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How Prior Information and Police Experience Impact Decisions to Shoot

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Social psychologists have relied on computerized shooting tasks to test whether race influences decisions to shoot. These studies reveal that under some conditions untrained individuals shoot unarmed Black men more than unarmed White men. We modeled the decision to shoot as a sequential sampling process in which people start out with prior biases and accumulate evidence over time until a threshold is reached, prompting a decision. We used this approach to test how prior information (a proxy for police dispatch information) and police experience influence racial bias in shooting decisions. When no prior information was given, target race biased the rate at which untrained civilians accumulated evidence, leading to a greater rate of shooting Black targets. For sworn police officers, the race of the target impacted prior bias, but not evidence accumulation. Moreover, officers showed no race bias in the observed decision to shoot. For both untrained civilians and sworn police officers, prior information about a target's race was sufficient to eliminate racial bias in shooting decisions both at the process and behavioral level. These studies reveal that factors present in real-world shooting decisions (dispatch information and police experience) can moderate the role that race plays both in the underlying cognitive processes and ultimately on the observed decision. We discuss the benefits of using a dynamic cognitive model to understand the decision to shoot and the implications of these results for laboratory analogues of real-world decisions.

Keywords: diffusion model, dispatch information, first person shooter task, officer-involved shootings, race bias

Supplemental materials: http://dx.doi.org/10.1037/pspa0000130.supp

In November 2014, a police officer responded to information from dispatch about a "Black male sitting on the swings . . . pointing [a gun] at people" (Lee, 2015a). When the officer arrived on the scene he shot the individual within seconds, killing him. The Black male was 12-year-old Tamir Rice, who was playing with an airsoft pistol. For many people, Rice's shooting represents bias in the use of lethal force against Black Americans (Lee, 2015b). Yet, at least in the case of Rice, prior information about the presence of a gun may have impacted the officer's decision independent of race. This raises the question of how *dispatch information*—information given to officers by police dispatch before seeing a suspect—might impact officers' decisions to shoot.

Researchers in fields such as criminal justice and sociology have long studied how officers make decisions to shoot and how race

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might influence these decisions by analyzing police shootings using police reports, public data sets, and observational methods. Although these methods are an important strategy for understanding the factors that influence use of lethal force, they suffer from at least three problems. First, conclusions depend on the completeness of the available data (James, Klinger, & Vila, 2014; James, Vila, & Daratha, 2013). If details are not recorded there is no way to understand how they impact decisions. Second, deadly force encounters are complex interactions involving multiple correlated factors (e.g., a suspect's race, demeanor, attire, and location). This makes it difficult to isolate the impact any one factor has on such decisions. Finally, relying on data collected after-the-fact precludes examining how those factors impact the decision process in the moment.

To address these problems researchers have created experimental tasks to isolate how factors such as race impact the decision to shoot (e.g., Correll, Park, Judd, & Wittenbrink, 2002; Plant & Peruche, 2005). Although these simplified shooting tasks lack the realism of police-civilian interactions, they allow researchers to more precisely isolate what factors influence decisions. The most extensively used experimental paradigm to study these decisions within psychology is the First-Person Shooter Task (FPST; Correll et al., 2002). In typical uses of this task, participants see pictures of Black men and White men holding either guns or harmless objects. They are told to press a "shoot" button if the object is a gun or a "don't shoot" button if the object is harmless. Participants earn points for correct decisions and incur penalties for errors.

The typical finding from these studies is that people under time pressure, particularly untrained civilians, are more likely to shoot armed and unarmed Black targets compared to White targets (Correll et al., 2002, 2007; Correll, Wittenbrink, Park, Judd, & Goyle, 2011). With less time pressure race bias shifts more to response times, with people being faster to shoot armed Black targets and slower to not shoot unarmed Black targets when compared with White targets (Correll et al., 2002; Plant & Peruche, 2005; Plant, Peruche, & Butz, 2005). Police officers tend to exhibit race bias in their response times when under time pressure (Correll et al., 2007; Sim, Correll, & Sadler, 2013). Yet, these behavioral results alone do not explain *how* race influences decisions to shoot at the process-level. To answer this question we use a computational model of decision-making in which the decision to shoot is modeled as a sequential sampling process (Pleskac, Cesario, & Johnson, 2017). According to the model, when people encounter someone who may be armed, they start with a prior bias to shoot or not. They then seek decision-relevant information and accumulate this information over time as evidence. When they obtain a sufficient amount of evidence they chose to shoot or not.

This model provides a framework for testing how factors such as race might influence shooting decisions. For example, people might shoot unarmed Black men more than unarmed White men because they show a *prior bias* to shoot (Takagi, 1974). Another possibility, not mutually exclusive, is that race impacts the decision *as evidence is collected.* Although early work using signal detection analyses found that participants set a lower criterion for shooting Black men (Correll et al., 2002), recent work with sequential sampling models supports the latter hypothesis (Correll, Wittenbrink, Crawford, & Sadler, 2015; Pleskac et al., 2017). On average, untrained civilians do not show a prior bias to shoot Black men more than White men. Rather, the race of a target may influence the interpretation of the object, perhaps through stereotypic associations between Black men and violence (Correll et al., 2015). Here we examine how dispatch information and police experience may impact the finding that race biases the decision to shoot as evidence is collected. We examine these two factors more closely next.

Dispatch Information

Laboratory shooting tasks complement real world data on police shootings because they allow precise control over what factors enter into a decision. Although this control is useful, it comes at the cost of potentially oversimplifying the decision environment. When critical pieces of information in the decision environment are missing from laboratory tasks, conclusions that can be drawn about such decisions outside the laboratory are limited and potentially misleading. For example, the dangerousness of the neighborhood (Correll et al., 2011; Kahn & Davies, 2017; but see Cox, Devine, Plant, & Schwartz, 2014; Pleskac et al., 2017), social class as indicated by clothing (Kahn & Davies, 2017; Moore-Berg, Karpinski, & Plant, 2017), officer fatigue (Ma et al., 2013), and racial prototypicality (Ma & Correll, 2011) all influence racial bias in laboratory shooting tasks. One additional limitation of laboratory shooting tasks that has not been examined is that participants often know nothing about a target until he appears on screen. This is despite the fact that officers often have dispatch information about a suspect before they interact with him or her.

What kind of dispatch information do officers typically receive and why might this information matter? Although the information requested by police dispatchers varies widely based on the situation, they generally ask four questions: *where* is the emergency, *what* is the emergency, *when* did it happen, and *who* is involved (Norcomm, 2017; Kobb, 2016). Answers to these questions are passed on to officers responding to the call. Importantly, the "*who*" question involves getting an accurate description of the suspect, including information about their sex, race, age, height, weight, hair color, and clothing. Thus, in many cases officers have information about the race and sex of the suspect far before they encounter them.

In the case of a crime, dispatchers also routinely ask whether weapons are present and pass this information onto officers (Broadbent et al., 2018). The presence of a weapon, particularly a gun, raises the priority of a call (Messinger et al., 2013). This information is not always accurate, however, because harmless objects are sometimes misidentified as weapons. This error is exemplified in the shooting of Tamir Rice as well as John Crawford (Balko, 2014), where officers received incorrect information that the suspect was holding a conventional firearm (both had airsoft replicas). In addition, officers sometimes receive bad information that a suspect is armed because civilians falsely report weapons to dispatch to get faster responses.[1] Although data about dispatch information are difficult to obtain because of a "near non-existence" of research on police dispatch (Gardett et al., 2016, p. 29), estimates from the Guardian's officer-involved shootings database (The Counted, 2016) suggest that in the majority of these shootings (64%) officers are given dispatch information, including information about the race of the suspect (29%) and whether they are armed (55%; see the online supplemental materials).

[1] We thank Lance Langdon (personal communication, June 1, 2016), the Director of the Ingham County 911 Central Dispatch Center for informing us of this issue.

Given the training officers receive for dealing with armed civilians, dispatch information about weapons likely has a strong influence on the decision to shoot. According to our model, because the information is communicated before officers arrive at the scene, the information that a suspect is armed may create a prior bias to shoot. Alternatively, this information might also impact an officer's perception of how threatening a suspect is acting as evidence is collected. The same action (e.g., reaching for a wallet) may be more threatening if officers expect the suspect to have a gun.

Dispatch information about a target's race might also impact the decision to shoot. Past work has shown that primes of Black male faces facilitate categorization of weapons when presented briefly (200 ms; Payne, 2001, 2006). This result suggests that providing dispatch information that a target is Black may create a prior bias to shoot. Alternatively, with greater time to process this information, participants may be better able to control any stereotypic associations between Black men and violence. This could help them respond more accurately and reduce the likelihood that race is used as evidence when making the decision to shoot.

In sum, dispatch information is routinely given to police officers who rely on this information to make decisions. Understanding the importance of this real-world cue for shooting decisions requires that laboratory shooting tasks approximate some form of dispatch information.

Policing Experience

A related problem with generalizing results from laboratory shooting studies to real world officer-involved shootings is that such studies typically recruit civilians who lack the training and experience of sworn officers. When studies have included officers, they typically outperform untrained civilians and show *less* race bias in their errors (Correll et al., 2007; James et al., 2013; Ma et al., 2013; Plant & Peruche, 2005; Sadler, Correll, Park, & Judd, 2012; Sim et al., 2013).

There are several reasons why officers might outperform students and show less bias. First, adults who choose to pursue a career in law enforcement may vary from the general population of adults in ways relevant to the decision to shoot, like being more cautious. Second, officers have more experience with quickly identifying objects in threatening circumstances. Finally, officers receive at least three types of training on use of force that civilians do not: basic training, field training, and in-service training (Morrison, 2006). Nationally, officers receive an average of 760 hr of basic training, 420 hr of field training, and 38 hr of annual in-service training (Stickle, 2016). However, it is difficult to identify which training or trainings—let alone what part of that training—impacts officer decisions in lethal force situations. This is because there is considerable variability in the content and format of training across departments (Sanders, Hughes, & Langworthy, 1995).

The impact of field experience and training on officer use of force decisions remains understudied (Stickle, 2016). However, studies that have examined experience and training have found that they generally improve outcomes related to force. Officers with more experience use less physical and verbal force (Paoline & Terrill, 2007) and officer-involved shootings decrease with officer age (McElvain & Kposowa, 2008). Officers with more training receive fewer complaints (Stickle, 2016) and are better able to resolve conflict with less force (Lee & Vaughn, 2010), but these benefits may be limited to additional in-service training (Lee, Jang, Yun, Lim, & Tushaus, 2010). One or more of these reasons may explain why officers outperform civilians when making lethal force decisions in laboratory experiments.

Our model offers a unique approach to understanding the ways in which officers might differ from civilians, both in terms of race bias in the decision to shoot as well as performance in general. For example, one possibility is that officers may be more cautious than untrained civilians. The gravity of shooting decisions may be more of a concern for officers as an incorrect decision could result in the death of an unarmed civilian. Another possibility is that officers may be better at using information relevant to the decision to shoot than untrained civilians. Officers might be better able to ignore irrelevant information such as race, which would explain why they do not show racial bias in shooting decisions compared with untrained civilians. A final possibility is that sworn officers may have slower responses than untrained students. Response times slow with age (Der & Deary, 2006; Pierson & Montoye, 1958), and officers, who are generally older than student populations, may have slower motor responses. This would suggest that slower response times by officers would not be attributable to differences in the decision process per se (e.g., being more cautious), but would instead be attributable to age-related slowing of the physical execution of a response. This effect would not be caused by experience but would rather be a side effect of aging that is naturally correlated with experience.

We now turn to a discussion of how we formally measure the impact of different factors like the race of a civilian, dispatch information, and police experience on shooting decisions using the drift diffusion model.

Drift Diffusion Model and Shooting Decisions

The drift diffusion model (DDM; Ratcliff, 1978; Ratcliff & McKoon, 2008) is a type of sequential sampling model used to explain decisions between two choices. It is the most widely used formal sequential sampling model in the cognitive sciences (Forstmann, Ratcliff, & Wagenmakers, 2016; Klauer, 2014; Ratcliff & McKoon, 2008; Ratcliff, Smith, Brown, & McKoon, 2016). The DDM assumes that for a given decision, people may start with a prior bias toward one choice or the other. Then they repeatedly sample noisy decision-relevant evidence from their environment, as approximated by the diffusion process. The information is noisy because the environment itself and the neural processes used to extract evidence introduce variability. When some internal threshold of evidence is met, the decision is made. The time it takes for evidence to reach this threshold is the predicted response time.

Why rely on the DDM over other models commonly used to understand the decision to shoot? One reason is that the DDM can separate biases that a person brings to the situation (indexed by start point) from biases that occur when processing the object in question (indexed by the drift rate). The DDM can do this because it simultaneously models decisions and the speed those decisions are made (see also Ratcliff & McKoon, 2008). In contrast, other formal decision-making models that focus solely on choices (e.g., signal detection and process dissociation; Green & Swets, 1966; Jacoby, 1991; Payne, 2001) are unable to make this distinction. In these models, biases are reflected in a single parameter (the criterion in signal detection, automatic processing in process dissociation). This makes it difficult to disentangle when bias originates in the decision process.

A second reason is that racial bias in the decision to shoot (among students) often shifts to response times rather than errors when participants have longer to respond (Correll et al., 2002). Signal detection and process dissociation models, which do not consider response times, are unable to identify racial bias in those cases. In contrast, the DDM identifies the role of race to a common process-level mechanism—the drift rate—regardless of whether bias manifests behaviorally in decisions or in response times (Pleskac et al., 2017).

Table 1 describes the parameters of the DDM. The model proposes that when faced with a decision such as the decision to shoot, people start out with an initial preference to shoot or not shoot, indicated by the relative start point β. They then accumulate evidence over time toward the shoot or do not shoot decision with average strength δ. When evidence reaches a threshold α, they make a decision. The model also estimates the length of response components unrelated to decision-making (e.g., motor response time) as nondecision time τ. Given a relative start point β, threshold α, drift rate δ, and nondecision time τ, the model predicts the probability of selecting to shoot or not and the response time distributions associated with each choice (see Figure 1). The model can also be used to test whether factors like race and dispatch information influence these decision parameters in similar or different ways.

Although the DDM can be used to test process-level predictions (e.g., officers are more cautious than students), interpreting its parameters as indexes of psychological constructs requires validation (Klauer, 2014). Some work has validated the psychological interpretations of DDM parameters within the FPST. For example, the threshold parameter is intended to measure how much evidence participants accumulate before making a decision and should be sensitive to manipulations that influence how much evidence participants can collect. Consistent with this hypothesis, Pleskac et al. (2017) showed that when the FPST response window was increased, the threshold parameter also systematically increased. Similarly, Pleskac et al. (2017) also demonstrated that blurring guns decreased drift rates (i.e., individuals took longer to accumulate evidence), validating it as a measure of evidence strength. Although promising, one feature of the model not yet validated is the relative start point as a measure of prior bias. The conclusion that race shapes shooting decisions by influencing how evidence is accumulated, and not because of a prior bias to shoot Black men,

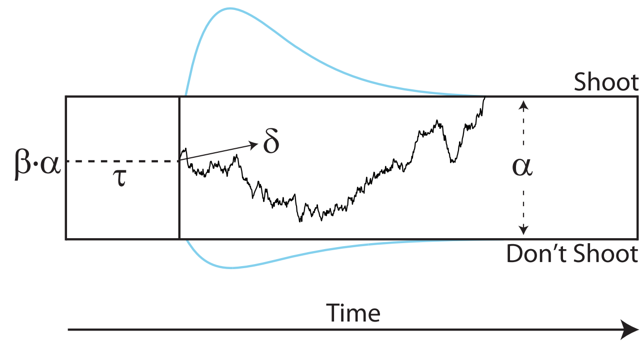

Figure 1. The diffusion model as applied to the decision to shoot. Individuals start with an initial preference to shoot or not shoot. This preference is indexed by the relative start point β, which determines the relative starting location between the two choice thresholds. Noisy information is accumulated in favor of the shoot or do not shoot decision with average strength δ. The amount of information needed to make a decision is indicated by the location of the thresholds. The bottom threshold is fixed at 0 and the location of the upper threshold is determined by the parameter α. The duration of other nondecision-related processes is indicated by tau τ. Distributions (in blue; gray in print) above and below the decision space indicate the model predicts the distribution of response times for both shoot and do not shoot decisions. See the online article for the color version of this figure.

is dependent on the relative start point serving as a valid index of bias. Thus, in this paper we validate the start point using two experimental manipulations that should influence prior biases: prior information that a target is armed and changing point-based payoffs in the FPST to encourage more shooting.

Effects of Race, Dispatch Information, and Police Experience on the Decision Process

We use our model of the decision process to outline different mechanisms by which race, dispatch information, and police experience could impact the decision to shoot. To formally test these hypotheses, we explicitly tie them to process-level predictions within the DDM. We consider multiple possibilities for how race, dispatch information, and police experience might influence the decision process.

Target Race

There are two plausible ways by which a target's race can impact the decision process to result in increased errors and faster

Table 1
DDM Parameters and Their Interpretations Within the FPST

Parameter	Interpretation
Relative start point (β)	Prior bias to favor shooting at the start of the evidence accumulation process, with $0 < \beta < 1$. Values above .50 indicate a bias to shoot.
Threshold (α)	Amount of evidence required to make a decision, with $0 < \alpha$. Hitting a threshold boundary triggers the decision to shoot or not shoot.
Drift rate (δ)	Average quality of information extracted from a stimulus at each unit of time, with $-\infty < \delta < \infty$. Higher absolute values indicate stronger evidence. Positive values indicate evidence to shoot.
Nondecision time (τ)	Length of all response components unrelated to decision-making, with $0 < \tau$. Reflects encoding time, motor response time, and other unknown contaminants. Measured in milliseconds.

Note. DDM = drift diffusion model; FPST = First Person Shooter Task.

responses to shoot armed Black targets relative to armed White targets. One mechanism is the relative start point β, such that participants set a higher start point for Black targets than for White targets (H1). A second way is via the evidence being accumulated δ, such that both the target's race and the object are processed as evidence when determining whether to shoot or not (H2). Assuming Black targets have a stereotypic association with violence this would lead to evidence accumulating faster toward the shoot threshold for both gun and nongun objects (i.e., more positive drift rates for guns and less negative drift rates for nongun objects), creating race bias in the decisions. Past work has supported the latter hypothesis—that race is most influential in evidence accumulation (Correll et al., 2015; Pleskac et al., 2017). However, this work has only focused on students rather than sworn officers, making it unclear whether officers might show a different pattern of race bias than students.

Weapon Information

We also consider how providing information that a target is armed would influence the decision process during the FPST. One possibility is that providing this information would bias participants to favor shooting, shifting the relative start point β closer to the shoot threshold (H3). Weapon information might also influence how participants accumulate evidence. Information that a target is armed may make the same action (e.g., holding an object) seem more dangerous, or race could be directly accumulated as evidence that a participant should shoot. This would result in an increased drift rate δ for targets when the information is correct and a decreased drift rate when the information is wrong (H4).

Race Information

As described above, the race of a target biases how untrained civilians accumulate information in the FPST. Providing information about the race of the target before the target appears on the screen might create—just like weapon information—a bias to shoot in participants' relative start point β (H5). This would be consistent with research demonstrating that briefly priming faces of Black men facilitates the categorization of weapons (Payne, 2001, 2006). However, a major difference between that work and how dispatch is used in the real world is that officers have considerably more time to digest that information. With more time, knowing a person's race beforehand may help them control the stereotypic associations between Black men and violence that lead to race being used as evidence. This might reduce or eliminate racial bias in drift rates δ (H6).

Police Experience

Officers have considerable field experience identifying threatening objects. This experience is likely to help them identify weapons, as indexed by stronger drift rates δ (i.e., drift rates for both guns and nonguns would be larger in magnitude) than untrained civilians (H7). At the same time, officers may be more cautious than students when deciding to shoot or not, as these decisions are more important for them. Within the DDM this would manifest as increased thresholds α for officers compared with civilians (H8). Finally, officers might respond slower than a young population (i.e., students) because their motor responses are slower as a result of age-related declines (H9). This slowing is predominantly attributable to an increase in the length of nondecision processes τ (Ratcliff, Thapar, Gomez, & McKoon, 2004; Ratcliff, Thapar, & McKoon, 2001, 2006; Thapar, Ratcliff, & McKoon, 2003).

General Method

Three studies tested how dispatch information and police experience impact the decision to shoot and how these variables might change the effects of target race on shooting decisions. Studies 1 and 3 tested how prior race and weapon information impacted shooting decisions and whether those effects depended on police experience. Untrained students and sworn police officers completed versions of the FPST where they received prior information (an operationalization of dispatch information) about the race of targets and whether they were armed. Study 2 validated the relative start point parameter using an experimental manipulation of payoffs. In all studies, data were examined with behavior-level analyses and process-level DDM analyses.

Behavior-Level Analyses

Although our predictions focused on the process-level, we tested behavior-level data with multilevel regression using the lme4 package in R (Version 1.1–13; Bates, Maechler, Bolker, & Walker, 2015). We used multilevel analyses to account for the variability both in terms of the targets and participants (see also Judd, Westfall, & Kenny, 2012). In all decision and response time analyses, we accounted for this variability by (a) allowing intercepts to vary for participants and targets, (b) allowing random slopes for objects for both participants and targets, (c) and by modeling the covariance between these effects. Choices were analyzed with a logistic link function and response times 2.5 standard deviations above a participant's mean were truncated to this value to reduce skew from inattentive responses (see Reifman & Keyton, 2010). All factors except for police experience were within-subjects factors. Parameter tables for all behavior-level analyses are listed in the online supplemental materials.

Process-Level Analyses

In all studies, we embedded the DDM in a hierarchical framework and estimated it using Bayesian methods (for a walkthrough, see Johnson, Hopwood, Cesario, & Pleskac, 2017). This method yields precise estimates of model parameters despite sparse data at the participant level. This approach is appropriate for tasks like the FPST, where participants complete a small number of trials per condition (typically 20–40 trials). The DDM was specified according to the guidelines set by Pleskac et al. (2017). Parameters were allowed to vary with the experimental manipulations, with the exception that only drift rate and nondecision time were allowed to vary by object. We ran the model in the Markov Chain Monte Carlo (MCMC) sampler JAGS (Version 4.20; Plummer, 2003) with the Wiener module extension (Wabersich & Vandekerckhove, 2014). Parameter effect tables and JAGS model code for each study are reported in the online supplemental materials.

Bayesian estimation provides an estimate of the posterior distribution of parameters after observing the data and in light of prior beliefs. The posterior distribution represents the degree of certainty regarding the parameters after observing the data. We allowed the data to dominate the posterior estimate by setting uninformative priors, and estimated the posterior distributions with a large representative sample via MCMC methods. Each sample in the MCMC chain provides a credible combination of parameter values in light of the data and the prior distribution. Kruschke (2014) recommends an effective chain sample size of 10,000 to precisely estimate the distribution of a parameter. Given our focus on comparing the condition level mean parameter estimates, all of these distributions had a minimum sample size of 8,000, and most were above 10,000. We also evaluated the chains for their representativeness and accuracy using the procedures outlined by Kruschke (2014). All condition level chains were inspected as well as a random subset of individual level chains. All parameters showed clear convergence.

We describe the posterior distributions for each parameter by their modal value and 95% highest density interval (HDI). The modal posterior value has the highest probability density and is thus the most credible estimate. Values within the 95% HDI also have higher probability density than values outside the interval and so are more credible (Kruschke, 2014). Because the (marginal) posterior distribution represents certainty about the value of a parameter, it can be used in hypothesis testing, although this approach is conceptually separate from the estimation procedure. In this approach we test whether a parameter's 95% HDI contains a null value. For each contrast, we report the difference in both the scale of the parameter and a standardized difference measure (Cohen's *d*), calculated using the condition level variability parameter.

We also verified that this Bayesian implementation of the DDM accurately predicted the FPST data. For each condition within each study we conducted posterior predictive checks for the predicted choice probabilities, mean response times, and response time distributions. Those checks are included in the online supplemental materials. The models gave a good account of the data, similar to the predictive checks from other work on this task (e.g., Pleskac et al., 2017).

FPST Procedure

Participants completed the FPST in PsychoPy (Version 1.83.01; Peirce, 2007) on a 24-in. monitor (20.88 by 10.75 in.). Stimuli were presented so they filled the screen without stretching (14.33 by 10.75 in.). Participants were seated approximately 18 in. away from the monitor but could adjust this distance. Sample sizes were determined according to Simonsohn's (2015) rule of thumb, which suggests a sample size of 2.5 times larger than work typical in the area ($N = 40$, Study 1; Correll et al., 2002). We therefore collected data from 100 participants and officers in each study unless otherwise noted.

On any given trial, participants were given prior race and/or weapon information (or not) for 2000 ms (see Figure 2 for a depiction of an example trial). This information served as an operationalization of the dispatch information officers receive in the field. If such information was not provided they saw a fixation point for the same amount of time. Because presenting prior information is a modification of the standard FPST procedure, we refer to the task as the modified FPST when prior information is used. Participants then saw one to four empty background scenes (e.g., parks, streets, office buildings). Each of these was presented for a random amount of time between 500 and 1000 ms in 100-ms increments. After these empty backgrounds were presented, a target appeared in a background at a random location holding a handgun or a harmless object (e.g., cell phone, wallet, soda can). Participants were instructed to press a button marked "shoot" if the target was armed or a button marked "don't shoot" if the target was holding a harmless object. Targets were 40 young to middle-aged men; 20 were White and 20 were Black.[2] Each target was photographed holding a handgun and a harmless object for a total of 80 pictures. Participants saw each picture twice (officers) or four times (students). Participants completed eight practice trials before the experimental task.

To encourage participants to respond as quickly as possible, we enforced a 650ms response window. In a departure from the standard FPST design (Correll et al., 2002), we did not give point-based feedback to participants, although they did receive feedback about their decision accuracy. This decision was driven by the choice to recruit officers. The purpose of point-based feedback is to mimic the payoffs that officers would receive on the job based on their decisions to use lethal force. Officers likely do not need this reminder and, in our experience, report that the point-based system trivializes these important decisions.

Study 1: The Role of Prior Information and Police Experience

The purpose of Study 1 was to test how two aspects of prior information influenced the decision to shoot: information about a target's race and information about whether the target was armed. In addition, by recruiting officers and students, we tested whether trained and untrained individuals responded differently to prior information.

Method

Participants. One hundred six undergraduates completed a modified version of the FPST with prior information. One participant was removed for not following instructions (always choosing to not shoot), and three participants were removed for responding carelessly (responding faster than 300ms on 20% or more trials). The remaining 102 participants ($M_{age} = 19.0$, $SD = 1.2$) were 72.5% White, 13.7% Asian, 3.9% Black, with 9.8% from other groups. Men (88.2%) were oversampled to better match the demographics of officers nationally, who are overwhelmingly male (87.8%; Reaves, 2015).

We also collected officer data from four different police departments in the Midwestern United States. We aimed to recruit 50 officers, which was the maximum number of officers we could recruit given our funding. Ultimately 51 officers from departments of various sizes (from 30–1,800 sworn officers) were recruited. The study was advertised to the officers during police training or shift briefings as a study of the role of dispatch information on police use of force. Race was not explicitly mentioned, although we cannot rule out the possibility that officers may have shared

[2] We thank Joshua Correll for sharing the stimuli used in Correll et al. (2002).

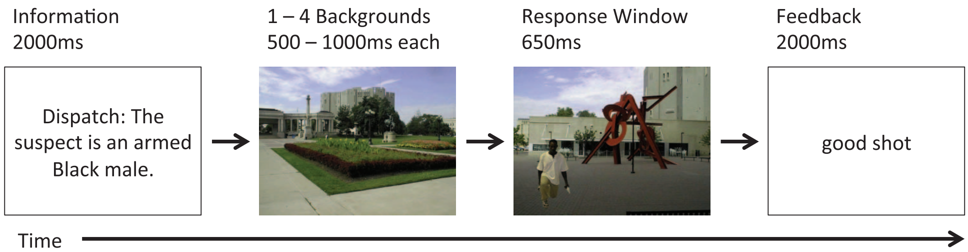

Figure 2. The modified FPST. On every trial participants first received accurate information about the race and sex of the target. On half of the trials they were informed (with 75% accuracy) that the target was armed. See the online article for the color version of this figure.

information about the study with their peers. Although the self-selection of officers limits the generalizability of our findings we made this trade-off given the scarce amount of research on officer decision-making. Officers completed the study in the department before or after their shift, or during their training. They either were paid $30 for their participation or did the study voluntarily. Officers were 68.6% men, with an average of 11.7 years of experience (SD = 9.5, range [0, 45]; not all officers reported their experience).

Procedure. Participants completed 160 trials (officers) or 320 trials (students) of the FPST. Figure 1 shows an example of one trial from the FPST. For this study, every trial began with the presentation of some information. With respect to demographic information, on all trials participants were given accurate information about the race and sex of the target (all targets were men). In the case of a Black male, participants would see the message "The suspect is a Black male." This design choice reflects the fact that misidentification of race and sex is unlikely for the targets in the FPST, who are easily categorized on these dimensions.

With respect to weapon information, on half the trials participants were told that the target was armed; on the other half of trials they were not given any weapon information and only received demographic information. For example, on a trial where weapon information was presented for a Black male target, participants would see the message "The suspect is an armed Black male." Because weapon information was accurate 75% of the time, targets were more likely to be armed when it was given (75% armed) than when it was not (25% armed). This made the information (and its absence) diagnostic as to whether the participant would encounter someone with a weapon, making the task more realistic. Participants were explicitly told that the demographic information would always be accurate, but that the weapon information would "generally (but not always) be correct."

Results

Behavior-level analyses. Figure 3 shows decision data for all conditions. Only two effects emerged. First, officers were less likely to make mistakes (M = .151, 95% CI [.123, .179]) than students (M = .212, 95% CI [.184, .241), $b = -.410$, OR = 0.66, SE = .108, $p < .001$.[3] Second, the predicted interaction between object and weapon information was significant, b = .776, SE = .075, $p < .001$. As expected, when prior weapon information correctly indicated that the target was armed, participants were less likely to make mistakes (M = .159, 95% CI [.133, .185]) than when the weapon information was incorrect (M = .202, 95% CI [.168, .236]), $b = -.288$, OR = 0.74, SE = .127, p = .023. In addition, when weapon information was not given (and unarmed individuals were more likely) participants were more likely to make mistakes for armed targets (M = .220, 95% CI [.185, .254]) than for unarmed targets (M = .147, 95% CI [.121, .172]), b = .487, OR = 1.63, SE = .126, $p < .001$.

The race by object interaction indicative of racial bias was not significant, $b = -.087$, SE = .217, p = .69, nor was the three-way interaction with weapon information, b = .154, SE = .150, p = .30. In sum, there was no evidence that students or officers were influenced by the race of a target when prior information was incorporated into the task. This effect was absent regardless of whether weapon and demographic information were provided or only demographic information was provided.

Figure 4 shows response times for correct choices. Officers (M = 614 ms, 95% CI [595, 633]) were slower to respond than students (M = 561 ms, 95% CI [546, 576]), b = 53.14, SE = 9.96, $p < .001$. Participants were also faster to respond to guns (M = 558 ms, 95% CI [541, 574]) than nonguns (M = 617 ms, 95% CI [602, 632]), $b = -59.34$, SE = 6.69, $p < .001$. Participants were also slightly faster to respond when they received prior information (M = 584 ms, 95% CI [570, 599]) than when they did not (M = 590, 95% CI [576, 605]), $b = -6.26$, SE = 2.65, p = .018.

There was also an interaction between object and prior information, $b = -23.74$, SE = 5.29, $p < .001$. Participants were faster to correctly respond to armed targets (M = 549 ms, 95% CI [532, 565]) than unarmed targets (M = 620 ms, 95% CI [604, 636]) when prior information stated that the target was armed, $b = -71.21$, SE = 7.19, $p < .001$. They were also faster to correctly respond to armed targets (M = 566 ms, 95% CI [550, 584]) than unarmed targets (M = 614 ms, 95% CI [599, 629]) when no weapon prior information was provided, but this difference was smaller, $b = -47.47$, SE = 7.19, $p < .001$.

Finally, there was an interaction between object and participant group, $b = -33.85$, SE = 8.44, $p < .001$. Officers were slower to correctly respond to unarmed targets (M = 575 ms, 95% CI [554, 598]) than armed targets (M = 652 ms, 95% CI [632, 672]),

[3] The likelihood of an error (reported in text as a proportion) and response times were calculated using the lsmeans package in R (Version 2.26-3; Lenth, 2016). Confidence intervals for logistic regression coefficients and condition means assume asymptotic normality and do not take into account degrees of freedom. As a result, differences between conditions may occasionally be significant even if their confidence intervals overlap.

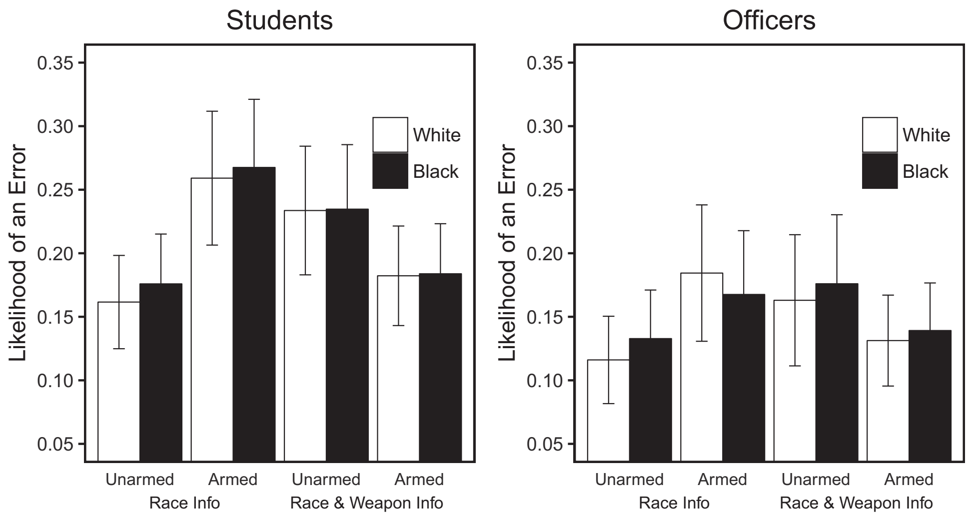

Figure 3. Model predicted likelihood of an error and 95% confidence intervals for students (left panel) and police (right panel). Confidence intervals are asymptotic.

$b = -71.21$, $SE = 7.19$, $p < .001$. Students were also slower to correctly respond to unarmed targets ($M = 540$ ms, 95% CI [522, 557]) than armed targets ($M = 582$ ms, 95% CI [566, 598]), but this difference was smaller, $b = -47.47$, $SE = 7.19$, $p < .001$.

Process-level analyses. By using the DDM we can examine how different components of the decision process were affected by race and prior information. Figure 5 shows condition-level estimates of the threshold, relative start point, drift rate, and nondecision time from the DDM.

Does race influence the decision process when prior information is given? We examined whether race influenced participants' relative start points (H1) or drift rates (H2). Similar to the behavioral analysis, race did not influence relative start points for students ($\mu_{diff} = .009$, $d = 0.18$, 95% HDI [−0.06, 0.44]) or officers ($\mu_{diff} = -.001$, $d = -0.02$, 95% HDI [−0.48, 0.44]). This lack of bias runs counter to the idea that giving accurate race information before each trial might increase the relative starting point for Black individuals (H5). There was also no credible evidence of a race by object interaction in drift rates for students ($\mu_{int} = 0.17$, $d = 0.29$, 95% HDI [−0.11, 0.62]) or officers ($\mu_{int} = 0.12$, $d = 0.14$, 95% HDI [−0.39, 0.68]). Thus, neither H1 or H2 was supported; race did not influence prior biases or how participants accumulated evidence when accurate race information was given in advance.

Does prior weapon information influence the decision process? In contrast to H3, participants' relative start points were *lower* when they were given information that a target was armed than when they only received race information. This was observed for both students ($\mu_{diff} = .070$, $d = 1.37$, 95% HDI [1.06, 1.67]) and officers ($\mu_{diff} = .072$, $d = 1.30$, 95% HDI [0.84, 1.84]). This

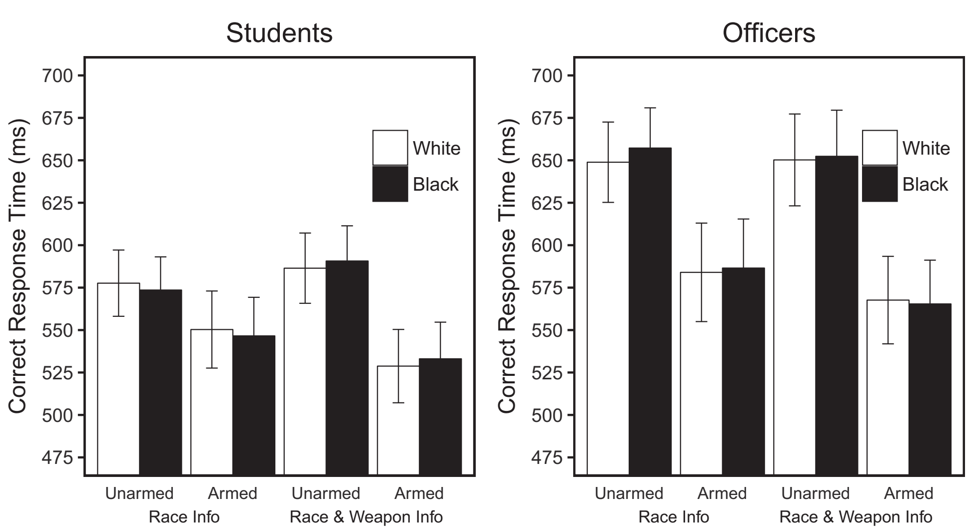

Figure 4. Model predicted correct response times and 95% confidence intervals for students (left panel) and police (right panel). Confidence intervals are estimated using model degrees of freedom.

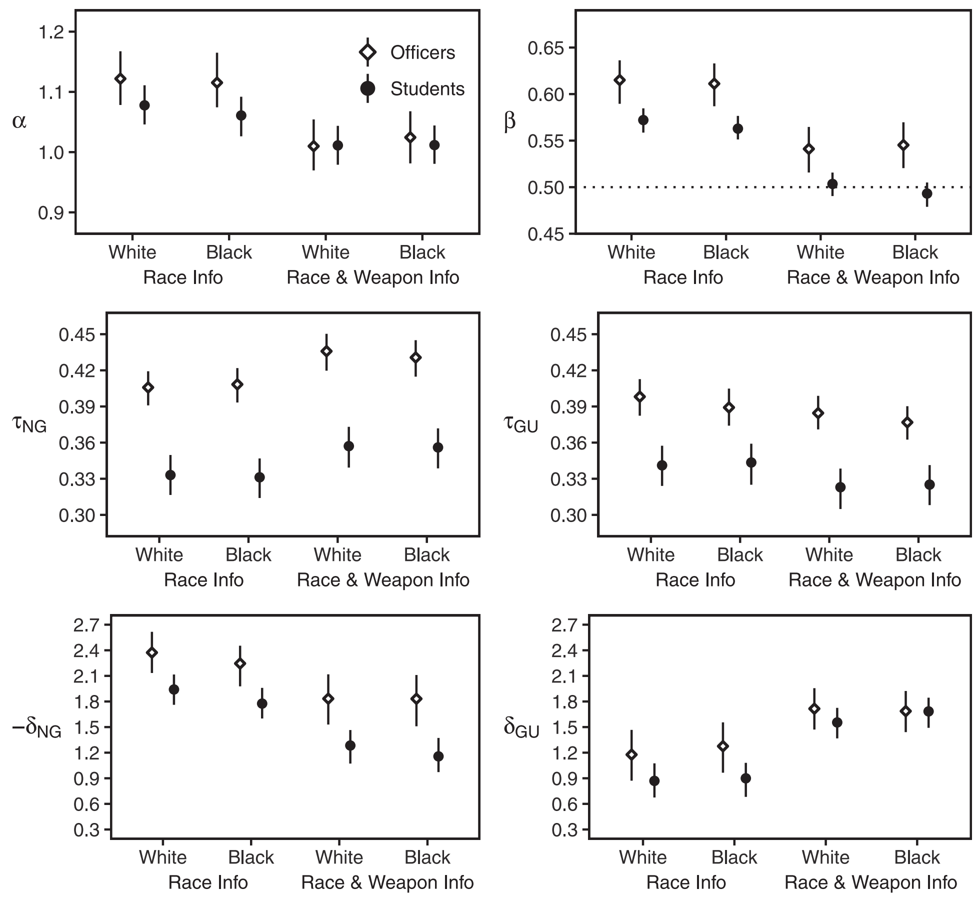

Figure 5. DDM parameters as a function of race, prior information, and object for Study 1. Markers represent mean posterior predictions at the condition level; bars are 95% HDI. NG = Nongun; GU = Gun.

is counterintuitive because the weapon information was reliable and was expected to bias participants to favor the shoot decision. (We address this finding in Study 2.)

We did however find evidence that prior information influenced participants' drift rates (H4). There was a strong interaction between prior weapon information and object, $\mu_{int} = 1.19$, $d = 1.56$, 95% HDI [1.22, 1.91]. When the information correctly identified that a target was armed, both students and officers showed stronger drift rates toward shoot than when only race information was provided, $\mu_{diff} = 0.62$, $d = 0.81$, [0.58, 1.04]. In contrast, when the information incorrectly identified a target was armed, participants showed weaker drift rates to not shoot than when only race information was given, $\mu_{diff} = -0.57$, $d = -0.75$, 95% HDI [−0.98, −0.52]. Thus, weapon information strongly shaped how participants accumulated evidence.

Does police experience influence the decision process? As predicted, officers were moderately better at identifying objects than students (H7), as evidenced by stronger drift rates ($\mu_{diff} = 0.35$, $d = 0.47$, 95% HDI [0.34, 0.64]. This tells us that untrained students perceptual processing of objects may differ from officers. Recall that officers were overall slower in their decisions than students (by 53ms according to the multilevel model). One reason for this might be that officers were more cautious than students, and indeed officers showed higher thresholds than students (H8). However, this effect was small, $\mu_{diff} = 0.028$, $d = 0.20$ [0.02, 0.42] translating into only a 15-ms difference in response time.[4] Instead, the predominant reason that officers were slower than students was longer nondecision times. Officers' nondecision times were on average 66 ms longer than students ($d = 1.09$, 95% HDI [0.93, 1.22]). This finding provides strong evidence that officers are slower because their nondecision processes take longer, perhaps as a result of a slowdown of motor responses with age. Note that officers' decisions were only 53 ms slower than students even though—all else equal—their longer nondecision times and thresholds would have resulted in them being much slower. This is because officers accumulated evidence more quickly than students, increasing response speed. These various counteracting components thus combine to produce the observed response times and shooting responses.

[4] To obtain the difference in response time between officer and student samples here and elsewhere we took the group-level diffusion model parameters for officers and calculated the predicted mean response. We then compared this value with what the predicted mean response time would be if officers showed the same threshold as students.

Discussion

Participants did not show racial bias in the decision process when accurate prior information about the race of a target was always given. Past work on shooting decisions, which has omitted prior information, found race-based differences in drift rates for Black and White targets (e.g., Correll et al., 2015; Pleskac et al., 2017). Because race bias was absent even when the race information was given alone—without weapon information—this suggests that accurate race information may be sufficient to eliminate bias in shooting decisions. However, Study 1 could not test this proposition because we did not include a condition where no prior information was given. We address this issue by manipulating the presence of race information directly in Study 3.

There were also several differences in performance as a function of police experience. Officers were more accurate and slower than students. The DDM revealed that this behavioral pattern was primarily attributable to two different and simultaneous mechanisms: officers were better at distinguishing objects than students (they had higher drift rates) and their nondecision processes took considerably longer than students. These two mechanisms result in slower response times for officers, and obscure that—all else equal—officers are faster and more accurate than students at distinguishing guns from harmless objects.

Finally, prior weapon information had strong effects on evidence accumulation. Officers' and students' drift rates were higher when the prior information was correct. This might be attributable to the prior information shifting people from an exploratory search strategy to a confirmatory one. Prior weapon information helps when the information is correct but hinders when it is incorrect. More surprising, information that the target was armed pushed participants' relative start point to favor *not* shooting. This was unexpected because the start point is thought to index prior bias.

This last counterintuitive finding raises questions about the validity of the DDM as a process model of the decision to shoot. Interpreting the relative start point as a measure of prior bias is dependent on it being sensitive to factors that should change biases. We addressed this issue in Study 2 by validating the start point parameter using an experimental manipulation of payoffs.

Study 2: Model Validation With Payoff Manipulation

The purpose of Study 2 was to validate the relative start point parameter as an index of bias to favor the shoot or do not shoot decision. We first conducted a simulation study (see the online supplemental materials) to test whether the DDM could detect simulated differences in relative start point in an experiment design similar to Study 1. Using the hierarchical model from Study 1 we simulated 100 data sets where there was a predicted condition difference in the relative start point as well as a difference in the drift rate. Then we fit the model to these data sets and found that we recovered the difference 85% of the time (95% HDI [77%, 91%]), with no evidence of bias in the other parameters.

We then sought empirical evidence that the relative start point was sensitive to experimental manipulations designed to influence this parameter. To test this, we manipulated the payoff matrix used in the standard FPST. If this influences the relative start point, it would provide construct validity for the interpretation of the parameter. We also used Study 2 to try to replicate earlier results that isolate the effect of race (without prior information of any sort) to drift rates (Correll et al., 2015; Pleskac et al., 2017).

Method

One hundred five undergraduate women completed two blocks of the FPST with decision payoffs manipulated between blocks.[5] Three participants were removed for careless responding. The remaining 102 participants (M_{age} = 19.0, *SD* = 1.4) were 78.4% White, 7.8% Black, 9.8% Asian, with 3.9% from other groups. Each block contained 160 trials and the order of blocks was counterbalanced across participants.

The basic structure of the FPST was the same as Study 1, except no prior information was provided. To encourage or discourage shooting decisions, we manipulated the payoff matrix for decisions across the two blocks (see Table 2). Participants were informed of these payoffs before the start of each block. In the *encourage shooting* block, shooting an armed target earned participants 25 points, whereas shooting an unarmed target only cost participants 5 points. In contrast, not shooting an armed target cost participants 25 points, whereas not shooting an unarmed target only earned participants 5 points. This creates a situation where choosing to shoot consistently leads to an average payoff of 10 points per trial (vs. −10 for not shooting) when collapsed across object type. In the *discourage shooting* block, the payoffs were mirrored so that choosing to *not* shoot consistently leads to an average payoff of 10 points per trial. In sum, the different payoff rates in the blocks should create a bias to shoot or not shoot.

Results

Behavior-level analyses. Figure 6 shows the decision data for all conditions (left panel). The predicted interaction between object and payoff was significant, $b = -1.960$, $SE = .052$, $p < .001$. As expected, when the payoff structure favored shooting, participants were more likely to shoot unarmed targets ($M = .387$, 95% CI [.343, .430]) and less likely to fail to shoot armed targets ($M = .188$, 95% CI [.161, .214]), $b = -1.004$, $OR = 0.36$, $SE = .112$, $p < .001$. In contrast, when the payoff structure favored not shooting, participants were less likely to shoot unarmed targets ($M = .206$, 95% CI [.175, .237]), and more likely to fail to shoot armed targets ($M = .402$, 95% CI [.361, .441]), $b = 0.956$, $OR = 2.60$, $SE = .112$, $p < .001$.

Importantly, we obtained evidence for race bias in participants' errors (as well as at the process-level, see below), as there was an interaction between race and object in errors, $b = -0.367$, $SE = .189$, $p = .052$. Descriptively, participants were more likely to shoot unarmed Black men ($M = .323$, 95% CI [.274, .372]) than unarmed White men ($M = .270$, 95% CI [.226, .314]) and less likely to shoot armed White men ($M = .304$, 95% CI [.261, .347]) than armed Black men ($M = .285$, 95% CI [.243, .326]), although both these effects were not significant, *ps* > .15. There was no evidence for a three-way interaction, $b = 0.121$, $SE = .104$, $p = .247$.

A multilevel regression was also run on the correct response times. Figure 6 (right panel) shows the response time data. Con-

[5] All studies came from the same pool of undergraduates. Men constitute less of this subject pool than women and were oversampled in Study 1 and 3, which were completed before Study 2. As students could only participate in one study, there were few men left to participate in Study 2. In addition, gender does not appear to moderate racial bias in shooting decisions (Correll et al., 2002).

Table 2
Payoff Values for the FPST by Block

Block	Shooting encouraged		Shooting discouraged	
	Armed target	Unarmed target	Armed target	Unarmed target
Shoot	25	−5	5	−25
Don't shoot	−25	5	−5	25

Note. FPST = First Person Shooter Task.

sistent with past work, participants were faster to respond to guns (M = 510 ms, 95% CI [496, 523]) than nonguns (M = 547 ms, 95% CI [535, 559]), $b = -37.38$, $SE = 5.49$, $p < .001$. This effect was qualified by an interaction with payoff, $b = -50.61$, $SE = 3.19$, $p < .001$. Participants were faster to correctly shoot armed targets (M = 495 ms, 95% CI [482, 509]) than unarmed targets (M = 558 ms, 95% CI [546, 571]) when the payoff structure favored shooting, $b = -62.69$, $SE = 5.83$, $p < .001$. They were also faster to shoot armed targets (M = 524 ms, 95% CI [510, 537]) than unarmed targets (M = 536 ms, 95% CI [524, 548]) when the payoff structure favored shooting, but this difference was smaller, $b = -12.07$, $SE = 5.83$, $p = .043$. There was no evidence of an interaction between race and object predicting response times, $b = -7.54$, $SE = 9.97$, $p = .454$, which is consistent with prior research (Correll et al., 2002; Pleskac et al., 2017) demonstrating that race bias primarily manifests in changes in errors—but not RTs—when response windows are short.

Process-level analyses. Figure 7 shows condition-level estimates of the threshold, start point, drift rate, and nondecision time. The central question was whether the relative start point would capture the effect of the payoff manipulation. There was a large effect of payoff manipulation on the relative start point, $\mu_{diff} = .037$, $d = 0.77$, 95% HDI [0.48, 1.05]. Participants showed an initial bias to favor the shoot response more when payoffs rewarded shooting ($\mu = .535$, 95% HDI [.525, .543]) than when they rewarded not shooting ($\mu = .497$, 95% HDI [.488, .506]). This provides convergent validity for the relative start point parameter as an index of bias.

We also tested whether the payoff matrix influenced other DDM parameters. Providing divergent validity for the relative start point, the payoff manipulation did not influence participants' thresholds ($\mu_{diff} = -0.012$, $d = -0.09$, 95% HDI [−0.30, 0.12]), or nondecision time (μ_{diff} = 8ms, $d = 0.11$, 95% HDI [−0.04, 0.24]). However, payoffs did influence participants' drift rates ($\mu_{diff} = 0.17$, $d = 0.22$, 95% HDI [0.07, 0.37]). This was qualified by a substantial interaction between drift rate and object, $\mu_{int} = 1.74$, $d = 2.17$, 95% HDI [1.81, 2.51].

For armed targets, participants showed stronger drift rates to shoot when shooting was encouraged than when it was discouraged ($\mu_{diff} = -1.08$, $d = -1.32$, 95% HDI [−1.54, −1.07]. For unarmed targets, participants showed stronger drift rates toward *not* shooting when shooting was discouraged than when it was encouraged ($\mu_{diff} = 0.68$, $d = 0.84$, 95% HDI [0.63, 1.09]. Both of these effects were large and demonstrate that the payoff manipulation influences multiple parts of the decision process.

Unlike Study 1, and replicating past research on race bias in the FPST, we found evidence of race bias in participants' drift rates (H2). There was a credible interaction between race and object, $\mu_{int} = 0.46$, $d = 0.57$, 95% HDI [0.23, 0.89]. Participants showed weaker drift rates to not shoot unarmed Black men than unarmed White men, $\mu_{diff} = 0.30$, $d = 0.39$, 95% HDI [0.16, 0.60]. In contrast, participants showed stronger drift rates to shoot armed Black men than armed White men, although this difference was not credible, $\mu_{diff} = -0.14$, $d = -0.18$, 95% HDI [−0.40, 0.04]. There was no evidence of a three-way interaction between race, object, and payoff structure, $\mu_{int} = -0.10$, $d = -0.12$ 95% HDI [−0.79, 0.53], and race did not impact any other parameters in the model.

Discussion

The results of Study 2 validated the relative start point as a measure of prior bias. When shooting was rewarded, participants' relative start

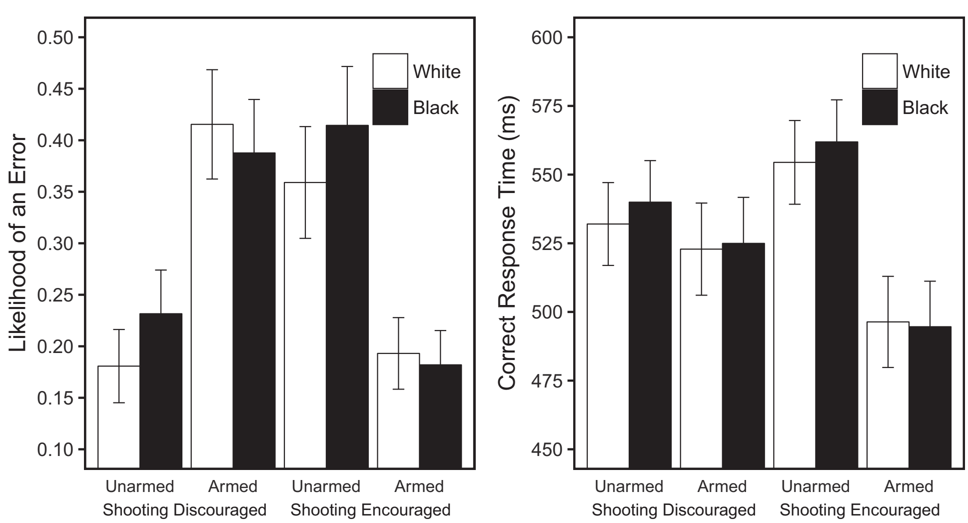

Figure 6. Model predicted likelihood of an error (left panel) and correct response times (right panel) with 95% confidence intervals. Confidence intervals for the likelihood of an error are asymptotic. Confidence intervals for response times are estimated using model degrees of freedom.

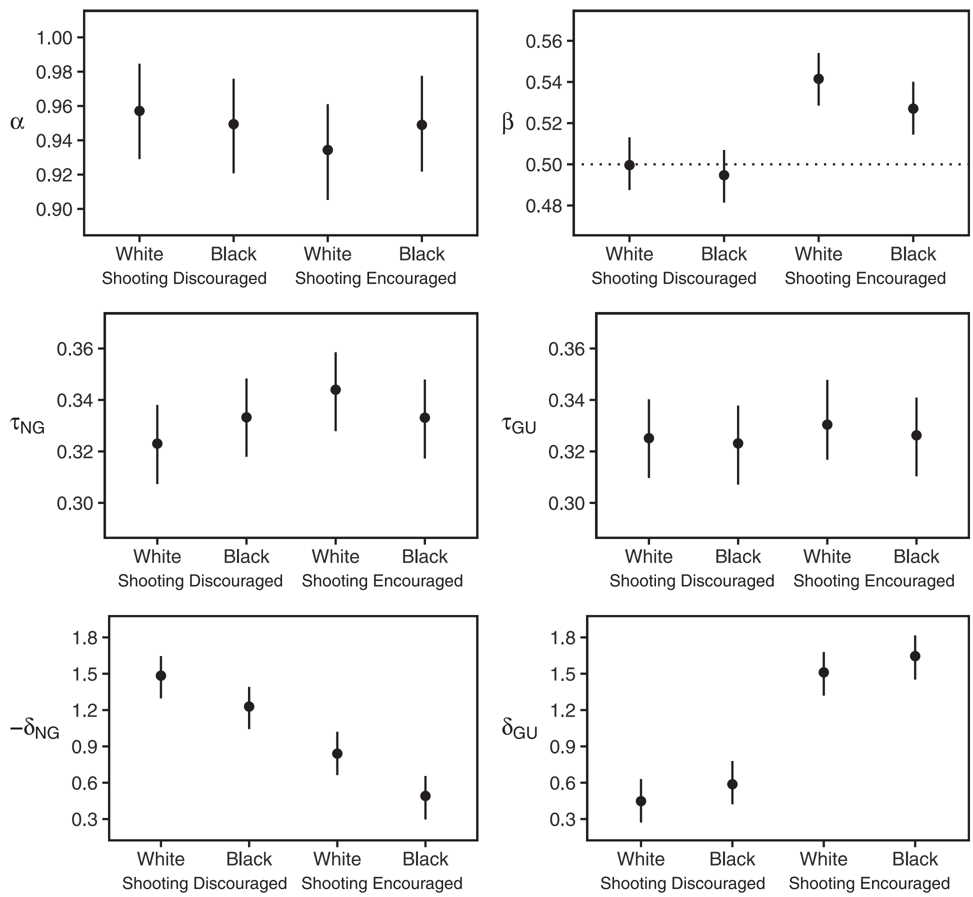

Figure 7. DDM parameters as a function of target race, payoff structure, and object for Study 2. Markers represent modal posterior predictions at the condition level; bars are 95% HDI. NG = Nongun; GU = Gun.

point shifted toward the shoot decision. When the opposite was true, it shifted toward not shooting. These results extend past research (Pleskac et al., 2017) demonstrating that DDM parameters index constructs relevant to psychologists (see also Voss, Rothermund, & Voss, 2004). We also observed that the payoff manipulation influenced the rate at which participants accumulated evidence (i.e., their drift rates). This finding parallels and clarifies results from Study 1. In both studies, encouraging shooting (giving information that the target was armed or giving higher payoffs for shoot decisions) changed participants' prior biases as well as how they accumulated information. Thus, factors that influence preferences to shoot may manifest as both a prior bias, as well as a perceptual or interpretive bias that occurs when the decision is being made.

Replicating past results, participants showed evidence of racial bias in their decisions, and the DDM isolated this effect to the drift rates. Racial bias was not found in Study 1, in which participants were always given prior information. This provides indirect evidence that prior information reduces race bias. Study 3 directly tested the moderating role of prior information by having officers and students complete either the FPST with prior information or the standard version of the FPST with no prior information. This condition replicated past FPST designs and allowed for a test of whether there was evidence for racial bias in shooting decisions when no prior information was given.

Study 3: Prior Information Versus No Prior Information

The goal of Study 3 was to replicate the findings from Study 1 and directly test whether prior information reduced racial bias in shooting decisions. In a blocked within-subjects design (Study 3a), students completed the modified FPST used in Study 1 (prior race information was always given, and prior weapon information was given half the time) as well as the standard task where no prior information was given on any trial. In a between-subjects design (Study 3b), officers completed the modified FPST or the standard FPST without prior information.[6] Because of these design differences, we analyzed the behavioral data from each study individually before analyzing the data together with the DDM.

Method

Participants. One hundred twenty undergraduates completed the FPST. Two students were removed for careless responding. The remaining 118 students (M_{age} = 19.4, *SD* = 2.3) were 75.4% White, 10.2% Asian, 4.2% Black, with 10.2% from other groups.

[6] The between-subjects design with officers was necessary because they participated in the study as a part of their training and did not have time to complete both versions of the FPST.

Men (89.8%) were again oversampled to better match the demographics of officers nationally. We also collected data from officers in a large Midwestern police department. One hundred two officers were recruited. Officers voluntarily completed the study in their department during a yearly training session. Two officers were removed for careless responding. The remaining 100 officers were 90.0% men, with an average of 7.39 years of experience (SD = 7.4, range [1, 30]). A majority of officers (79%) were White, 11% were Black, and 10% were from other groups.

Procedure. Students participated in the laboratory. They completed 160 trials of the modified FPST described in Study 1 and 160 trials of the standard FPST without prior information. Task order was counterbalanced. Officers participated in a quiet room in the training academy. They were randomly assigned to complete either 160 trials of the modified FPST from Study 1 or 160 trials of the standard FPST task. The design of the modified FPST was identical to that used in Study 1. Participants always received accurate demographic information before each trial; on half of the trials they also received information about whether the target was armed with 75% accuracy. This allowed us to compare performance on trials where only race information was presented, where race *and* weapon information were presented, and where *no* information was presented (i.e., the standard FPST).

Results

Study 3a: Behavior-level analyses for students. Figure 8 shows the decision data (left panel). The key question was whether prior information eliminated racial bias in shooting decisions, regardless of whether that information included only the race of the target or race information plus whether they were armed. In support of this hypothesis, there was a significant interaction between race, object, and prior information, $b = 0.416$, $SE = 0.101$, $p < .001$. When students received prior information, they showed no evidence of racial bias, $b = -0.038$, $SE = 0.206$, $p = .85$. However, when students did not receive prior information, they showed evidence of bias, $b = -0.453$, $SE = 0.206$, $p = .028$. This interaction was driven by an increased likelihood to shoot unarmed Black men (M = .231, 95% CI [.188, .275]) compared with unarmed White men (M = .183, SD = .113), $b = 0.295$, $OR = 1.34$, $SE = 0.158$, $p = .062$. Students were also more likely to fail to shoot armed White men (M = .263, SD = .113) than armed Black men (M = .244, SD = .122), although this was not significant, $b = -0.159$, $OR = 0.85$, $SE = 0.164$, $p = .33$.

There was also an interaction between condition and prior information, $b = -0.127$, $SE = 0.050$, $p = .012$. When they did not receive prior information, students were more likely to fail to shoot armed targets (M = .240, 95% CI [.206, .275]) than they were to shoot unarmed targets (M = .207, 95% CI [.177, .238]), $b = 0.195$, $OR = 1.22$, $SE = 0.111$, $p = .077$. This difference was not observed when prior information was given, $b = 0.069$, $OR = 1.07$, $SE = 0.111$, $p = .53$.

A multilevel regression with identical predictors was run on the correct response times. Figure 8 (right panel) shows the response time data for all conditions. Students were faster to correctly respond when targets were armed (M = 512 ms, 95% CI [500, 524]) than unarmed (M = 564 ms, 95% CI [544, 568]), $b = -44.13$, $SE = 5.38$, $p < .001$. They were also faster to respond when given prior information (M = 527 ms, 95% CI [516, 538]) than not (M = 541 ms, 95% CI [531, 552]), $b = -14.62$, $SE = 1.64$, $p < .001$. Finally, there was an interaction between prior information and object, $b = -7.47$, $SE = 3.28$, $p = .023$. Students were faster to respond to armed targets (M = 521 ms, 95% CI [509, 534]) than unarmed targets (M = 562 ms, 95% CI [550, 574]) when given prior information, $b = -47.86$, $SE = 5.74$, $p < .001$. They were also faster to respond to armed targets (M = 503 ms, 95% CI [491, 515]) than unarmed targets (M = 550 ms, 95% CI [539, 563]) when not given prior information, but this difference was smaller, $b = -40.39$, $SE = 5.74$, $p < .001$.

Follow-up analyses were conducted on errors in the modified FPST only. These analyses tested differences between when race

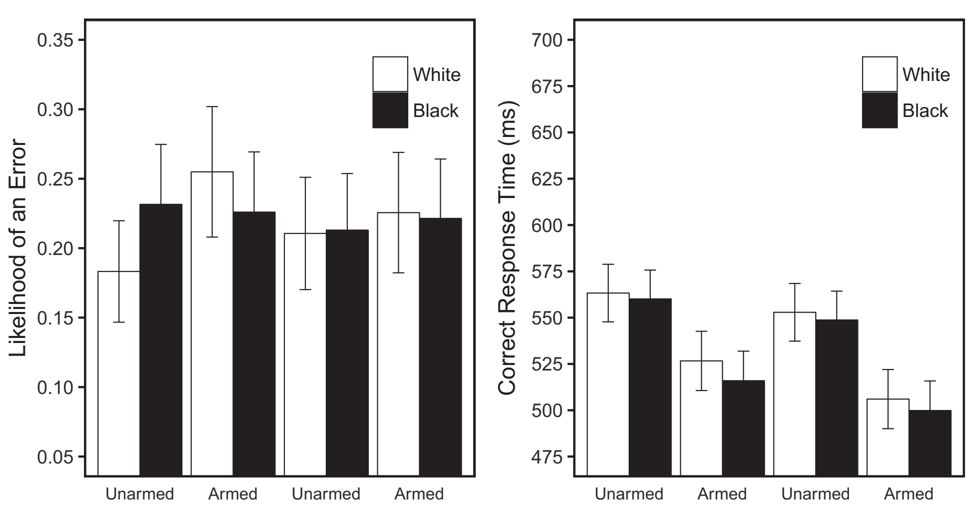

Figure 8. Model predicted likelihood of an error (left panel) and correct response times (right panel) with 95% confidence intervals for students. Confidence intervals for the likelihood of an error are asymptotic. Confidence intervals for response times are estimated using model degrees of freedom.

information was given versus when both race and weapon information were given and are directly comparable with the analyses conducted in Study 1. The expected interaction between object and information was significant, $b = -1.340$, $SE = 0.078$, $p < .001$. When the weapon information correctly identified a target as armed, students were less likely to make an error ($M = .189$, 95% CI [.159, .219]) than when no weapon information was given ($M = .329$, 95% CI [.283, .375]), $b = -0.571$, $OR = 0.57$, $SE = 0.121$, $p < .001$. When the weapon information incorrectly identified a person as armed, students were far more likely to make an error ($M = .292$, 95% CI [.249, .334]) than when no prior information was given ($M = .185$, 95% CI [.156, .215]), $b = 0.769$, $OR = 2.16$, $SE = 0.120$, $p < .001$. Thus, there was strong evidence that students were using the prior information presented to them.

Focusing on the response time data in the modified FPST, students were faster to respond when targets were armed ($M = 508$ ms, 95% CI [495, 522]) than unarmed ($M = 556$ ms, 95% CI [544, 569]), $b = -48.21$, $SE = 6.15$, $p < .001$. There was a significant interaction between object and information, $b = -41.49$, $SE = 5.51$, $p < .001$. When the weapon information correctly identified targets as armed, students were faster to correctly respond to armed targets ($M = 498$ ms, 95% CI [484, 511]) than unarmed targets ($M = 567$ ms, 95% CI [553, 580]), $b = -68.96$, $SE = 6.71$, $p < .001$. When the weapon information incorrectly identified targets as armed, they were also faster to correctly respond to armed targets ($M = 519$ ms, 95% CI [504, 533]) than unarmed targets ($M = 546$ ms, 95% CI [534, 559]), but this difference was smaller $b = -27.47$, $SE = 3.41$, $p < .001$.

Study 3b: Behavior-level analyses for officers. The same analyses were conducted for officers' data, but with type of task as a between-subjects factor. Decision data is displayed in Figure 9 (left panel). Unlike students, officers often show no bias in shooting error rates (Correll et al., 2007). Consistent with past findings, the race by object interaction indicative of bias was not significant, $b = -.187$, $SE = .229$, $p = .414$. There was also no three-way interaction between race, object, and prior information, $b = .232$, $SE = .170$, $p = .172$. Although officers observed performance was not significantly influenced by race, officers did show the same descriptive pattern as students (shooting unarmed Black men 1.8% more than unarmed White men).

Figure 9 (right panel) shows the response time data for all conditions. There was only a main effect of object: officers were faster to respond when targets were armed ($M = 557$ ms, 95% CI [538, 577]) than unarmed ($M = 634$ ms, 95% CI [612, 655]), $b = -76.33$, $SE = 8.53$, $p < .001$.

Follow-up analyses on errors in the modified FPST revealed the expected interaction between object and prior information, $b = -1.267$, $SE = 0.130$, $p < .001$. When weapon information identified a target as armed, officers were less likely to make an error for armed targets ($M = .145$, 95% CI [.116, .173]) than for unarmed targets $M = .217$, 95% CI [.116, .173]), $b = -0.492$, $OR = 0.61$, $SE = 0.158$, $p = .002$. When no weapon information was provided, officers were far more likely to make an error for armed targets ($M = .250$, 95% CI [.202, .298]) than unarmed targets ($M = .133$, 95% CI [.099, .167]), $b = 0.776$, $OR = 2.17$, $SE = 0.157$, $p < .001$. Thus, there was strong evidence that officers, like students, were using the prior information presented to them.

Turning to the response time data in the modified FPST, officers were faster to respond when targets were armed ($M = 556$ ms, 95% CI [532, 581]) than unarmed ($M = 628$ ms, 95% CI [601, 655]), $b = -71.65$, $SE = 8.45$, $p < .001$. Like Study 1, there was a significant interaction between object and information, $b = -25.92$, $SE = 7.83$, $p < .001$. Officers were faster to correctly respond to armed targets ($M = 547$ ms, 95% CI [523, 571]) than unarmed targets ($M = 632$ ms, 95% CI [603, 659]) when weapon information stated the target was armed, $b = -84.63$, $SE = 9.30$, $p < .001$. They were still faster to correctly respond to armed targets ($M = 565$ ms, 95% CI [540, 591]) than unarmed targets ($M = 624$ ms, 95% CI [597, 651]) when no weapon information was given, but this difference was smaller $b = -58.67$, $SE = 9.33$, $p < .001$.

Process-level analyses. Figure 10 shows condition-level estimates of the threshold, relative start point, drift rate, and nondecision time. We started by examining whether race biased partic-

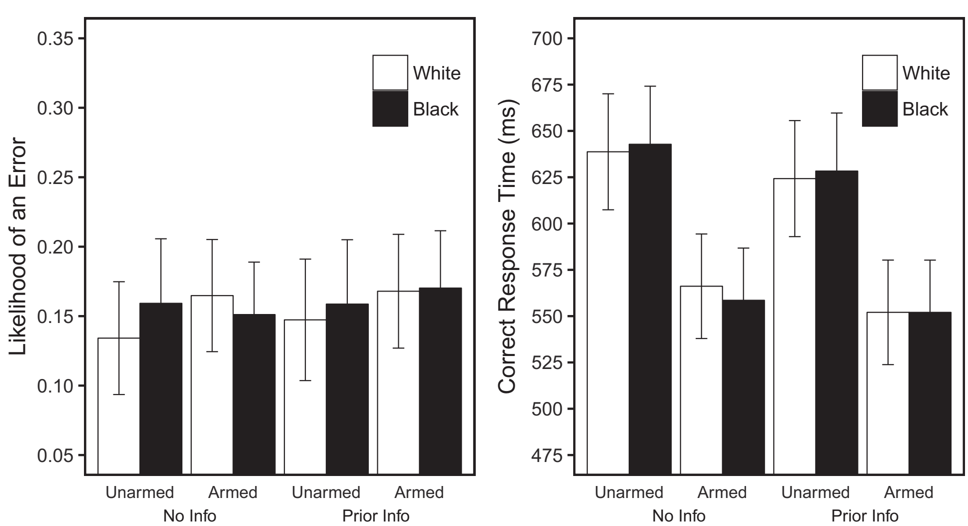

Figure 9. Model predicted likelihood of an error (left panel) and correct response times (right panel) with 95% confidence intervals for officers. Confidence intervals for the likelihood of an error are asymptotic. Confidence intervals for response times are estimated using model degrees of freedom.

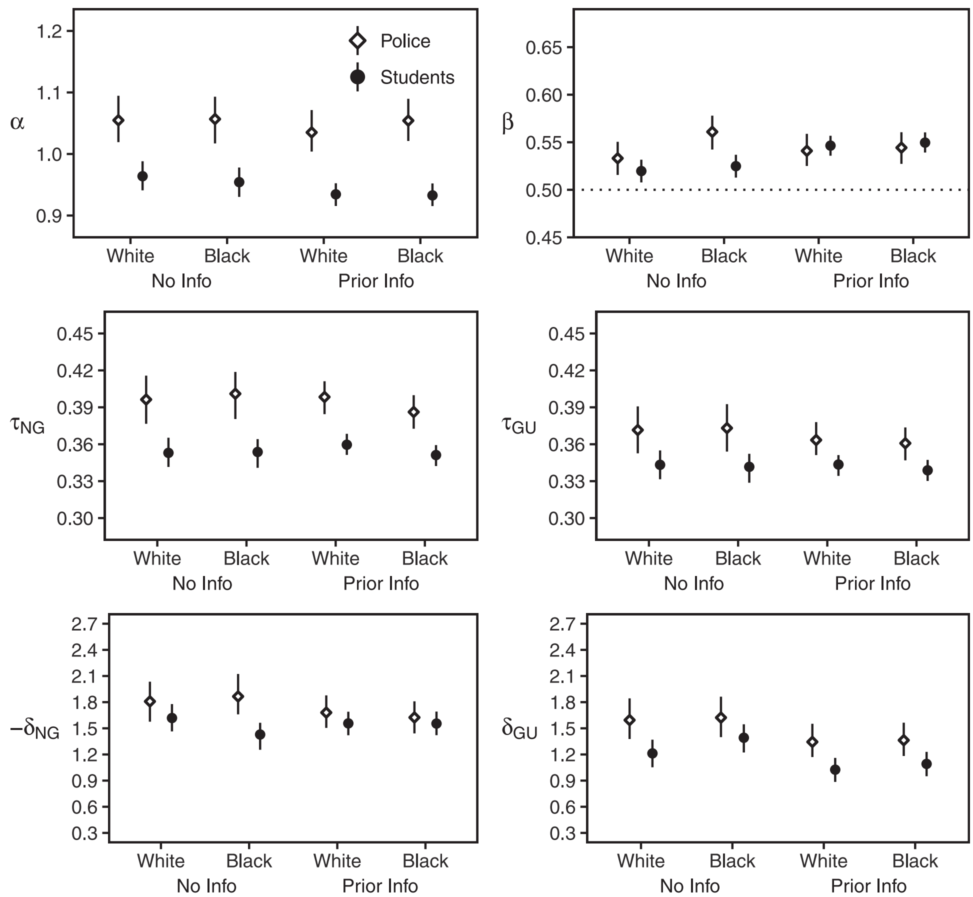

Figure 10. DDM parameters as a function of target race, prior information, and object for Study 3. Prior information is collapsed across trials where only race information was presented versus where both race and weapon information was presented to facilitate comparisons to the no prior information condition. Dots represent mean posterior predictions at the condition level; bars are 95% HDI. NG = Nongun; GU = Gun.

ipants' relative start point (H1) or their drift rates (H2) when participants completed the FPST without prior information, as this is the most direct comparison to prior research.

Does race influence the decision process? Consistent with the results from Study 2, when students did not receive prior information, race influenced drift rates, $\mu_{int} = 0.36$, $d = 0.34$, 95% HDI [0.05, 0.66]. Race did not influence students' relative start point, $\mu_{diff} = -.006$, $d = -0.09$, 95% HDI [−0.32, 0.19]. In contrast, when officers did not receive prior information their drift rates were not influenced by the race of the target, $\mu_{int} = -0.05$, $d = -0.07$, 95% HDI [−.79, .62]. Instead, their relative start point was higher for Blacks than it was for Whites, $\mu_{diff} = .030$, $d = 0.60$, 95% HDI [0.05, 1.12]. Neither students nor officers showed any bias in start point or drift rates when prior information was provided. Thus, prior information about a target's race and whether he was armed was sufficient to eliminate racial bias at the process-level for both officers and students.

Does prior weapon information influence the decision process? Participants' relative start points were again *lower* when they were given information that a target was armed, relative to when they only received accurate race information (in contrast to H3). This was observed for both students ($\mu_{diff} = .044$, $d = 0.64$, 95% HDI [0.42, 0.89]) and officers ($\mu_{diff} = .074$, $d = 1.55$, 95% HDI [0.98, 2.19]). We also replicated the strong interaction in drift rate between prior information and object, $\mu_{int} = 1.69$, $d = 1.94$, 95% HDI [1.65, 2.26]. When the weapon information correctly identified a target was armed, students and officers showed stronger drift rates toward shoot than when only race information was provided, $\mu_{diff} = 0.93$, $d = 1.07$, [0.87, 1.27]. In contrast, when the weapon information incorrectly identified a target was armed, participants showed weaker drift rates to not shoot than when only race information was given, $\mu_{diff} = -0.77$, $d = -0.89$, 95% HDI [−1.08, −0.70]. This was strong evidence for H4, that weapon information influences how participants accumulate evidence.

Does prior race information influence the decision process? We tested whether race information exacerbated prior biases to shoot Black targets more than White targets (H5) by comparing relative start points when no race information was given to when only accurate race information was given. There was no evidence that prior biases changed as a function of this information, $\mu_{int} = .003$, $d = 0.05$, 95% HDI [−0.34, 0.46]. We also tested whether accurate race information alone was sufficient to eliminate race biases in drift rates for students and in the start point for officers

(H6). Students showed no bias in drift rates when they received race information $\mu_{int} = -0.05$, $d = -0.05$, 95% HDI [−0.41, 0.32], or race information and weapon information $\mu_{int} = -0.07$, $d = -0.07$, 95% HDI [−0.45, 0.30]. Officers' relative start points were not credibly higher for Black targets than White targets when they received race information $\mu_{diff} = -.019$, $d = -0.38$, 95% HDI [−1.09, 0.34], or race information and weapon information $\mu_{diff} = .012$, $d = 0.25$, 95% HDI [−0.44, 1.02]. In sum, these results support the conclusion that prior accurate race information alone is sufficient to reduce biases in the decision process.

Does police experience influence the decision process? We replicated the finding that officers accumulated evidence more quickly than students (H7); officers' drift rates were higher than students', $\mu_{diff} = 0.24$, $d = 0.28$, 95% HDI [0.18, 0.38]. Officers' thresholds were again higher than students' thresholds ($\mu_{diff} = 0.108$, $d = 0.74$, 95% HDI [0.59, 0.90]). This difference was much larger than Study 1 ($d = 0.20$) and translates into a 36-ms difference in response times. Finally, officers' nondecision times were on average 32 ms longer than students' ($d = 0.40$, 95% HDI [0.32, 0.49]). This accounted for the other half of the response time difference between officers and students.

Discussion

Study 3 demonstrated that when no prior information was provided, race influenced how students and officers reacted to targets at the process-level. Consistent with past work using student samples (Correll et al., 2015; Pleskac et al., 2017), students showed racial bias in how they accumulated evidence to shoot in the standard FPST paradigm. However, prior work has not examined officer decisions from the lens of the DDM. We found that officers showed a relative starting bias to shoot Black targets in the absence of prior information, despite no evidence of bias when analyzing officer decisions or response times alone. Why did officer bias in the start point not translate into behavioral differences in shooting decisions? This is partially attributable to officers' higher decision thresholds. High thresholds correspond to slower decisions and fewer errors, and so the small prior bias to favor the shoot decision for Black targets was mitigated by increased caution. In other words, as officers made few mistakes in general, there was less room for race to impact the decision to shoot. This pattern also explains why officers descriptively show the same pattern of bias in shooting decisions as students, even though this pattern was not significant.

To explicitly demonstrate how changes in threshold can decrease racial bias in shooting responses attributable to prior biases, we ran a simulation analysis on the officer data from Study 3 (see the online supplemental materials). Holding race bias in start point constant, as threshold increased race bias in decisions decreased to zero. Although race bias was *descriptively* evident under most threshold levels, we replicated the Study 3 finding that it would not have been significant when looking at the likelihood of errors as a function of race. This difference was attributable to officers' high threshold combined with a small relative start point bias (3%). Under less ideal circumstances (e.g., a group of officers with a stronger relative start point bias under greater time pressure) we would expect this bias to influence decisions. This discrepancy highlights the importance of using a process approach like the DDM, which can show how biases at the process-level can be masked at the behavior-level by other components of the decision.

We also found that giving prior information about a target eliminated racial bias in shooting decisions. Prior information reduced bias at both the behavioral and process-level, even though bias manifested in different parts of the decision process for students and officers. Although accurate information about the presence of a weapon had a strong influence on how participants accumulated evidence, providing accurate information about the race of a target to students and officers was sufficient to prevent racial bias relative to when no information was given at all.

Summary of Results

We used the DDM to test nine different mechanisms by which race, prior information, and police experience could impact the decision to shoot. Table 3 lists the hypotheses, ties them to

Table 3
Summary of Evidence for Hypotheses Across Studies

Hypothesis	Process-level prediction	Evidence: Study 1	Evidence: Study 3
H1: Students and officers will show a prior bias to shoot Black targets.	Higher start point β for Black targets than White targets.	No	Officers[a]
H2: Students and officers will accumulate race as evidence for the decision to shoot.	Higher drift rate δ for Black targets than White targets.	No	Students[a]
H3: Information that a target is armed will create a prior bias to shoot.	Start point β closer to shoot when information that a target is armed is given.	No	No
H4: Information that a target is armed will make objects seem more dangerous.	Increased drift rate δ when weapon information is correct; decreased drift rate δ when it is incorrect.	Yes	Yes
H5: Information that a target is Black will create a prior bias to shoot.	Start point β closer to shoot for Black targets than White targets when race information is given.	No	No
H6: Information that a target is Black will prevent race from being accumulated as evidence to shoot.	Race information reduces drift rate difference δ between White and Black targets.	NA	Yes
H7: Officers will be better at identifying objects than students.	Higher drift rate δ for officers than students.	Yes	Yes
H8: Officers will be more cautious than students.	Higher threshold α for officers than students.	Yes	Yes
H9: Officers will have slower motor responses than students.	Longer non-decision processes τ for officers than students.	Yes	Yes

Note. H6 was not tested in Study 1 because race information was always given. NA = Not applicable.
[a] Only when prior information was not given; this information was always given in Study 1.

process-level predictions, and details whether they were supported. We found that target race only influenced the decision process when prior information was not given. When prior information was not given, officers showed start point biases to shoot Black targets (H1) and students accumulated race as evidence for the decision to shoot (H2). These effects translated to a race bias in students' shooting behavior but not officers' shooting behavior. In the case of the officers, the lack of racial bias in their behavior was due to their higher thresholds for making decisions.

In terms of prior information, information that a target was armed shifted participants' start points to favor *not* shooting, rather than shift them to favor shooting (H3). Study 2 confirmed that this counterintuitive result was not attributable to problems with the DDM by validating the start point parameter using an experimental manipulation of payoffs. Information that a target was armed did, however, make objects seem more dangerous in that the estimated drift rates under these conditions pointed more strongly to shooting (H4). Information that a target was Black did *not* shift participants' start points to favor shooting (H5). Rather, race information was sufficient to reduce racial bias in students' evidence accumulation (H6) as well as in officers' prior biases.

Finally, officers were better at identifying objects than students (H7), were more cautious (H8), and had slower motor responses (H9).

General Discussion

Officers responding to an emergency call typically receive, at minimum, demographic information about the person in question from dispatch. Pertinent information about the individual, such as whether he or she is armed, is also passed on to officers. Despite these policies, research has studied shooting decisions in the absence of dispatch information. Although this is a reasonable starting point, extrapolating these results to real-world decisions where officers have dispatch information may present a skewed view of the degree to which racial bias is present.

Dispatch Information and Police Experience as Moderators of Racial Bias

The current studies found that students and officers reliably showed racial bias in the decision to shoot at the process-level when they were not given prior information (a proxy for the information officers get from police dispatch). These process-level biases were eliminated when students and officers received prior demographic information. Thus, *accurate demographic information might reduce racial bias at the process-level*. Moreover, even when officers showed a process-level bias to shoot Black men, it did *not* impact their shooting responses as they showed increased caution. Thus, *the effects of formal police training might reduce racial bias at the behavior-level*. These results suggest that racial bias in shooting decisions, as observed in laboratory studies, might be more likely when an officer is relatively untrained, has no dispatch information about a person, and has to make the decision in a short amount of time.

The fact that race did not influence the decision to shoot when prior information was provided raises the question of the pervasiveness of racial bias in officer shooting decisions. Work using the standard FPST with officers has found mixed results. Sometimes officers show no racial bias in their decisions (Correll et al., 2007). However, officers who routinely interact with minority individuals involved in gang-related crime do show bias (Sim et al., 2013), albeit when no dispatch information is presented.

Research on racial disparities in the real world has also shown mixed evidence for the existence of bias in shooting decisions. Although some research has found evidence of anti-Black bias in officer use of lethal force (Jacobs & O'Brien, 1998; Ross, 2015; Scott, Ma, Sadler, & Correll, 2017; Sherman & Langworthy, 1979; Takagi, 1974), other research has not supported such a conclusion (Brown & Langan, 2001; Cesario, Johnson, & Terrill, in press; Fyfe, 1978, 1982; Fryer, 2016; Geller & Karales, 1981; Goff, Lloyd, Geller, Raphael, & Glaser, 2016; Inn, Wheeler, & Sparling, 1977; Klinger, Rosenfeld, Isom, & Deckard, 2016; MacDonald, Kaminski, Alpert, & Tennenbaum, 2001; White, 2016). The latter set of work has found that apparent racial disparities in police use of lethal force sometimes disappear when controlling for other factors such as crime rates.[8] Although a full discussion of whether officers use lethal force disproportionately for Black civilians is beyond the scope of this research, our work adds that bias in lethal force may be more likely in situations where novice officers have little advance information about the person they encounter.

Why Does Accurate Race Information Reduce Racial Bias?

We focused on two pieces of information commonly given to officers by dispatch, information about the race of a suspect and whether he was armed. One might predict that (accurate) race information would exacerbate racial biases by priming stereotypes of violence for Black men. Conversely, one might predict that advanced knowledge of a suspect's race would allow individuals to better control those stereotypes, reducing bias. Although the DDM does not directly speak to whether stereotypes are applied or suppressed, evidence at the process-level is more consistent with the latter hypothesis. Race bias for officers (in the relative start point) and students (in the drift rate) was reduced when they knew the race of the target beforehand.

At first glance these results may seem inconsistent with existing priming work using faces of Black and White men. Payne (2001, 2006) found that individuals primed briefly (200 ms) with faces of Black men identified weapons faster and more accurately than when primed with faces of White men. Payne reasoned that this was attributable to automatic (i.e., efficient) associations between Black men and violence that facilitated weapon categorization. However, when individuals are exposed to these primes for longer periods of time, they are better able to suppress the activation of such stereotypes, and even respond in counterstereotypic ways (Blair & Banaji, 1996). In the current studies, participants always were exposed to the dispatch information for 2000 ms, giving them ample time to apply such corrective strategies.

One caveat to the conclusion that accurate race information alone is sufficient to eliminate racial bias has to do with the structure of the FPST used in this study. Participants always

[8] Although officers may not use *lethal* force disproportionately against Black individuals, there is evidence for greater law enforcement use of *nonlethal* force (e.g., Tazer use) more with Black individuals than White individuals (Fryer, 2016; Goff et al., 2016).

received demographic information about targets, whereas information that the target was armed was given only on half of those trials. However, because the weapon information was accurate 75% of the time, the base rate of encountering an armed target when only race information was provided was 25%. This means that although weapon information was not *explicitly* provided, participants might have been able to infer that encountering an armed target was unlikely. The degree to which participants identified this pattern and acted upon it is unclear. Future work should directly test whether race information alone is sufficient to reduce racial bias by testing this information in isolation from weapon information.

Although we found that prior accurate race information reduced racial bias at the process-level among both officers and students, another caveat is that this manipulation is an imperfect proxy for how dispatch information is presented. Officers receive dispatch information that is continuously updated for several minutes while the officer travels to an incident. In contrast, our presentation of dispatch information was simplified, focusing on race and weapon information presented for a relatively short period of time. The dynamic stream of dispatch information in the real world may have stronger or unique effects on officer decisions. Information about *why* the officer is called to the scene and *what* has taken place are also likely have important influences on how officers respond to an individual, regardless of their race and whether they are armed.

A broader reason why prior accurate race information may reduce racial bias in the decision to shoot is by its role in reducing ambiguity. Considerable research has stressed that stereotypes are more likely to be used in situations where information is ambiguous (for a review, see Macrae & Bodenhausen, 2000). Consistent with this account, research on shooting decisions shows that increasing decision time (Correll et al., 2002) or providing information about the dangerousness of a neighborhood (Correll et al., 2011) reduce racial bias in shooting errors. Similarly, giving participants information about the race of the target beforehand (as well as information about the presence of a weapon) should also reduce ambiguity. This perspective suggests that it is not race information per se that reduces bias, but the effect of this information on reducing uncertainty. Additional research could test this by manipulating the reliability of race information. As prior race information becomes less certain, the race of a target should play more of a factor in the decision to shoot.

Shooting Decisions at the Process-Level

In our computational model of the decision to shoot, participants start out with a bias to shoot or not. They then collect information until they reach a threshold, at which point a decision is made. We used the DDM to study this decision because its parameters map well onto these components, enabling us to test how race, dispatch information, and police expertise influence shooting decisions at the process-level. This approach isolated the mechanism by which race influences shooting decisions and demonstrated this mechanism varies between students and officers. When dispatch information was not given, students showed racial biases in evidence accumulation that manifested in the drift rate. In contrast, officers showed prior biases to shoot Black men that manifested in the relative start point. These student results are consistent with prior research (Correll et al., 2015; Pleskac et al., 2017), whereas the officer findings present the first research showing how the shooting decision process unfolds for trained officers.

Despite racial bias in the relative start point, officers did not show biases in their shooting responses because of their greater skill at identifying objects and higher decision thresholds relative to students. These factors reduce errors, minimizing the impact of race. This nuanced finding presents a novel advantage of the DDM approach; such effects are difficult to detect when examining decisions or response times in isolation as officers did not show significant evidence of bias in either outcome. In addition, the DDM was able to simultaneously identify changes in performance due to increased caution versus increased skill, as the former increases response times while the latter decreases response times. Other cognitive models (e.g., signal detection or process dissociation) that do not take into account response time information are unable to distinguish such accounts. Finally, one additional benefit is that this approach clarifies the circumstances in which we would likely see racial bias in an officer's decisions—in situations where an officer's threshold is low (e.g., under extreme time pressure), the officer has a greater prior bias (e.g., the suspect has a history of violent behavior), and no dispatch information is given.

The final advantage of the DDM is its ability to disentangle whether dispatch information reduced bias in the decision process by compensating for bias or correcting it. For example, although students showed racial bias in how they accumulated evidence (drift rate), giving them race information might have allowed them to set a relative start point that favored not shooting Black men, eliminating bias in errors (compensation). However, we saw that the race effect for both students and officers was wiped out at its source—students no longer showed bias in the drift rate and officers no longer showed it in the start point (correction). Thus, when accurate race information was given the effect of target race was on shooting decisions was corrected for and not just compensated.

Weapon Information and Prior Bias

Although the results of Study 2 validated the relative start point as an index of prior bias, there still is the question of why providing information that a target is likely to be armed would bias participants to favor not shooting, as indicated by the relative start point shifting closer to the do not shoot threshold. Although the diffusion model does not provide an explanation for why the start point would shift in a counterintuitive fashion, we speculate that it may be caused by the uncertainty of the weapon information. Although participants are warned that the information is generally but not always correct, they do not know the exact rate at which the information is wrong (25%). Insofar as participants want to avoid shooting unarmed men, they may overcorrect their prior bias to shoot (Sommers & Kassin, 2001; Wegener & Petty, 1997), resulting in *less* of a bias to shoot. This explanation could be tested directly by varying the accuracy of prior weapon information and testing how bias changes. We predict that as the accuracy of the information increases, the counterintuitive bias to favor not shooting would reverse. The description of the information given to participants may also matter; describing the information as "generally" accurate may cause participants to overcorrect their bias if in fact the accuracy of the information is near ceiling.

If participants showed a prior bias to favor not shooting after receiving information that a target was armed, how does the model account for the fact that participants in fact were *more* likely to shoot unarmed men? The DDM reveals that this is attributable to weapon dispatch information influencing how people accumulated information. Participants accumulated information more quickly when targets were correctly described as armed. This overwhelmed the counteracting change in prior bias to favor not shooting. This might be attributable to dispatch information changing how people search for information. In the absence of any dispatch information, individuals may search in an *exploratory* way, asking, "what object is that person holding?" When participants receive information that the person is armed, they may search for *confirmatory* information, asking, "is the person holding a gun?"

Although the current experiments do not directly test whether dispatch information influences search strategies, participant self-reports suggest it is a possibility. Multiple officers and students reported that they tried to ignore the information. The behavioral data clearly show these attempts were unsuccessful. If participants were trying to avoid using the information (i.e., avoid prior biases), it may have leaked into their search strategies instead. A central finding of research on confirmation bias is that individuals are unaware they are searching for expectation consistent information (Mynatt, Doherty, & Tweney, 1977; Wason & Johnson-Laird, 1972, for a review see Nickerson, 1998). This would explain why participants thought they had ignored the information even though they accumulated evidence more quickly when it was correct.

Expertise Effects and Caveats

Officers were more accurate and slower than students when making decisions because of several separate process-level mechanisms. First, officers were slower than students because their nondecision processes took longer. This may be attributable to officers being older than college students, although we did not record officer' ages and so cannot test this directly. Second, officers were more accurate than students because they were better at distinguishing guns from harmless objects, as indicated by their higher drift rates. Without this process-level analysis, it might be tempting to conclude that officers' slower and more accurate performance was entirely due to increased cautiousness. However, only in Study 3 was there evidence that officers were substantially more cautious as indicated by their response threshold. This may reflect officers' increased attention to the task or increased caution out of fear of being seen as biased.

A caveat to the above conclusions is that we rely on untrained civilians (students) as a comparison with police officers in a quasi-experimental design. A more ideal design that would reduce confounds would be to compare veteran officers to recently trained police recruits. We attempted to reduce these confounds by matching student demographics to officers nationally (majority male, majority White), but police recruits would be a more natural comparison group. For this reason, we refrain from making causal statements that—for example—officer experience (in years) is responsible for officers increased ability to distinguish guns from harmless objects. This could just as easily by attributable to self-selection; individuals who choose to become officers might be better at this skill. Alternatively, the training recruits receive could be the reason that officers (of any tenure) outperform students.

However, even using recruits as a control group is not a panacea, as expertise is naturally correlated with aging. Although individuals can become police officers at any age, veteran officers will always be older than they were when they were recruits. This is important when considering that officers' response times were far slower than students' response times. Although research has shown that the slowing of response times with age is primarily attributable to increases in the length of nondecision processes (Ratcliff et al., 2001, 2004; 2006; Thapar et al., 2003), understanding the independent role of expertise will require controlling for officer age. Similarly, we cannot rule out that increased officer caution might be due to some other unknown variable. Nonetheless, although there are limitations when comparing officer samples with student samples, our data show clear process and behavior-level differences between the groups. Future research should attempt to clarify whether these differences are due to experience or some other correlated variable.

Training and Policy Implications

The result that prior weapon information influences decisions by impacting the information accumulation process has implications for officer training. Both officers and students accumulated information more slowly when weapon information was incorrect, but officers outperformed students because they were better at identifying objects. Identifying which aspects of their training or field experience improve officers' performance is key to further improving their ability to distinguish guns from harmless objects. Once identified, weapon identification training could be used strategically to assist officers who are particularly poor at quickly identifying objects, as measured by tasks like the FPST.

Training officers in object identification is most likely to help officer decisions in high-pressure situations where they need to rapidly identify weapons. Such training would be just one component of a broader use of force training focused on addressing other factors that officers must consider when using force (e.g., intent of the person, presence of bystanders). In many cases these other components may be more important in predicting whether an officer decides to use force. Nonetheless, given the gravity of accidentally shooting an unarmed individual, this training has a place within a multifaceted approach to improve officer decisions.

Another way to tackle the issue that unreliable dispatch information increases mistakes in officer decisions to shoot is to consider the role of policy in shaping the information that dispatch passes onto officers. In the current studies, giving incorrect dispatch information increased the likelihood that participants mistakenly shot unarmed men. Similarly, in the case of Tamir Rice, dispatch did not share the information from the 911 caller that the pistol was "probably fake" and that he was "probably a juvenile" (Smith, 2015). However, if the uncertainty of this information had been passed onto the officer, this may have changed how he approached the situation and ultimately his decision to shoot. One policy change that could reduce these mistakes would be for dispatchers to ask 911 callers to report how confident they are about the information they give, particularly weapon information. The uncertainty of those judgments then would be passed on to officers, who would be able to use force that is appropriate to not only the level of threat, but also the *likelihood* of threat. The limitation of this approach is that even if such policies are implemented this will not prevent officers from getting incorrect information when it is intentionally misreported. Dispatchers, especially in metropolitan areas, often receive and pass on false reports that weapons are present on scene. Thus, even if dispatch policies are improved, training to identify objects in high-pressure situations is still needed.

Officers also showed a prior bias to shoot Black men when no dispatch information was provided. However, this effect obscures substantial variation between officers in the degree of that bias. Examination of the officer-level relative starting biases reveals that although most officers showed some anti-Black prior bias, certain officers showed up to four times as much bias as the group average (.12 vs. .03). Insofar as these prior biases impact real-world shooting decisions, they represent an opportunity to create targeted interventions to help officers most at risk of making biased shooting decisions. Given that these biases occur *before* officers interact with a civilian, officers might benefit from counterstereotypic training programs targeted toward police officers. Such programs have reduced implicit bias with civilians (Devine, Forscher, Austin, & Cox, 2012; Forscher, Mitamura, Dix, Cox, & Devine, 2017; but see Carnes et al., 2015), and would need to be tested with and tailored to police officers.

It might be tempting to conclude from this study that there is racial bias in officer shooting decisions. We find such a conclusion premature for several reasons. First, as noted above, bias at the process-level does not always manifest in behavior. This is particularly true if there are counteracting processes that reduce the expression of bias, such as increased caution. Second, officers are a heterogeneous group of individuals. Bias in a small group of officers may not translate to bias at the department or national level. Conversely, bias at the department level does not mean that all individual officers within that department are biased. Finally, this bias disappears when dispatch information is present. Because officers frequently receive dispatch information before responding to a call, current laboratory results that do not incorporate this information may overestimate the degree to which racial bias is present in real-world police-civilian interactions.

It would also be premature to conclude there is *not* racial bias in officer shooting decisions. One reason is attributable to our use of a convenience sample of officers. The officers who volunteered for our study might differ in racial bias from the general population of officers. We cannot rule out this possibility, but practical limitations prevented us from recruiting a random sample. Second, although our analysis at the *group-level* revealed no bias in officer shooting decisions, this overlooks the fact that there is variability in that bias among officers. Moving forward, focusing on bias at the *officer-level* may be an effective way to identify and reduce racial bias in officer shooting decisions. A lack of evidence for bias at the group-level can result if bias is uncommon among officers (Walker, 2001, 2005).

Furthermore, if only a minority of officers show racial bias in shooting decisions it would be inefficient and costly to retrain all officers.[9] Data on officer use of force complaints support the hypothesis that some officers are more likely to exhibit deviant behavior. In the first half of 2015, 5% of officers from the New York Police Department were responsible for 80% of citizen use of force complaints; 14% of all officers were responsible for all complaints (Civilian Complaint Review Board, 2015). If bias in the decision to use lethal force is similarly distributed among officers, it would be more effective to target officers in particular need of help, perhaps with additional training.

If racial bias varies among officers, a profitable way forward would be to follow officers who engage in behaviors deemed problematic by departments and communities of interest to understand what individual differences predict such behavior. Initial work (Goff & Kahn, 2012) has identified that concerns about appearing biased and concerns about masculinity might predict differential use of force as a function of race. Linking performance in simulated shooting tasks like the FPST to actual job performance would also help validate whether such tasks reliably predict the problematic behaviors they are intended to simulate. The goal of this work would be to find a constellation of measures and tasks that aid in the selection and recruitment of officers.

Conclusion

Instances like the shooting of Tamir Rice and many others have become catalysts for broader concerns about racial disparities in police use of force. This work illustrates how the integration of social cognition, experimental psychology, and cognitive modeling can begin to illuminate how the decision to shoot is made and when and how race might enter the decision. Our results show that when no prior information was given, the race of the target biased the rate at which untrained civilians accumulate evidence to shoot, whereas for police officers the race of the target impacted prior biases. Regardless, prior information effectively eliminates the biasing effect of race. This pattern of results suggests that in some cases the accuracy of the dispatch information itself may be an important factor in whether an officer shoots or not.

[9] Bias reduction training may have additional benefits in areas other than shooting decisions. However, here we focus specifically on the effects of such training on shooting decisions.

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