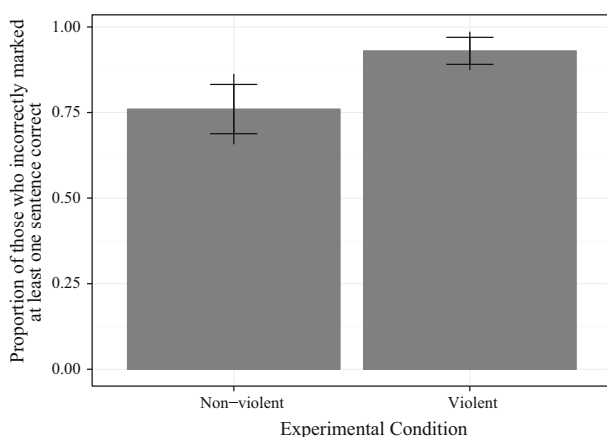


Fig. 2 Proportion of those who incorrectly marked at least one sentence incorrect: Experiment 2. Bars represent 95 % confidence intervals. Lines on bars represent 83 % confidence intervals, which show the point at which bars cannot overlap if the proportion difference is significant at the 95 % level

Experiment 3: Method

We conducted a third experiment for three reasons: (1) to rule out the possibility that Experiment 1 and 2 results are just an aversion effect (i.e., individuals were put off by the violence in the sentences rather than motivated by it to cheat), (2) to test additional possible mediators, and (3) to present individuals with more direct exposure to human violence.

Participants

We solicited 603 different participants from MTurk. Once again, participants were from the United States and over the age of 18. We targeted 600 participants (and received an extra three before the experiment was closed) due to the additional conditions in this experiment, as well as past research suggesting small effects from video treatments. We sought to ensure sufficient power to detect results.

Task

In this experiment, we hired participants to watch and evaluate movie clips, a common task on MTurk. Participants were randomly assigned to see two short videos (less than five minutes combined) that were both (1) violent (Violent Video condition), (2) nonviolent but meant to be intense or arousing (Intense Video condition: designed to help us separate hostility from arousal effects), or (3) nonviolent and not arousing (Boring Video condition). The clips came from popular movies: the violent clips came from *The Bourne Identity* and *Shoot ‘Em Up*, the nonviolent but intense clips from *Unstoppable* and *2012*, and the boring clips came from *The Straight Story* and *How to Make an American Quilt*. After watching the video clips, participants answered questions measuring the two potential mediating variables we suggested earlier—hostility and dehumanization—as well as a third potential mediator: arousal. We included a measure of arousal to explore an alternative theory: that violent media might lead to an increase in unethical behavior simply because it increases physical arousal. Previous research suggests that exposure to violence can lead to increased physical arousal and can lead to an impulsivity of behavior (Anderson and Bushman 2001)—in this case a lapse in ethical judgment.

After answering these questions, participants in all three experimental conditions viewed another movie clip, 10 min in length, intended to be boring and long enough to generate an incentive for participants to skip watching the movie. The movie clip came from the movie *Blue* by Derek Jarman. The clip consisted of a blue background for the entire 10 min and a monotone voice speaking with

subtitles. The directions to the study concluded with the following: “We are paying you to watch each movie clip *in its entirety* before advancing to the next screen” (emphasis as in original). At the end of the study, we asked participants the following question regarding whether they had earned their pay: “Did you watch all of the movie clips in their entirety?” As the study was done online, we were able to capture how long participants viewed each video. To measure participant lying, we simply recorded those who did not watch all of the last video but said that they did.

To test for mediation, we asked participants to respond to a perceived arousal scale (Anderson et al. 1995), a state hostility scale (Anderson et al. 1996), and to the same dehumanization scale described in Experiment 1.

Results

Figure 3 presents the results with three key findings. First, exposure to violence via the media did not affect the ethics of our female participants (proportion of females lying in the violent condition = 0.28, 95 % CI 0.20, 0.37; boring condition = 0.35, 95 % CI 0.25, 0.44; and intense condition = 0.32, 95 % CI 0.23, 0.41; univariate χ^2 tests show no statistically significant differences, the same holds true for logistic regression comparison; all $p > 0.25$). Recall that in experiments 1 and, 2, females reacted to violent sentences to the same degree as males. While their different response in this experiment result was unexpected, it is consistent with some previous research.²

Second, exposure to human violence in these movie clips did dramatically affect the proportion of males lying (violent condition = 0.54, 95 % CI 0.43, 0.65; boring condition = 0.44, 95 % CI 0.34, 0.54; and intense condition = 0.37, 95 % CI 0.27, 0.47). Males in the violent video condition lied more than in the boring condition ($\chi^2(1) = 1.79$, $p = 0.091$) and the intense condition ($\chi^2(1) = 4.97$, $p = 0.013$) and the boring and intense conditions did not differ from each other ($\chi^2(1) = 0.90$, $p > 0.25$). A multivariate test shows that males in the violent video condition lied more than all other conditions ($\chi^2(1) = 10.95$, $p < 0.001$). Thus, the evidence provides support for our main hypothesis for males.

As shown in Table 1, we also found evidence for the partial mediating role of hostility: those for whom the violent movies increased hostility levels were those most likely to lie for pay. We found no evidence for dehumanization or arousal as mediators.³

² Although previous research has not explored the effects of exposure to violence on ethics for females (or males), it does indicate that females exhibit revulsion to depictions of violence (Malamuth and Check 1981).

³ Three different mediation tests using PROCESS by Hayes (2013) are used to test whether dehumanization, perceived arousal, or state

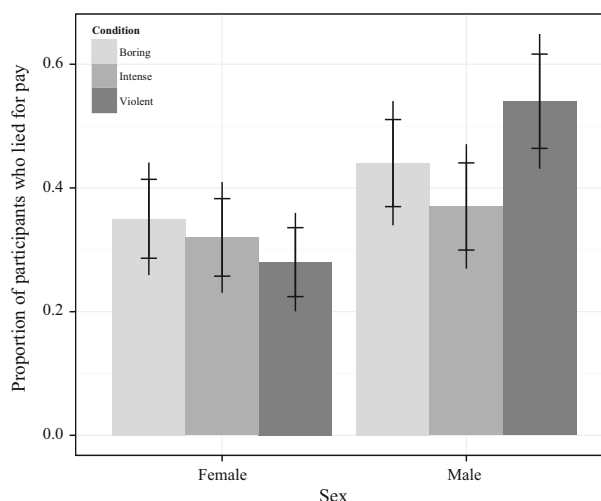


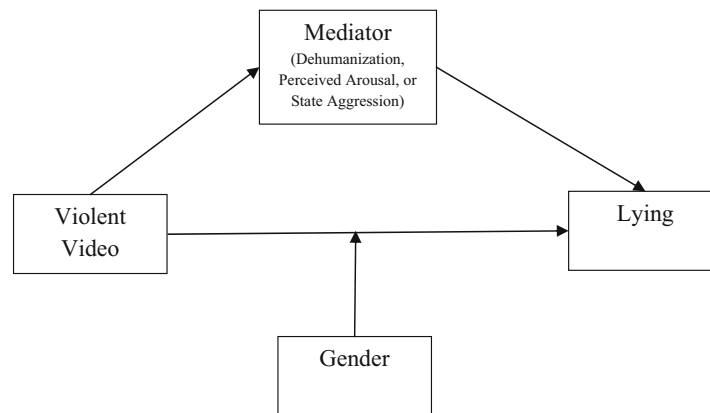
Fig. 3 Proportion of participants who lied for pay: Experiment 3. Bars represent 95 % confidence intervals. Lines on bars represent 83 % confidence intervals, which show the point at which bars cannot overlap if the proportion difference is significant at the 95 % level

Correlational Study: Method

These three experiments provide causal evidence that exposure to human violence can motivate less ethical decision making in a business setting. The experimental evidence is more compelling when we recall that it comes from a real-world (field) setting, Amazon’s online labor market, rather than from a lab. Even so, questions about the generalizability of our results still apply. As such, we supplement this causal evidence with correlational data and careful modeling showing that companies headquartered in locations marked by greater human violence are more likely to fraudulently misstate their financial statements and exhibit more aggressive financial reporting. Although such evidence cannot be interpreted causally, the fact that it is consistent with the observable implications of our experimental findings is yet another support for the argument.

Footnote 3 continued

aggression mediate the relationship between watching violent videos and lying. Given that results differed by gender, Gender is included as a moderator between violent videos and lying (supplementary tests using gender as a moderator for the relation between violent video and the mediator or the mediator and lying showed that both genders responded relatively similarly to the violent video). The arousing and boring video are collapsed into one condition and compared against the violent video. Panel B reports indirect effect tests (direct tests for males showed significant relations between violent video in all tests, with p value < 0.05 ; thus state aggression is a partial and not full mediator of the relation).

Table 1 Experiment 3 mediation results**Panel A:** Mediation Model for Testing**Panel B:** Mediation Testing Results for Indirect Effects

Possible Mediator	Effect	Effect	Z	P-value
Dehumanization	Indirect Effect of Violent Video on Lying	<0.01	0.19	0.85
Perceived Arousal	Indirect Effect of Violent Video on Lying	-0.04	-1.37	0.17
State Aggression	Indirect Effect of Violent Video on Lying	0.11	2.23	0.01

Data Sources

To perform our tests, we gathered data from several sources. We first gathered company financial statement data from the financial database Compustat and market security data (price and number of shares outstanding) from the CRSP database. We then gathered data for our dependent variables, proxies for fraud and aggressive financial reporting. We analyzed data from 1995 to 2008 because the database of SEC enforcement actions covers this time period.

We assumed that business executives living in areas of greater crime were exposed to more human violence (through local news media channels and/or direct exposure) than those living in areas with minimal crime. Thus, we proxied for exposure to human violence using violent crime data from the *Crime in the United States* report provided yearly by the FBI starting in 1995. This report included rates of violent and property crime by city, state, and metropolitan statistical area (MSA). We merged all these data sources to perform the analyses.

Empirical Model

We estimated the following regression model to explore the degree to which violent crime was associated with

financial reporting irregularities (IRREG) between 1995 and 2008.

$$\begin{aligned} \text{IRREG} = & \alpha + \beta_1 \text{Violence} + \beta_2 \text{Property} + \beta_3 \text{Size} \\ & + \beta_4 \text{ROA} + \beta_5 \text{LEV} + \beta_6 \text{Big4} + \beta_7 \text{BM} \\ & + \beta_8 \text{Loss} + \beta_9 \text{Op Risk} + \varepsilon \end{aligned}$$

We employed three measures to proxy for financial reporting irregularities (*IRREG*). First, we used accounting-related SEC enforcement actions (accounting and auditing enforcement release, AAER) for violations of the Foreign Corrupt Practices Act from the Karpoff et al. (2008a, b) database. SEC enforcement actions are generally interpreted as clear evidence of financial reporting irregularities (Dechow et al. 1995)—that is, the senior management of a company clearly and materially violated financial reporting rules. For fiscal years in which an enforcement action by the SEC resulted, the variable AAER takes a value of one, zero otherwise. Because this dependent variable was an indicator variable, we estimated a logistic regression model; the remaining dependent variables were continuous so we used ordinary least squares for estimating those models.

For our second dependent variable, we used Dechow and Dichev's (2002) accruals quality measure (AQ). This commonly used measure is estimated as the residual of a regression of accruals (the non-cash component of a firm's

earnings) on past, current, and future operating cash flows (the cash component of a firm's earnings):

$$\Delta WC_{it} = \beta_0 + \beta_1 CFO_{it-1} + \beta_2 CFO_{it} + \beta_3 CFO_{it+1} + \varepsilon_{it},$$

where ΔWC is the Change in working capital from year $t - 1$ to year t , deflated by average total assets, CFO is the Cash flows from operations for year $t - 1$, t , or $t + 1$, deflated by average total assets.

The intuition behind this model is that a firm with high-quality financial reporting should have a good mapping between accruals and cash flow (i.e., a low residual).⁴ Firms for which cash flows exhibit low explanatory power (high residuals) were interpreted as having poor financial reporting and exercising undue discretion, a common measure of fraudulent practices. We estimated the model by industry, using the absolute value of the residual as the estimate of accruals quality.⁵

We used the accounting and governance risk ratings (AGR) as our final dependent variable. This is a proprietary measure of risk provided by GMI Ratings. Prior research shows AGR is as good as, or better than, all other academic measures at predicting accounting irregularities (Price et al. 2011). The AGR measure broadly measures the risk that accounting irregularities and corporate governance weaknesses would adversely affect a particular company (e.g., class-action litigation, restatements, SEC enforcement actions, bankruptcies, and other risks). This measure takes a value between 0 and 100, with higher values indicating greater risk.

Our independent variable of interest was *VIOLENCE*: the log of the yearly violent crime rate per 100,000 people in the MSA of a firm's headquarters. We also included *PROPERTY*: the log of the yearly property crime rate per 100,000 people. The FBI measures violent crime as the sum of the following: murder, rape, robbery, and assault. Property crime is the sum of: burglary, theft, and vehicle theft.

We included a fairly standard set of control variables in our regression to account for other factors that affect managers' choice to engage in questionable financial

reporting activity (McGuire et al. 2011). We included *Size*, the natural log of the market value of a firm's equity at the end of the previous fiscal year. Next, we included the return on assets, *ROA*, from the previous year to control for performance. We also controlled for prior year's leverage, *LEV*. We controlled for the prior year's book-to-market ratio of a firm, *BM*, a proxy for growth opportunities. We included an indicator variable *Loss*, which equals one if a firm has had negative earnings in the current or previous 2 years. Finally, we controlled for operating risk, *Op Risk*, measured as the standard deviation over the previous 5 years of operating cash flows deflated by average assets. To control for outliers, we took the natural log of crime rates and firm size; all other regression variables were winsorized at the top and bottom one percent. We report various sensitivity tests to ensure our models are robust to various different specifications.

Results

Table 2 presents the results of a regression of accounting irregularities on crime rates and other control variables. Panel A presents the results of the logistic regression of *AAER* on crime rates and other control variables. The coefficient on *VIOLENCE* is positive and significant, 0.42 with 95 % CI [0.18, 0.66]. This suggests that the predicted probability of committing fraud increased by 27 % for a company in a low violent crime area (first quartile of violent crime) versus a company in a high violent crime area (third quartile of violent crime) holding all else constant.

Table 2, Panels B and C replicate the analysis in Panel A using our two other dependent variables. In Panel B, which explores the influence of violence on the AGR measure, the coefficient is 2.15, 95 % CI [1.47, 2.83], a prediction that a company in a high violent crime area would have an AGR score 3 % higher than a company in a low violent crime area holding all else constant. In Panel C, which explores the influence of violence on accruals quality, the coefficient is 0.003, 95 % CI [0.000, 0.005], meaning that accruals quality increased (representing an increased likelihood of fraudulent reporting) for the companies in our dataset by 25 % for companies in high versus low violent crime areas.

The results in Panels A through C show that the effect of property crime on accounting irregularities was inconsistent, sometimes positive, sometimes negative, and sometimes indistinguishable from zero. That violent crime was reliably related to all three measures of financial irregularities, but property crime was not, is evidence that violence, and not the fact that a crime has taken place, drives our results. This evidence is consistent with observable implications of the three previous experiments.

⁴ Consider for example a firm that makes sales on account; this increases revenue in the current period with no increase in cash. However, in a future period, cash is collected. This regression model will account for this delay in cash collection. A large residual would occur when cash is not collected, which could result when revenues are fraudulently reported.

⁵ Using the absolute value treats negative and positive deviations as poor quality; a firm can deliberately understate or overstate earnings. An alternative is to use the signed residual and interpret overstatements of earnings as more problematic. Our overall inferences are unchanged if we use the signed residual instead of the absolute value. Also, we note that because future cash flow is included in this model, it cannot be used in a predictive fashion; however, we use it to test for the association between a firm's accruals quality and violence.

Table 2 Effect of crime on financial reporting irregularities

	Panel A: AAER				Panel B: AGR				Panel C: AQ			
	Est.	95 % CI		<i>P</i> val	Est.	95 % CI		<i>P</i> val	Est.	95 % CI		<i>P</i> val
		Lower	Upper			Lower	Upper			Lower	Upper	
Intercept	-14.10	-16.47	-11.73	<0.0001	5.21	-2.70	13.11	0.1966	0.098	0.068	0.127	<0.0001
VIOLENCE	0.42	0.18	0.66	0.0007	2.15	1.47	2.83	<0.0001	0.003	0.000	0.005	0.0463
PROPERTY	0.30	0.01	0.60	0.0448	0.70	-0.65	2.04	0.3086	-0.004	-0.007	0.000	0.0258
Size	0.37	0.31	0.42	<0.0001	1.06	0.71	1.41	<0.0001	-0.003	-0.003	-0.002	<0.0001
ROA	0.39	-0.02	0.79	0.0604	-5.14	-6.77	-3.52	<0.0001	-0.027	-0.037	-0.018	<0.0001
LEV	-0.19	-0.52	0.14	0.2656	-4.71	-6.56	-2.86	<0.0001	0.013	0.006	0.019	0.0001
BIG4	-0.14	-0.41	0.14	0.3257	-2.75	-3.87	-1.63	<0.0001	-0.009	-0.013	-0.004	0.0001
BM	-0.02	-0.04	0.00	0.0572	-0.33	-0.50	-0.16	0.0002	-0.002	-0.002	-0.001	<0.0001
Loss	0.55	0.36	0.74	<0.0001	4.85	3.86	5.85	<0.0001	0.004	0.000	0.007	0.023
Op_Risk	0.25	-0.53	1.02	0.5311	12.17	8.82	15.51	<0.0001	0.115	0.097	0.133	<0.0001
<i>N</i>	43,948				37,785				37,397			
<i>R</i> ²	0.0468				0.084				0.166			

This table includes the results from the estimation of regressions of financial irregularities (SEC enforcement actions, AAERs, in Panel A; AGR risk score in Panel B; accruals quality AQ in Panel C) on metropolitan area violent crime (*VIOLENCE*) and property crime (*PROPERTY*) levels for sample firms' headquarters and other control variables. The sample period is 1995–2008. The regression in Panel A is a logistic regression because the dependent variable is binary, the *R*² is a max rescaled pseudo *R*². The regressions in Panels B and C are ordinary least squares with standard errors clustered by firm and year

We performed several robustness tests on our regression analyses. First, in addition to measuring violent crime as reported by the FBI, we also measured violent crime excluding rape, and property crime excluding robbery, because previous research indicates these crimes have a mixture of violent and nonviolent motives (Anderson et al. 1997). The inferences are unchanged using this alternative measurement of crime. Second, we tested whether our results were robust to multiple different models. The results are robust to models that include/exclude fixed effects or use standard errors clustered by firm. The inclusion of fixed effects helps control for correlated omitted variables, and results estimated without fixed effects but with standard errors clustered by firm and year control for possible lack of independence of observations. In all specifications, we observe an association between violent crime and financial reporting irregularities that is different from zero.

General Discussion

We began with the question: can exposure to human violence impact an individual's ethical decision making at work? The results from three different experiments suggest a clear “yes:” exposure to violent media, both print and video, can motivate individuals to be less ethical in a business setting. They also provide evidence for a

mechanism driving this relationship, showing that exposure to human violence increases aggressive thoughts and feelings towards others, and that these thoughts and feelings motivate less ethical decision making.

Evidence from a correlational analysis of exposure to violence and corporate fraud for companies in the United States 1995–2008 provides additional confidence in our answer. We show that companies headquartered in more violent areas are associated with less ethical financial reporting, a finding that although not casual, is consistent with our theory. Thus, across multiple experiments and using complimentary methodologies, the results suggest that the negative effects of violent media extend beyond the increase in interpersonal aggression documented in previous work to other areas of society, including business ethics.

These findings raise concerns about additional damaging effects on society from rise of violent media in recent decades. Violent media appears to not only shape attitudes towards and expectations from others in ways that increase aggressive behavior towards them (as has been shown in previous research), but these ways of thinking also appear to affect other behaviors, like the decision to lie or cheat another. Given this potential negative impact to society of poor business ethics, we suggest additional areas for research on this important topic.

First, our results were inconsistent across genders, suggesting a need for additional research. While both men and

women responded to violently worded media, only men responded to violent videos. Second, we measured ethical behavior directly after contact with violent media. Although our correlational data would suggest the duration of the effect is strong enough to impact decision making long-term, additional experiments capable of exploring how repeated violent media exposure influences the strength and duration of the violent media effect on ethics will be beneficial. Third, violent media differs in many ways (e.g., intensity, detail, justifications for violence, etc.). We encourage future research that tests whether these results are robust to different types of violent media. Finally, we encourage additional research exploring factors that can reduce the effects of media exposure to human violence on ethics.

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