Journal of Experimental Social Psychology 61 (2015) 120–130

Contents lists available at ScienceDirect

Journal of Experimental Social Psychology

journal homepage: www.elsevier.com/locate/jesp

Is the evidence from racial bias shooting task studies a smoking gun? Results from a meta-analysis☆

Yara Mekawi *, Konrad Bresin

University of Illinois at Urbana-Champaign, United States

HIGHLIGHTS

- Laboratory shooter tasks have yielded mixed results regarding racial shooter biases.
- This study reports a meta-analysis of racial shooter biases.
- Shooter biases were significant for shooting threshold and reaction time.
- State level gun laws and proportion of non-Whites moderated shooter biases.
- Implications for training of police officers and gun owners are discussed.

ARTICLE INFO

Article history:
Received 4 September 2014
Revised 20 August 2015
Accepted 21 August 2015
Available online 23 August 2015

Keywords:
Shooting task
Shooting threshold
Racial shooter biases
Meta-analysis

ABSTRACT

The longstanding issue of extrajudicial police shootings of racial and ethnic minority members has received unprecedented interest from the general public in the past year. To better understand this issue, researchers have examined racial shooter biases in the laboratory for more than a decade; however, shooter biases have been operationalized in multiple ways in previous studies with mixed results within and across measures. We meta-analyzed 42 studies, investigating five operationalizations of shooter biases (reaction time with/without a gun, false alarms, shooting sensitivity, and shooting threshold) and relevant moderators (e.g., racial prejudice, state level gun laws). Our results indicated that relative to White targets, participants were quicker to shoot armed Black targets ($d_{av} = -.13$, 95% CI [−.19, −.06]), slower to not shoot unarmed Black targets ($d_{av} = .11$, 95% CI [.05, .18]), and more likely to have a liberal shooting threshold for Black targets ($d_{av} = -.19$, 95% CI [−.37, −.01]). In addition, we found that in states with permissive (vs. restrictive) gun laws, the false alarm rate for shooting Black targets was higher and the shooting threshold for shooting Black targets was lower than for White targets. These results help provide critical insight into the psychology of race-based shooter decisions, which may have practical implications for intervention (e.g., training police officers) and prevention of the loss of life of racial and ethnic minorities.

Although police brutality toward racial and ethnic minorities has been a pervasive problem in the United States for decades (Binder & Scharf, 1982; Department of Justice, 2001; National Center for Injury Prevention and Control, 2014), these issues have been a particularly salient topic of public debate since the acquittal of George Zimmerman for the murder of Trayvon Martin (Black Lives Matter, 2015; Mays, Johnson, Coles, Gellene, & Cochran, 2013). In particular, the past year involved unprecedented media coverage of extrajudicial shootings of racial and ethnic minorities (most often Black men) by police (e.g., Michael Brown, Tamir Rice, Walter Scott). In response to similar cases that have occurred in the past decade, researchers have used a novel laboratory task to examine this phenomenon in the hopes that such research will eventually lead to interventions that will prevent the unjustified loss of life of people of color. In a typical first-person shooter task (Correll, Park, Judd, & Wittenbrink, 2002), participants are shown images of Black and White targets holding either a gun or a neutral object (e.g., cell phone) and given less than one second to respond whether or not they should "shoot" or "not shoot" the target. Over a dozen studies have investigated shooter biases using targets from various

☆ YM conceptualized the study. YM and KB searched for studies, coded studies, conducted data analysis, and wrote the manuscript. Both authors made revisions to the manuscript and approved the final version of the manuscript for submission. The authors would like to thank Dr. Joshua Correll, Dr. E. Ashby Plant, Dr. Jack Glaser and Dr. Sang Hee Park for their cooperation and immediate willingness to retrieve datasets for inclusion in these analyses.

* Corresponding author at: Psychology Department, University of Illinois Urbana-Champaign, 603 East Daniel Street, Champaign, IL 61820, United States.
E-mail address: yaramekawi@gmail.com (Y. Mekawi).

http://dx.doi.org/10.1016/j.jesp.2015.08.002
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racial groups, using different stimuli, and identifying various individual differences in shooting decisions. This shooter bias has been operationalized in a number of ways (e.g., reaction time, shooting threshold), with mixed results both within and across studies and operationalizations. Given the importance of the topic and the mixed results in the literature, we conducted a meta-analysis of shooting task studies to synthesize the research about how target race affects shooting decisions.

A cursory reading of the literature would suggest that the evidence for a racial shooter bias is quite strong, as all published papers on the topic have significant results suggesting a bias against Black (versus White) targets. Upon further analysis, however, it becomes clear that researchers do not operationalize shooter biases in the same way across studies and null and significant results exist for each definition. For example, some studies have found that participants are quicker to shoot armed Black (versus White) targets and slower to not shoot unarmed Black (versus White) targets (Correll et al., 2002; Correll, Park, Judd, & Wittenbrink, 2007; Park, Glaser, & Knowles, 2008; Sadler, Correll, Park, & Judd, 2012), while others have not found reaction time differences (Harmer, 2012; Taylor, 2011). More relevant to shootings of unarmed individuals, some studies have focused on trials where participants shoot when no gun is present (i.e., false alarms). Some of these studies have shown that participants are more likely to shoot unarmed Black (versus White) targets (e.g., Plant & Peruche, 2005) while others have not (e.g., Sadler et al., 2012). However, one limitation to this type of operationalization is that it ignores correct trials. Participants who make a large amount of errors may have a tendency to respond to shoot (i.e., bias to respond).

Hence, to understand racial biases in shooting decisions, some researchers have used signal detection theory to estimate two parameters representing decision-making processes. The first parameter, called d' or sensitivity, represents the strength of the signal relative to noise, with larger values indicating a stronger signal (i.e., more sensitive). Research has not consistently found differences based on target race when looking at shooting sensitivity (e.g., Correll et al., 2002). Another parameter, c, denotes the threshold for responding. A value of zero signifies an observer who evenly balances responding (i.e., shoot, don't shoot). Participants who have a negative value have a bias toward shooting (i.e., liberal threshold), and participants with a positive value have a bias toward not shooting (i.e., conservative threshold). Some previous studies have found that participants have a more liberal shooting threshold (i.e., bias to shoot) for Black (versus White) targets (e.g., Correll et al., 2002), whereas others have not (e.g., Taylor, 2011).

Given the many operationalizations of shooter biases and inconsistencies in which operationalizations are reported across studies, the results from this paradigm are difficult to interpret. Of particular concern is the possibility that researcher degrees of freedom in how to operationalize shooter bias increases the likelihood of false positive research findings (Simmons, Nelson, & Simonsohn, 2011). For instance, if a researcher analyzes all operationalizations of shooter bias, there is a higher chance of a Type I error than if they only analyzed one. This, combined with selective reporting of operationalizations (i.e., those that have significant results), could make the results from this paradigm appear more robust than they really are. It should be noted that it is possible that some operationalizations may have true effect, whereas others might not. Therefore, we sought to determine the effect size for each operationalization of shooter bias across studies.

Inconsistent results across studies might also be explained by variables that differ between the studies. To better explain variance in shooter biases across studies, we were also interested in investigating the role of three relevant variables that may moderate racial shooter biases: racial heterogeneity of community, strictness of gun laws, and prejudicial attitudes.

One moderator that has been identified in shooting task studies is quantity of interaction with racial out-groups. Some studies found a negative association between intergroup contact and relative reaction time (Correll et al., 2002; Correll et al., 2007), suggesting that more contact is associated with a quicker decision to shoot Black targets. This is consistent with criminology research, which finds a positive association between proportion of Black individuals in a neighborhood and Whites' fear of crime (Chiricos, Hogan, & Gertz, 1997). Others have found a positive association between intergroup contact and shooting threshold for White targets, suggesting that the more contact with out-group members, the more conservative they were in their shooting threshold for their own group (Kenworthy et al., 2011). However, Kenworthy et al. (2011) did not find a correlation between shooting threshold for Black targets and contact with Blacks. Thus, it is unclear whether the effect is unreliable, or whether it differs based on the shooter bias outcome analyzed. To clarify the role of inter-group contact in moderating shooter biases, we used percentage of non-Whites in the community where the data was collected as a proxy for inter-group contact.

Second, prior research has found that permissive (vs. restrictive) gun laws (e.g., less government regulation) are associated with more extrajudicial shootings, with Black victims disproportionately affected (Miller, Hemenway, & Azrael, 2007; Price, Thompson, & Dake, 2004). The culture of gun ownership may affect how careful people are in making shooting decisions in a way that affects racial and ethnic minorities more than Whites. People living in states with more permissive gun laws are more likely to own and use guns and therefore may be less inhibited in their shooting decisions. Thus, we examined whether the strictness of gun laws in the states where participants were recruited moderated racial shooter biases.

Finally, endorsement of prejudicial attitudes was frequently assessed in shooting task studies. It is possible that to the extent that an individual is prejudiced toward Blacks, he or she is more likely to have a shooter bias toward Black (versus White) targets. Despite how intuitive it may seem that prejudicial attitudes play a role in shooter biases, results have been largely inconsistent (e.g., Correll et al., 2002, 2007). Given that studies varied in other ways, we also examined a number of methodological differences between studies (e.g., response time window, sample type).

This meta-analysis was conducted to answer the following questions:

(1) What are the overall effects of shooter biases for all operationalizations of shooter biases (reaction time with/without gun, error rate, shooting sensitivity, shooting threshold)?

(2) Does racial heterogeneity of community moderate shooter biases?

(3) Does the strictness of gun laws moderate shooter biases?

(4) How strong is the association between prejudicial attitudes and shooter biases?

1. Method

1.1. Study inclusion

Studies were identified by searching the words “shooting bias” and “shooter bias” using PsychINFO and Google Scholar. We also examined papers that had cited Correll et al.'s (2002) original study examining shooter biases. Initially, papers were included if they were deemed relevant based on a review of the title and brief review of the abstract. Papers were excluded at this stage if they clearly did not involve a racial shooter bias task. This search identified 35 papers.

The 35 papers were reviewed closely for inclusion in the meta-analysis. To be considered for the meta-analysis, papers had to include a) some variant of the Correll et al. (2002) shooting task,

b) a comparison of White male targets to Black male targets[1], and c) sufficient information to calculate effect sizes for at least one operationalization of shooter bias was included in the paper. We contacted authors when relevant data were missing, and 90% responded with the requested data. We also unsuccessfully attempted to elicit unpublished data. However, we did include unpublished dissertations as an attempt to detect and counteract the possibility of publication bias (Ferguson & Brannick, 2012). The final analysis included 16 papers with a total of 42 different studies or samples ($N = 3427$).

1.2. Coding

M's and *SD*'s for reaction time with/without a gun, false alarms, shooting sensitivity, and shooting threshold were coded and used to calculate Cohen's d_{av} (Lakens, 2013). We calculated the variance of d_{av} using the equation provided by Dunlap, Cortina, Vaslow, and Burke (1996), which takes into account the correlation between dependent measures. Given that no study reported the necessary correlations, we used the correlations based on our data (Mekawi, Bresin, & Hunter, in press; $r = .7$ for RT, $r = .8$ for false alarms, $r = .7$ for d', and $r = .2$ for c). A small number of studies used between-subject manipulations to examine their effects on the shooter bias (e.g., reading an article about a Black or White person committing a crime; Correll et al., 2007). Unfortunately, there were too few studies to combine them into meaningful categories for calculating effect sizes to compare against the normative effect. This, combined with our interested in the shooter bias, in the absence of other factors, led us to just code the control condition for these studies.

All effect sizes were calculated by subtracting White target trials from Black target trials. The implication of this is that different operationalizations of shooter bias should have different signs (i.e., positive or negative effect). For reaction time to gun trials, a racial bias would be indicated by a *negative* effect size, indicating faster decision making for Black targets with guns relative to White targets with guns. For reaction time to no gun trials, a *positive* effect size would be indicative of a bias against Black relative to White targets (i.e., slower to identify a Black target without a gun). Similarly, a positive effect size for false alarms would indicate a bias against Black relative to White targets (i.e., more false alarms for Black relative to White targets). There is no clear prediction for the direction of the shooting sensitivity effect. Finally, a *negative* effect size for shooting threshold would indicate a lower threshold for shooting Black (versus White) targets. To summarize, positive effect sizes would be expected for reaction time to no gun trials and false alarms, whereas negative effect sizes would be expected for reaction time to gun trials and shooting threshold.

We coded multiple demographic variables to characterize the studies' samples (i.e., age, % of women, % of White participants, % of Asian participants, % of Black participants, % of Latino/a participants). Table 1 displays the racial demographics of participants across the studies. We also coded sample type into three groups: undergraduates, community members, and police officers/cadets. In terms of methodology, we coded the response time window for reaction times, as it has been suggested that shorter times lead to smaller reaction time effects and larger false alarm effects (Correll et al., 2002; Plant, Peruche, & Butz, 2005). We also coded whether the study used Correll et al.'s (2002) materials, which depict full body images with an environmental background, or stimuli that depicted faces with objects (e.g., gun or soda can) superimposed (e.g., Plant & Peruche, 2005).

We also coded the correlation between shooter bias and individual differences measures that were reported in five or more studies (in order to have enough studies to pool effect sizes), namely awareness of discrimination against Blacks (Wittenbrink, Judd, & Park, 1997), personal endorsement of stereotypes, awareness of cultural stereotypes (e.g., Correll et al., 2002), implicit and explicit motivation to control prejudice (Dunton & Fazio, 1997), and contact with Blacks (e.g., Correll et al., 2002). In all but one study (Kenworthy et al. 2011, who used a difference score of c) the shooter bias was parameterized as a difference score of reaction time, with larger values indicating a larger shooter bias.

To code the strictness of gun laws, we used the 2013 Brady Campaign State Scorecard (Brady Campaign to Prevent Gun Violence, 2013). In this report, each state is given a score based on multiple categories of gun laws (e.g., background checks, firearms in public places, gun owner accountability) with scores ranging from 0 (the most permissive gun laws) to 100 (the most restrictive). For example, in the sub-category of "dealer regulation", a state that requires firearms dealers to be licensed would likely receive a score of "6" whereas a state that does not require licensure would likely receive a score of "0". As another example, in the sub-category of background checks, states that establish categories of persons deemed ineligible to purchase or possess firearms (e.g., history of serious mental illness) would receive a score of "5," whereas states without these categories would likely receive a score of "0". The range of our sample (17–75.5) was similar to that of the United States as a whole (6–89), though somewhat more constricted. Only two studies did not specifically indicate the city where the data was collected, and thus the first authors of those papers were contacted to confirm the data collection site. Two studies used samples of police officers from multiple states; thus these two studies were not used in analyses using state level variables.

Table 1
Study characteristics for studies included in the meta-analysis.

	M	SD	Range
% Women ($k = 39$)	48.89	22.89	39–100
% White ($k = 39$)	67.64	22.73	7.25–100
% Black ($k = 34$)	9.74	18.08	0–100
% Asian ($k = 36$)	7.71	11.07	0–100
% Latino/a ($k = 20$)	10.03	12.28	0–49.6

Note. k = number of studies reporting relevant information.

[1] For completeness, we also examined out-groups other than Blacks that have been included in studies using the shooing task. The results for White women compared with White men as targets ($k = 4$) suggested a bias against shooting women. Compared with targets who are men, participants were slower to shoot armed women ($d_{av} = .25$, 95% CI [.17, .32]), slower to not shoot unarmed women ($d_{av} = .99$, 95% CI [.50, 1.48]), made less errors for unarmed targets ($d_{av} = -.26$, 95% CI [−.40, −.12]), and had a higher shooting threshold ($d_{av} = .40$, 95% CI [.24, .67]). However, the effect for shooting sensitivity was not significantly different from zero ($d_{av} = .03$, 95% CI [−.14, .21]).
For Asian relative to White targets ($k = 4$), the effect was not significantly different from zero for reaction time on gun trials ($d_{av} = -.55$, 95% CI [−1.20, .10]), reaction time on no gun trials ($d_{av} = -.07$, 95% CI [−.12, .27]), or shooting threshold ($d_{av} = .25$, 95% CI [−.15, .65]). There was a significant effect for false alarms ($d_{av} = .35$, 95% CI [.05, .65]) and sensitivity ($d_{av} = -.13$, 95% CI [−.20, −.06]), indicating that relative to White targets, participants are more likely to make errors and have less sensitivity for Asian targets.
For Latino targets ($k = 2$), the two studies showed significant effects for all three operationalizations of shooter bias reported. Participants were faster to shoot armed Latino targets ($d_{av} = -.66$, 95% CI [−1.20, −.11]) and unarmed Latino targets ($d_{av} = -.47$, 95% CI [−.82, −.12]) relative to White targets. Moreover, in comparison with White targets, participants had more sensitivity for Latino targets ($d_{av} = .35$, 95% CI [.24, .46]).
Only two studies examined Muslim/Middle Eastern targets with the same operationalization of shooter bias (i.e., shooting threshold). The results showed a lower shooting threshold for Muslim or Middle Eastern targets relative to White targets ($d_{av} = -.66$, 95% CI [−1.20, −.11]).
Overall comparison of groups is difficult due to missing data for all operationalizations of shooter biases. While no clear pattern emerges for all out-groups, participants seem to have a relatively lower threshold for groups stereotyped as being dangerous (men, Muslim or Middle Easterners, but not Asians) though other operationalizations of shooter biases do not follow this pattern (e.g., participants were less sensitive to shooting Asians but more sensitive to shooting Latinos). Further research is needed to clarify how generalizable various shooter biases are to other out-groups.

The percentage of non-Whites was coded from the US and/or Canada census data for the most recent census.[2] This allowed us to use the data from all studies, regardless of whether researchers assessed participants' frequency of intergroup contact. This was calculated based on the city where the data collection occurred. The average percentage of non-Whites (M = 24.43%, SD = 12.46) was slightly lower than the national average (36.3%). Table 2 displays the values of the main moderators for each study.

1.3. Data analysis

Data were analyzed using the Metafor package in R (R Development Core Team, 2010; Viechtbauer, 2010). We used a mixed effects model with restricted maximum likelihood estimation for parameters. Study effects were weighted by sample size. For individual effect sizes and parameters, we report 95% confidence intervals. To test for heterogeneity in effect sizes, we calculated the Q statistic, which tests the null hypothesis of no heterogeneity. First, we tested the average effect for each operationalization of shooter bias. Second, for effects with significant heterogeneity, we conducted our primary moderator analyses by including gun law score and percentage of non-Whites in separate analyses. Third, we explored whether study characteristics or methodological considerations moderated effect size. Fourth, we looked at the results for studies correlating the shooter bias to self-report measures. Finally, we checked for publication bias using the tandem procedure proposed by Ferguson and Brannick (2012). This procedure consists of calculating the fail-safe-N (i.e., the number of missing studies to make the effect not significant), the rank order correlation of funnel plot asymmetry, and the trim-and-fill procedure.

2. Results

2.1. Overall effects

2.1.1. Reaction time for gun trials

The effect size for reaction time for gun trials was significantly different from zero and in the expected negative direction (k = 32, d_{av} = −.13, 95% CI [−.19, −.06]). This suggests that across all studies, participants were faster to shoot armed Black targets relative to armed White targets. There was significant heterogeneity in this effect, $Q(31)$ = 159.72, p < .001. The top panel in Fig. 1 displays the forest plot for this effect.

2.1.2. Reaction time for no gun trials

The effect size for reaction time for no gun trials was also significant and in the expected positive direction (k = 32, d_{av} = .11, 95% CI [.05, .18]). This suggests that across all studies, participants were slower to not shoot unarmed Black targets relative to unarmed White targets. Again, there was significant heterogeneity in this effect, $Q(31)$ = 150.57, p < .001. The bottom panel of Fig. 1 displays the forest plot for this effect.

2.1.3. False alarms

The effect size for false alarm differences was not significant (k = 28. d_{av} = −.01, 95% CI [−.11, .09]). This suggests that across all studies, the rate of false alarms for shooting unarmed targets was not different for Black and White targets. There was significant heterogeneity in this effect, $Q(27)$ = 553.18, p < .001. The forest plot for this effect is shown in Fig. 2.

2.1.4. Shooting sensitivity (d')

The effect for shooting sensitivity was not significant (k = 30, d_{av} = .07, 95% CI [−.01, .15]). This suggests that across all studies, participants' shooting sensitivity was not different for Black and White targets. Again, there was significant heterogeneity in this effect, $Q(29)$ = 162.74, p < .001. The top panel of Fig. 3 shows the forest plot of this effect.

2.1.5. Shooting threshold (c)

The effect for shooting threshold was significant and in the expected negative direction (k = 29, d_{av} = −.19, 95% CI [−.37, −.01]), suggesting that across all studies, participants had a lower shooting threshold (i.e., bias to shoot) for Black targets relative to White targets. As with the other effects, there was significant heterogeneity $Q(28)$ = 404.30, p < .001. The bottom in Fig. 3 shows the forest plot for this effect.

These data lead us to conclude that there are no overall effects of shooting sensitivity or false alarm rate based on target race, but that there are small negative effects for reaction time for reaction time to armed targets and shooting threshold and a small positive effect for reaction time to unarmed targets. Relative to White targets, participants were quicker to shoot armed Black targets, slower to not shoot unarmed Black targets, and more likely to have a liberal shooting threshold for Black targets.

2.2. Main moderators

2.2.1. Gun laws

We found that the strictness of gun laws significantly moderated the effect size for two operationalizations: false alarms and shooting threshold. For false alarms, the relation was negative (b = −.007, CI [−.010, −.004], k = 28), and explained 45% of the variance across studies. To interpret this effect, we calculated estimated effect sizes for − 1 SD and + 1 SD from the mean. In states with more permissive gun laws (− 1 SD) there was a small positive effect (d_{av} = .20, 95% CI [.08, .32]; more false alarms for Black targets), whereas in states with relatively restrictive gun laws, the effect was negative (d_{av} = −.17, 95% CI [−.06, −.57]; less false alarms for Black targets), suggesting greater bias in states with more permissive gun laws. For shooting threshold, the effect was positive (b = .01, CI [.005, .017], k = 28), indicating that in states with restrictive (versus permissive) gun laws, the effect size was small and non-significant (d_{av} = .07, 95% CI [−.13, .28]) compared with states with more permissive gun laws (d_{av} = −.54, 95% CI [−.77, −.30]), where the effect was medium to large and negative (i.e., lower threshold for Black targets). This explained 32% of the heterogeneity in effect sizes. Gun law strictness did not moderate the effects of shooting sensitivity (k = 30. p = .169), reaction time for gun trials (k = 32, p = .108), or reaction time for no gun trials (k = 32, p = .354). These data suggest that in states with more permissive (versus restrictive) gun laws, the false alarm rate for shooting Black (versus White) targets is higher, and the shooting threshold for shooting Black (versus White) targets is lower. Consistent with our predictions, stricter gun laws were associated with less shooter biases toward Black targets relative to White targets.

To rule out the hypothesis that this effect could be explained by state-level political orientation, we adjusted for political orientation of the state. We operationalized state-level political orientation by using the percentage of the popular vote in the 2012 presidential election for the liberal candidate (i.e., Barack Obama) for studies in the United States and popular vote by province for the 2006 Prime Minister

[2] Based on a reviewer suggestion, we compared studies conducted in Canada to those conducted in the United States. In these analyses, we found we found evidence of moderation for all operationalizations aside from shooting sensitivity (p = .906). For both reaction time types the effects from studies conducted in Canada were small and not significant (Gun trials: d_{av} =.04 [-.06, .15], k = 9; No gun trials: d_{av} = −.03 [−.15, .08], k = 9); however, the effect for studies conducted in the United States were significant and in the expected direction (Gun trials: d_{av} = −.19 [−.26, −.13], k = 23; no gun trials: d_{av} = .17 [.10, .24] , k = 23). For false alarms, there was no effect for the studies in conducted in United States (d_{av} = 08 [−.03, .19], k = 20) and there was a significant positive effect studies conducted in Canada (d_{av} = −.20 [−.33, −.03], k = 8), which is evidence of a bias to make more errors for White versus Black targets. Finally, for shooting threshold there was a complete reversal. For studies conducted in the United States there was a significant negative effect (d_{av} = −.42 [−.60, −.23], k = 19) and for studies conducted in Canada, there was a significant positive effect (d_{av} = .25 [−.01, .50], k = 10), suggesting a bias toward lower thresholds for Black versus White targets in the United States and a bias for higher thresholds for Black versus White targets in Canada.

Table 2
Study characteristics for key moderator variables.

Authors (year)	N	Age	Gun laws	% non-White	RW	Stim type	Sample type
Akinola and Mendes (2011)	78	41.2	74.5	33.4	850	Correll	Police Officers
Correll et al. (2002) S1	40		28.5	12	850	Correll	Undergraduates
Correll et al. (2002) S2	44		28.5	12	630	Correll	Undergraduates
Correll et al. (2002) S3	48		28.5	12	850	Correll	Undergraduates
Correll et al. (2002) S4 BP	25		28.5	31.1	850	Correll	Community
Correll et al. (2002) S4 WP	21		28.5	31.1	850	Correll	Community
Correll et al. (2007) S1 NO*	113	38.4			850	Correll	Police Officers
Correll et al. (2007) S1 DO	124	37.9	28.5	31.1	850	Correll	Police Officers
Correll et al. (2007) S1 Com	127	35.5	28.5	31.1	850	Correll	Community
Correll et al. (2007) S2 DO	31	35.6	28.5	31.1	630	Correll	Police Officers
Correll et al. (2007) S2 Com	45	36.8	28.5	31.1	630	Correll	Community
Correll, Wittenbrink, Park, Judd, and Goyle (2011)	55	18.87	59	55	630	Correll	Undergraduates
Harmer (2012)	89	20.16	75.5	16.2	630	Correll	Undergraduates
Hunsinger (2011) S1	214	20.21	74.5	21.1	700	Correll	Undergraduates
Husinger (2010) S2	180	20.03	74.5	21.1	700	Correll	Undergraduates
Hunsinger (2011) S3	72	20.31	74.5	21.1	700	Correll	Undergraduates
Mekawi et al. (in press)	290	19.21	32.2	59	1000	Plant	Undergraduates
Miller, Zielaskowski, and Plant (2012) S2	50	19.5	17	42.6	630	Plant	Undergraduates
Musolino (2012)	123	23	75.5	16	630	Correll	Undergraduates
Park et al. (2008)	58		81.27	40.5	None	Correll	Undergraduates
Park and Glaser (2011) CG	27		81.27	40.5	None	Correll	Undergraduates
Plant and Peruche (2005)	50		17	39.9	630	Plant	Police Officers
Plant et al. (2011) Study 2	122		17	42.6	630	Plant	Undergraduates
Plant et al. (2005) S1 ET	125	19	17	42.6	630	Plant	Undergraduates
Plant et al. (2005) S2 CG	60	19	17	42.6	630	Plant	Undergraduates
Plant et al. (2005) S3 ET	61	18.64	17	42.6	630	Plant	Undergraduates
Sadler et al. (2012) S1	69		28.5	12	850	Correll	Undergraduates
Sadler et al. (2012) S2*	224				850	Correll	Police Officers
Taylor (2011) S1 PR	47	27.72	75.5	16	630	Correll	Police Officers
Taylor (2011) S1 PO	49	38.63	75.5	16	630	Correll	Police Officers
Taylor (2011) S1 UG	50	20.84	75.5	16	630	Correll	Undergraduates
Taylor (2011) S1b	74	21.82	75.5	16	630	Correll	Undergraduates
Taylor (2011) S2 PR	50	26.94	75.5	16	630	Correll	Police Officers
Taylor (2011) S2 PO	30	39.90	75.5	16	630	Correll	Police Officers
Taylor (2011) S2 UG	50	20.29	75.5	16	630	Correll	Undergraduates

Note. RS = response window, Stim type = stimulus type, S = study, BP = Black participants, WP = White participants, Com = community, DO = Denver officers, NO = national officers, CP = control participants, ET = early trials, PO = police officers, PR = police recruits, UG = undergraduates, Correll = full body images on backgrounds based on Correll et al. (2002), Plant = images of faces with objects superimposed based on Plant and Peruche (2005), Gun laws = Brady Scorecard score, where larger (relative to smaller) numbers indicate more restrictive gun laws; % non-White = % of non-White in the city where the study was conducted * because officers were from multiple states, gun laws and % non-White could not be calculated.

elections in Canada. When this was included in the model, the effects for false alarms ($b = -.008$ [−.01, −.006], $k = 19$) and shooting threshold ($b = .012$ [.007, .018], $k = 27$) were still significant and largely unchanged in magnitude. Surprisingly, in both cases, percentage of the popular vote for liberal candidates had the opposite relation with effect sizes compared with the gun law effects. For false alarms, higher percentage of liberal voting was related to more errors for Black versus White targets, $b = .007$ [−.000, .015], $k = 19$. For shooting threshold, higher percentage of liberal voting was related to lower shooting thresholds for Black versus White targets, $b = -.023$ [−.042, −.004], $k = 27$. Although these results are counterintuitive, there is likely an issue of restriction of range, given that the liberal candidate won the electoral votes for the United States elections (for studies conducted in the United States: $M = 52\%$, $min = 50\%$, $max = 61\%$; for studies collected in Canada: $M = min = max = 39\%$). It is possible that with more variability in liberal voting a more intuitive association would be found. More importantly, these results suggest that state-level political orientation does not account for the association between gun laws and effect sizes of shooter biases.

2.2.2. Proportion of non-Whites

We found that the proportion of non-Whites in the city where the sample was recruited moderated the effect for shooting threshold ($b = -.015$, CI [−.028, −.002], $k = 28$), which explained 14% of the heterogeneity. The negative slope indicates that in cities with a greater proportion of non-Whites, there was a larger shooter bias against Black versus White targets. For example, cities one *SD* below the mean had no significant difference between Black and White targets, ($d_{av} = .03$, 95% CI [−.23, .30]); however, in cities one *SD* above the mean there was a significant effect with lower thresholds for Black versus White targets ($d_{av} = -.38$, 95% CI [−.62, −.14]), suggesting that more racially diverse cities had more shooter bias. The proportion of non-Whites only marginally moderated the false alarm rate ($b = .006$, CI [−.001, .013], $k = 28$), which explained only 6% of the variance. This effect was positive, indicating that proportion of non-Whites was related to a larger bias (e.g., −1 *SD*: $d_{av} = -.09$, 95% CI [−.24, .04]; +1 *SD*: $d_{av} = .07$, 95% CI [−.06, .21]). The proportion of non-Whites did not moderate the effects of shooting sensitivity ($k = 29$, $p = .509$), reaction time for gun trials ($k = 31$, $p = .481$), or reaction time for no gun trials ($k = 31$, $p = .367$). The results suggested that data collected in a city with a greater proportion of non-Whites had larger shooter biases in terms of shooting threshold (lower shooting threshold for Black versus White targets) and false alarm rate (more errors for Black versus White targets).

2.3. Methodological moderators

2.3.1. Response time window

Given the large number of studies using 630 milliseconds as the response window ($k = 22$), we contrasted this with all other windows (ranging from 700 milliseconds to no time limit). We found that response window moderated the effect size for reaction time for both gun ($b = .10$, CI [.05, .16]) and no gun trials ($b = -.12$, CI [−.17, −.06]). For gun trials, the effect size was significant when the window was larger than 630 milliseconds ($k = 15$, $d_{av} = -.24$, 95% CI [−.32, −.16]), but not when the window was 630 milliseconds ($k = 16$, $d_{av} = -.02$, 95% CI [−.10, .06]). Similarly, for no gun trials, the effect was significant for longer windows ($k = 16$, $d_{av} = .24$, 95% CI [.16,

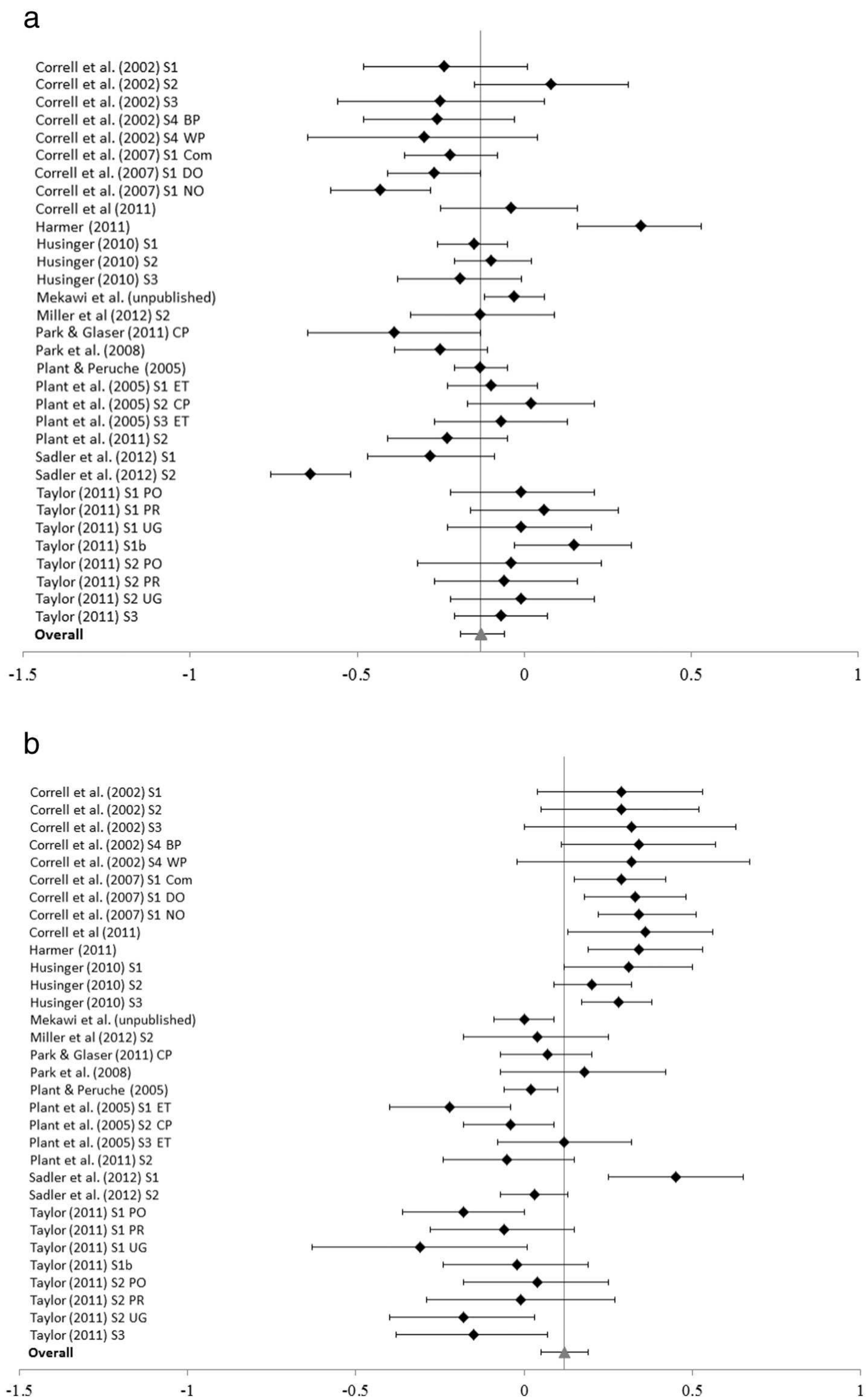

Fig. 1. Forest plot for reaction time to gun trials (top panel) and no gun trials (bottom panel). For gun trials, a negative effect was expected (faster to shoot armed Black versus White targets). For no gun trials, a positive effect was expected (slower to not shoot unarmed Black versus White Targets). Note that S = study, BP = Black participants, WP = White participants, Com = community, DO = Denver officers, NO = national officers, CP = control participants, ET = early trials, PO = police officers, PR = police recruits, and UG = undergraduates.

.31]), but not for the 630 millisecond window ($k = 15$, $d_{av} = -.01$, 95% CI [−.04, .09]). The effect of response time window was not significant for false alarm rate ($k = 28$, $p = .915$), shooting sensitivity ($k = 30$, $p = .410$), or shooting threshold ($k = 28$, $p = .122$). These results make sense given that shorter response windows reduce variability in reaction times and diminish the ability to detect significant reaction time effects. However, it does not appear that shorter response windows differentially increase the number of errors for Black versus White targets as has been proposed in the literature (e.g., Correll et al., 2002).

2.3.2. Stimulus type

We found that in terms of reaction time, the stimulus type only marginally moderated the effect size for gun trials ($b = -.06$, CI [−.13, .01], $k = 32$) and no gun trials ($b = .06$, CI [−.01, .13], $k = 32$). Correll et al.'s (2002) task depicts full body images of Black and White

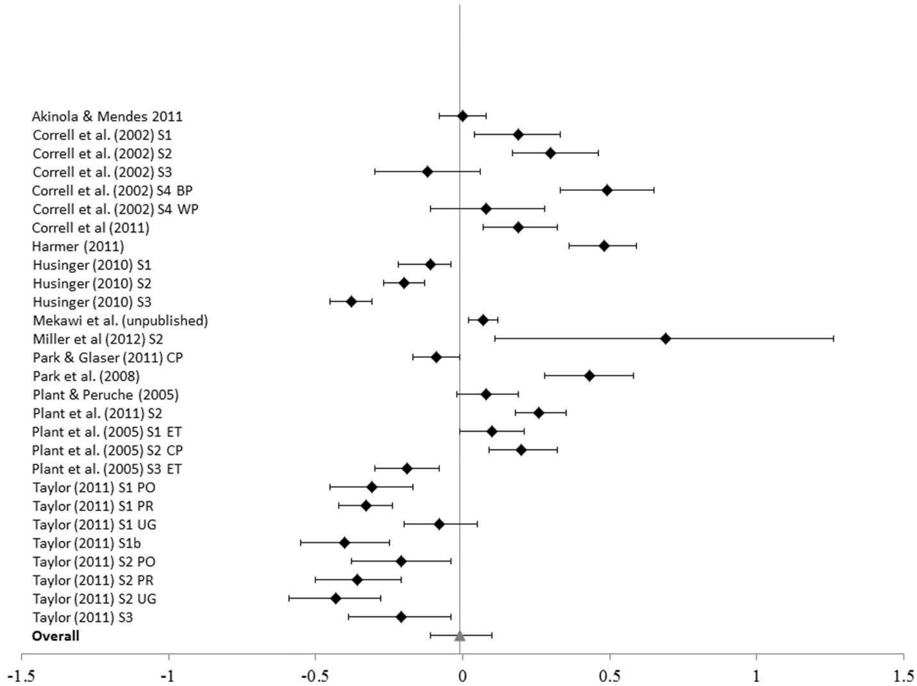

Fig. 2. Forest plot for false alarms. A positive effect was expected (more errors for shooting unarmed Black versus White targets). Note that S = study, BP = Black participants, WP = White participants, CP = control participants, ET = early trials, PO = police officers, PR = police recruits, and UG = undergraduates.

men with an environmental background, holding either a gun or a neutral object (e.g., cell phone). Plant and Peruche's (2005) task depicts Black and White faces with a gun or neutral object superimposed. Given that many of the studies using Correll et al.'s (2002) task had a shorter response time window, we adjusted for response window and in both cases the effect of stimulus was no longer significant ($p > .74$). Moreover, the effect of response window remained significant. In terms of false alarm rates, the effect of stimulus type significantly moderated the effect ($b = -.18$, CI [$-.27$, $-.08$], $k = 28$). The effect size for studies using Plant and Peruche's (2005) materials was positive ($k = 8$, $d_{av} = .25$, CI [.09, .42]), suggesting that there were more false alarms for Black versus White targets, whereas for studies using Correll et al. (2002)'s materials the effect size was negative ($k = 20$, $d_{av} = -.11$ CI [$-.21$, $-.01$]). This effect was still significant when adjusting for response time window. Stimulus type significantly moderated the effect for shooting sensitivity ($b = .09$, CI [.01, .17], $k = 30$). For studies using Plant and Peruche's (2005) materials, there was no effect for shooting sensitivity ($d_{av} = .06$, CI [$-.21$, .08], $k = 9$) whereas for studies using Correll et al.'s (2002) materials, there was a significant effect ($d_{av} = .12$, CI [.03, .21], $k = 21$), suggesting greater sensitivity for shooting Black vs. White targets in these studies. Finally, stimulus type did not moderate the effect of shooting threshold ($k = 29$, $p = .346$). In sum, there does not appear to be an overall moderating effect of stimulus type across different operationalizations of shooter biases. The effect sizes of shooting threshold biases (and to some extent reaction time) were, however, the most reliable across stimuli, possibly suggesting that they should be the main focus of investigations.

2.3.3. Sample type

To examine whether the sample type moderated the effect size we used two effect coded variables: one that contrasted undergraduates with police officers and recruits, the other contrasting community members with police officers and recruits. For all operationalizations of shooter bias, adding these two variables to the model did not significantly improve the fit (Q[2] ranged from .30–4.31, p's ranged from .115–.859). Based on suggestions from a reviewer, we also conducted a follow-up analysis where undergraduates and community members were compared with police officers. In this case, the effect of sample type was also not significant for any operationalization of shooter bias (p's ranged from .125–.942). Thus, it does not appear that police officers, community members, and undergraduates have different shooter biases in general.

2.3.4. Comparison of moderators

Given that for some operationalizations of shooter bias, there were multiple significant moderators, we conducted follow-up analyses entering all moderators that were significant when entered individually into one model. As noted above, for both reaction time measures, response window was the only significant moderator when both stimulus type and response window were entered into the model. For shooting sensitivity, there was only one significant moderator therefore no further analyses were needed.

For false alarms, when all three significant moderators were added to the model, neither percentage of non-White ($b = -.001$, 95% CI [$-.008$, .005]), nor stimulus type ($b = -.09$, 95% CI [$-.22$, .03]) were still significant. However, strictness of gun laws was still significantly related to effect size ($b = -.005$, 95% CI [$-.009$, $-.001$]. For states with permissive gun laws, the effect was positive ($d_{av} = .20$, 95% CI [.05, .35]) suggesting more false alarms for Black (versus White targets), whereas for states with strict gun laws the effect was not significantly different from zero ($d_{av} = -.08$, 95% CI [$-.24$, .07]). These results possibly suggest that the association between percentage of non-White/stimulus type and effect size were due to shared variance with gun laws (percentage of non-Whites: ($r = -.42$, 95% CI [$-.65$, $-.11$]; stimulus type: $r = -.41$, 95% CI [$-.65$, $-.09$]). However it is also possible that the more comprehensive model reduced statistical power. Similar to the results for false alarms, the results for shooting threshold showed that gun laws ($b = .010$, 95% CI [.003, .017]), but not percentage of non-Whites ($b = -.003$, 95% CI [$-.017$, .011]), was related to effect size. In states with permissive gun laws (-1 SD), the effect size was medium in size and in the direction of lower thresholds for Black versus White targets ($d_{av} = -.49$, 95% CI [$-.81$, $-.16$]). In states, with strict gun laws ($+1$ SD) the effect was not significantly different from zero ($d_{av} = .07$, 95% CI [$-.14$, .28]). Again these results could indicate shared variance or changes in statistical power.

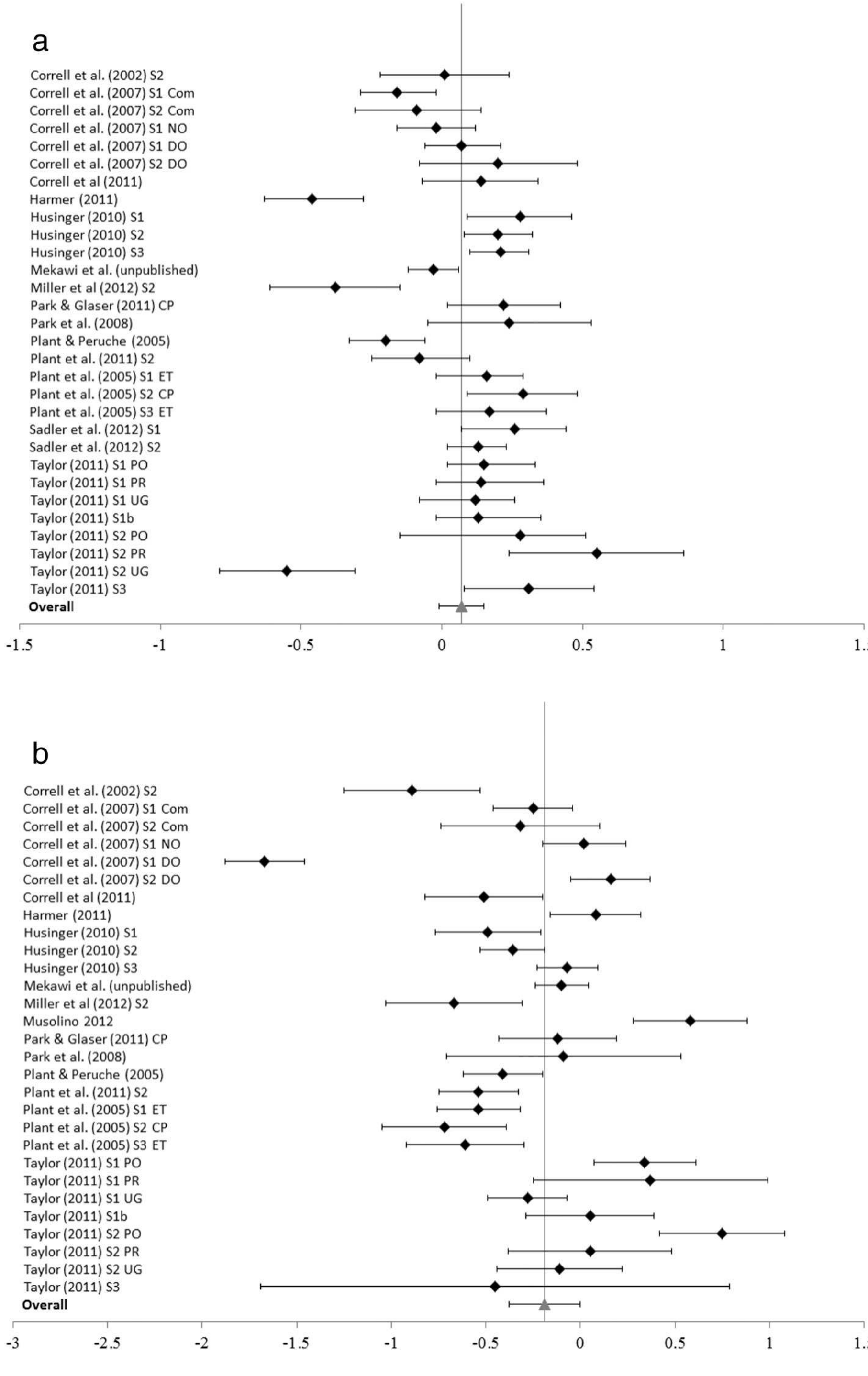

Fig. 3. Forest plots for shooting sensitivity (top panel) and shooting threshold (bottom panel). There was no directional prediction for shooting sensitivity. For shooting threshold, a negative effect was expected (lower threshold for deciding to shoot Black versus White targets). Note that S = Study, BP = Black participants, WP = White participants, Com = community, DO = Denver officers, NO = national officers, CP = control participants, ET = early trials, PO = police officers, PR = police recruits, and UG = undergraduates.

2.3.5. Correlation results

We coded the correlation between shooter bias (in all cases but one operationalized as a difference in reaction time) and six different individual difference measures. The relation between shooter bias and awareness of discrimination against Blacks was small and not significantly different from zero ($r = .03$, 95% CI [−.06, .14], $k = 5$). Personal endorsement of stereotypes ($r = .08$, 95% CI [.01, .15], $k = 6$) but not knowledge of cultural stereotypes ($r = .06$, 95% CI [−.01, .13], $k = 7$) had a small significant relation with shooter bias, such that greater endorsement of stereotypes was related to more shooter bias. Neither implicit ($r = -.02$, 95% CI [−.11, .06], $k = 5$) nor explicit motivation to control prejudice ($r = .03$, 95% CI [−.10, .17], $k = 5$), had a significant relation with shooter bias. Finally, consistent with the results for percentage of non-Whites in the community, there was a significant positive relation between shooter bias and contact with out-groups ($r = .14$, 95% CI [.04, .23], $k = 6$), suggesting that people who had more contact with Blacks had a larger shooter bias. None of these effects had significant heterogeneity (p's ranged from .13–.62), aside from explicit control of prejudice, which had marginal heterogeneity, $Q(4) = 9.17$ $p = .05$. Given that these effects are based on a small number of studies (k's range from 5 to 7), these results should be interpreted with caution. With that in mind, the results seem to suggest that prejudicial attitudes, at best, have very small relations with shooter biases.

2.3.6. Publication bias

We assessed for publication bias in four ways. First, we calculated the fail-safe-N using three different methods for the operationalizations of shooter bias that were significant (i.e., gun trials, no gun trials, and shooting threshold). All methods (Rosenthal, Orwin, and Rosenberg) suggested that there was no effect of publication bias (N's ranged from 392 to 956) on the gun trials effect. Similarly, there was no effect of publication bias on the no gun trials effect (N's ranged from 360 to 635) or shooting threshold (N's ranged from 514 to 758). Second, we calculated the rank order correlation of funnel plot asymmetry and found no evidence for publication bias for the gun trials effect ($\tau = .02$, $p = .884$), no gun trials effect ($\tau = .03$, $p = .809$), or shooting threshold ($\tau = .04$, $p = .749$). Third, we used the trim-and-fill procedure, which suggested that there were no missing studies. Finally, given that we included many unpublished dissertations (41% of our studies), we compared the effect size of published and unpublished studies (i.e., dissertation studies). We found that the effects were larger for published studies for the included operationalizations of shooter biases. For gun trials, the effect size for published studies was −.20 (CI: [−.32, −.08], $k = 20$) and for unpublished studies was −.01 (CI: [−.10, .08], $k = 12$). For no gun trials, the effect size for published studies was .16 (CI: [.02, .29], $k = 20$) and for unpublished studies it was .04 (CI: [−.06, .15], $k = 12$). For shooting threshold, the effect size for published studies was −.44 (CI: [−.76, −.12], $k = 16$) and for unpublished studies it was .11 (CI: [−.11, .35], $k = 13$). Thus, while there does not seem to be any publication bias across studies used in the meta-analysis, our results suggest that studies with larger effect sizes are more likely to be published versus unpublished (i.e., dissertations).

3. Discussion

In summarizing a decade of research on racial shooter biases, we found that across all studies, there were significant effects for reaction time and shooting threshold biases. Compared with White targets, participants were quicker to shoot armed Black targets, slower to not shoot unarmed Black targets, and were more likely to have a liberal shooting threshold for Black targets. They were not, however, more likely to be sensitive to or have a higher false alarm rate for Black (versus White) targets.

The significance of reaction time and shooting threshold biases – but not false alarm rates or shooting sensitivity – presents an avenue for theorizing about what drives different shooter biases. Given that people respond to stereotype-consistent information more quickly than stereotype-inconsistent information (Blair & Banaji, 1996; Devine, 1989), it is possible that reaction time biases are a result of participants' awareness or endorsement of stereotypes about Blacks and criminality/dangerousness. Specifically, participants may be faster to shoot Black (versus White) targets with a gun because it fits with a racial stereotype. This interpretation is partially supported by the correlation between reaction time biases and endorsement of stereotypes. However, this relation was quite small, suggesting other factors may be at play.

The real world implications of reaction time biases may not be as devastating as with the shooting threshold bias because reaction time biases are calculated based on correct decisions. For example, if a person takes a few extra milliseconds to decide *not* to shoot an unarmed Black person, the consequences are low. Conversely, if that person has a liberal shooting threshold and is generally more likely to make the decision to shoot Black targets, then the outcome for the Black person has the potential to be lethal. It is therefore important to understand what factors may put people at risk to having different types of biases. One theory posited by Correll, Hudson, Guillermo, and Ma (2014) to explain why in their studies, police officers, in contrast to community members and college students, do not show shooting threshold biases, is because police officers might develop more cognitive control from training to overcome these biases. They are, however, still susceptible to reaction time biases due to implicit or explicit knowledge or endorsement of stereotypes about Blacks. More studies specifically designed to determine what factors predict different shooter biases are needed. It is clear that more work on the construct validity of different operationalizations of shooter biases is needed.

We found that studies conducted in states with more permissive gun laws (i.e., less laws regulating gun usage) had bigger effects for false alarm rates and shooting threshold biases. This effect was still present when adjusting for general liberalness of the state as indexed by state voting behavior, suggesting that the effect is somewhat specific to gun laws. Our results do not allow us to draw conclusions about what characteristics of states with permissive gun laws explain the association with individual racial shooter biases; however, we offer two possibilities that may be tested in future research. First, research shows that support for permissive gun control is related to conservative political ideology (Branscombe, Weir, & Crosby, 1991). Other research has found that political conservatism is related to prejudicial attitudes against Blacks (e.g., Reyna, Henry, Korkfmacher, & Tucker, 2006; Sidanius, Pratto, & Bobo, 1996). Therefore, to the extent that racial shooter biases are a form of prejudice, political conservatism (and the underlying need to manage uncertainty and threat; Jost, Glaser, Kruglanski, & Sulloway, 2003), may explain the gun control findings.

A second possibility is that the gun culture in states with permissive gun laws affects the wiliness to shoot possible perpetrators of a crime in a way that affects Black targets more than White targets. On the surface, this makes sense because states with more permissive gun laws also have more extrajudicial shootings of racial and ethnic minorities (Price et al., 2004). Moreover, Coa, Cullen, Barton, and Blevins (2002) found that willingness to shoot a perpetrator of a crime was positively related to childhood socialization to guns, fear of crime, and recent increases in the number of Black residents in the neighborhood. This possibly suggests that people with more exposure to guns (e.g., people from states with permissive gun laws) are more willing to use guns against perpetrators of crime, who they assume, due to stereotypes, to be racial and ethnic minorities. Thus, it is possible there are cumulative effects of permissive gun laws, willingness to shoot, and race-related fear of crime that, together, contribute to racial shooter biases. Alternatively, individuals who are more willing to shoot may be influencing policy in such a way that may increase the likelihood of permissive gun laws (e.g., voting for political candidates in line with their views on gun laws). Though plausible, without further research these hypotheses should be considered tenuous.

The results of our racial proportion moderator analysis showed that studies conducted in cities with a higher proportion of non-Whites had a larger effect for shooting threshold. The correlational results also showed that contact with Blacks was related to larger shooter biases. These findings stand in contrast to intergroup contact theory (Pettigrew & Tropp, 2006), which suggests that contact with members of an out-group should reduce prejudicial behavior. For intergroup contact to be effective, however, specific conditions are required (e.g., common goals, equal status; Allport, 1954/1988; Pettigrew, 1998) that could not be assessed in our analysis. Our finding is in line with other work showing that changes in racial/ethnic makeup of a neighborhood increases fear of crime, gun ownership, and willingness to shoot perpetrators (Coa et al., 2002; Young, 1985). Thus, in the absence of the necessary conditions, more contact with non-Whites might make Whites more afraid of crime and thus more willing to shoot Blacks, whom they might perceive to be perpetrators of crime. When both proportion of non-Whites and gun laws were in the same model, it appeared that the results for gun laws were more robust. Therefore, it is possible that the overlap between racial makeup of a city and state level gun laws explain these results. Reduced statistical power associated with more complex models, however, should also be considered as an explanation.

Our results also allow us to make some recommendations about conducting and reporting of racial shooter bias studies. First, the effects are generally small, so researchers may need large samples to detect effects. For example, according to G*Power, to have .80 power to detect a within-subjects effect size of 0.13, 467 participants are required. This is much larger than the average 85 participants in the studies included in this meta-analysis. Second, researchers should at least report results for the two reaction time measures and shooting threshold, as they are the most reliable. For full transparency and if space allows, it would be ideal for researchers to report all operationalizations of shooter biases. Such standardization in reporting across studies should help reduce the likelihood of researchers only publishing operationalizations that have significant results. Third, if researchers are interested in focusing on reaction time biases, they should uses response windows longer than 630 milliseconds, as the effects are essentially zero with shorter timeframes. Researchers more interested in shooting threshold could use any response window according to our results. Finally, our results do not suggest that stimulus type or sample type have particularly important effects on the results.

This meta-analysis contributes to the literature by quantitatively examining the effect sizes of shooter biases and examining moderators that may further explain these effects. Two important limitations, however, should be considered. First, the proportion of non-Whites in the region where the participants were recruited does not give us information about the quality or quantity of inter-group contact, which may be important mediating variables. The fact that participants live in a city with more non-Whites does not necessarily imply that they have more contact with them. Second, given that gun law strictness is a policy-level variable, it is difficult to infer how it reflects individual attitudes, particularly based on the data available to us.

This meta-analysis highlights a number of stimulating future directions and recommendations for this area of study. First, future work should be focused on understanding what the shooter biases really measure and what psychological processes affect them (e.g., What is the nomological network for shooting threshold?). Second, researchers should seek to understand the psychological factors that explain how community level variables are associated with individual level behavior. For example, researchers should investigate both the quantity and *quality* of contact to better understand the racial heterogeneity findings in the context of intergroup theory. Researchers should also specifically assess attitudes about guns (e.g., willingness to shoot perpetrators of crime) to better understand the role of gun laws in shooting decisions. Third, the inconsistency in self-reported prejudice correlations suggests that it may be fruitful to investigate other racial biases that may be relevant to racially prejudiced behavior (e.g., dehumanization).

Given the potentially high translational value of research on shooter biases, continued work in this area should be a priority to researchers interested in social justice across various domains (e.g., psychology, political science, law). Understanding the factors that contribute to shooter biases in the laboratory may provide critical insight into targets of change for interventions designed for gun-owners and law enforcement officers to reduce the frequency of unjustified shootings of racial and ethnic minorities. Given the impact of these shootings not only on families of victims, but on entire communities' sense of safety and wellbeing (Mays et al., 2013; Sha'Kema & Thomas, 2015; Thomas & Blackmon 2015), research identifying effective interventions is needed to maintain the basic human rights of racial and ethnic minorities, keep communities safe, and increase the effectiveness of policing.

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