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Reverse Deterrence in Racial Profiling: Increased Transgressions by Nonprofiled Whites

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A controlled experiment tested the possibility that racial profiling—disproportionate scrutiny of a minority racial group by sanctioned authorities—would have a "reverse deterrent" effect on the illicit behavior of members of a nonprofiled majority group. Research participants given a task involving extremely difficult anagrams were given the opportunity to cheat. White participants randomly assigned to a condition in which two Black confederates were obtrusively singled out for scrutiny by the study administrator cheated more than Whites in a White-profiling condition and a no-profiling control condition, and more than Black participants in all three conditions. Black participants cheated at comparable levels across the three experimental conditions. The effect of the profiling of Blacks was consequently a net increase in cheating. The results indicate that racial profiling may be counterproductive.

Keywords: racial profiling, deterrence, discrimination, race

Racial profiling is the use of race or ethnicity by law enforcement officials as a basis for judgment of criminal suspicion. Anecdotes from celebrities and public officials (Russell, 1998), surveys of the public (e.g., Newport, 1999), statistical and econometric analyses of archival data (e.g., Harcourt, 2004, 2007), and labor-intensive field studies (e.g., Lamberth, 1994, 1998), as well as court rulings based in part on disclosures of internal departmental memos, indicate that minorities, particularly African Americans and Hispanics, in many jurisdictions have been subjected to stops and searches by police at rates disproportionate to their representation in the population and their rates of offending.

The purpose of this report is not to document the extent of racial profiling in the U.S. There is ample evidence that it is widespread (see e.g., Harris, 2002; Withrow, 2006) and there are also credible studies indicating the absence of profiling in some locales (e.g., RAND, 2004). The purpose of this report is to document that racial profiling can have unintended, counterproductive consequences vis-à-vis the targeted behavior. Racial profiling is supported by some commentators and practiced by some law enforcement officials because they believe it is a rational, effective strategy—if some racial or ethnic groups are more likely to commit crime, racial profiling would increase incapacitation of criminals. Racial profiling may also be appealing because it could *deter* crime. Deterrence theory holds that as the likelihood and/or severity of punishment increase, to the extent that that is perceived by individuals contemplating committing crimes, the rate of offending will decrease. Deterrence is a pillar of criminological theory embraced by the general public, lawmakers, and some prominent scholars (Blumstein, Cohen, & Nagin, 1978; Paternoster, 1987; Pratt, Cullen, Blevins, Daigle, & Madensen, 2006). To the extent that minority group members are aware of increased, targeted vigilance by the police, deterrence theory holds that they should reduce their participation in profiled crimes.

Yet the costs of racial profiling are many. Lay people and constitutional scholars alike argue that racial profiling is illegitimate because it violates the civil liberties of minorities and leads to their overrepresentation in the U.S. criminal justice system (e.g., Harris, 1999). Racial profiling also likely contributes to the racial gap in perceived legitimacy of the police and courts (Peffley & Hurwitz, 2010). Furthermore, racial profiling can also *cause* or exacerbate the very criminal justice statistical disparities that are used to justify it (Glaser, 2006), indicating the potential for a self-fulfilling prophecy. The present purpose is to investigate an as yet unexamined potential consequence of racial profiling—the possibility that profiling of minorities has a *reverse deterrent* effect, emboldening individuals who do not fit the profile to commit transgressions they would not otherwise have carried out.

Racial Profiling and Reverse Deterrence

Even if minorities were more likely to commit certain crimes, reasonable extensions of deterrence theory hold that racial profiling is still a questionable method of crime control (Glaser, 2006; Harcourt, 2004, 2007). Racial profiling represents a special case with regard to deterrence because it involves the concentration of resources on one group, which necessitates a dilution for others. If deterrence works because those inclined to commit crimes per-

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ceive and respond to changes in expected cost (probability of capture times punishment) of crime, then potential criminals in the nontargeted population (e.g., Whites) should, inferring a lower probability of capture, commit more crime. Harcourt (2007) contended that racial profiling will likely result in a net increase in drug trafficking because Whites are the majority group and, primarily because of racial differences in economic motivations for crime, Whites' criminal activity would be more responsive to changes in probability of being apprehended. Similarly, Glaser (2006) illustrated via mathematical simulations that when responsiveness to changes in probability of capture (i.e., deterrence) is incorporated in models of profiling there tends to be a higher overall crime rate and fewer criminal captures relative to models excluding deterrence.

In summary, racial profiling has been criticized for not only discriminating against members of minority groups (Harris, 1999) but for being a flawed strategy because it may increase crime (Glaser, 2006; Harcourt, 2004, 2007).

The present study is the first to test experimentally the effect of racial profiling on the rates of a profiled transgression. We manipulated the presence of racial profiling and observed its effect on the profiled behavior. In order to achieve experimental control and the ethical treatment of research participants while still attaining ecological validity and psychological realism, we operationalized "crime" as cheating on a difficult test in a classroom setting. Although cheating on a test is not illegal behavior, students know that it is unethical behavior subject to severe punishment, but associated with benefits if undetected.

As suggested by Harcourt (2004, 2007) and Glaser (2006), and consistent with deterrence theory, we postulated that when Blacks were profiled for cheating, White participants would feel greater impunity and be more likely to cheat than when either Whites were profiled or no profiling occurred. A simple application of the logic of reverse deterrence theory would also lead us to expect that Blacks would cheat more when Whites were profiled than when either Blacks were profiled or no profiling occurred. Our predictions for the behaviors of Black participants, however, were not this straightforward. First, given the extent to which Blacks experience profiling on a regular basis, we reasoned that Black participants might chronically feel watched by authority figures and would not risk cheating. Second, given that racial profiling in particular, and discrimination more generally, typically targets Blacks, and not Whites, Black participants should be less likely to perceive scrutiny of Whites as being race-based, and would not feel greater impunity. In other words, Blacks (and Whites) are less likely to have a mental schema for White profiling and, therefore, less likely to recognize it as such.

Method

Participants

Two-hundred and ninety-nine students from undergraduate psychology courses at Georgia Southern University participated to partially fulfill a course requirement or for extra credit. Data from Black and White participants were analyzed ($n = 278$). Of these participants, 71.9% were White, 60.4% were women, and the mean age was 20.1 ($SD = 3.5$). Preliminary analyses indicated that there were no gender effects so gender is not further discussed.

Design

This experiment involved a 2 (Participant Race: Black vs. White) $\times$ 3 (Profiling: Black vs. White vs. None) between-subjects design. Participants were recruited from an online subject pool and from classes. Once participants provided their names, a research assistant looked at Facebook photos to determine whether the participant appeared to be Black or White. Participants were then randomly assigned to one of the three profiling conditions and were e-mailed their session date and time. Our original intent was to run all groups with same-race participants so as to preclude spontaneous assumptions by participants of racial profiling. However, whenever scheduling procedures failed to achieve same-race groups (except for confederates), those sessions were run as controls (i.e., with no confederates singled out). This resulted in two types of no-profiling control conditions: same-race and mixed-race.

Procedure

This experiment used a test with extremely difficult anagrams (e.g., gnimimtyana, drinpomlea) developed by Vargas, von Hippel, and Petty (2004) to give participants an unobtrusively measurable opportunity to cheat. The procedure has been shown to have good construct validity as a measure of cheating. Specifically, Vargas et al. found that cheating scores on this anagram test were positively related to a partially structured measure of dishonesty and unrelated to impression management and high school grade point average. Consequently, it afforded a valid dependent measure of a serious, dishonest transgression that was unlikely to be confounded by cognitive performance effects.

The procedure was administered in a classroom to small groups of 2–9 (median = 6) same-race participants plus the two confederates in the experimental conditions. The experimenter explained that participants would be completing an anagram test that was a measure of "cognitive skills," followed by demographic and attitude questions. Participants performed the task individually, seated apart, and could not see what the other participants were doing. The experimenter then stated that, because some people had tried to cheat in the past, she would be monitoring for cheating. At this point, the racial profiling experimental treatment began. Participants randomly assigned to the Black (White) profiling condition observed the experimenter move closer to the group, stare directly in the face of each participant and Black (White) confederate for two seconds, and then point to the two Black (White) confederates, saying, "I'd like you and you to move up front so I can see you better." The Black (White) confederates looked puzzled, and then moved to two desks at the front of the classroom, next to the experimenter's desk.

After completing a practice anagram, participants were instructed that they had up to 15 minutes to complete the 15 anagrams. The cover story that this study assessed cognitive skills was purposefully constructed to provide participants with a subtle incentive to cheat. Cheating on this task would allow participants to show more cognitive skills. To enhance the legal relevance of the cheating task (i.e., there are benefits and sanctions associated with rule breaking), we told participants that others had tried to cheat in the past. The experimenter's stern look at each participant, combined with the expressed concern, was designed to convey the

unacceptability of cheating. In line with Vargas et al. (2004), to provide participants with another incentive to cheat, they were told that when they had finished they could score their own tests using the answers on the page following the test page, complete the remaining questionnaires, and then leave whenever they were finished. Thus, to the extent that participants would feel some pressure to complete some items before continuing to the next survey and leaving, cheating on this task would also be rewarded by a savings in time.

The experimenter then moved to the front of the room, sat on the instructor's desk, and, in the profiling conditions, stared at the confederates and did not look at any of the participants for the 15-minute time period. In the control condition, the experimenter intently read a book or periodical and did not look at any of the participants. The remaining questionnaires were demographics, Social Dominance Orientation (SDO; Pratto, Sidanius, Stallworth, & Malle, 1994), a manipulation check ("Did the researcher monitor for cheating during the study?" "How closely was the experimenter monitoring *you* for cheating?") and a probe question about the purpose of the study. Due to a collating error, participants did not receive all questions of the SDO and it was dropped from analyses. In response to the question about the purpose of the study, three participants indicated that one race was singled out, and two mentioned cheating. All other participants responded in line with the cover story. Analyses excluding these five participants yielded results equivalent to those reported below. The experimenters and confederates were blind to the purpose and hypothesis of the experiment.

Results

Cheating Score

Three pairs of coders, blind to experimental condition, participant race, and the hypothesis, coded each of the 15 anagrams for whether the participant tendered a solution to the anagram (i.e., wrote a word) or not, and if so, whether the participant solved it correctly or incorrectly. Using Vargas et al.'s (2004) coding strategy, the coders then coded each anagram for whether the participant cheated or not (interrater reliability was high; Cohen's Kappa ranged from .92–.96). A word was coded as a cheat if the participant answered correctly but there was no indication of work (e.g., dots around letters, slashes through letters), an attempt to solve the word, or erasure marks of such work.

Because three of the anagrams were relatively easy for the participants to solve (*world*, *whelp*, and *equinox*), consistent with the analytic procedures from Vargas et al. (2004), they were not included in the analyses. Each participant had a cheating score ranging from 0 to 12.

Rate of cheating. Figure 1 presents the results of the primary test of our hypothesis, investigating the independent and interactive effects of experimental condition and participant race on rate of cheating. To test whether this pattern reflects a significant interaction of participant race and experimental condition, we conducted a two-way analysis of variance (ANOVA). Pearson's r is provided as a standardized index of effect size. Although the main effect of participant race was only trending toward statistical significance, $F(1, 272) = 2.55$, $p = .112$, $r = .1$, with Blacks cheating less overall than Whites, the effect of experimental condition was significant, $F(2, 272) = 3.13$, $p = .045$, $r = .15$. These main effects were qualified by the predicted interaction, $F(2, 272) = 4.22$, $p = .016$, $r = .17$. LSD post hoc simple comparisons revealed that the mean cheating rate for White participants in the Black-profiled condition was significantly higher than all other means (all ps $< .01$). No other means differed significantly from each other (all ps $> .37$).

The significant main effect of condition reflects that, collapsing across Black and White participants, there was more cheating overall in the Black-profiled condition than in the control and White-profiled conditions (both ps $< .001$) and no significant difference between the latter two conditions ($p = .594$). This test of the condition effect, combining cheating rates of Black and White participants, offers an assessment of the *net* effect of profiling on cheating. The net effect of profiling will, of course, vary as a function of the ratio of size of minority and majority popu-

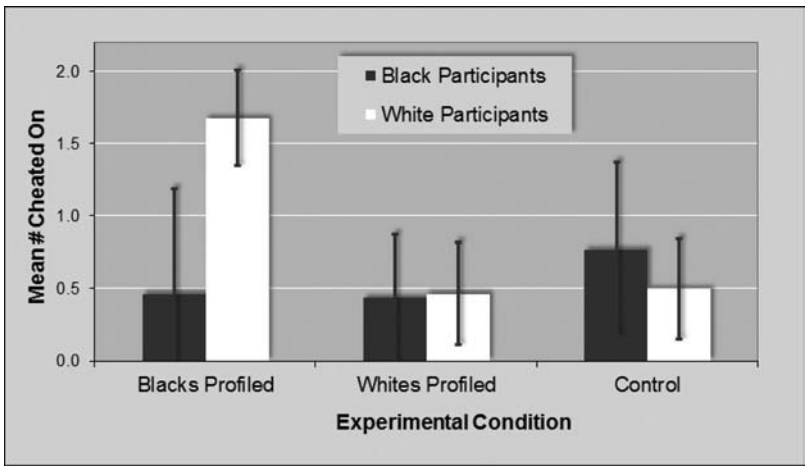

Figure 1. Average number of items cheated on out of the 12 most difficult anagrams, by condition and participant race. Error bars represent 95% confidence intervals.

lations, their baseline offending rates, and the intensity of the profiling. Nevertheless, this test shows that profiling can, at least under some circumstances, lead to a net increase in transgressions—profiling of Blacks caused more cheating overall than not profiling them.

To address any concerns about the positive skew of the distribution of the number of items cheated on, the ANOVA was replicated with a square-root transformation on the cheating variable, yielding equivalent results. Similarly, we tested the effects of participant race and profiling condition on the *proportion* of participants who cheated at all (i.e., cheating coded dichotomously as 0 = *did not cheat* and 1 = *cheated on one or more items*). As with the square-root-transformation analysis, this approach mitigates the likelihood that results are due to a small number of participants cheating on a large number of items. Although this state of affairs would not render the results uninteresting or unimportant, it would indicate a narrower problem and possibly implicate different policy responses. Nevertheless, the analysis of the proportions that cheated revealed a nearly identical pattern of results, with roughly the same (low) proportion cheating in all conditions *except* for Whites in the Black-profiled condition, who cheated at a rate of at least 15 percentage points higher than in all five other conditions. In this analysis, Black participants in the Black-profiled condition cheated at a somewhat lower rate than all the other conditions. However, probably because of the inherently lower statistical power of tests with binomial rather than continuous dependent variables, the interaction was only trending toward significance.

Group racial composition. Because, with the exception of the confederates, groups in which participants were run were same-race, differences in rates of cheating as a function of Black- versus White-profiling could be attributed to the groups' racial compositions. For example, for White participants, the two profiling conditions also differed in that the White-profiling condition had all-White groups (including confederates) and the Black-profiling condition involved having two Black students in the room. The mere presence of Black students may have promoted a higher rate of cheating for reasons other than a presumed decline in probability of detection. Because the no-profiling control groups varied in whether they were same-race or mixed-race, we were able to conduct an additional test of the effect of racial profiling, comparing the rates of cheating in the profiling conditions with those in the mixed-race control conditions.

Racial composition does not appear to have influenced White participants in the no-profiling control condition. The average number of items cheated on was exactly 0.5 in both the mixed-race ($SEM = 0.12$, $n = 34$), 95% CI [0.26, 0.74] and same-race ($SEM = 0.12$, $n = 32$), 95% CI [0.25, 0.75] control conditions. Among Black participants, on the other hand, there was a nonsignificant tendency to cheat more in the same-race ($M = 1.0$, $SEM = 0.47$, $n = 11$), 95% CI [−0.05, 2.05] than in the mixed-race ($M = 0.55$, $SEM = 0.31$, $n = 11$), 95% CI [−0.14, 1.24] control conditions, $p = .257$ (LSD post hoc comparison), but this comparison is low in statistical power.

The critical comparison is that Whites cheated significantly more in the Black-profiled experimental condition than they did in all other conditions, including the mixed-race no-profiling control condition, all ps $< .001$, and in all the other experimental conditions, White participants' cheating rates did not differ (all ps $> .923$). Accordingly, the higher rate of cheating among Whites in the Black-profiled condition cannot be attributed to the mere presence of Black test-takers.

Another possible confound was that groups in which White participants were run tended to be larger than Black participant groups (by 1.6 participants, on average), because there were many more White students in the subject pool and so they tended to show up for sessions in larger numbers. If people cheat more in larger groups (perhaps feeling more anonymous) this could explain the results. This concern is not particularly serious because White groups were larger across all three conditions, and it was only in the Black-profiled condition that Whites cheated more. Nevertheless, we investigated the possibility and found group size and cheating to be uncorrelated across the sample, within conditions, and within race. In fact, there was the tendency for a slight negative (nonsignificant) correlation. Controlling for group size in an analysis of covariance replication also revealed that the effects are in no way artifacts of group size.

Discussion

The goal of this study was to investigate experimentally the effects of racial profiling on the rate at which people commit the profiled transgression. We theorized that heightened surveillance of members of a minority group would increase illicit activity in the majority group—that it would have a *reverse deterrent* effect. We found that White participants in the Black-profiled condition cheated more than participants in any other condition. Although cheating on a test of this sort is not a crime, it is a dishonest behavior that is a particularly serious transgression in academic settings. These results indicate that racial profiling could increase crime among nonprofiled groups, having a counterproductive effect.

We also found that Blacks did not cheat more when Whites were ostensibly profiled. There are a number of plausible reasons for this finding. First, because of the prevalence of racial profiling, Blacks may chronically feel as if they are being monitored by authorities for transgressions. Second, for Blacks, being lower status, minority group members, the White identity of others may be less salient (Guinote, Mauro, Pereiva, & Monteiro, 2007; Simon & Brown, 1987) and so they may have been less likely to infer that White-targeted racial profiling was happening when Whites were targeted. Similarly, there may have been no preexisting mental schema of White profiling to be activated when Whites were singled out. In other words, when participants saw White, as opposed to Black, confederates being scrutinized, it was unlikely that they would infer that it was because of their race. Additionally, as legal and economic theorists have speculated (Harcourt, 2007; Persico, 2002), the economics of transgressing are different for minorities than for majority group members, and so they may be less sensitive to changes in expected costs (i.e., they are less "cost-elastic"). Harcourt's thesis holds that in the case of crime, minority group members may be less elastic in response to policing because: (a) they have fewer legal alternatives (i.e., educational and employment) available to replace crime as a source of income; and (b) punishment is more severe for minority group members than

majority group members, so those who abide by the law are less likely to start committing crime when the probability of detection decreases. In the case of this experiment, Black participants may have been more concerned about the academic and social consequences of getting caught cheating in this task than were White participants. It's likely that the Black participants were already aware of negative stereotypes regarding their racial group; cheating on the task would only confirm such stereotypes. Additionally, discrimination in society may lead Black participants to view their alternative options as being more narrow. Thus, they could be less responsive to the changes in costs of cheating. Of course, these are post hoc explanations that are in need of empirical testing.

We theorized that White participants would transgress more in the Black-profiled condition due to feelings of impunity. To explore this possibility, we administered a probe question after the cheating measure, asking participants to rate on a 7-point scale how much they thought they were being monitored. We found that, not surprisingly, participants in the profiling conditions reported feeling more monitored than those in the control condition, but did not find that Whites in the Black-profiling condition reported feeling less monitored than in other conditions. Although the results of our analysis of the probe question, asked after the primary task, do not provide direct support for our mediation hypothesis, this does not mean that the model is incorrect. A strength of our study is that our primary dependent variable was behavioral. Given that the measure of the mediating variable was an explicit self-report, and that it was asked after the cheating task, it was more susceptible to social desirability effects. More importantly, as Bullock, Green, and Ha (2010) have demonstrated, it is very difficult to demonstrate an underlying mechanism with a single statistical procedure or even within a small number of studies. Our findings call for follow-up research to *manipulate* variables that mediate the reverse deterrent effect. Such manipulations could alter participants' perceived likelihood of being caught cheating, the expected benefits of cheating, and the expected academic and social costs of being caught cheating.

The standard defense of profiling is that it reduces crime. This is based on the presumption that profiling promotes the efficient allocation of police resources to groups with presumed higher rates of crime, thereby leading to more criminal captures and greater deterrence. Our thesis holds that, to the extent that deterrence functions as criminological canon holds, profiling one group should have a reverse deterrent effect on criminally inclined individuals in other groups. The net effect of profiling could be to increase crime, as was observed with the rate of transgressions in the experiment reported here. It should be noted that a net increase in crime resulting from reverse deterrence in racial profiling would not have to rely on the asymmetric effects observed here (i.e., that Blacks did not cheat more when Whites were profiled). The mere circumstance that an elevated number of people from the *larger* (majority) group will offend when the minority is profiled could lead to a higher overall offending rate. The asymmetry we observed here would only exacerbate that problem.

In the future, it will be important to replicate our findings with other samples and operationalizations of crimes, and to develop studies that clarify the underlying processes involved. For now, the possibility that the net effect of profiling could be to increase crime, combined with legal and civil rights problems, as well as dramatic racial disparities in the criminal justice system, should be considered by law enforcement executives and legislators when setting policy related to racial profiling.

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