

Race–Crime Congruency Effects Revisited: Do We Take Defendants’ Sexual Orientation Into Account?

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Abstract

Decades ago, social psychologists documented a juror decision-making bias called the *race–crime congruency effect*: a tendency to condemn Black men more than White men for stereotypically Black crimes but to do the reverse for stereotypically White crimes. We conducted two high-powered experiments ($N = 2,520$) to see whether this pattern replicates and to examine whether it is attenuated when the defendant is gay. When participants reported on what the average American juror would do (Experiment 1), we observed greater harshness toward Black defendants accused of stereotypically Black crimes but not the previously documented reversal for stereotypically White crimes. Defendant sexual orientation did not moderate this pattern. When participants reported their own judgments about the same criminal cases (Experiment 2), they expressed greater harshness toward White (vs. Black) defendants and toward heterosexual (vs. gay) defendants. These effects were not moderated by crime type. Implications for the race–crime congruency effect are discussed.

Keywords

racial bias, crime perceptions, stereotyping, sexual orientation

Racial disparities in the criminal justice system are extensive and undeniable (Western, 2006), but the question of whether racial stereotyping contributes to these disparities has been more controversial (e.g., Mears, Cochran, & Lindsey, 2016). Decades ago, social psychologists conducted a number of experiments to test the hypothesis that Black versus White defendants might be judged differently by mock jurors, even when the evidence against these defendants is exactly the same (e.g., Bodenhausen & Wyer, 1985; Gordon, Bindrim, McNicholas, & Walden, 1988; Jones & Kaplan, 2003). These studies confirmed the existence of a *race–crime congruency* bias in juror decision-making. This bias describes jurors’ tendency to think in ways that disadvantage Blacks relative to Whites for stereotypically Black crimes, like gang violence, but that also disadvantage Whites relative to Blacks for stereotypically White crimes, like insider trading.

As an illustrative example of research on the race–crime congruency effect, Jones and Kaplan (2003) asked participants to say how confident they were in the guilt of defendants who were either Black or White and who were accused of either grand-theft auto (a “Black” crime) or embezzlement (a “White” crime). Participants perceived Black men as more guilty than White men when the crimes were stereotypically Black (Cohen’s $d = 1.18$), but they perceived White men as more guilty than Black men when the crimes were stereotypically White (Cohen’s $d = 1.12$). Other studies produced similar findings (Bodenhausen & Wyer, 1985; Gordon et al., 1988).

However, these studies were all underpowered by contemporary standards, with per-condition n s ranging from 15 to 20 across various experiments. Thus, these estimates of race–crime congruency effects are undoubtedly imprecise.

We revisit race–crime congruency effects in light of three issues. First, there is a need for a high-power test of the race–crime congruency bias, given that earlier studies had low statistical power. Second, the contours of racial stereotyping change over time. Research examining large, nationally representative samples of Americans has documented very dramatic changes in racial attitudes across the 20th century (Schuman, Steeh, Bobo, & Krysan, 1997), including a growing tendency to conceal or suppress any racial biases one may hold (Dovidio & Gaertner, 1986, 2000; Sigall & Page, 1971). In some contexts, normative pressures to express *pro*-Black biases have also emerged (Axt, Ebersole, & Nosek, 2016; Plant & Devine, 2001). It is possible, therefore, that the expression of racial biases found in earlier decades has changed over time or even reversed. Third, psychologists are becoming increasingly aware that people perceive others in light of their multiple,

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intersecting social identities (Cole, 2009; Else-Quest & Hyde, 2016; Kang & Bodenhausen, 2015). However, we know very little about how intersectional person perception operates in the domain of juror decision-making (e.g., Mitchell, Haw, Pfeiffer, & Meissner, 2005). We consider the possibility that a defendant's sexual orientation will attenuate the racial biases of mock jurors.

The expression of racial biases is contingent on the racial prototypicality of potential targets (e.g., Eberhardt, Davies, Purdie-Vaughns, & Johnson, 2006; Maddox, 2004), with highly prototypic group members being much more likely to be targeted for racial discrimination than their less prototypic counterparts. As intersectionality theorists have noted, the most prototypic members of socially subordinated groups are those who are *not* subordinated along other dimensions of their identities (Purdie-Vaughns & Eibach, 2008). Thus, when asked to think of a prototypical Black person, most people will bring to mind a heterosexual man. If heterosexual Black men are more prototypic of Black Americans than are gay Black men, gay Black men may be more likely than their heterosexual counterparts to evade racial biases. Prior research in the domain of personnel selection substantiates the notion that gay Black men are, at least sometimes, evaluated less negatively than heterosexual Black men (Pedulla, 2014).

There is an additional reason why gay Black men (vs. heterosexual Black men) may be less targeted for racial biases in the specific context of criminal judgments. Recent studies have documented various ways that race is gendered, with African Americans seeming more masculine to perceivers than European Americans (e.g., Galinsky, Hall, & Cuddy, 2013; Johnson, Freeman, & Pauker, 2012). In line with this pattern, crimes that are commonly considered to be stereotypically Black tend to be ones having a hypermasculine character (e.g., Gordon et al., 1988), such as physical aggression. Gay men, however, are commonly stereotyped as gender-inverted (Blashill & Powlishta, 2009; Kite & Deaux, 1987), meaning that people assume they have more feminine characteristics than heterosexual men. For this reason, participants may view them as less likely than their heterosexual counterparts to commit the kinds of masculine offenses that are stereotypically linked to Black men.

How might mock jurors respond to White defendants with a minority sexual orientation? Because of the gender-inversion notions inherent in common gay stereotypes, gay White men may seem even less likely to commit hypermasculine, stereotypically Black kinds of crimes than their heterosexual counterparts. Additionally, like gay Black men, gay White men are less likely to seem like prototypical members of their racial group than heterosexual group members. This implies that gay White men may be seen as less likely to engage in the kinds of white-collar crimes that are stereotypically associated with their racial group, compared to heterosexual White men. In summary, there are several compelling reasons to believe that the expression of race bias in mock juror judgments could be moderated by defendants' sexual orientation.

We conducted two high-powered experiments that provide insight into the issues outlined above. In both, we asked

participants to evaluate defendants whose race was either Black or White, whose alleged crimes were either stereotypically Black or stereotypically White, and whose sexual orientation was either gay or heterosexual. In Experiment 1, we asked them to report on how the average American juror would judge defendants. Following past research, we adopted this approach as a strategy for reducing social desirability biases regarding the suppression of prejudice (e.g., Calabrese et al., 2017; Cox & Devine, 2015; Devine & Elliot, 1995; Ghavami & Peplau, 2012). The logic is that participants will tend to project their own biases onto the vague "average person," without the threatening possibility that they will be seen as personally endorsing such views (for evidence that people use their own personal characteristics to judge the prevalence of psychological attributes in a larger population, see Krueger & Clement, 1994).

In the second experiment, we asked new participants to report how they personally would respond to the exact same case materials. Although potentially more vulnerable to the previously noted rise in race-related social desirability biases, these instructions more closely replicate those of the original research that stimulated the present studies. Moreover, some scholars have argued that Americans' hesitations about overtly expressing explicit racial biases may even be on the decline as our society becomes more diverse (Craig & Richeson, 2014). Given the rise in explicit racism during the Obama presidency (Pasek, Stark, Krosnick, Tompson, & Payne, 2014) and the possibility that the candidacy and ensuing presidency of Donald Trump has emboldened people with latent racist tendencies (Pettigrew, 2017), it is a question of intrinsic interest whether participants' own self-reported reactions align with the reactions they project onto the average American.

Experiment 1

Method

Participants and Design

We recruited a total of 1,314 individuals from the Mechanical Turk website. Our final sample consisted of more than 150 participants per condition, far larger than the earlier experiments that inspired this study.¹ This sample had a mean age of 36.03 ($SD = 11.47$), was 54.5% male, 44% female, and 1.5% other or unspecified gender, and was relatively White (77%), followed by Asian (8.1%), Latinx (6.3%), and African American (6.2%). The sample was overwhelmingly heterosexual, with 3.7% identifying as gay or lesbian. Participants covered the gamut of educational attainment and political ideology, although the distribution was biased toward liberalism ($M = 5.17$ on an 11-point scale ranging from 1 = *extremely liberal* to 11 = *extremely conservative*, $SD = 2.75$). Thus, the sample was in several ways more diverse than the student samples used in prior studies of race bias among mock jurors. Participants were randomly assigned to one of the conditions of a 2 (defendant race: Black or White) \times 2 (defendant sexual orientation: heterosexual or

homosexual) \times 2 (crime type: stereotypically Black or stereotypically White) between-subjects design. As noted below, there were three different specific crimes within each crime type condition, and each participant saw only one of these crimes.

Materials and Procedure

We identified potential racially stereotypic crimes based on past research and conducted a pilot test using a different group of participants (sampled from the same population as the main study; $N = 77$) in order to confirm that these stereotypical associations are still evident in the minds of contemporary social perceivers. We selected as “Black” crimes drive-by shooting, gang violence, and street gambling. As “White” crimes, we selected embezzlement, computer hacking, and insider trading. Pilot participants rated these crimes, in a randomized order, on the degree to which they seemed stereotypic of White versus Black individuals (from 1 = *very White* to 7 = *very Black*) and on the degree to which they implied that defendants were guilty (from 1 = *not at all* to 7 = *very much*). As intended, the three stereotypically White crimes were seen as “Whiter” ($M = 2.34$, $SD = 1.26$) than the three stereotypically Black crimes ($M = 5.22$, $SD = 1.12$), $t(76) = 46.92$, $p < .001$, $M_{diff} = 2.87$, 95% confidence interval (CI) [2.75, 2.99], Cohen’s $d = 2.43$; additionally, both of these means differed significantly from the race-neutral midpoint of the response scale, $ps < .001$. Finally, we verified that the “White” ($M = 5.19$, $SD = 1.05$) and the “Black” crime descriptions ($M = 5.18$, $SD = 1.06$) did not meaningfully differ in the extent to which they implied the defendant was guilty, $t(76) = 0.11$, $p = .91$, $M_{diff} = 0.02$, 95% CI [-0.30, 0.34], Cohen’s $d = 0.02$.

For the main experiment, we created case summaries involving defendants accused of each of these crimes, modeled on the earlier work of Jones and Kaplan (2003).² All crime descriptions featured the charge against the defendant as well as a brief description of his defense. This description also contained an indication of the defendant’s sexual orientation by manipulating information about the sex of his romantic partner. For example, the description of gang-related violence read:

The defendant was charged with assaulting two men in a parking lot on the basis of gang membership. In his defense, the defendant stated that his own gang membership had nothing to do with the assault, but that he was instead protecting his girlfriend [boyfriend] from being harassed by these men.

Past research has commonly manipulated the race of defendants by giving them racially distinctive names or including a photo. However, names carry many psychological and demographic connotations other than race (e.g., Kasof, 1993), and faces similarly contain many cues that are used to infer an individual’s characteristics beyond race per se (e.g., Oosterhof & Todorov, 2008). Race can of course be explicitly specified, but past research indicates that group stereotypes are less likely to be activated by relatively pallid verbal category labels, compared to more vivid kinds of information (Beckett & Park,

1995). As a novel strategy to deal with these issues, we manipulated race by including a heavily pixelated photo of the defendant (ostensibly for purposes of protecting his privacy) that obscured specific facial features that might convey qualities such as warmth or competence but which clearly conveyed race (see Figure 1).

We told participants that the study concerned the judgments American jurors make in legal contexts. Depending on randomization condition, they read one crime summary and made several judgments based on it. First, they stated whether they thought the average American juror would regard the defendant as guilty or not guilty and then they rated their confidence in this verdict (from 0 = *not at all* to 10 = *extremely*). Next, they completed a 5-item index of negativity toward the defendant (adapted from Bodenhausen and Wyer, 1985). For this index, participants rated the extent to which the average American juror would feel “that the case against the defendant is strong,” “that the defendant likely committed this crime,” and “that if no action is taken, the defendant would likely be charged with this crime again” (all from 0 = *not at all* to 10 = *extremely*). In addition, they stated whether the average American juror would recommend disciplinary action against the defendant (from 0 = *definitely not* to 4 = *definitely would*) and they stated what level of punishment the average American juror would recommend, assuming the defendant is guilty (from 0 = *the minimum provided by the law* to 4 = *the maximum provided by the law*). After completing their case judgments, participants answered a few questions designed to check their attentiveness to the case materials, and they reported their own demographic characteristics (age, gender, race, sexual orientation, education, and political orientation). No other measures were collected.

Results

Based on earlier research, which involved defendants who were highly likely to be assumed to be heterosexual, we would expect to see that when it comes to stereotypically Black crimes, heterosexual Black defendants would be judged more harshly than heterosexual White defendants; regarding stereotypically White crimes, these same Black defendants should be judged less harshly than their White counterparts. When the defendants are characterized as gay men, we predicted that this pattern would be attenuated.

We included all participants in the reported analyses.³ We began by examining participants’ confidence-weighted verdicts (i.e., their verdict, where *not guilty* = -1 and *guilty* = 1, multiplied by their confidence, which could range from 0 to 10). An analysis of variance (ANOVA) on the this index indicated a main effect of target race, with Black defendants ($M = 6.21$, $SD = 5.03$) seeming more likely to be guilty than White defendants ($M = 5.48$, $SD = 4.99$), $F(1, 1287) = 6.98$, $p = .008$, $M_{diff} = 0.74$, 95% CI [0.19, 1.28], Cohen’s $d = 0.16$. This main effect was qualified by an interaction with crime type, $F(1, 1287) = 7.46$, $p = .006$, $\omega_p^2 = .005$. Means for this interaction are depicted in Figure 2.



Figure 1. Sample face stimuli showing Black (A) and White (B) defendants; facial stimuli were adapted from the MR2 database (Strohinger et al., 2016).

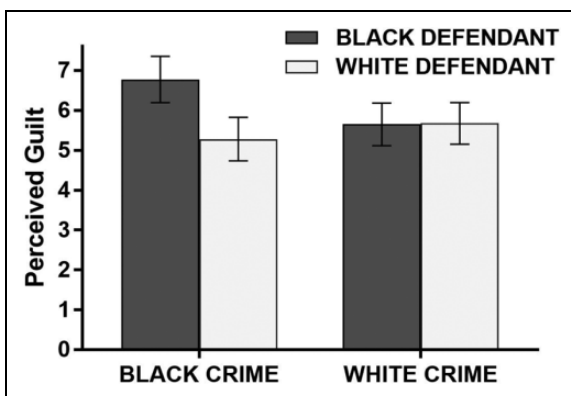


Figure 2. Mean perception of guilt (and 95% confidence intervals) as a function of defendant race and crime type; scores could range from -10 to $+10$.

Decomposing this interaction reveals that defendant race affected perceived culpability for “Black” crimes, $M_{\text{diff}} = 1.54$, 95% CI [0.74, 2.35], $p < .001$, Cohen’s $d = 0.31$, but had no discernible effect on perceptions of “White” crimes, $M_{\text{diff}} = -0.02$, 95% CI [-0.76, 0.72], $p = .95$, Cohen’s $d < 0.01$. Contrary to our hypothesis, this pattern was not qualified by defendant sexual orientation, $F(1, 1287) = 1.31$, $p = .25$, $\omega_p^2 < .001$, nor were there any other effects involving sexual orientation, $ps > .57$. Thus, we clearly replicated earlier work showing a bias against Black defendants in the case of stereotypically Black crimes, but we did not replicate the corresponding bias against White defendants in the case of stereotypically White

crimes. Nor did we find any evidence that defendant sexual orientation significantly moderated these results. We examine the results pertaining to sexual orientation more closely after presenting the remaining positive findings.

The remaining dependent measures reflected different facets of negative reactions to the defendant (his perceived likelihood of recidivism, the severity of recommended punishment, etc.). These ratings were all substantially intercorrelated, $0.36 < r_s < 0.76$, and an exploratory factor analysis revealed that they formed just one factor. Thus, we standardized participants’ responses and averaged these ratings in a single composite index of negativity. This composite index (M_z) was quite reliable, $\alpha = .85$. An ANOVA on this variable revealed a pattern similar to the verdict measure, with a main effect of defendant race in the same direction, $F(1, 1306) = 22.56$, $p < .001$, $M_{\text{diff}} = 0.21$, 95% CI [0.12, 0.29], Cohen’s $d = 0.19$, and in this case also a main effect of crime type in which participants’ ratings reflected greater harshness toward defendants accused of “Black” crimes than toward defendants accused of “White” crimes, $F(1, 1306) = 5.89$, $p = .015$, $M_{\text{diff}} = 0.11$, 95% CI [0.02, 0.19], Cohen’s $d = 0.13$. Most importantly, we again found a significant interaction between defendant race and crime type, $F(1, 1306) = 16.25$, $p < .001$, $\omega_p^2 = .012$, shown in Figure 3. Decomposing this interaction, we again found that participants’ ratings reflected greater harshness toward Black defendants charged with “Black” crimes than toward White defendants charged with these same crimes, $M_{\text{diff}} = 0.38$, 95% CI [0.26, 0.50], $p < .001$, Cohen’s $d = 0.49$. However, when defendants were

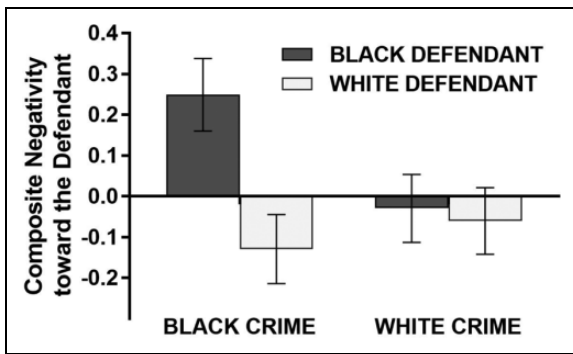


Figure 3. Mean standardized negativity toward the defendant (and 95% confidence intervals) as a function of defendant race and crime type (Experiment 1).

accused of “White” crimes, there was no effect of defendant race, $M_{diff} = 0.03$, 95% CI $[-0.08, 0.15]$, $p = .60$, Cohen’s $d = 0.04$. Again, there were no discernible effects of the defendant’s sexual orientation, all $ps > .23$.

Given that there were good reasons to expect that race biases would be moderated by the target’s sexual orientation, it is important to evaluate how strongly the data disconfirm this hypothesis. To address this issue, we used an equivalence testing procedure called the two one-sided tests (TOST) procedure (Lakens, 2017). This procedure allowed us to compute, for each combination of defendant race and crime type, whether the effect of defendant sexual orientation was significantly *higher* than a lower boundary of practical equivalence to zero as well as whether it was significantly *lower* than an upper boundary of practical equivalence to zero. We set our lower and upper boundaries to Cohen’s ds of -0.34 and 0.34 , respectively, which are the smallest non-zero effects we had sufficient statistical power to rule out.⁴ In addition to using equivalence testing, we computed Bayes factors (BF_{01}) to estimate the strength of evidence in favor of our null findings (Kruschke & Liddell, 2017). These Bayes factors indicate how much more likely it is that sexual orientation has a null influence on participants’ responses than it is that sexual orientation has some alternative influence on participants’ responses that the researchers specify. For ease of comparison to the equivalence testing procedure, we set our alternative influence to there being an effect of at least $d = 0.34$. All equivalence test results and Bayes factors are reported in Tables 1 and 2, which correspond to each of our dependent variables.

In general, all equivalence tests were significant (all $ps < .025$), indicating that all effects of sexual orientation were significantly closer to $d = 0$ than they were to the least nonzero effects we had adequate power to observe. Furthermore, our Bayesian analyses bolster our support in favor of the null hypothesis. For all combinations of defendant race and crime type, the null hypothesis was at least 2.53 times more likely than the alternative hypothesis. Thus, there is good reason to believe that defendant sexual orientation had a *very* small influence on participants’ responses—so small that it was practically equivalent to zero.⁵

Experiment 2

Method

We recruited $N = 1,206$ new Mechanical Turk workers to participate in an experiment that was structurally identical to the experiment we report above. The new sample was demographically similar to that of Experiment 1 in terms of race (73% White, 8.9% African American, 8.5% Asian, 6.5% Latinx, 3.1% other), gender (50.5% male, 47.9% female, 1.6% other), age ($M = 34.97$, $SD = 11.38$), sexual orientation (only 3.2% identified as gay or lesbian), and political ideology ($M = 5.01$, $SD = 2.75$, using the same 11-point scale as before).

Again, our aim was to recruit approximately 150 people per condition, and again we made no data exclusions.⁶ However, we made one important change in this experiment: We told each participant that we were interested in “how you would respond to this case if you had been a juror.” Using the same cover story as Jones and Kaplan (2003), we further told participants, “Our goal is to see how your judgments, based on this shortened court case description, compare with those of the actual jurors.”

Results

We calculated our dependent measures exactly as in Experiment 1. When we subjected participants’ confidence-weighted verdicts to an ANOVA, we found just one effect. Participants viewed White defendants ($M = 4.20$, $SD = 5.70$) as more guilty, on average, than Black defendants ($M = 2.63$, $SD = 6.40$), $F(1, 1191) = 20.87$, $p < .001$, $M_{diff} = -1.60$, 95% CI $[-2.29, -0.91]$, Cohen’s $d = 0.26$. No other main effects or interactions were statistically significant (all $ps > .11$).

The negativity composite measure ($\alpha = .82$) was subjected to the same analysis, and we again found that participants judged White men ($M = 0.09$, $SD = 0.74$) more harshly than Black men ($M = -0.09$, $SD = 0.78$), $F(1, 1198) = 18.03$, $p < .001$, $M_{diff} = -0.19$, 95% CI $[-0.27, -0.10]$, Cohen’s $d = 0.23$. In addition, we found that participants judged men accused of “Black” crimes ($M = 0.06$, $SD = 0.78$) more harshly than they judged men accused of “White” crimes ($M = -0.06$, $SD = 0.75$), $F(1, 1198) = 8.01$, $p = .005$, $M_{diff} = 0.12$, 95% CI $[0.04, 0.21]$, Cohen’s $d = 0.15$. Although descriptively the pro-Black bias was greater for stereotypically White versus stereotypically Black crimes, there was no significant interaction between defendant race and crime type ($p = .096$; see Figure 4). Defendant sexual orientation did not moderate either of these main effects ($ps > .599$); however, we did observe a main effect of defendant sexual orientation such that participants judged heterosexual defendants ($M = 0.07$, $SD = 0.75$) more harshly than gay defendants ($M = -0.07$, $SD = 0.78$), $F(1, 1198) = 9.59$, $p = .002$, $M_{diff} = -0.14$, 95% CI $[-0.22, -0.05]$, Cohen’s $d = 0.18$. Overall, the fact that participants reported less negative judgments of Black (vs. White) and gay (vs. heterosexual) defendants, independent of crime type, is consistent with the possibility that participants’ responses can become contaminated by social desirability concerns when they are asked to

Table 1. Equivalence and Bayesian Tests for Effects of Defendant Orientation on Weighted Verdicts.

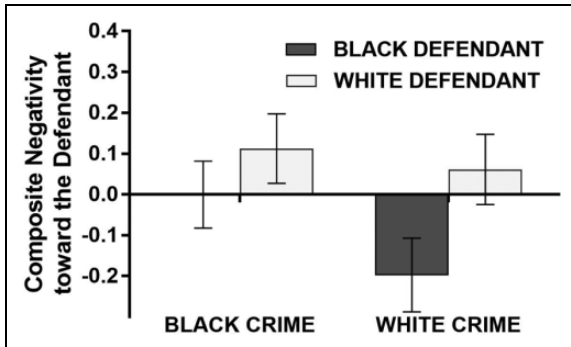
Defendant Type	M_{gay} (SD)	$M_{\text{heterosexual}}$ (SD)	M_{diff} [95% CI]	d	TOST result		BF_{01}
					t	p	
Black, "Black" Crime	6.65 (4.30)	6.91 (4.21)	-.26 [-1.42, 0.90]	-.06	2.35	.010	3.54
Black, "White" Crime	5.70 (4.87)	5.61 (5.01)	.09 [-0.98, 1.16]	.02	2.93	.002	4.18
White, "Black" Crime	5.62 (5.48)	4.93 (5.87)	.69 [-0.41, 1.78]	.12	1.96	.025	2.53
White, "White" Crime	5.56 (5.05)	5.79 (4.75)	-.24 [-1.29, 0.81]	-.05	2.72	.003	3.95
Across all types	5.83 (4.97)	5.80 (5.05)	0.03 [-0.52, 0.58]	<.01	6.01	<.001	7.82

Note. TOST results that are statistically significant indicate that effects of defendant sexual orientation were significantly greater than $d = -.34$ and significantly lower than $d = .34$. BF_{01} = ratio of evidence in favor of the null hypothesis over the alternative hypothesis; CI = confidence interval; M = mean; SD = standard deviation; TOST = two one-sided tests.

Table 2. Equivalence and Bayesian Tests for Effects of Defendant Orientation on Negativity Composite.

Defendant Type	M_{gay} (SD)	$M_{\text{heterosexual}}$ (SD)	M_{diff} [95% CI]	d	TOST result		BF_{01}
					t	p	
Black, "Black" Crime	.25 (.70)	.25 (.69)	-.01 [-0.19, 0.17]	-.01	2.81	.003	3.98
Black, "White" Crime	.01 (.80)	-.07 (.83)	.08 [-0.09, .25]	.10	2.23	.013	3.03
White, "Black" Crime	-.11 (.81)	-.15 (.86)	.04 [-0.13, .21]	.05	2.61	.005	3.84
White, "White" Crime	-.10 (.75)	-.02 (.75)	-.08 [-0.24, -0.08]	-.11	2.18	.015	2.78
Across all types	-.003 (.78)	.003 (.80)	-.006 [-0.09, 0.08]	<.01	6.03	<.001	7.85

Note. TOST results that are statistically significant indicate that effects of defendant sexual orientation were significantly greater than $d = -.34$ and significantly lower than $d = 0.34$. BF_{01} = ratio of evidence in favor of the null hypothesis over the alternative hypothesis; CI = confidence interval M = mean; SD = standard deviation; TOST = two one-sided tests.

**Figure 4.** Mean standardized negativity toward the defendant (and 95% confidence intervals) as a function of defendant race and crime type (Experiment 2).

report their own explicit judgments about minority defendants. We discuss other possible interpretations below.

General Discussion

The present research casts a new light on the race–crime congruency effects documented in decades past. In that earlier work, the evidence indicated that people condemned Black men more than White men for “Black” crimes but that when the crimes were “White,” the effect was reversed (e.g., Gordon et al., 1988). In Experiment 1, we replicated half of this pattern. Specifically, Black defendants were judged more harshly when accused of stereotypically Black crimes, but there was no effect of defendant race when accusations

involved stereotypically White crimes. To circumvent concerns about social desirability, we framed the instructions in the first experiment in terms of how the average juror would respond. This procedure allows respondents to project their own biases onto this nondescript “average person” without personally owning up to them (e.g., Devine & Elliott, 1995). In Experiment 2, where participants were asked to report their own personal judgments, this interaction pattern disappeared; in these circumstances, there was no evidence of a race–crime congruency effect. Instead, case judgments reflected a general tendency to judge minority defendants less harshly regardless of crime type.

There can be little doubt that most members of contemporary American society do not want to appear racist or homophobic, and they take pains to avoid such appearances under many circumstances (Dovidio & Gaertner, 1986). Differences in social desirability concerns thus provide one plausible account for the differing results across these two experiments. However, in addition to reflecting projections derived from participants’ own racial attitudes, their judgments in Experiment 1 may also reflect, at least in part, personal theories about the prevalence of relevant social attitudes. From this perspective, it is interesting that people expect others to exhibit enhanced harshness toward Black defendants accused of stereotypically Black crimes, irrespective of the defendant’s sexual orientation. That is, they evidently expect race biases to trump any biases related to sexual orientation, given the lack of any practically significant effects of defendant sexual orientation on case judgments in Experiment 1.

Other studies have documented that, in contexts where negative racial biases do emerge, gay Black men can elicit more favorable responses than heterosexual Black men (Pedulla, 2014; Wilson, Remedios, & Rule, 2017). These other studies examined much more positive judgment contexts (i.e., hiring decisions and leader selection), while our study focused on the negative context of crime and punishment. Racial biases may exert a stronger, less easily mitigated influence in negative vs. positive judgmental contexts. In addition, affirmational racial stereotypes (e.g., that “Black men *do* commit violent homicides”) may be harder to modify via cross-cutting social identities than negational racial stereotypes (e.g., that “Black men *do not* make good leaders”). Future research should directly investigate these interesting possibilities.

Past research also indicates that in general, people spontaneously categorize and attend to targets’ race more quickly than to other dimensions of targets’ identities (Ito & Urland, 2003; see Todorov, Olivola, Dotsch, & Mende-Siedlecki, 2015, for a review). In situations where perceivers are motivated to reach a quick judgment, this initial processing bias toward race may end up being their primary consideration regarding the target’s social identity. Future research should examine situations that vary in terms of respondents’ motivation and opportunity to render more thoughtful judgments about criminal defendants. It could be that instructions to report one’s own judgments (which may have greater implications for one’s personal reputation and presumed values) produce greater motivation to think more extensively, compared to making a prediction about others’ reactions. This difference might explain why respondents were influenced only by race-related considerations in Experiment 1, whereas the defendant’s sexual orientation (as well as race) did influence their overall harshness in Experiment 2.

It would also be desirable for future research to examine additional kinds of crimes. Although we selected a set of crimes that were considered highly racially differentiated and we found a consistent pattern across them, there are many other crimes that might carry racial assumptions. Perhaps there are still some kinds of “White” crimes that would selectively elicit greater harshness toward White versus Black defendants, as shown in earlier research. Of course, it is also important to recognize that the informal responses of individuals who casually read about a court case and report their beliefs (or the average American’s beliefs) could be very different from those of actual jurors who must make a high-stakes decision about a real case. However, the public perception of court cases is a topic of interest in its own right (e.g., Bodenhausen, 1990), and the public’s absorption and application of crime-related racial stereotypes that are commonly propagated in the mass media (e.g., Gilliam & Iyengar, 2000) is well worth investigation.

Americans’ racial biases have indeed changed over time. While Americans of 30 years ago freely admitted to biases against Black men, our data suggest that they are much less willing to do so today. Americans of today, if anything,

evinced a pro-Black bias when asked to report on their own views. Yet their pro-Black views diverge from what they believe to be the average American’s views. The average American, according to our participants, still condemns Black men for engaging in stereotypically Black actions. This bias may look different from how it looked 30 years ago, but it continues to pervade the American psyche—just not as explicitly as did before.

Declaration of Conflicting Interests

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Supplemental Material

The supplemental material is available in the online version of the article.

Notes

1. The reported data were collected in two waves, with mutually exclusive participant samples; we collapsed the two waves into one omnibus analysis in order to maximize efficiency of reporting and statistical power. Our total sample size gives us 80% power (assuming $\alpha = .05$) to detect effect sizes as small as Cohen’s $f = 0.08$ in our full-factorial analyses of variance. Cohen (1969) considered effects to be small if they were at or below Cohen’s $f = 0.10$.
2. Although we followed Jones and Kaplans’s (2003) method of creating brief court case descriptions, others (e.g., Bodenhausen & Wyer, 1985) have created full-fledged case files, including pieces of incriminating and exonerating evidence that mock jurors can evaluate. We opted for the former method for its simplicity and for the cross-condition control that it affords. See Supplemental Material for our exact stimuli, instructions, and survey questions.
3. All conclusions remain the same, regardless of whether we exclude the minority of individuals ($n = 135$) who failed to notice our sexual orientation manipulation (see Supplemental Material, for analyses excluding these individuals). Fluctuations in degrees of freedom are due to missing data.
4. We computed these boundaries using Lakens’s (2017) power calculator for the two one-sided tests procedure. Boundaries are determined by desired power and alpha levels ($1 - \beta = .80$, $\alpha = .05$) as well as per-condition sample size ($n = 150$).
5. We also conducted exploratory analyses to see whether any of our demographic variables predicted harshness toward defendants in this Experiment, and in Experiment 2. See Supplemental Material for a summary.
6. As with Experiment 1, all conclusions remain the same, regardless of whether we exclude the minority of individuals ($n = 81$) who failed to notice our sexual orientation manipulation (see Supplemental Material, for analyses excluding these individuals).

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