Three Essays on Reducing Outcome Bias and Racial Disparities in Decision Making

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In this dissertation, I investigate the presence of outcome bias and racial disparities in various societal institutions such as the labor market, the criminal justice system, and electoral politics. The primary focus of this study is to understand the underlying mechanisms that cause these biases and to propose solutions to mitigate their impact on decision-making.

The first paper is a co-authored paper with Dr. Robizon Khubulashvili and Dr. Sera Linardi. We ask if visually observing effort can reduce outcome bias. Outcome bias is pervasive and persistent across different environments. In our noisy gift-exchange game, where agents can perform a real effort task to improve principals' lottery win probability, we replicate outcome bias in effort rewarding when the effort is only numerically observable. To investigate the role of principals' beliefs about effort cost, we employed a visual treatment in which principals watch a 30-second video of the agents performing the task. We show that visually observing agents' work corrects asymmetry in rewarding effort. The post-experiment survey suggests that the mechanism through which visually observing effort reduces the outcome bias in reciprocating effort is informing principals about the cost of effort.

In the second paper, I study the effect of Black Lives Matter Protests on racial disparities in nonfatal police-civilian interactions. Protests against police brutality and systemic racism have been prevalent in the United States, and most recently hastened by the killing of George Floyd. This paper evaluates how George Floyd protests affect racial disparities in nonfatal police-civilian interactions using police practice data across 17 cities in 12 states and a combination method of regression discontinuity (RD) and difference-in-differences (DiD). The results show that the protests have not impacted the proportion of African Americans in stops, but have reduced the proportion of African Americans in arrests from 30% to 26%. When dividing all interactions into cases in daylight and in darkness, the decreased effect of the protests only holds during the daytime, instead of nighttime when

public supervision is absent. It suggests that Black Lives Matter protests did affect nonfatal police-civilian interactions when it comes to race. However, the day-night differences imply that the decrease in police interactions with African Americans may not be due to the change in police attitudes/beliefs. It is possible that it is a temporary change yielding to strong public attention at that time.

My third paper explores the racial differences in politicians' persistence in elections. Empirical data from California city council elections and a close election regression discontinuity design (CERDD) suggest that losing an election causes 70% attrition in rerunning for office. After a loss, however, nonwhite candidates are 59% more likely to run for office again compared to white candidates. The possibility of winning the subsequent election remains the same for different racial groups conditional on rerunning. As such, the persistence of losing nonwhite candidates contributes to closing the racial representation gap.

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Preface

This dissertation represents the culmination of several years of hard work, perseverance, and support from a number of individuals without whom this accomplishment would not have been possible. Foremost, I would like to express my gratitude to my committee members, Sera Linardi, Daniel Jones, Luke Condra, and Marco Castillio, for their invaluable guidance and direction throughout the research process. In particular, I am indebted to my primary advisors, Sera and Dan, for their unwavering encouragement and belief in me. Their mantra of "do whatever makes you happy" has been a constant source of inspiration and motivation throughout my PhD journey. From the very beginning, Sera urged me to speak up and share my ideas, and her guidance has been instrumental in shaping my research interests and approach. Dan, as co-chair of my committee, has consistently provided thoughtful feedback and patiently advised me on various methodological issues. His support has made completing this dissertation a much smoother and more rewarding process. I would also like to express my appreciation to Luke for his detailed feedback on my papers. Working with Marco and Sera on the Jail project has been a formative experience that has deepened my interest in criminal justice research. Finally, I want to thank my husband, Keith, for putting me in the position to spend all of my time doing something that I love.

1.0 Introduction

In our daily lives, we are often faced with circumstances that are beyond our control. Our gender, race, and the random luck of life are all factors that can significantly impact our decision-making and shape our experiences. Unfortunately, these same factors can also lead to biases and discrimination, particularly in areas such as the labor market, the criminal justice system, and electoral politics.

This dissertation seeks to explore the nature of these biases and discrimination, with the ultimate goal of developing strategies to reduce their impact. By delving deeply into these issues, we hope to gain a better understanding of the mechanisms that perpetuate them and identify ways in which we can work to address them. Using rigorous examinations, this dissertation studies how outcome bias and racial disparities influence decision-making and suggests how to reduce them. The second chapter focuses on outcome bias. The third one is about racial disparities. The last chapter is a combination of outcome bias and racial disparities.

The second chapter explores the impact of random luck on decision-making and presents a potential solution for reducing its influence. Real-world situations often involve outcomes that are affected by various factors, making them a noisy function of intentional choice or effort. Even when the level of effort is fully known, decision-makers may still be tempted to use the outcome to reciprocate the intentions of others toward them. To simulate this situation, my coauthors and I designed a noisy gift-exchange game, in which agents can perform a real effort task to improve the probability of their principals winning a lottery.

Drawing on previous research on reciprocity, we propose a framework that incorporates altruism and perceived intentions in decision-making. Our model predicts that the more effort an agent contributes, the more likely the principal is to perceive the recipient as well-intentioned, leading to greater reciprocity. However, this relationship may be affected by the mental or physical cost of effort.

Our experimental results confirm the presence of outcome bias in effort rewarding when effort is only numerically observable. To investigate the role of principals' beliefs about the cost of effort, we introduce a visual treatment in which principals watch a brief video of the agents performing the task. Our findings demonstrate that visually observing agents' work corrects the asymmetry in rewarding effort. Moreover, we provide suggestive evidence that the mechanism through which visual observation reduces outcome bias in reciprocating effort is by informing principals about the cost of effort.

Overall, our study highlights the potential impact of random luck on decision-making. We propose visual observation as a potential intervention to reduce outcome bias in effort rewarding and improve decision-making. Our findings contribute to the existing literature on reciprocity and decision-making under uncertainty.

The third chapter looks at the effect of Black Lives Matter protests on racial disparities in nonfatal police-civilian interactions. Police practices are characterized by racial bias from police stops, to investigations, arrests, and use of (nonlethal and lethal) force in the United States. Protests against systemic racism within the criminal justice system have been prevalent in the United States in recent years. Starting from the killing of Eric Garner in 2014, the hashtag blacklivesmatters on social media has rendered Black Lives Matter (BLM) a leading proponent of civil rights, racial justice, and police reform. Recently, in the wake of the police killing of George Floyd in May 2020, one of the largest episodes of BLM protest erupted nationally.

This paper evaluates how George Floyd protests affect racial disparities in nonfatal police-civilian interactions using police practice data across 17 cities in 12 states and a combination method of regression discontinuity (RD) and difference-in-differences (DiD). The results show that the protests have not impacted the proportion of African Americans in stops, but have reduced the proportion of African Americans in arrests from 30% to 26%. When dividing all interactions into cases in daylight and in darkness, the decreased effect of the protests only holds during the daytime, instead of nighttime when public supervision is absent. It suggests that Black Lives Matter protests did affect nonfatal police-civilian interactions when it comes to race. However, the day-night differences imply that the decrease in police interactions with African Americans may not be due to the change in police attitudes/beliefs. It is possible that it is a temporary change yielding to the strong public attention at that time.

This paper contributes to the literature in two ways. First, it examines the effect of BLM protests on nonfatal police practices when it comes to race. Second, it uses daytime to proxy the supervision environment to explore the effect of protests on police practices. This paper also provides nationwide timely and detailed police practice data at the individual level.

The last chapter delves into the persistence of politicians in elections and how it varies across racial groups, which sheds light on the drivers of the racial representation gaps in elected offices in the US. While prior research has predominantly explored the demand perspective by examining how racial attitudes and behaviors among voters restrict minority office-holding, there has been little research from the supply perspective, which examines if minorities are underrepresented due to differences in candidate entry and reentry. This paper addresses this gap by assessing the impacts on individuals who have already expressed interest in elected office, specifically, those who have run but lost in the past.

The study utilizes a close election regression discontinuity design (CERDD) and heterogeneous-by-race impacts to investigate whether there is differential attrition between nonwhite and white candidates in response to an electoral loss, as well as how rerunning choices affect subsequent office-holding. The analysis draws on data from 4,617 marginal candidates from city council elections in California.

The findings reveal that narrowly losing white candidates are 16.2 percentage points likely to rerun for office, while losing nonwhite candidates are 25.7 percentage points likely to rerun an election, which is significantly higher than their white counterparts. After a narrow loss, nonwhite candidates are 59% more likely to run for office again compared to white candidates. The probability of winning the subsequent election remains the same for all rerunning candidates. Therefore, the persistence of losing nonwhite candidates contributes to closing the racial gap. This paper is the first to use causal inference to study racial differences in politician persistence and their contribution to minority representation. The results have implications for policies aimed at promoting diversity and inclusion in elected offices.

2.0 The Role of Visually Observing Effort on Outcome Bias

2.1 Introduction

In most real-world situations, outcome is a noisy function of intentional choice (effort). Even when effort is fully known, we are tempted to use the outcome to reciprocate others' intentions toward us. As a result, we reward more in response to successful outcomes compared to unsuccessful ones, even when the amount available for reward is fixed. This empirical phenomenon is called "outcome bias". Examples of outcome bias could be found across different fields, sports (Gauriot and Page, 2019; Lefgren et al., 2015), politics (Wolfers et al., 2002; Cole et al., 2012), finance (Bertrand and Mullainathan, 2001), medicine (Sarsons, 2017), and many more. In this paper, we investigate if the outcome bias is robust to visually observing effort in experimental settings.

To study this issue, we designed a noisy gift-exchange game. The gift-exchange paradigm is relevant for many settings where one party contributes first, and the other one then may or may not reciprocate.[1] Our game starts with the agent performing a real effort task, followed by a dictator game. Agents have an opportunity to exert effort in a real effort task to improve the principals' lottery win probability. Principals in a dictator game are in one of the two information treatments. In the Numerical Information treatment, principal observes the outcome of the lottery and numerical measures of the agent's effort. In the Visual Information treatment, principal sees the numerical measures and watches a 30-second video of the agent performing the task. In both treatments, after receiving respective information, principals decide on reciprocating agents' efforts.

We find that overall principals reward agents 23 cents more after a win than a loss, an effect that is attributable to the difference in pie size. However, what we are interested in is the difference in rewarding effort. When the principal is informed of agent's effort, she

[1] It has been widely applied in different settings such as philanthropy (Falk, 2007) and labor (Brown et al., 2004). Falk (2007) found that charity donation frequency increased when potential principals received small gifts. Kocielnik et al. (2018) shows the existence of the reciprocity mechanism in digital platforms: Wikipedia articles that provide more utility to readers attract more donations.

rewards each 1 std deviation increase in effort by 0.18 in the case of a win and 0.08 in the case of a loss. However, the principals in the Visual treatment do not exhibit this asymmetry, rewarding each 1 std deviation increase in effort by 0.12 no matter whether they won or lost. The post-experiment survey suggests that the mechanism through which visually observing effort reduces the outcome bias in reciprocating effort is informing principals about the cost of effort (a proxy for the cost of effort).

The application of this finding could be far-reaching in environments where it is hard to distinguish luck from effort. For example, one practical implication is to employ this mechanism to reduce tip unfairness in delivering the food. Food delivery had been popular all over the world before COVID-19. The demand for it has risen dramatically during the COVID-19 crisis. Running late and other small mistakes are commonly caused by bad luck which can greatly affect tipping. Most of the workers in the food delivery industry are those who struggle to make ends meet. Our finding suggests that showing the delivery process through animation simulation would reduce tip unfairness. The empirical implementations could also be used in the medical industry (letting the families of patients watch surgeries through a glass wall may reduce the medical malpractice arguments), finance (making the investment decision more transparent), and other economic behaviors.

Experimental economics literature (de Oliveira et al., 2017; Brownback and Kuhn, 2019; Charness and Levine, 2007) has explored outcome bias and found that principals rely on the outcome when they decide on rewarding or punishing agent's effort even when they can observe numerical effort levels and the amount available for rewarding is fixed. Charness and Levine (2007) and Brownback and Kuhn (2019) examined induced-effort experiments and found that both effort and outcome influence giving. Brownback and Kuhn (2019) sparked our curiosity about the role of a non payoff-relevant dimension of effort. They find that principals, even after perfectly observing the numerical measures of effort contribution, still believe that the winning agents are more hard workers than the losing agents. Our study builds on these by designing a new visual intervention that shows not only numerical effort measures but also how hard the agent worked on the task. We used a real-effort task and found that after watching a 30-second video of the agent's work, outcome bias disappeared.

Our paper presents a practical and innovative approach to manipulate the visual observability of real-effort tasks. We use mouse click and movement data to replicate performance videos, providing a way for agents to visually see the effort. To achieve this, we adapted the counting zeros task to the Emoji Selection Task and constructed the video using mouse data. This method is beneficial for online experiments where screen recording is challenging.

2.2 Framework

In this section, we describe the intuition behind our research question. Fundamentally, this is a gift-exchange game between the first-mover agent (Player B) and the second-mover principal (Player A). Our main focus in the model as well as in our experiment is Player A. Player A has an opportunity to give to Player B out of a budget Π. Denote Player A's utility as $u_A(\pi_A, \pi_B)$, where π_A and π_B are, respectively, shares of Player A and Player B from the entire pie $\Pi = \pi_A + \pi_B$. We require Player A's utility function to be increasing in both arguments. For simplicity we assume the payoffs are in Cobb-Douglas form, where ρ indicates how much Player A cares about Player B's payoff.[2]

$$u_A(\pi_A, \pi_B) = \pi_A^{(1-\rho)} \cdot \pi_B^{\rho} \tag{1}$$

Earlier in the game, Player B had an opportunity to complete a real effort task for Player A. Higher performance in the task (e) translates to a higher lottery win probability for Player A. The extensive literature on reciprocity has shown that generosity towards others increases with our perception of others' good intentions towards us. This perception comes not only from the direct impact of others' actions towards us, but also from what it had cost them

[2]Our characterization is similar to Nax et al. (2015) and departs from standard models such as Fehr and Schmidt (1999); Charness and Rabin (2002) in that it allows non-zero shares in environments where A's giving affects B's payoff linearly. The intuition holds for more general sets of utility functions where utility maximization assigns non-zero weight to the other player's payoff amount that is increasing in how much Player A cares about Player B.

to take those actions Zhang and Epley (2009).[3] Real-effort tasks create an opportunity for a "social dimension" of effort that may be rewarded differently than induced effort.[4]

Letting c be what A believes to be B's per-unit cost of effort, A's belief of what B had sacrificed for her can be represented by ce. We define ρ as a function of A's general altruism a and A's reciprocity towards B, which is an increasing function of ce. One simple form this could take is equation 2.

$$\rho = a + ce \tag{2}$$

The literature has suggested that the parameters a and c above are not fixed for Player A and may instead be dependent upon the lottery outcome ($\omega = \{W, L\}$). Some studies have shown that people appear more generous to strangers after experiencing success compared to failure in games,[5] suggesting that a is a function of ω. More importantly, in a principal-agent setting, Brownback and Kuhn (2019) show that principals reward agents more for the same level of (induced) effort when they win the lottery. They suggested that this is because principals believe the agents that are associated with the win are working harder (i.e, are generally higher contributors) than those associated with a loss even when efforts are perfectly known. This is in line with our model of reciprocity as being based not only on the payoff-relevant dimension of others' contributions (e), but also from its non-payoff relevant dimension (c). Their results also suggest that this other dimension is subjective and highly affected by outcomes. All of this implies that both, a and c could be a function of ω, resulting in the equation 3.

$$\rho = \mathbf{1}_{[\omega=W]} \cdot a_H + \mathbf{1}_{[\omega=L]} \cdot a_L + (\mathbf{1}_{[\omega=W]} \cdot c_H + \mathbf{1}_{[\omega=L]} \cdot c_L)e \tag{3}$$

Notice, equation 3 does not imply that we require a and c to be a function of ω. Instead, it allows us to test if a_H and a_L or c_H and c_L are statistically different from each other or

[3]See Van Dijk et al. (2001). Literature shows that individuals usually derive a certain utility or disutility from real effort tasks Sprinkle (2000) which is different from induced-effort tasks, where the cost of effort is fixed and known for each effort level Brown et al. (2004). For example, think about canvassing a neighborhood for signatures for a ballot measure vs taking politicians out to dinner.

[4]While Dutcher et al. (2015) and Brüggen and Strobel (2007) find no difference between rewarding induced-effort and real effort tasks, Lezzi et al. (2015), Heinz et al. (2012), Gneezy and List (2006) do.

[5]See, for example Isen et al. (1973), Sahoo and Misra (1983), de Oliveira et al. (2017). These games have either no material prizes for winning or have controlled for wealth effects in the analysis.

not.

Having defined ρ, we now derive Player A's giving as a function of ρ. Returning to Player A's utility function in equation 1 and normalizing the pie size to $\Pi = 1$, we arrive at equation 4 below, which is maximized when $\pi_B = \rho$.

$$\max_{\pi_B} \ (1 - \pi_B)^{(1-\rho)} \cdot \pi_B^{\rho} \tag{4}$$

Substituting equation 3 into $\pi_B = \rho$ and denoting the indicator function as W if the outcome is 1, we derive A's giving to B as a function of lottery outcomes in equation 5.

$$\pi_B = a_L + (a_H - a_L) \cdot W + c_L \cdot e + (c_H - c_L) \cdot W \cdot e \tag{5}$$

Equation 5 motivates our regression model of interest in equation 6,

$$\pi_B = \alpha + \beta_1 W + \beta_2 \cdot e + \beta_3 \cdot W \cdot e + X'\beta_x + \epsilon \tag{6}$$

where α captures baseline altruism, β_1 is the increase in altruism when A experiences a win $(a_H - a_L)$, β_2 is the baseline rate with which A rewards B's effort, and β_3 is the increased appreciation for B's effort when A wins $(c_H - c_L)$. X are control variables such as pie size Π, gender, and age.

Our primary interest is β_3, which is the coefficient capturing outcome-based reciprocity for contributed effort. A non-zero β_3 shows that A's reward for an effort contribution of e from B depends on A's lottery outcome, suggesting the role of subjectivity in interpreting the intention behind e. As for the sign of β_3, laboratory results tend to show negative outcome bias while empirical studies reflect both positive and negative outcome biases: in other words, agents are rewarded for being lucky and penalized for being unlucky. This suggests that $\beta_1 > 0$ or $\beta_3 > 0$ if $\beta_1 = 0$. This subjectivity, and consequent outcome bias in rewarding effort, may change when Player A can observe Player B during the working process.

This leads us to the two treatments in our experiment. In the Numerical treatment, Player A is given full information about Player B's contribution to her lottery chances in the form of numerical measures (e). In the Visual treatment, Player A sees a video of Player

B's work process in addition to knowing the numerical measures. This allows A to not only know e, but also the costs (c) that went into B's production of e (time, attention).[6] Table 1 summarizes our information treatments. The discussions above imply several testable hypotheses for the treatments.

Treatment	Information Available to principals
Numerical	ω, e
Video	ω, e, c

Table 1: Treatments

Hypothesis 1: In the Numerical treatment $\beta_2 > 0, \beta_3 > 0$

In the numerical treatment, Player A is informed about the effort level e, so we expect β_2 to be positive. However, in the absence of information on c, Player A substitutes her own biased assessment of c. Following findings in previous literature that have suggested that lucky agents are considered hard workers, we expect that the same level of effort is seen as coming from a more worthy source in the case of a win, resulting in $\beta_3 > 0$.

Hypothesis 2: The Visual treatment reduces the outcome bias in rewarding effort (β_3) compared to the Numerical treatment.

In the visual treatment, Player A gains information about c by observing how Player B performed the task. This counters the subjectivity that arises when Player A uses the outcome to infer the cost of effort, resulting in a smaller β_3.

[6] van Rijn et al. (2011) shows how video as an experimental treatment conveys more information than a written description.

2.3 Experimental Design

We designed a noisy gift-exchange game to test our hypotheses discussed below. Agents move first and have an opportunity to do a real-effort computer task that can increase principals' chances of lottery success. Principals observe the outcome of the lottery before deciding how much, if any, to give to the agents. We employ a between-subject design where a principal will give under one of two informational treatments: Numerical (information about the agent's effort), and Visual (information in addition to the numerical information).

We employed neutral language in the study: principals were referred to as "Player A", and agents were referred to as "Player B". Player A faces either Lottery 1 (win – receive \$2.5 or lose – receive \$1.5) or Lottery 2 (win – receive \$3.5 or lose – receive \$2.5). Player A's payoff is determined by a baseline 50-50 chance of winning or losing. Player B can change this winning chance up to a max of 75% (and a min of 25%) by performing a real effort task. After observing the lottery outcome (and other information depending on treatment), Player A is asked how much is she willing to share out of her payoff with Player B who completed that task. We ran the experiment asynchronously by recruiting Player Bs first and Player As several weeks later.

We conducted our study as an online experiment through Amazon Mechanical Turk (MTurk), using oTree (Chen et al., 2016). To identify and exclude careless respondents from the study, each participant had to complete a short comprehension quiz including a computation question which requires careful attention (see Appendix A). If they did not answer all questions correctly after two tries, they were excluded from the experiment. We recruited 628 subjects in total. More than 50% of the subjects (320) were excluded from the experiment because they failed the quiz after 2 tries and received only the show-up fee of \$1. A total of 308 effective participants are used in our analysis.

2.3.1 Agent's real effort task and payoffs

As in Brownback and Kuhn (2019), agents in different treatments are not aware of their treatment status. This implies we are not studying the effects of different treatments on the entire principal-agent game, instead, we fix agents' environment to focus on the effects of different information treatments on principals' giving.

We developed the Emoji Selection Task which asks participants to select one specific kind of emojis from two very close kinds of emojis. The task is similar to Abeler et al. (2011), where subjects had to count the number of zeros in a string. Since our experiment is conducted remotely through Mturk instead of a physical computer lab, we were concerned that the lack of supervision may induce subjects to employ digital tools as a shortcut to performing laborious real-effort tasks. For example, the text from a character-counting task can be copied and pasted into an automatic counter.[7] We therefore used emojis (which cannot be easily automatically counted) to replace the numbers. To make the process easily observable, we adapted the counting task to a selecting task and programmed the screen such that the emoji gets a little bigger when selected and shrinks back down to its original size if deselected. The task is displayed in Figure 6 in Appendix.

In each grid, there are 50 "correct" emojis (peace signs) and 50 "incorrect" emojis (fingers crossed signs). Player B is informed that each selected correct emoji increases Player A's probability of winning by 0.5 percentage points, while each selected incorrect emoji decreases it by the same amount. The probability of winning goes up to 75% if Player B performs the task for Player A perfectly, stays at 50% if Player B skips the task or does not put in any effort, and goes down to 25% if Player B does the opposite of what she is supposed to do. Player B is aware of how these grids affect Player A's expected payoff and the possibility of receiving future shares from Player A.

Regardless of the number of grids completed and their emoji-selecting performance, all Player Bs earned a flat fee of $2. They were told they will receive whatever Player A's share was (if anything) in several weeks. For more information about Player Bs' earnings see Appendix A.

[7] Using PDF or images may not completely overcome the problem since Smallpdf and OnlineOCR and similar free websites can convert them into text in which numbers or characters can be easily recognized.

2.3.2 Principal's Giving Environment: Treatments

Each Player A played 6 rounds, making a total of six sharing decisions. Figure 1 shows the timeline of each round. In each round, Player A is randomly matched with one completed grid from a random Player B.[8] Player A then plays one of two lotteries: Lottery 1 ($2.5 or $1.5) or Lottery 2 ($3.5 or $2.5).[9] Win probabilities vary from 75% to 25% and are determined by Player B's performance on the selected grid. After the outcome is revealed, Player A observes additional information depending on the treatment they are in. Finally, the sharing screen is displayed. In all treatments, this happens 30 seconds after the subject learned of the lottery outcomes.

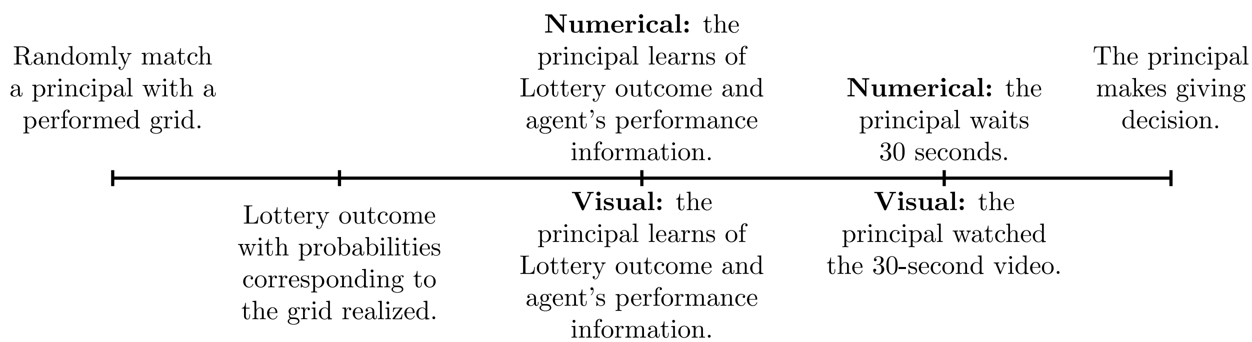

Figure 1: Timeline for each round.

In the Numerical treatment, Player A received information in the form of numerical information on Player B's performance. For example, they might see "Player B selected 49 peace signs and 3 fingers crossed signs and changed your winning chance from 50% to 73%". In the Visual treatment, in addition to the information received in the Numerical treatment, Player A watches a 30-second video of Player B's process of completing the given grid.[10] To make sure Player As watched the 30-second video, the timer was automatically stopped if a

[8]Since the focus of the paper is Player A, matching with Player B's completed grids should have been random across treatments. Otherwise, if Player B's knew the treatment they were in, it could have affected their behavior making identification of Player As responses problematic. Therefore, we had to fix Player B's environment across treatments while varying Player A's information.

[9]Varying between the two lotteries allows us to control for the wealth effect.

[10]Since, on average, Player Bs spent 49 seconds on each grid, the 30-second video permits principals to see most of the work.

subject switched to a different window on their computer and the experiment web-page was not on the main screen. We will describe the video replication in more detail below.

2.3.2.1 The Video Replication

The goal of the Visual treatment is to inform Player A of Player B's process of completing the grid. We, therefore, wanted to avoid potential confoundment from in-group preferences, attractiveness, or racial and gender discrimination. We also do not want to impose the burden of using additional technology (a Zoom recording, for example) on our MTurk subjects. Therefore, instead of recording the subjects' cameras or screens while working on the Emoji Selection Task, we recorded their mouse movements and mouse clicks on the grid as a variable in oTree. For replication, we used JavaScript with the time series data collected from player Bs to replay the video. In the reconstructed video, emojis enlarged when they were clicked and shrank back to normal size when they are deselected.[11] This technique may be useful in allowing visibility to be manipulated in online real-effort experiments.

2.4 Results

2.4.1 Summary Statistics

Our experiment was pre-registered at AEA.[12] Based on power analysis[13], we recruited a total of 165 principals (Player As): 78 in the Numerical treatment and 87 in the Visual

[11]Figure 6 shows agents' screen but principals in the visual treatment also see the similar screen where emojis change the size as agents select or dis-select them.

[12]See preregistration details here: The Observability of Real effort Input and Generosity.

[13]We use a one-tailed test, alpha = 0.05, beta (power) = 0.2 to calculate our sample size. 1) For a general dictator game, under the situation that a dictator needs to allocate 100 units between herself and the recipient, previous studies with similar subjects have shown that the variability is approximately normally distributed with a standard deviation of 25, the minimal relevant difference equals 10. The required sample size for each group is 78 (Gruener (2018)). 2) For a dictator game with real effort design (Heinz et al. (2012)), if the minimal relevant difference is 10 with a standard deviation of 24. The required sample size for each group is 71. Considering information from these two pieces of literature, we will have 80 samples for each treatment.

treatment.[14] Table 15 in Appendix A provides detailed summary statistics. Across all treatments, 39% of Player A subjects are female and the average age is 37 years. Subjects were generally altruistic - out of the extra $1 that was given to them at the end of the experiment to donate to their favorite charity, an average of 38c was donated. There is no difference in the distribution of gender, age, or altruism across treatments.

Player A shared 68c on average with agents out of an average pie size of $2.69 (around 25%). This pie is made up of a starting endowment of $2 or $3 (average $2.62) and lottery wins or losses (+/- 50c). We use the standardized effort for agents' effort in the analysis. Agents' average effort was 73% of the maximum, which increased principals' likelihood of winning the lottery from 50% to 68%. Across all treatments, 57% of the lotteries had a winning outcome. Giving was on average 23c more in the case of wins compared to losses. The distribution of these variables is the same across treatments.

Since the focus of this paper is Player As' behavior we put Player Bs' summary statistics in Appendix A.

2.4.2 Effect of Treatments

The average amount shared in both treatments is, respectively, 72c, and 62c. There are no statistically significant differences in sharing across treatments. However, these averages obscure differences in how effort is reciprocated.

We investigate how principals reciprocate agents' effort contributions more closely through regressions in Table 2. Our baseline specification in equation 7 is motivated by the reduced-form equation 5 from our theoretical framework. Dependent variable Y - principal's giving to agent - is regressed against agents' winning outcome (coefficient - β_1), which indicates how principals reward effort differently at a single point after a win and a lose (which is not our interest), efforts (coefficient - β_2), and our primary interest: the interaction between winning and effort (coefficient - β_3) which captures the outcome bias in rewarding effort.

In all models, we control for round numbers, wealth effects, and individual characteristics (gender, age, altruism) in matrix X. Subjects join our session independently at asynchronous

[14] As a reminder: the principal knows the lottery outcome and agents' performance metrics in the Numerical, while in the Visual treatment principals additionally watch a 30-second video of the agent's work).

times, so we include fixed effects of their start hour as a session control. Standard errors are clustered at the individual level.

$$Y = \alpha + \beta_1 \cdot Win + \beta_2 \cdot Effort + \beta_3 \cdot Win \cdot Effort + X'\beta + TimeFE + \epsilon \tag{7}$$

In Models 1 and 2 of Table 2 we regress each treatment separately. In Model 1, the Numerical treatment, giving increases with effort (0.0817 in the case of losses), and the sensitivity to effort increases substantially more (by 0.1) after a win, providing strong evidence of outcome bias in effort reciprocation. However, this asymmetric rewarding of effort disappears in the Visual treatment (Model 2) where the correlation between giving and effort is larger (0.12) with no significant differences across lottery outcomes.

In Model 3, we use the regression model in equation 8 to investigate if the asymmetric rewarding of effort in the Numerical treatment is corrected by the video in the Visual Treatment. Specifically, we are interested if β_7 cancels out the effects of β_3. In other words, we test if $\beta_3 + \beta_7 = 0$ when we pool the Numerical and Visual treatments together.

$$\begin{aligned} Y =& \alpha + \beta_1 \cdot Win + \beta_2 \cdot Effort + \beta_3 \cdot Win \cdot Effort + \\ & + \beta_4 \cdot Visual + \beta_5 \cdot Visual \cdot Win + \beta_6 \cdot Visual \cdot Effort + \beta_7 \cdot Visual \cdot Win \cdot Effort + \\ & + X'\beta + TimeFE + \epsilon \end{aligned} \tag{8}$$

Indeed Table 2 Model 3 shows that the Visual treatment corrects this bias. While maximum effort is rewarded with an additional \$0.0884 (p=0.077) in the Numerical treatment, the Visual treatment overcomes this bias (-\$0.150 (p=0.048)). The linear combination of the two coefficients ($\beta_3 + \beta_7$) is -0.0616 and it is not statistically different from zero with p=0.287, confirming that outcome bias in reciprocating effort is corrected by the information received from the video.

	(1)	(2)	(3)
	Numerical	Visual	Num.&Visual
Win	-0.0254	-0.00481	-0.000920
	(0.0506)	(0.0492)	(0.0521)
Effort	0.0817**	0.120***	0.0836**
	(0.0347)	(0.0361)	(0.0352)
Win x Effort	0.1000**	-0.0560	0.0884*
	(0.0471)	(0.0560)	(0.0497)
Visual treatment			-0.0524
			(0.0790)
Visual x Win			-0.0195
			(0.0739)
Visual x Effort			0.0154
			(0.0520)
Visual x Win x Effort			-0.150**
			(0.0752)
Constant	0.0102	-0.204	-0.0159
	(0.309)	(0.259)	(0.233)
Observations	468	522	990
Clusters	78	87	165
R-squared	0.529	0.315	0.374

*** p<0.01, ** p<0.05, * p<0.1. We control for round and wealth effects, individual characteristics (gender, age, altruism), and session start hour fixed effects in all specifications. Robust standard errors in parentheses are clustered at the individual level.

Table 2: Giving as a function of lottery outcomes and agent's earlier effort contribution.

2.4.3 Mechanism

To explore the mechanisms behind our experimental results, we surveyed principals about the task and their feelings towards the agents on a 5-point scale at the exit survey. Table 15 summarizes our survey results, where a higher number indicates higher agreement. The visual treatments significantly affected two beliefs. First, compared to the Numerical treatment, the video decreased principals' perception of task annoyance from 2.64 to 2.23 (-0.41, p=0.03). Second, principals in the visual treatment were more confident that they understood how the agents worked.

Which subjective belief serves as the main mediator affecting the outcome bias? In Section 4.3, Table 2 Model 3 highlights that β_7 (visual treatment) cancels out the outcome bias seen in the numerical treatment. How do principals' beliefs impact the cancellation effect? In Table 3, we added each subjective belief as a control variable in the pooled data. In Model (2), the coefficient of annoyance is positive and significant, indicating that principals' perception of task annoyance (a proxy for the unit cost of effort) is a crucial driver of reciprocity. Moreover, the coefficients on Visual x Win x Effort decrease and become insignificant after controlling annoyance. However, in Models (3) - (7), β_7 remains significant. This implies that the primary effect of the Visual treatment on outcome bias stems from how the video altered principals' perceptions of how annoying the task is – the cost of effort.

To further investigate whether task annoyance can affect outcome bias as a mediator variable, we control how annoying the task was in each treatment. Table 4, Models (2) and (4) indicate that principals who find the task more annoying appreciate agents' efforts by sharing more in every treatment. More importantly, after controlling for the principal's perception of how annoying the task is, Model (2) shows that the outcome bias in reciprocating effort (Win x Effort interaction) disappears in the numerical treatment. This finding is consistent with Table 3, suggesting that outcome bias stems from the subjective evaluation of principals on how deserving the agent is. Specifically, the mechanism of visibility in reducing outcome bias stems from the precision and confidence in effort cost.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Win	-0.000920	0.00707	-0.00192	-0.00209	-0.0118	-0.00668	-0.00321
	(0.0521)	(0.0512)	(0.0514)	(0.0523)	(0.0516)	(0.0513)	(0.0523)
Effort	0.0836**	0.0936***	0.0899**	0.0839**	0.0898**	0.0854**	0.0827**
	(0.0352)	(0.0332)	(0.0345)	(0.0352)	(0.0350)	(0.0350)	(0.0356)
Win x Effort	0.0884*	0.0618	0.0704	0.0886*	0.0759	0.0921*	0.0908*
	(0.0497)	(0.0491)	(0.0502)	(0.0496)	(0.0511)	(0.0485)	(0.0501)
Visual treatment	-0.0524	-0.0150	-0.0498	-0.0557	-0.0857	-0.0764	-0.0538
	(0.0790)	(0.0731)	(0.0771)	(0.0775)	(0.0784)	(0.0765)	(0.0787)
Visual x Win	-0.0195	-0.0315	-0.0226	-0.0179	0.00657	-0.00116	-0.0166
	(0.0739)	(0.0743)	(0.0742)	(0.0740)	(0.0714)	(0.0711)	(0.0735)
Visual x Effort	0.0154	-0.00186	0.0125	0.0147	0.000363	0.0134	0.0143
	(0.0520)	(0.0500)	(0.0510)	(0.0518)	(0.0493)	(0.0496)	(0.0518)
Visual x Win x Effort	-0.150**	-0.116	-0.129*	-0.150**	-0.128*	-0.154**	-0.151**
	(0.0752)	(0.0741)	(0.0742)	(0.0751)	(0.0717)	(0.0715)	(0.0754)
Task is annoying		0.0878***					
		(0.0292)					
Task is difficult			0.0597				
			(0.0390)				
I know how recipients worked				0.00707			
				(0.0303)			
Recipient is like me					0.104***		
					(0.0351)		
Feel close to recipient						0.0839**	
						(0.0348)	
Recipient had worked for me							0.0270
							(0.0423)
Constant	-0.0159	-0.169	-0.126	-0.0439	-0.218	-0.221	-0.112
	(0.233)	(0.242)	(0.247)	(0.262)	(0.255)	(0.234)	(0.269)
Observations	990	990	990	990	990	990	990
R-squared	0.374	0.395	0.381	0.374	0.398	0.392	0.376

*** p<0.01, ** p<0.05, * p<0.1. We control for round and wealth effects, individual characteristics (gender, age, altruism), and session start hour fixed effects in all specifications. Robust standard errors in parentheses are clustered at the individual level.

Table 3: Effects of different post-experiment survey variables on giving

2.5 Conclusion

Most of the time we are in an environment where outcome is affected by random noises. In such settings, accurately evaluating effort is essential. When success is positively affected by effort and effort is not observable, we might use the outcome to infer effort. As a result, we give more in the case of a successful outcome, or a win, compared to a loss. Literature on outcome bias has shown that there is asymmetry on giving (reciprocity) even when the effort

	(1)	(2)	(3)	(4)
	Numerical		Visual	
Win	-0.0254	-0.0189	-0.00481	-0.00557
	(0.0506)	(0.0495)	(0.0492)	(0.0488)
Effort	0.0817**	0.0903***	0.120***	0.112***
	(0.0347)	(0.0330)	(0.0361)	(0.0354)
Win x Effort	0.1000**	0.0752	-0.0560	-0.0423
	(0.0471)	(0.0468)	(0.0560)	(0.0553)
Task is annoying		0.0907**		0.103**
		(0.0360)		(0.0460)
Constant	0.0102	-0.162	-0.204	-0.321
	(0.309)	(0.326)	(0.259)	(0.254)
Observations	468	468	522	522
R-squared	0.529	0.550	0.315	0.338

*** $p<0.01$, ** $p<0.05$, * $p<0.1$. We control for round and wealth effects, individual characteristics (gender, age, altruism), and session start hour fixed effects in all specifications. Robust standard errors in parentheses are clustered at the individual level.

Table 4: Effects of annoyance on giving in each treatment

is perfectly observable. Where can this (non-payoff relevant dimension) be coming from?

We study how the change in information availability for judging an agent's contribution affects the principal's decision to reward (reciprocate) the agent's effort. We find that displaying only the numerical effort leads principals to reward effort asymmetrically by rewarding effort more after a win compared to a loss. Such an outcome bias disappears in the Visual treatment where principals watched a 30-second video on how the agents performed the task.

Our contribution is twofold in experiments. First we contribute to the literature on outcome bias and show that visually observing effort corrected the outcome bias in reciprocating effort. Also, unlike other outcome bias papers, we used real effort instead of induced effort. This change allowed us to investigate a new dimension of effort observability – the visual. Second, we contribute to experimental economic methodology by providing a novel and practical way to manipulate the visual observability of real effort tasks using mouse click and mouse movement time-series data to replicate the performance video.

3.0 The Effect of Black Lives Matter Protests on Racial Disparities in Nonfatal Police-Civilian Interactions

3.1 Introduction

Police practices are characterized by racial bias from police stops, to investigations, arrests, and use of (nonlethal and lethal) force in the United States (Kochel et al., 2011; Ross, 2015; West, 2018; Pierson et al., 2020). Protests against systemic racism within the criminal justice system have been prevalent in the United States in recent years. Starting from the killing of Eric Garner in 2014, the hashtag blacklivesmatters on social media has rendered Black Lives Matter (BLM) a leading proponent of civil rights, racial justice, and police reform. Recently, in the wake of the police killing of George Floyd in May 2020, one of the largest episodes of BLM protest erupted nationally (Reny and Newman, 2021).

Literature on the effects of BLM protests mainly focuses on two types of outcomes: public attitudes (Sawyer and Gampa, 2018; Mazumder, 2018, 2019; Wasow, 2020; Reny and Newman, 2021) and fatal police interactions (Skoy, 2021; Campbell, 2021). For public attitudes, Reny and Newman (2021) conclude that protests decreased racial resentment and favorability toward the police, and increased perceived anti-Black discrimination among low-prejudice and politically liberal Americans. Mazumder (2019) finds that protests reduce racial resentment mainly through attitude changes of young people. Using protests between 1960 and 1972, Wasow (2020) identifies the different effects of nonviolent and violent protests on votes for Democrats and Republicans. As for fatal police interactions, Skoy (2021) suggests a decrease in fatal interactions between Black civilians and police in the month after the protests. Campbell (2021) examines the long-term effect of protests and finds that they have decreased fatal police interactions 15% - 20%.[1]

This paper asks have the protests affected the racial disparity in nonfatal police interactions. To explore it, I collect individual-level police practice data across 17 cities in 12 states. All data include race information of the involved civilians and other demographic

[1] For more details about the literature please see Table 16 in the Appendix.

information. In total, there are 796,854 stops[2] and 303,855 arrests cases from January 1st, 2019 to December 31st, 2020.[3]

Using RD-in-time and DiD, my data show that the 2020 George Floyd protests have significantly reduced the proportion of African Americans in police arrests from 30 to 26 percentage points. However, the effect of the protests is not significant on stops. When breaking down the cities based on the protest characteristics, the decreased effect holds in the "high protest areas" and the "early protest areas". These results are robust to alternative specifications.

To explore the mechanisms driving the change in police interactions with African Americans after the BLM protests, I employ a day-night division for all police practice cases inspired by the method of "veil of darkness" Grogger and Ridgeway (2006). In arrests, the protests only reduce the share of African Americans during the daytime. There is no evidence that the race distribution of police arrests during nighttime has changed after the protests. For stops, the effect of the protests on the proportion of African Americans remains insignificant during daytime and nighttime. This suggests that the change in police interactions when it comes to race after the protests is a temporary change yielding to the strong public attention at that time.

For nonfatal police interactions[4], Morgan and Pally (2016) and Shjarback et al. (2017) study the effect of the protests on "de-policing" in arrests and stops. Cheng and Long (2022) confirm this reduction in police nonfatal activities after the protests using a more strict method in the same time period. These three papers talk about the intensity of police interactions with the whole population after the protests. How protests affect nonfatal police behaviors in the racial composition is understudied in previous literature.

This paper contributes to the literature in two ways. First, it examines the effect of BLM protests on nonfatal police practices when it comes to race. Second, it uses daytime to proxy the supervision environment to explore the effect of protests on police practices. This paper

[2]They include 629,105 traffic stops (79%) and 167,749 pedestrian stops (21%).

[3]The stops and arrests data are distinct data sources – arrests are not restricted to arrest-made resulting from traffic stops.

[4]Nonfatal police interactions are more common. According to https://ucr.fbi.gov, law enforcement made over 10 million arrests in 2019. Baumgartner et al. (2018) show that more than 20 million Americans are stopped each year for traffic violations.

also provides nationwide timely and detailed police practices data at the individual level.

The rest of the paper proceeds as follows. Section 4.2 provides a simple theoretical frame. Section 3.3 describes my data and methods. Section 4.3 presents results. Section 3.5 explores the possible mechanisms and Section 4.4 concludes.

3.2 Framework

To explore the effect of protests on police encounters, I use a regression discontinuity in time approach to examine how the protests change the proportion of African Americans in police stops and arrests. The effects of the protests can be written as:

$$\tau_s = \lim_{P_{t\downarrow 0}} \frac{Pr(S|Post, B)}{Pr(S|Post)} - \lim_{P_{t\uparrow 0}} \frac{Pr(S|Pre, B)}{Pr(S|Pre)} \tag{9}$$

$$\tau_a = \lim_{P_{t\downarrow 0}} \frac{Pr(A|Post, B)}{Pr(A|Post)} - \lim_{P_{t\uparrow 0}} \frac{Pr(A|Pre, B)}{Pr(A|Pre)} \tag{10}$$

where S and A are binary variables indicating whether the officers decide to stop or arrest a suspect. Post and Pre represent after or before the protests. B denotes the subject as Black or African American. I use a regression discontinuity in time to show the effect of the protests. The running variable P_t refers to the number of days before (< 0) or after (> 0) the protests. $P_{t\downarrow 0}$ is when the running variable approaches zero from the right (among the days after the protests). $P_{t\uparrow 0}$ is when it approaches zero from the left (among the days before the protests).

The first part of Equation 9 is the relative risk of an African American suspect being stopped compared to other racial groups exactly after the protests. The second part is the relative risk of an African American subject being stopped before the protests. Due to the dynamic changes around the protests, I use the relative risk as my main outcome variable to control the changes in police exposure and other unobserved factors. Equation 10 is the change of the relative risks of an African American subject being arrested around the protests. τ_s and τ_a represent the changes in relative risks of being stopped or arrested due to the protests.

How do protests change racial disparities in police stops and arrests? Literature shows the George Floyd protests have increased the perceived anti-Black discrimination in public opinion (Reny and Newman, 2021). On one hand, police opinion or attitudes might have changed as a part of public opinion, directly affecting racial disparities both in stops and arrests. On the other hand, the protests can also impact racial gaps in an indirect way. Because of the increase in public attention on policing, the cost of being perceived as racial profiling increased, which may also impact police behavior. In this case, we expect arrests - which are more visible to the public - to be more impacted than stops. It is also possible that the protests have not impacted the racial disparities in nonfatal police practices. The two pathways and the no-effect possibility introduce three hypotheses:

Hypothesis 1: $\tau_s < 0$ and $\tau_a < 0$

Direct Effect on Police Attitudes: The proportion of African Americans decreases both in stops and arrests after the protests. Then police change their interactions with African Americans. The direct mechanism works. Police have changed their attitudes and behaviors.

Hypothesis 2: $\tau_s = 0$ and $\tau_a < 0$[5]

Indirect Effect on Perceived Increase in Public Monitoring: The protests have not affected the proportion of African Americans in police stops. But the share of African Americans in arrests has reduced. Under this hypothesis, protests do not work in changing police attitudes towards African Americans. Instead, the protests work through public supervision. Stops usually happen in a fast-drive environment which is less likely to be affected by public attention. But for arrests, when the public pays more attention to police practice or misconduct, the cost of being perceived as racial profiling has increased after the protests. The share of African Americans in arrests decreases. In this way, the indirect mechanism works. Police have not changed their attitudes, but behaviors have changed due to highlighted monitoring.

[5] More realistically, it can also be written as $\tau_a < \tau_s \leq 0$

Hypothesis 3: $\tau_s = 0$ and $\tau_a = 0$

The protests have not affected the relative risk of an African American subject being stopped or arrested. Police have not changed their behaviors in the nonfatal interactions with African Americans.

To further disentangle the two mechanisms, inspired by the method of "veil of darkness" proposed by Grogger and Ridgeway (2006) where darkness serves as a proxy of visibility, the protests might work differently during different daylights. The effect of the protests from public monitoring is more feasible during the daytime. In this way, we can divide all cases into daytime and nighttime. Considering the two pathways now, if the direct pathway that the protests change police attitudes works, there should be no day-night difference. The protests would reduce the proportion of African-Americans in stops and arrests both during daytime and nighttime. Hypothesis 1 will be expanded as: $\tau_{s-day} = \tau_{s-night} < 0$, and $\tau_{a-day} = \tau_{a-night} < 0$. If the indirect pathway works where police changed their behaviors because of public scrutiny, the only situation where the protests could work is police arrests during the day. The hypothesis 2 can be written as $\tau_{s-day} = \tau_{s-night} = \tau_{a-night} = 0$, and $\tau_{a-day} < 0$.

3.3 Methods

3.3.1 Data

This paper draws on two main categories of outcomes: stops and arrests. The data are from the official websites of local law enforcement agencies (local governments, police departments, and regional data centers). Police Data Initiative (PDI)[6] lists local law enforcement agencies that might provide open and timely police practice data. Based on the search results in PDI, I checked the websites of 19 agencies for stop data and another 20 agencies for arrest data. To get more open data across the United States, I searched 43 websites for

[6]https://www.policedatainitiative.org/datasets/

police practice data.[7]

This study requires individual-level data: including race information of the stop or arrest subjects; incident time has to be precise to the hour and minute because of the day-night examination; and data dating back to the year 2020 or earlier.[8] Among all searches, stop data from ten cities and arrest data from a different set of eight other cities satisfied these requirements.[9] The ten cities where police stop data are used in this paper are: Chicago, Fayetteville, Gaithersburg, Louisville, Middletown, Minneapolis, New Orleans, New York, Philadelphia, and Seattle. After removing the observations for which race is missing or unknown, it includes 542,411 observations for the year 2019 and 254,443 for the year 2020. The stops and arrests data are distinct data sources – arrests are not restricted to those resulting from traffic stops. Arrest data are from another eight cities: Baltimore, Bremerton, Chandler, Charleston, Fayetteville, Lincoln, Pittsburgh, and Tucson.[10] Arrest data includes 171,407 observations for the year 2019 and 132,448 for the year 2020. Police practice data also includes the gender and age information of the subjects. Officers' demographic information (race, gender, and age) is available in Louisville and Seattle.[11]

Some additional analysis takes advantage of more detailed investigation data, stemming from police data. Nine out of the ten cities have some investigation resolutions after the stop.[12] It includes no action taken, warning issued, citation issued, and arrest made. "Arrest made" is one of the investigation resolutions. But note that "arrest made" is different from the arrest data used in this paper. Arrest data is a whole new data set that is drawn from different cities.

The George Floyd protests happened in 2020, but 2020 was a rare year. The onset of the COVID-19 pandemic brought a nationwide lockdown which dramatically impacted almost all aspects of life, including policing. Both the pandemic and protests may cause police stops to drop tremendously and rapidly. To better control the effect of the lockdown, I retrieved the

[7]More search details are in Table 17 in the Appendix.

[8]Technically, it requires racial distribution daily data but separates daytime and nighttime cases. It is not a natural data format. In this way, having individual-level data can solve this problem.

[9]Cities provide stop and arrest data separately.

[10]Fayetteville is the only city that includes both stop and arrest data.

[11]I checked the heterogeneity for officers' race. The results are not significant. It might be due to the lack of information from other cities or the low variety of officers' races: 19% of the officers are Black officers.

[12]More information about investigation data availability is in Table 18.

lockdown start date for each city.[13] For more details please see Table 19 in the Appendix.[14]

Different cities started the protests at different times with different magnitudes. I retrieved the protest dates using the keyword "George Floyd" for each sample city from the Armed Conflict Location and Event Data Project (ACLED).[15] I use the frequency of the total protests for each city after the murder of George Floyd until the end of June 2020 as the index of the magnitude.[16] I use the median to divide all cities into "high protest areas" and "low protest areas". I also use the date of the first protest dividing cities at the median to group cities into "early protest areas" and "late protest areas".

3.3.2 RD and DiD

The main method used in this study is a combination of regression discontinuity (RD) and difference-in-differences (DiD). The regression discontinuity in time used in this study is due to the random timing of the police killing of George Floyd followed by the nationwide protests. Therefore, the "running variable" is the date indicated as X_i. The cutoff point (indicates as C_0) in this study is the day of May 28, 2020, which is the first day after the outbreak of nationwide Black Lives Matter protests in the wake of the killing of Floyd.[17] This study uses a "donut-RD" proposed in Barreca et al. (2011) by removing the observations in the immediate vicinity of the treatment threshold. Donut-RD is employed when noises exist around the cutoff. In this case, the spread of massive information on social media started on May 26th, two days before the cutoff. It is clear that the protests developed into nationwide movements on May 31st, two days after the cutoff. Therefore, using the first day after the outbreak of protests as the cutoff, and removing the noisy days around the cutoff is the most

[13] It is from the website of https:// https://www.timeanddate.com/holidays/us/lockdown-day-1

[14] Nebraska, where Lincoln is located, did not have a specific lockdown start time. I use the latest date in the sample which is April 7th for Lincoln's lockdown start time when it comes to the situations where I have to strict the analysis under lockdown time.

[15] ACLED is a disaggregated data collection, analysis, and crisis mapping project. ACLED collects the dates, actors, locations, fatalities, and types of all reported political violence and protest events around the world. The website is https://acleddata.com/about-acled/.

[16] Another magnitude index is the participant size for each protest. However, most of the sizes are unavailable. The available protest sizes are ambiguous. For example, the words "large" and "hundreds" are used to describe the size, making it hard to quantify.

[17] George Floyd was killed on the evening of May 25. Starting on May 27, protests spread across the United States. This set is consistent with the media coverage analysis in Reny and Newman (2021).

accurate way to use RD in this study.

Following the recent protocol for regression discontinuity analysis, a polynomial of order 1 is used. The bandwidths are selected separately for stops and arrests after adding city fixed effects (Calonico et al., 2014). The outcome variables are collapsed on the date when selecting the bandwidths due to the large observations. The optimal bandwidths are 68 days and 64 days for stops and arrests respectively. The analysis for police investigation is using the 68-day bandwidth.[18]

Difference-in-differences is employed to control for seasonal trends by comparing the years 2019 and 2020. Due to the multiple impacts of the COVID-19 lockdown, the main model limits the 2020 data to when the lockdown already started. The main model for how protests affect stops or arrests when it comes to race is:

$$\begin{aligned} Y_i = \beta_0 + \beta_1 * D_i + \beta_2 * X_i + \beta_3 * T_i + \beta_4 * D_i * X_i + \beta_5 * D_i * T_i \\ + \beta_6 * X_i * T_i + \beta_7 * D_i * X_i * T_i + \gamma * C_i + e_i \end{aligned} \tag{11}$$

where i denotes each individual. Y_i is equal to 1 if the civilian involved in the police encounter is African American, and 0 otherwise. D_i equals 1 if the date is after May 28th. $D_i = 0$ if $X_i < C_0$, in this case, is considered the pre-protest period. $D_i = 1$ if $X_i >= C_0$ which is the post-protest period. May 28th, 2019 is the placebo or pre-period cutoff to employ the method of DiD. T_i has two values, 2020 (post) and 2019 (pre), which makes β_5 the parameter of interest. β_5 is the coefficient of the interaction term of the cutoff and the year 2020. It tells us how George Floyd protests affect the proportion of African Americans among stops and arrests. Other parameters capture how the proportion of African Americans in stops and arrests changes over time. γ is the city fixed effect. The results are also clustered at the city level. Due to the small number of clusters, clustered standard errors are calculated via bootstrap.

[18] The investigation information is conditional on stops, it makes sense to use consistent bandwidth as the stop data. Arrest data is from a different pool.

3.3.3 Daytime and Nighttime

Inspired by the method of "veil of darkness" proposed by Grogger and Ridgeway (2006) where darkness serves as a proxy of visibility, the protests might work differently during different times. Gau et al. (2022) discusses public scrutiny and low-visibility decisions. These papers suggest that the main effect of the protests might be from public supervision which is more feasible during the daytime.

To examine the mechanisms of why or how police changed their behaviors, I will explore the effect of protests under different natural lights which coincides with if police behavior is under public watch. I retrieved the sunrise and sunset time of each city in my samples through Python codes at the local time. The police practice data and sun time data sets are merged by city and date which is the main data used in the mechanism part. All police practice is divided into daytime and nighttime cases. Daytime represents the time from sunrise to sunset, otherwise nighttime.

3.4 Results

3.4.1 Summary Statistics

In this section, I document general trends in policing during the study period. Figure 2 plots the total weekly police stops and arrests in 2020 (black solid lines) and 2019 (grey solid lines) in my sample. The grey dashed vertical line denotes the lockdown start date (March 20th) based on Table 19. The black dashed vertical line indicates the first day after the murder of George Floyd which is May 26th.

There are two clear trends we can tell from Figure 2. 1) The pandemic had a large impact on police behaviors. 2) 2019 and 2020 follow nearly the same seasonal changes for stops and arrests, levels are different though. 2019 can be used to differ out seasonality.[19]

Table 5 shows the total daily stop or arrest cases before and after the protests within

[19]The evidence of de-policing is not clear after the protests if we take into consideration the lockdown and the seasonal changes. The effects of the protests on de-policing are not the main focus of this paper.

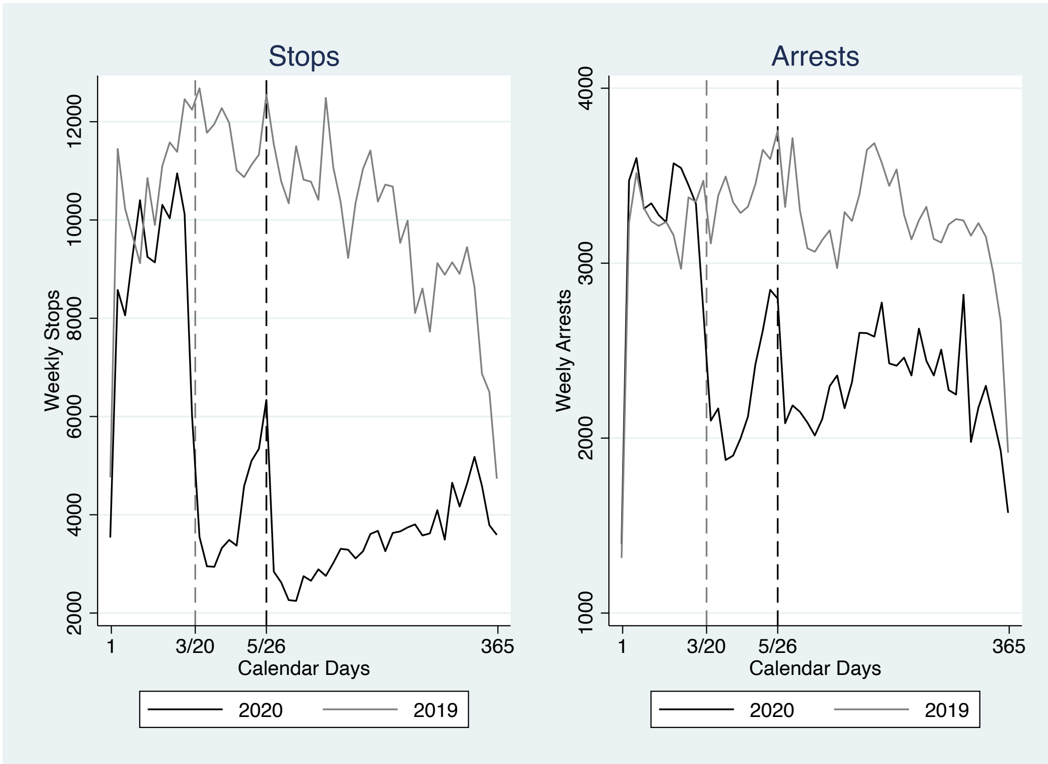

Figure 2: Weekly police stops and arrests in 2019 and 2020

the optimal bandwidths which are 68 days and 64 days for stops and arrests respectively. All samples are restricted under lockdown time. The average daily stop is 540 before the protests, while it is 386 after the protests. There are declines for both African Americans and other racial groups. The proportion of African Americans dropped 1.3 percentage points after the protests.

The average daily arrest is 286 before the protests, while it is 309 after the protests. There is a slight increase in arrests for both African Americans and other racial groups. The proportion of African Americans dropped by 0.6 percentage points in arrests after the protests. Figures 7 and 8 present graphical evidence of the effect of protests on the proportion of African Americans among stops and arrests. The observations are collapsed by city and days. It looks like there is no clear disparity for stops. For arrests, a discontinuity is presented at the threshold, with a jump of nearly 3 percentage points.

		All	African Americans	Other Racial Groups	Black Share
Stops	Before the protests	539.9	320.5	219.5	59.4%
		(176.1)	(94.9)	(84.8)	
	After the protests	385.7	224.2	161.6	58.1%
		(63.7)	(38.8)	(34.4)	
Arrests	Before the protests	285.5	85.3	200.2	29.9%
		(111.7)	(33.7)	(82.2)	
	After the protests	308.9	90.5	218.4	29.3%
		(53.7)	(19.3)	(40.7)	

Notes: the observations in this table are from the optimal bandwidths which are 68 days and 64 days for stops and arrests respectively. The samples are limited to the lockdown time. Standard deviations are in parentheses.

Table 5: Daily police stops and arrests before and after the protests

3.4.2 Main Results

In this section, I employ the main method – a combination of donut-regression discontinuity in time (DRDiT) and difference-in-differences (DiD) to show how George Floyd protests affect police-civilian nonfatal interactions when it comes to race. Table 13 is the main table of this paper. Models (1) and (3) use all observations in the optimal bandwidths, 68 days and 64 days for stops and arrests, respectively. Models (2) and (4) limit the samples under lockdown time but still within the optimal bandwidths.[20] Let us start from the stops in specifications (1) and (2). For stops, the protests have increased the share of African Americans by 2.4 or 2.3 percentage points which is not statistically significantly different from zero. The protests have not impacted the racial composition of police stops.

For arrests, in specifications (3) and (4) of Table 13, the coefficients of -0.033 and -0.040 suggest that the protests have decreased the proportion of African Americans by about 3.3 to 4 percentage points, which is significant at the 5% significance level. Figure 2 suggests

[20] If I only use the samples after the lockdown to select bandwidth, it will only be a handful of days and might be biased. In this paper, I use the original optimal bandwidth and do different robustness checks to control the effect of COVID-19.

Proportion of African Americans	(1) Stops	(2) Stops	(3) Arrests	(4) Arrests
Protests	0.024	0.023	-0.033**	-0.040**
Bootstrapped p-value	0.415	0.389	0.039	0.031
Bandwidth	68	68	64	64
Only lockdown	No	Yes	No	Yes
Observations	276,647	270,542	98,385	92,525
R-squared	0.115	0.115	0.303	0.310

Notes: City fixed effects are included in every specification as controls. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 6: Protests on the share of African Americans

that the onset of the COVID-19 pandemic has largely affected police behaviors. Models (2) and (4) might be better than Models (1) and (3) in this situation. Taking Model (4) as the main effect for the arrests, the protests have decreased the proportion of African Americans by 4 percentage points, from 30.1% to 26.1%. That means the protests have significantly decreased the share of African Americans in arrests by 13%. Table 20 separates stops into traffic stops and pedestrian stops. The results are consistently positive and insignificant for stops.

To strengthen the causal interpretation of the main results, Table 7 splits the sample cities into different groups based on the frequency of the peaceful protests and the date of the first protest in each city. Specifications (1) to (4) divide the cities into "high protest areas" and "low protest areas" according to the total protest times. Specifications (5) to (8) group the cities into "early protest areas" and "late protest areas" based on the first protest date. The regression results are also clustered at the city level. Due to the small number of clusters, p-values are adjusted by bootstrap tests. All observations in this table are under lockdown period and within the optimal bandwidth.

In Table 13, there is no evidence that the protests have impacted the proportion of African Americans in police stops regardless of the protest characteristics. For arrests,

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	High Protest Areas		Low Protest Areas		Early Protest Areas		Late Protest Areas	
	Stops	Arrests	Stops	Arrests	Stops	Arrests	Stops	Arrests
Protests	0.035	-0.089***	0.024	-0.016	0.042	-0.063***	-0.019	-0.061
P-value	0.438	0.000	0.750	0.500	0.375	0.000	0.688	0.188
Constant	0.643	0.344	0.345	0.157	0.660	0.228	0.427	0.269
Observations	172,830	44,767	97,712	47,758	138,110	28,394	132,432	64,131
R-squared	0.000	0.011	0.005	0.001	0.001	0.004	0.010	0.001

Notes: The observations from the optimal bandwidths and limited to the lockdown time. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 7: Protest characteristics on the share of African Americans

in "high protest areas" (Model 2), the protests have decreased the proportion of African Americans by 8.9 percentage points. But this impact does not remain in "low protest areas" (Model 4). Similarly, in "early protest areas" (Model 6) where the first protest happened on May 30th or earlier, the protests have cut the share of African Americans in arrests by 6.3 percentage points. There is no statically significant evidence in "late protest areas" (Model 8). Also, the coefficients of the protests in "high protest areas" and "early protest areas" are larger than the general effect of the protests in arrests, which is 4 percentage points. The results are consistent if we do not limit the samples to the lockdown time. These results strengthen the interpretation that local protests impacted arrest patterns, rather than simply identifying trends in nationwide responses to the murder of George Floyd. In the future, additional tests can be conducted. If the effects of the protests are driven by the need for greater visibility, the effects should be stronger in areas with higher population densities compared to those with lower population densities.

3.4.3 Assumption and Robustness Checks

Regression discontinuity assumes that other potentially relevant variables be continuous at the cutoff point. Table 8 is the assumption check table that uses the same method (Donut-

RD and DiD) to explore the effect of the protests on other demographic variables: gender and age. The bandwidths are selected separately for gender and age in stops and arrests after adding city fixed effects. The optimal bandwidths are 44, 45, 55, and 52 days, respectively. There are no discontinuities for these demographics. No evidence shows that the protests have changed police behaviors when it comes to gender and age.

	(1)	(2)	(3)	(4)
	Stops		Arrests	
	Female	Age	Female	Age
Protests	0.007	-0.293	0.011	-0.954
P-value	0.570	0.875	0.305	0.781
Bandwidth	44	45	55	52
Observations	176,651	94,869	83,343	74,124
R-squared	0.057	0.031	0.014	0.003

Notes: City fixed effects are included in every specification as controls. The samples are limited to the lockdown time. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 8: Protests on distribution of gender and age in stops and arrests

Table 9 reports the results of a placebo, where I use May 28th in 2019 as the cutoff to run regression discontinuity and replicate the specifications in Table 13. After May 28th in 2019, the proportion of African Americans decreased in stops according to Models (1) and (2). This is unexpected. A possible reason could be that different people have different traveling plans during the Memorial Day holiday. There is no difference in police arrests before and after the cutoff point. This table emphasizes the importance of using difference-in-differences with regression discontinuity in this study to control the seasonal and holiday effects.

Figure 3 is a robustness check figure which varies the methods, bandwidths, and donut sizes. The two top panels use the optimal bandwidths of 68 days for stops and 64 days for arrests. The two bottom panels use a double length of optimal bandwidths to explore the effect of the protests, which are 136 days for stops and 128 days for arrests. The two left panels are using the main method: the combination of RD and DiD. The two right

Proportion of African Americans	(1) Stops	(2) Stops	(3) Arrests	(4) Arrests
Protests	-0.018**	-0.017**	0.008	0.011
P-value	0.029	0.034	0.570	0.320
Bandwidth	68	68	64	64
Match lockdown in 2020	No	Yes	No	Yes
Observations	213,998	209,448	59,093	55,672
R-squared	0.125	0.125	0.302	0.307

Notes: City fixed effects are included in every specification as controls. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 9: Police-civilian interactions around May 28th in 2019

panels only employ the RD method. The gray lines show the 90% confidence intervals of the coefficients for the stops, while the black lines are for arrests. The three lines represent, in order, different donut sizes which are 0 days (no donut, regular RD), 2 days (the main method in this paper), and 4 days (double donut size of the main method). The results are consistent with Table 13. The protests have not impacted the proportion of African Americans in stops (gray lines in four panels). However, the protests have decreased the proportion of African Americans in arrests.

3.5 Mechanisms

3.5.1 Day-night differences in stops and arrests

Section 4.2 proposed two pathways. The first one is that police may change their attitudes or opinions directly. The second pathway is that the protests might change police behaviors because of public supervision. Figure 4 integrates the literature review and these two possible pathways.

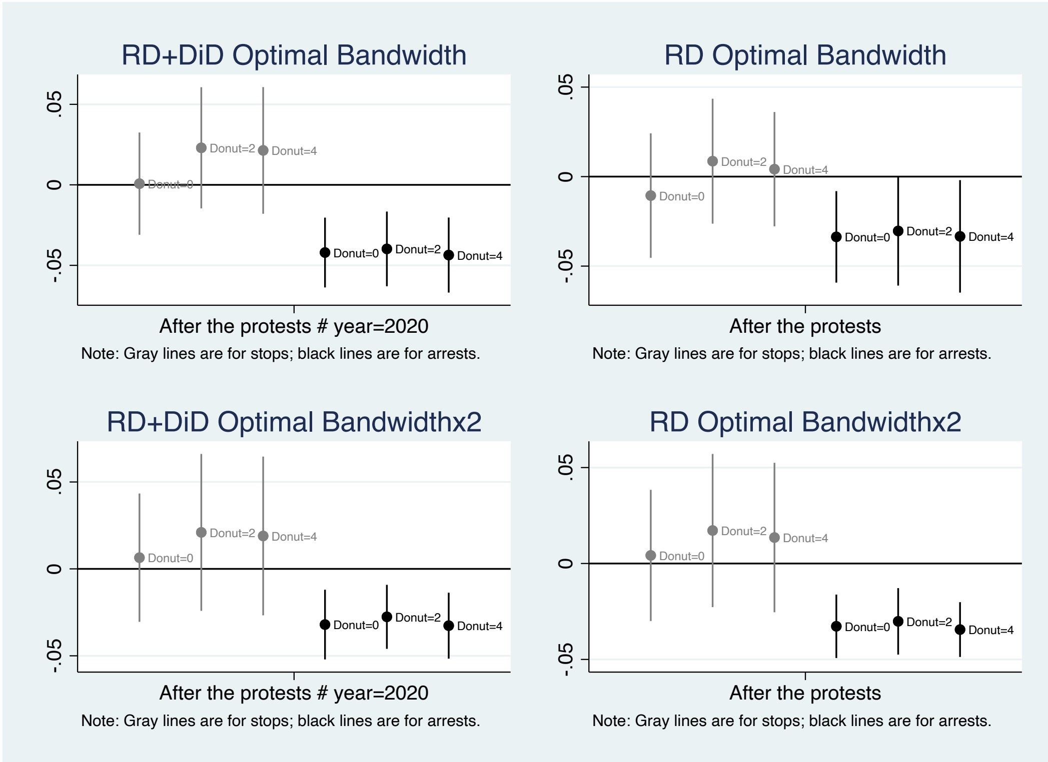

Figure 3: Protests on police-civilian interactions

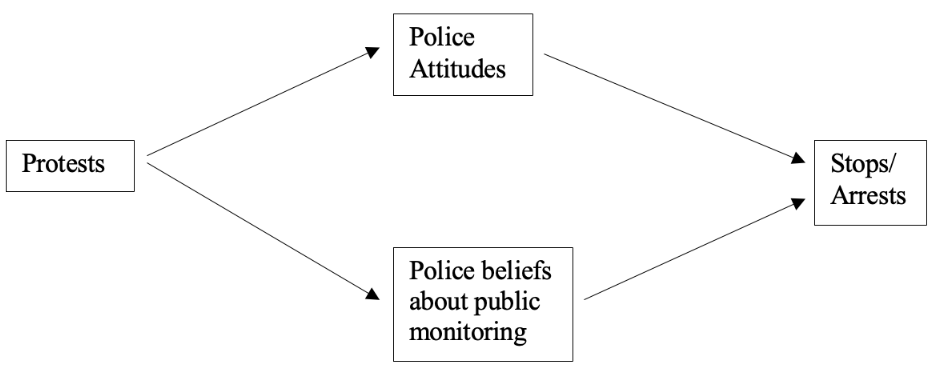

Figure 4: Pathways of the protests on police-civilian interactions

If the upper (direct) pathway works (protests have changed police attitude toward African Americans), the daytime and nighttime should be consistent for different police-civilian interactions. If the second pathway works, there might be a difference between daytime and nighttime.

	(1)	(2)	(3)	(4)
	Stops		Arrests	
Proportion of African Americans	Daytime	Nighttime	Daytime	Nighttime
Protests	0.012	0.031	-0.040**	-0.038
P-value	0.491	0.575	0.039	0.117
Observations	156,795	113,747	57,727	34,798
R-squared	0.137	0.092	0.342	0.262

Notes: The observations from the optimal bandwidths and limited to the lockdown time. City fixed effects are included in every specification as controls. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 10: Protests on the share of African Americans daytime and nighttime

Table 10 shows that the protests have not impacted the police stops when it comes to race during daytime and nighttime. There is a decreased effect on arrests in both daytime and nighttime, but the effect is only significant during daytime. The main changes of the protests on interactions between police and African Americans are driven by the visibility of police behaviors. It confirms the second pathway mechanism. The protests increase public attention or at least the police think the protests have brought increased public scrutiny, especially during the day. Of course, some caution in interpretation is warranted as the magnitude of coefficients in Columns (3) and (4) are similar.[21]

		(1) No Action Taken	(2) Warning Issued	(3) Citation Issued	(4) Arrest Made
Daytime	Black x Protests	0.030***	0.051	-0.038	-0.019
	P-value	0.000	0.500	0.625	0.391
	Observations	122,077	78,315	139,705	139,822
	R-squared	0.609	0.114	0.274	0.084
Nighttime	Black x Protests	-0.026	0.048	0.001	-0.020
	P-value	0.500	0.125	0.984	0.828
	Observations	91,624	55,918	101,093	103,952
	R-squared	0.612	0.062	0.139	0.058

Notes: Observations in this table are from the optimal bandwidth 68 days before and after the protests. The samples are limited to the lockdown time. City fixed effects are included in every specification as controls. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 11: Protests on police investigations daytime and nighttime

3.5.2 Day-night differences in police investigations

Table 11 shows the effect of protests on racial disparities in police investigations during daytime and nighttime. The coefficients of the interaction term in Black X Protests compare different resolution rates for African-Americans and other racial groups. The only significant effect is for the no-action-taken rate. African Americans have a higher no-action-taken rate during the day, which is significant at the 1% level. This effect is not significant and is negative during nighttime. The difference between daytime and nighttime implies that police are less likely to take action after stopping African Americans during the day. This result is consistent with the results in Subsection 3.5.1. Police have only changed their behaviors

[21] Since the coefficients in Models (3) and (4) are close, I ran three robustness checks for arrests during daytime and nighttime: using the method RD, separating high and low protest areas, and splitting the early and late protest areas. The results are consistent: RD results are the same as Table 10. The protests worked for high protest or early protest areas only during daytime. I will also use investigation data to further explore the mechanism in the following subsection.

during the daytime when it comes to race after the protests.

3.6 Conclusion

In this paper, I study how the Black Lives Matter protests impact nonfatal police-civilian interactions (stops, investigations, and arrests) when it comes to race. The protests have increased the proportion of African Americans in stops by 2.3 percentage points. But it is not significant. Pierson et al. (2020) concludes that being Black makes them 3-4% more likely to be stopped. Protests have not closed this racial gap. The protests have decreased the proportion of African Americans in arrests by 4 percentage points. It has decreased by 13%. This number is close to the estimated drop in fatal police interactions (15-20%) from Campbell (2021) resulting from earlier BLM protests. The effect is even larger in cities with higher protest intensity.

When dividing police encounters into cases during daytime and nighttime, the decreased effect of the protests only holds during the daytime. At nighttime, when public supervision is absent, the protests did not change the racial compositions in police interactions. This suggests that BLM protests did affect nonfatal police-civilian interactions when it comes to race. However, the day-night differences imply that the decrease in police interactions with African Americans may due to police beliefs about public monitoring. It is not because of the direct change in police attitudes toward African Americans.

Notably, the regression discontinuity design only explores the local effect of the protests. The long-term effect of the protests is to be addressed by future researchers.

4.0 Racial Differences in Politician Persistence

4.1 Introduction

There are racial representation gaps in elected offices at all levels in the US. Nonwhites make up approximately 40% of the US population, yet in the 118th Congress, only 12% of the Senate and 28% of the House of Representatives belong to racial and ethnic minorities.[1] According to the National Conference of State Legislatures (NCSL), there is an average gap of -13.48 percentage points between the percentage of nonwhite representatives in state legislatures and the population in 2020.[2] Using nationwide data, Ricca and Trebbi (2022) concludes that "nonwhite minorities are collectively underrepresented by approximately 8.4 percentage points" in the city council.

What are the drivers of the racial representation gap and its persistence? This question can be answered from the demand perspective by asking how racial attitudes and behaviors among voters restrict minority office-holding (Citrin et al., 1990; McDermott, 1997; Kam, 2007; Telles et al., 2011), and from the supply perspective by exploring if minorities are underrepresented because of differences in candidate entry (Shah, 2014; Canon, 2020). This paper focuses on the latter perspective.

This paper focuses on the supply side. In part to yield a comparable sample for the sake of causal inference, I assess the impacts on people who have already expressed interest in elected office, namely, people who have run but lost in the past. As the path to political office is not characterized by electoral success alone, different choices across race groups after a failed electoral attempt are potential determinants of eventual office holding and political representation. Indeed, depending on the direction of the effect, differential rates of re-running after a loss could partially explain or could mitigate the racial representation gap.

I use a close election regression discontinuity design (CERDD) and heterogeneous-by-

[1] Pew Research Center report link
[2] NCSL 2020 Data link

race impacts to investigate whether there is differential attrition between nonwhite and white candidates in response to an electoral loss. I also test how the rerunning choices affect the subsequent office-holding.

Using 4,617 marginal candidates from city council elections in California, my results show that narrowly losing white candidates are 16.2 percentage points likely to rerun for office while losing nonwhite candidates are 25.7 percentage points likely to rerun an election, which is significantly higher than their white counterparts. Therefore, after a narrow loss, nonwhite candidates are 59% more likely to run for office again compared to white candidates. The possibility of winning the subsequent election remains the same for all rerunning candidates. As such, the persistence of losing nonwhite candidates contributes to closing the racial gap.

Understanding the determinants of representation, including at the local office, is important. In light of growing evidence on the impacts of descriptive representation on policy outcomes. Minority representation impacts outcomes for minorities in political development (Banducci et al., 2004; Gleason and Stout, 2014; Grumbach and Sahn, 2020), education (Kogan et al., 2021), labor market outcomes (Nye et al., 2015), housing (Beach et al., 2018), and policing (Sass and Mehay, 2003; Bulman, 2019). Moreover, studying this question at a local level is important as local governments often represent an entry point in political careers aimed at higher office (Frendreis et al., 1990).

This paper provides causal evidence of racial differences in political trajectories for individuals who initially express interest in running for office. It contributes to the literature in two ways. First, previous research studies racial differences in political trajectories from the ambition aspect – whether winning candidates pursue a higher office (Shah, 2015). This paper contributes to understanding the racial differences in political trajectories from the persistence perspective – whether losing candidates leave politics. Second, it provides another perspective on electoral reentry: while there is (mixed) evidence on a gender difference (Wasserman, 2018; Thomsen and King, 2020; Bernhard and de Benedictis-Kessner, 2021), little research has been conducted on racial differences. Notably, the work on gender either finds no difference or that women (the underrepresented group) are less likely to run than men.

The rest of the paper proceeds as follows. Section 4.2 provides a simple theoretical frame.

Section 4.3 describes my data and results. Section 4.4 concludes.

4.2 Framework

The main goal of this study is to estimate the persistence of political candidates in response to an electoral loss and investigate whether there are any differences in political persistence among candidates of different races. A comparison of the future political involvement of election winners and losers could lead to biases due to unobserved candidate characteristics that are likely correlated with the electoral outcome. To isolate the effect of an electoral loss distinct from the unobserved characteristics between winning and losing candidates, this paper employs a close election regression discontinuity design (CERDD) proposed by Lee (2008), which restricts the comparison of winners and losers to candidates in close elections – elections where the outcome is mainly decided by chance. Specifically, the main analysis includes only the last-placed winners and the first-placed losers.

The main outcome variable in this paper is whether an individual runs for the city council election again in four years. The running variable is the marginal vote share (MVS) for candidate i in election year t denoted as MVS_{it}, which for winning (losing) candidates is the difference between their vote share and that of the first loser (last winner). The effect of losing an election on subsequent political participation can be written as:

$$\tau = \lim_{MVS_{it} \uparrow 0} E[Y_{i,t+4}|MVS_{it}] - \lim_{MVS_{it} \downarrow 0} E[Y_{i,t+4}|MVS_{it}] \tag{12}$$

where $Y_{i,t+4}$ represents whether candidate i runs for office again in four years. The treatment effect, denoted as τ, is the disparity of $Y_{i,t+4}$ at the marginal vote share threshold. This refers to the jump in $Y_{i,t+4}$ as the marginal vote share approaches zero from the left (among the losing candidates) and the right (among the winning candidates). Assuming that the attributes of candidates who barely won and barely lost are continuous throughout the threshold for winning, this empirical strategy provides causal estimates of the effect of losing on the likelihood of re-running for office.

This paper focuses on investigating racial differences in politician persistence. It can be examined in two ways. On one hand, we can test heterogeneity in the effect of losing by race of the candidates. On the other hand, we can estimate the re-running decisions of the candidates whose marginal vote share approaches zero for different racial groups separately. The first examination can be written as:

$$\tau_m = \lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] - \lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] \tag{13}$$

$$\tau_w = \lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] - \lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] \tag{14}$$

where M_i denotes a minority or nonwhite candidate while W_i is a white candidate. τ_m and τ_w are the effect of an electoral loss on following political participation for nonwhite and white candidates. Suppose that $\tau_m \neq \tau_w$, the effect of an electoral loss on candidates' subsequent participation is not the same for nonwhite and white candidates. The inequality could be driven by differences in the likelihood of re-running for office among winning candidates:

$$\lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] \neq \lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] \tag{15}$$

The inequality could also be driven by differences among losing candidates:

$$\lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] \neq \lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] \tag{16}$$

Both inequalities could hold at the same time. There is also a scenario in which treatment effects are homogeneous across minorities and whites, but both inequalities hold. In this case, the jumps are the same because the discontinuous behaviors in winning and losing candidates are the same for both nonwhite and white politicians.

The second examination involves investigating racial differences separately among winning and losing candidates. This allows us to directly test Equations 15 and 16, which capture racial differences in victories and racial differences in losses, respectively.

$$\tau_v = \lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] - \lim_{MVS_{it\downarrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] \tag{17}$$

$$\tau_l = \lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, M_i = 1] - \lim_{MVS_{it\uparrow 0}} E[Y_{i,t+4}|MVS_{it}, W_i = 1] \tag{18}$$

where τ_v and τ_l are the racial disparities of future political participation among victorious and losing candidates.

The regression model to calculate the four effects τ_m, τ_w, τ_v and τ_l can be written as:

$$\begin{aligned} Y_{i,t+4} = \alpha + \beta Lost_{it} + \gamma M_i + \delta(M_i \times Lost_{it}) + f(MVS_{it}) + \\ Lost_{it} \times f(MVS_{it}) + M_i \times f(MVS_{it}) + M_i \times Lost_{it} \times f(MVS_{it}) + \epsilon \end{aligned} \tag{19}$$

where the new terms are $Lost_{it}$, an indicator variable that takes a value of one if the candidate i lost the election at year t. The coefficient β represents the effect of losing for white candidates, and γ shows the difference in subsequent political participation between nonwhite and white narrow winners. The coefficient δ denotes the extra effect of an electoral loss for nonwhites relative to whites. δ can also be explained as the additional racial disparities in re-running for office for losing candidates compared to winners.

Theoretical parameters and regression statistics can be connected as follows: τ_m, which is the effect of a loss on following political participation of a nonwhite candidate. It is identified as $\beta + \delta$. Similarly, τ_w, which is the effect of a loss on a white candidate is β. τ_v, the racial disparity for winning candidates, equals γ. The racial gap in runner-ups, τ_l is $\gamma + \delta$.

4.3 Results

4.3.1 Data and Descriptive Statistics

The main data source for this paper is the California Elections Data Archive (CEDA), – a joint project of the Center for California Studies and Institute for Social Research of California State University, Sacramento, and the Secretary of State – which collects and compiles local election returns.[3] The data used in this paper were downloaded from the old CEDA website which covers data from 1995 to 2014 in municipal elections. Election returns

[3] CEDA can be downloaded in PDF format from this website.

include candidates' full name, ballot designation, incumbency status, election date/location, number of votes earned, total votes, number of individuals to be elected for a given office, and whether the candidate was elected.

CEDA data lacks demographic information. Beach and Jones (2017) collected the ethnicity information by 1) contacting the cities, 2) reaching out to ethnic organizations which maintain lists of government officials that are of Hispanic or Asian origin, 3) searching for pictures of candidates, and then asking Amazon Mechanical Turk subjects to identify the ethnicity.[4] I contacted the authors and asked for the data at the candidate level. Each candidate has a unique ID, where the ethnicity information is identified.

The main outcome variable is whether an individual runs for the city council election again in four years. Since the final election cycle analyzed is 2014, any observations of individuals running for office 2011 and later are dropped because it is unclear whether they re-run for office.

In order to implement the close election regression discontinuity design, I limit the candidates to those who are the last-placed winners and the first-placed losers. Therefore, I only analyzed races where the number of candidates exceeded the number of open seats. The running variable for this analysis is margin vote share. Winning candidates have a positive margin vote share, which is calculated as the difference between their vote share and that of the first-placed loser. Losing candidates have a negative margin vote share, which is calculated as the difference between their vote share and that of the last-placed winner.

This paper reports on a subset of candidate data from California local elections between 1995 and 2010. Panel A of Table 12 presents summary statistics for marginal candidates who were either the last-placed winners or the first-placed losers. Although race and ethnicity are multifaceted, in this study, they are divided into two categories: white alone (white) and not white alone (non-white). These candidates will be used to select the optimal bandwidth. Of the 4,617 marginal candidates, 25.6% are nonwhite. The elected rate for nonwhite marginal candidates is 56.9 percent. For white candidates, the rate is 60.2 percent. The probability of rerunning is approximately 45% for all marginal candidates. 33.1% of the sample candidates are elected in four years. Of the 2,077 rerunning candidates, white candidates have an

[4]More details please see the pages 117-118 in Beach and Jones (2017)

election rate of 75.4% in phase t+4, while nonwhites have a rate of 67.9%. Panel B of Table 12 presents the summary statistics of close marginal candidates whose marginal vote share is less than 11.2 percentage points. The patterns are similar to those in Panel A.

	Panel A: Marginal Candidates			Panel B: Close Marginal Candidates		
	(1) All	(2) Nonwhite	(3) White	(4) All	(5) Nonwhite	(6) White
Elected in t	0.594	0.569	0.602	0.583	0.557	0.590
Rerun in t+4	0.450	0.462	0.446	0.444	0.469	0.437
Elected in t+4	0.331	0.314	0.336	0.320	0.316	0.321
Observations	4,617	1,183	3,434	3,421	811	2,610
Nonwhite Share		25.6%			23.7%	
Conditional on rerun						
Elected in t+4	0.735	0.679	0.754	0.720	0.674	0.735
Observations	2,077	546	1,531	1,520	380	1,140
Nonwhite Share		26.3%			25.0%	

Note: This table shows summary statistics for the marginal candidates who are the last-placed winners and first-placed losers in Panel A, and close marginal candidates whose marginal vote share is within 11.2 percentage points.

Table 12: Summary Statistics

4.3.2 Main Results

The left panel of Figure 5 presents graphical evidence of the effect of losing on subsequent political participation. The graph plots candidates' probability of running for office in year $t + 4$ on the y-axis against their marginal vote share in year t on the x-axis. The dotted vertical line at zero represents the winning threshold. Candidates to the right of the zero threshold won their elections in t, while to the left of zero, candidates lost their elections in year t. A second-degree polynomial is fit on each side of the zero threshold. Both sides present a relatively flat relationship: there are no obvious increase or decrease trends. A clear discontinuity is presented at the threshold, with a magnitude of jump of about 40 percentage points. The jump represents the deterrence effect of losing, which is described in Lee (2008) as a component of incumbency advantage.

The right panel of Figure 5 presents the main results of this paper. It divides the sample by the candidates' race. Nonwhite candidates are represented by black dots in bin scatter (bins of width equal to 0.0125 percentage points) and black fitted lines, while white candidates are represented by gray solid diamonds in bin scatter and gray fitted lines. For both groups, there is a discontinuity at the threshold that is consistent with the jump observed for all candidates in the left panel. At the winning threshold, where the gray vertical line intersects with the winning (right) fitted lines, there is no racial gap for future political involvement. However, at the losing threshold, visual evidence shows that nonwhite candidates are more likely to rerun for office compared to their white counterparts. The linear fit graph is consistent with the non-linear fit graph.[5] Table 21 in Appendix presents the regression results of the effect of the electoral outcome on subsequent political participation by race.

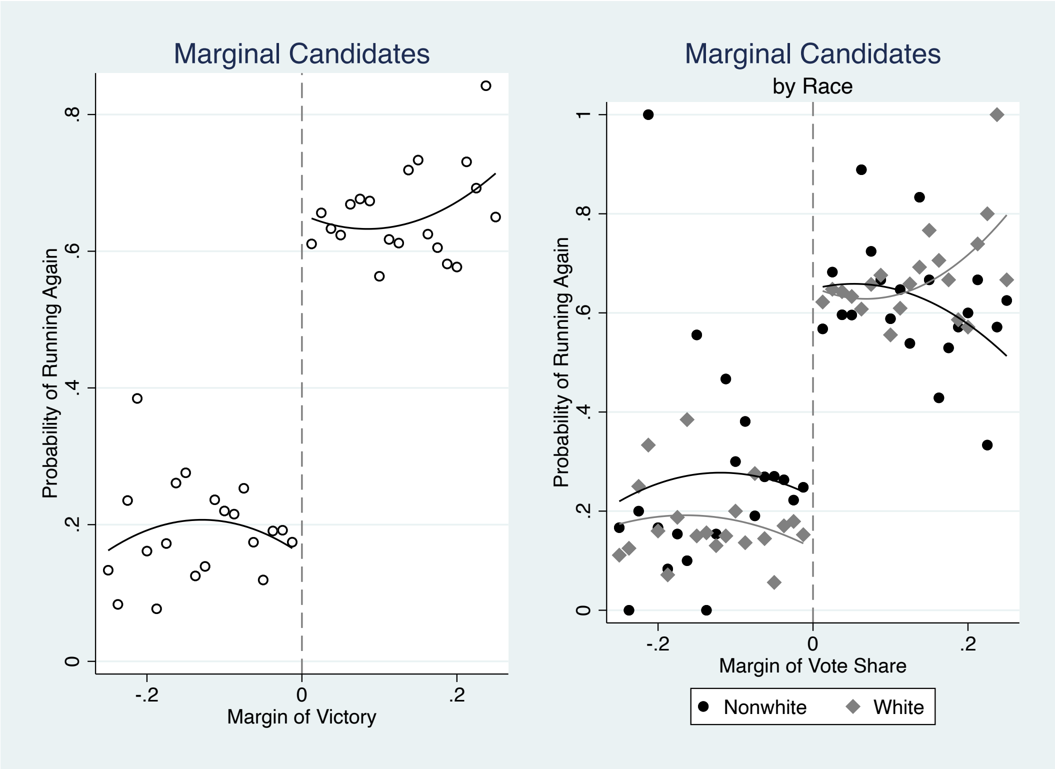

Figure 5: Effect of Electoral Outcome and Race on Subsequent Political Participation

[5]Please see Appendix 9.

Table 13 presents the results of the regression model in Equation 19, which corresponds to the right panel of Figure 5. The outcome variable is whether a candidate runs again for office in four years. Optimal bandwidths are selected separately for each panel. County and year fixed effects are included in each specification, and standard errors are clustered at the county level. Columns 1 through 4 use a local linear regression with a first-order polynomial in the running variable. Marginal vote share and samples are restricted by various bandwidths around the cutoff for winning. The first column uses the optimal bandwidth computed from Calonico et al. (2014), while the second and third columns use twice and half the optimal bandwidth, respectively. The fourth column uses the sample of all marginal candidates, without restriction on the range of marginal vote share. The fifth column uses a second-order polynomial in the margin of victory for all marginal candidates.

Different models show consistent results for all coefficients. Starting with β_1, which represents the deterrence effects for white candidates, losing the previous election causes a decline of 42.6 to 47.6 percentage points in the probability of running in the following election. β_2 measures the racial differences in rerunning for winning candidates, and the coefficients remain low and insignificant, indicating no racial difference in subsequent political participation for winning candidates. The coefficients on the interaction term Lost $\times$ Nonwhite indicate that nonwhite candidates are 7 to 13 percentage points more likely to run for office again after a loss relative to white candidates. The coefficients are significant across all specifications except for Model 3. It is possible that restricting the bandwidth to half of the optimal bandwidth cut too many samples, leading to a higher robust standard error and the non-significant coefficient in Model 3.

Our main estimates are taken from the first column of the local linear specification with optimal bandwidth. Winning white candidates have a 62.7 percentage point likelihood of running for office again, compared to 59.2 percentage points for winning nonwhite candidates. There is no significant racial difference in subsequent political participation for winning candidates. In contrast, losing white candidates are 16.2 percentage points likely to rerun for office, while losing nonwhite candidates are 25.7 percentage points likely to rerun an election, a difference that is statistically significant (p-value for $\beta_1 + \beta_3$ is 0.01). Nonwhite candidates are 58.6% ((25.7-16.2)/16.2) more likely to rerun for office than white candidates

	(1)	(2)	(3)	(4)	(5)
	Polynomial order one				Polynomial order two
	Optimal bw	2xOptimal bw	0.5xOptimal bw	Full sample	Full sample
Lost	-0.465***	-0.467***	-0.426***	-0.476***	-0.462***
	(0.022)	(0.019)	(0.027)	(0.016)	(0.026)
Nonwhite	-0.035	0.015	0.001	-0.014	0.007
	(0.037)	(0.028)	(0.071)	(0.020)	(0.033)
Lost*Nonwhite	0.130**	0.102**	0.072	0.123***	0.113**
	(0.053)	(0.047)	(0.084)	(0.035)	(0.053)
White winner mean	0.627	0.621	0.610	0.638	0.613
Bandwidth	0.112	0.224	0.056	-	-
Observations	3,419	4,135	2,629	4,615	4,615
R-squared	0.234	0.229	0.238	0.230	0.231

Note: The outcome variable is whether a candidate runs again for office in 4 years. Standard errors are clustered at the county level. County and year fixed effects are included in each specification. Significance levels are indicating as: *** p<0.01, ** p<0.05, * p<0.1.

Table 13: Effect of Electoral Outcome and Race on Subsequent Political Participation

after a loss. These results suggest that minorities are more resilient than white candidates in their political participation following a loss. Table 22 in Appendix summarizes the results. Table 23 in Appendix replicates Table 13 using samples from Hispanic and white alone candidates. The results are consistent.

The empirical findings for the parameters from Section 4.2 are summarized below:

Deterrence effect for nonwhite candidates: the effect of a loss on subsequent political participation for nonwhite candidates, τ_m, is -0.335.

Deterrence effect for white candidates: the effect of a loss on subsequent political participation for white candidates, τ_w, is -0.465.

Racial disparity for winning candidates: τ_v, equals -0.035. It is not statistically significant, indicating no racial differences for winning candidates in subsequent political participation.

Racial disparity for losing candidates: the racial gap in runner-ups, τ_l, equals 0.095. It is also the main interest of this paper. Losing nonwhite candidates are 25.7 percentage points likely to rerun an election, compared to 16.2 percentage points for losing white candidates. Nonwhite candidates are 59% more likely to rerun for office relative to their white counterparts after a loss.

4.3.3 Consequence of the Resilience

Will this resilience pay off? Table 14, which shows the winning probabilities in election year $t+4$, provides insights. The two columns differ in how candidates who do not run again are coded. The outcome variable for the first specification is not conditional on rerunning, and the regression includes all marginal close election candidates. Candidates who do not rerun are coded as having lost the election, and their chance of winning in election year $t+4$ is set to 0. Column (2) shows the winning possibility conditional on rerunning, where the candidates who do not rerun are excluded from the regression. This explains the difference in observations. The optimal bandwidths are selected separately for each analysis.

When not conditioning on rerunning, the coefficient of Lost x Nonwhite is significantly greater than zero, indicating that nonwhite candidates who lost in phase t are more likely to be elected in phase $t+4$ than their white peers. When conditioning on rerunning, the coefficient of Lost x Nonwhite is large enough to cancel out the negative effect on "lost", albeit not significantly different from zero. This would suggest that conditional on re-running, losing non-white candidates win at roughly the same rate as winning non-white candidates. In other words, there is reason to be persistent in the sense of re-running after a loss.

Both results confirm that the higher rerunning probability or extra resilience of losing nonwhite candidates pays off by holding the same winning possibility in the following elections. Note that the results remain robust if we use the optimal bandwidth (11.2 percentage points) shown in Table 13. Therefore, the persistence of losing nonwhite candidates contributes to closing the racial representation gap.

Due to data limitations, I was unable to conduct a full suite of covariate balance checks for the regression discontinuity design. Additionally, since my dataset only includes city

	(1) Not Conditioning on Rerunning	(2) Conditioning on Rerunning
Lost	-0.365***	-0.153
	(0.030)	(0.094)
Nonwhite	-0.077	-0.055
	(0.057)	(0.055)
Lost*Nonwhite	0.168**	0.178
	(0.067)	(0.135)
Bandwidth	0.078	0.072
Observations	3,046	1,300
R-squared	0.204	0.131

Note: The outcome variable is whether a candidate wins in the next election. Column 1 codes candidates who do no run again as a lose. Column 2 codes not rerunning candidates as missing values. Standard errors are clustered at the county level. County and year fixed effects are included in each specification. Significance levels are indicating as: *** p<0.01, ** p<0.05, * p<0.1.

Table 14: Effect of Electoral Outcome and Race on Subsequent Political Winning Probability

council elections, it is possible that some candidates may be running for different offices in the following elections.

4.4 Conclusion

This paper investigates racial differences in electoral reentry after a loss in local government elections. Empirical data from California city council elections and a close election regression discontinuity design reveal that losing nonwhite candidates are more likely to rerun an election compared to losing white candidates. The possibility of winning the next election is the same for different racial groups. Therefore, the persistence of losing nonwhite candidates contributes to closing the racial representation gap. This suggests that under-representation in local government might be due to racial differences in the initial candidacy.

It should be noted that this study may not be generalizable to other states since California is not representative of the average state in the United States. Nonetheless, research on minority representation in electoral politics has been hindered by a lack of data, and this paper is the first to explore the racial differences in subsequent political participation using causal inference. The study demonstrates that there is no significant racial difference in rerunning for office among narrow winning candidates. However, there could be racial gaps for higher electoral levels, such as state legislators, House of Representatives, and even senators.

This paper addresses the puzzle of racial differences in persistence, with nonwhite candidates being more likely to run for office. It also raises more questions. What drives these gaps? One possibility is that the occupations of minority candidates before running for office are community engagement positions that motivate them to run for office. Other drivers could be racial differences in personality and other characteristics. Moreover, different politicians may estimate their chances of winning differently in the next election, driving different decisions in rerunning for office. Future research is needed to explore these issues continuously.

Appendix A Outcome Bias Paper Appendix

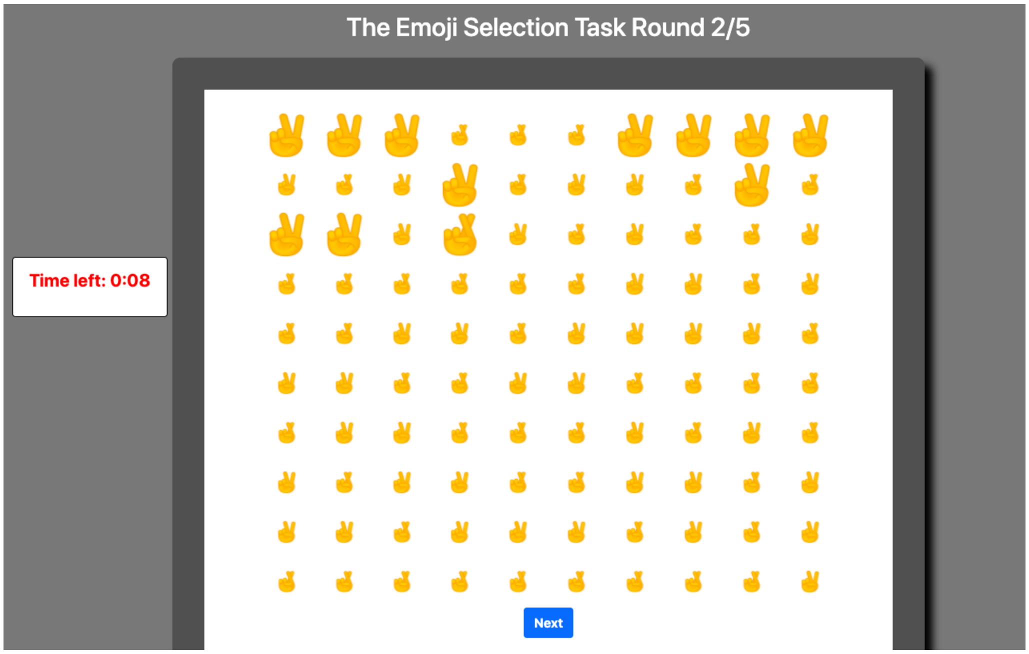

Figure 6: Agent's screen while working on the emoji selection task.

The text in red displays time remaining to complete the task in the current round. Large emojis are selected while small ones are not. Principals in the visual treatment also see similar screen where emojis change the size as agent select or dis-select them.

After reading the instructions, all the subjects had to answer the following 4 comprehension quiz questions. If they failed the first time, they were redirected to the instructions page; after re-reading the instruction, they had to re-take the same quiz again, but the order of the multiple choice options were randomized.

1. Who is responsible for performing the emoji selection task?
   - o Player A
   - o Player B
2. If 45 peace signs were selected and 1 fingers crossed sign was selected, what is the possibility that you will win the game?
   - o 44%

Summary Statistics.			
Variable	Numerical Mean	Visual Mean	Num. & Visual p-value
Donor demographics			
Female	0.41	0.36	0.48
Age	37.08	38.05	0.58
Donation to unrelated nonprofit	0.39	0.35	0.51
N	78	87	
Giving to recipient			
Donation to recipient	0.72	0.62	0.31
Donor's pie size ($)	2.72	2.68	0.27
Donor's endowment ($2 or $3)	2.63	2.61	0.15
Fraction of time donor's project is successful	0.59	0.57	0.53
N	468	522	
Beliefs about the task recipient had performed			
Task is annoying	2.64	2.23	0.03
Task is difficult	2.13	2.05	0.64
I know how recipients worked	3.53	3.92	0.03
Feelings towards recipient			
Recipient is like me	3.20	3.31	0.57
Feel close to recipient	2.97	3.14	0.40
Recipient had worked for me	3.94	3.92	0.92

P-values are from regressions clustered at the individual level for "Giving to recipient" session.

Table 15: Summary Statistics

- o 50%
- o 72%
- o 88%

3. Who is responsible for allocating the money?
   - o Player A
   - o Player B
4. If your final outcome is $X, how much will Player B receive from Player A?
   - o $0
   - o $2
   - o $4
   - o Anywhere between $0 to $X

We recruited 61 Player Bs to perform the task. The subject pool is similar to that of Player As, which we will see later, with an average age of 36 years old and a gender breakdown of around 40% female and 60% male. About 10% of the grids were skipped, but most Player Bs put maximum effort into the task. More than half of the grids were completed with very minor errors (95% or more correct), and 36% of the grids had no errors whatsoever. In a small number of grids (4%), mostly incorrect emojis were selected, harming Player A's win probability. The 60-second time limit was not binding - on average, Player Bs spent 49 seconds on each round. We imposed the time limit to control for an extra compounding factor to measure hard work – time it took subject to complete the work and only focus on how much work is done. Also, without the time control, 30-second video that principal's watched in the Visual treatment might not have been a good representation of how the agent performed the task if the agent spend for example 5 minutes on the selection task. On average, Player Bs received rewards of 68c per grid from Player As.

There were no significant time trends in completion time or performance across rounds, suggesting little learning of how the task is performed. Most player Bs put maximum effort on the task: about 36% of them selected all correct emojis and 0 incorrect emojis; more than half of the tasks are almost perfectly done (increased the winning chance to 74% or more). There is no difference for player Bs' performance across all rounds (p=0.795). Player Bs spent 49.2 seconds (Std. Dev.=13.76) on average for each round's selection task. There is no difference in time spending across all rounds (p=0.134). Their average productivity increasing the winning chances is about 1.65 percentage points per second (Std. Dev.=1.32). There is no difference in productivity across all rounds (p=0.279). The results from time spending and productivity also prove this task is mainly effort-related and not skill-related.

We asked player Bs about their beliefs on how much player As will share with them under different information conditions. They predicted player As will share about $0.86 out of $2.5 if they only know the outcome is a loss. The predicted sharing amount will significantly increase to $1.13 (numerical information) or $1.07 (visual information) if player As know that player Bs selected all correct emojis and maximize their winning chances.

Appendix B Policing Paper Appendix

Citation	Method	Field	Duration	Location	Obs	Main Results	Mechanism
Campbell (2021)	DID	police homicide	14-19	country wide	1,571 census places	15-20% decrease 300 fewer deaths	body camera - community perception - less force
RENY and NEWMAN (2021)	RDiT	public opinion on police + discrimination	Jul19 -Sep20	country wide	378k individuals	low-prejudice: ↓ police ↓ discrimination high-prejudice: unchanged	x location attention to politics attention to media e.g. MSNBC x FOX
Skoy (2021)	state month FE	fatal interactions with police	10-17 following month	country wide	4.8k counties	↓ 0.225 per 10 m blacks	NA
Wasow (2020)	IV	voting	60-72	country wide	9k counties	nonviolent ↑ violent ↓	rights - subordinate riots - dominant
Mazumder (2019)	DID	racial attitudes	10-18	country wide	148k individuals	↓ racial resentment	- young
Mazumder (2018)	OLS	racial political SP attitude	06-11	country wide	150k individuals	↓ racial resentment ↑ democrats ↑ AA	NA
Sawyer and Gampa (2018)	OLS	racial attitude	09-16	country wide	1.4m individuals	↓ pro-White	NA

Table 16: Effects of BLM protests literature review

No.	City	State	InUse	Search results	Start Date	End Date	Update	Links
Stop:	From police data initiative: https://www.policedatainitiative.org/datasets/							
1	DC	NA	N	no 2020 data	1-Jan-10	31-Dec-17	Unknown	https://mpdc.dc.gov/node/1310236
2	Denver	CO	N	no race info	31-Dec-10	20-Jul-21	Daily	https://www.denvergov.org/opendata/c
3	Fayetteville	NC	Y	by file	1-Jan-16	31-Dec-21	Daily	https://data.fayettevillenc.gov/datasets/f
4	Gaithersburg	MD	Y	by file	1-Jan-12	31-Dec-21	Unknown	https://data.montgomerycountymd.gov.
5	Lincoln	NE	N	no 2020 data	1-Jan-18	31-Dec-19	Yearly	https://opendata.lincoln.ne.gov/search?
6	Louisville	KY	Y	by file	1-Jan-09	31-Dec-21	Monthly	https://data.louisvilleky.gov/dataset/lmp
7	Middletown	NY	Y	by quarter	1-Jan-18	31-Dec-21	Quaterly	https://wallkillpd.org/document-center/
8	New Orleans	LA	Y	by file	1-Jan-10	31-Dec-21	Daily	https://data.nola.gov/Public-Safety-and
9	New York	NY	Y	by year	1-Jan-18	31-Dec-20	Yearly	https://www1.nyc.gov/site/nypd/stats/r
10	Northampton	MA	N	not individual data	1-Jan-14	31-Dec-15	Unknown	https://northamptonpd.com/calls-for-se
11	Owensboro	KY	N	not individual data	1-Jan-15	31-Dec-19	Yearly	https://police.owensboro.org/wp-conte
12	Palo Alto	CA	N	no race info	1-Jan-18	31-Dec-20	Yearly	https://data.cityofpaloalto.org/search/?q
13	Philadelphia	PA	Y	by year	1-Jan-18	31-Dec-21	Daily	https://www.opendataphilly.org/datase
14	Sacramento	CA	N	no 2020 data	1-Jan-19	10-Nov-19	Unknown	https://data.cityofsacramento.org/datase
15	San Diego	CA	N	no 2020 data	1-Jan-14	30-Jun-18	Yearly	https://data.sandiego.gov/datasets/polic
16	San Francisco	CA	N	not individual data	1-Jan-16	31-Dec-20	Quarterly	https://www.sanfranciscopolice.org/yo
17	Seattle	WA	Y	by file	15-Mar-15	31-Dec-21	Daily	https://data.seattle.gov/Public-Safety/T
18	Waterbury	VT	N	no 2020 data	1-Jan-18	31-Dec-19	Yearly	https://vsp.vermont.gov/communityaffa
19	Winnoski	VT	N	can't open	NA	NA	NA	https://winooskipolice.com/fair-and-im
Stop:	Learn from crime talks: the cities might have open data							
20	Chicago	IL	Y	by year	1-Jan-16	31-Dec-21	Yearly	https://home.chicagopolice.org/statistic
21	Minneapolis	MN	Y	by file	31-Oct-16	31-Dec-21	Daily	https://opendata.minneapolismn.gov/da
Arrest:	From police data initiative: https://www.policedatainitiative.org/datasets/							
1	Baltimore	MD	Y	by file	1-Jan-15	31-Dec-21	Daily	https://data.baltimorecity.gov/datasets/a
2	Bremerton	WA	Y	by month	1-Jan-18	31-Dec-21	Monthly	http://www.bremertonwa.gov/Docume
3	Chandler	AZ	Y	by file	1-Jan-15	31-Dec-21	Daily	https://data.chandlerpd.com/catalog/arr
4	Charleston	SC	Y	by year	1-Jan-18	31-Dec-21	Monthly	https://www.arcgis.com/apps/MapSeri
5	DC	NA	N	no 2020 data	1-Jan-12	31-Dec-19	Yearly	https://mpdc.dc.gov/node/1347766
6	DC	NA	N	no race info	1-Jan-16	31-Dec-21	Semi-yearly	https://mpdc.dc.gov/node/208852
7	Fayetteville	NC	Y	by file	1-Jan-10	31-Dec-21	Daily	https://data.fayettevillenc.gov/search?c
8	Gaithersburg	MD	N	no race info	1-Jun-21	31-Dec-21	Daily	https://data.montgomerycountymd.gov.
9	Hampton	VA	N	no 2020 data	1-Jan-19	15-Nov-19	Yearly	https://data.hampton.gov/dataset/DRU
10	Hampton	VA	N	no 2020 data	1-Jan-19	15-Nov-19	Yearly	https://data.hampton.gov/Government/
11	Lincoln	NE	Y	by year	1-Jan-18	31-Dec-20	Yearly	https://opendata.lincoln.ne.gov/search?
12	Long Branch	NJ	N	not individual data	1-Jan-15	31-Dec-19	Yearly	https://www.longbranch.org/juvenile-a
13	Los Angeles	CA	N	no 2020 data	1-Jan-10	31-Dec-19	Yearly	https://data.lacity.org/Public-Safety/Ar
14	Newport News	VA	N	can't download	NA	NA	NA	https://www.nnva.gov/2229/Open-Dat
15	North Bergen	NJ	N	not individual data	1-Jan-16	31-Dec-21	Monthly	http://www.northbergenpolice.com/rep
16	Northampton	MA	N	not individual data	1-Jan-03	31-Dec-15	Yearly	https://northamptonpd.com/department
17	Providence	RI	N	sent a msg	1-Jun-21	31-Dec-21	Daily	https://data.providenceri.gov/Public-Sa
18	San Francisco	CA	N	not individual data	1-Jan-16	31-Dec-20	Quarterly	https://www.sanfranciscopolice.org/yo
19	Tucso	AZ	Y	by year	1-Jan-18	31-Dec-21	Yearly	https://gisdata.tucsonaz.gov/datasets?q
20	Waterbury	VT	N	no 2020 data	1-Jan-13	31-Dec-16	Yearly	https://data.vermont.gov/Public-Safety/
Arrest:	Learn from crime talks: the cities might have open data							
21	Pittsburgh	PA	Y	entire	1-Jan-18	31-Dec-21	Daily	https://data.wprdc.org/dataset/arrest-da
22	Chicago	IL	N	no time	2017	2021	Unknown	https://home.chicagopolice.org/statistic

Table 17: Data collection process

City	State	No Action Taken	Warning Issued	Citation Issued	Arrest Made
Chicago	IL	Y	N	Y	Y
Fayetteville	NC	Y	Y	Y	Y
Gaithersburg	MD	Y	Y	Y	Y
Louisville	KY	N	Y	Y	N
Middletown	NY	N	N	N	N
Minneapolis	MN	N	N	Y	N
New Orleans	LA	Y	Y	Y	Y
New York	NY	N	N	N	Y
Philadelphia	PA	N	N	N	Y
Seattle	WA	N	N	Y	Y

Table 18: Police investigation data availability

	City	State	Lockdown
1	Chandler	AZ	3/31/20
2	Tucson	AZ	3/31/20
3	Chicago	IL	3/21/20
4	Louisville	KY	3/26/20
5	New Orleans	LA	3/20/20
6	Baltimore	MD	3/23/20
7	Gaithersburg	MD	3/23/20
8	Minneapolis	MN	3/27/20
9	Fayetteville	NC	3/30/20
10	Lincoln	NE	NA
11	Middletown	NY	3/20/20
12	New York	NY	3/20/20
13	Philadelphia	PA	4/2/20
14	Pittsburgh	PA	4/2/20
15	Charleston	SC	4/7/20
16	Bremerton	WA	3/24/20
17	Seattle	WA	3/24/20

Table 19: Lockdown start date for each city

Proportion of African Americans	(1) Traffic Stops	(2) Traffic Stops	(3) Pedestrian Stops	(4) Pedestrian Stops
Protests	0.026	0.020	0.022	0.031
P-value	0.401	0.602	0.250	0.250
Bandwidth	68	63	68	44
Observations	207,546	195,424	62,996	40,706
R-squared	0.124	0.126	0.006	0.006

Notes: The samples are limited to the lockdown time. City fixed effects are included in every specification as controls. Standard errors are clustered at the city level. P-values are adjusted by the Wald test. Bootstrapped p-values are shown below the coefficients. Statistical significance levels are the following: *** p<0.01, ** p<0.05, * p<0.1

Table 20: Protests on traffic and pedestrian stops

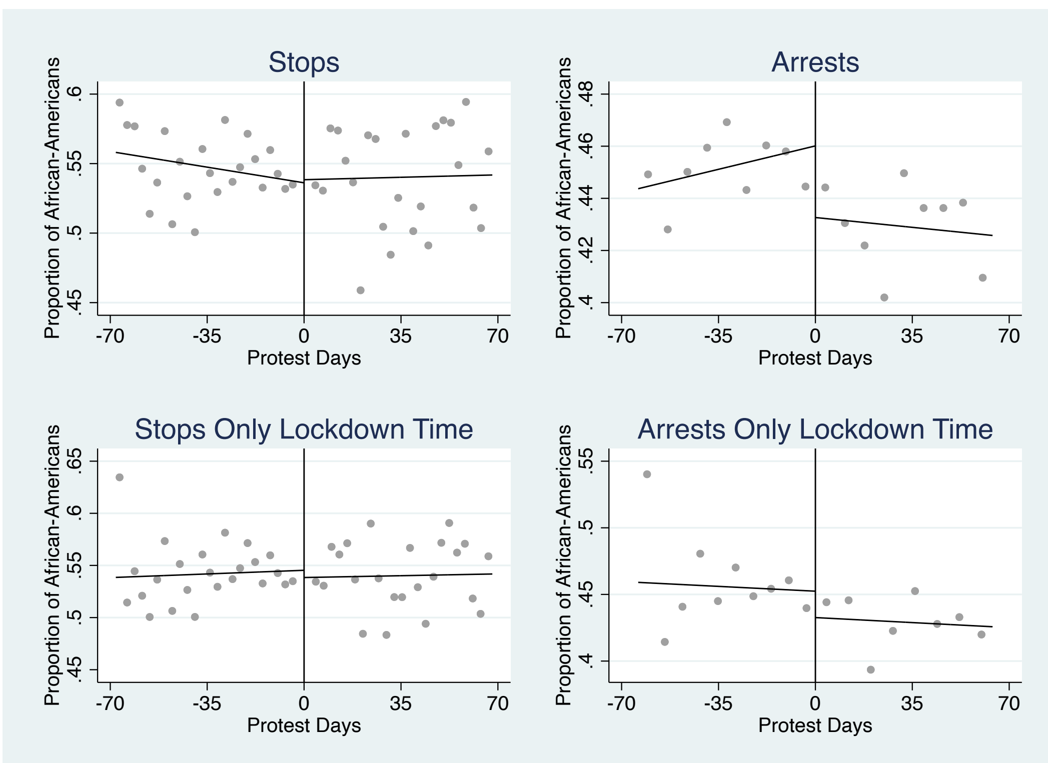

Figure 7: Protests on the proportion of African Americans

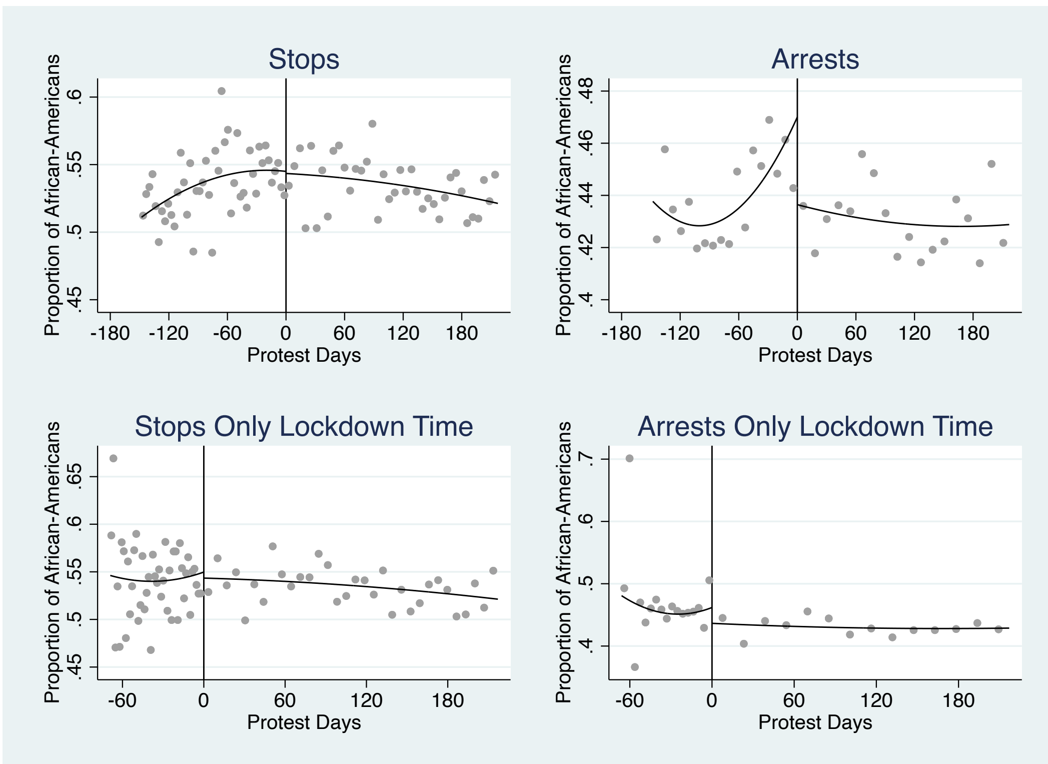

Figure 8: Protests on African American Stops and Arrests [Polynomial=2]

Appendix C Politician Persistence Paper Appendix

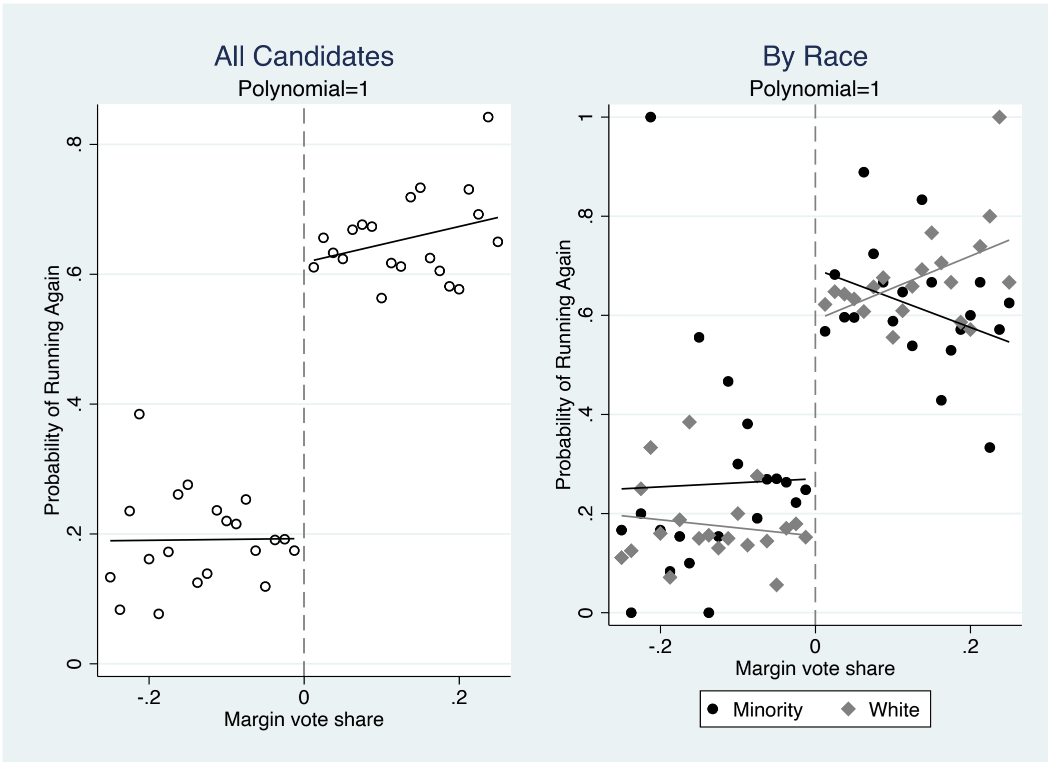

Figure 9: Effect of Electoral Outcome and Race on Subsequent Political Participation (linear)

		(1)	(2)	(3)	(4)	(5)
		Polynomial order one				Polynomial order two
		Optimal bw	2xOptimal bw	0.5xOptimal bw	Full sample	Full sample
Panel A: All Candidates	Lost	-0.435***	-0.442***	-0.411***	-0.445***	-0.437***
		(0.021)	(0.020)	(0.025)	(0.017)	(0.026)
	Winner mean	0.620	0.625	0.610	0.635	0.617
	Bandwidth	0.112	0.224	0.056	-	-
	Observations	3,419	4,135	2,629	4,615	4,615
	R-squared	0.231	0.227	0.234	0.227	0.228
Panel B: Nonwhite Candidates	Lost	-0.314***	-0.371***	-0.323***	-0.357***	-0.359***
		(0.058)	(0.047)	(0.096)	(0.032)	(0.051)
	Winner mean	0.577	0.614	0.585	0.627	0.621
	Bandwidth	0.079	0.158	0.040	-	-
	Observations	713	912	489	1,175	1,175
	R-squared	0.205	0.185	0.209	0.189	0.190
Panel C: White Candidates	Lost	-0.454***	-0.459***	-0.414***	-0.474***	-0.461***
		(0.023)	(0.021)	(0.036)	(0.016)	(0.027)
	Winner mean	0.623	0.625	0.592	0.638	0.614
	Bandwidth	0.076	0.152	0.038	-	-
	Observations	2,305	2,853	1,638	3,432	3,432
	R-squared	0.263	0.261	0.270	0.254	0.256

Note: The outcome variable is whether a candidate runs again for office in 4 years. Standard errors are clustered at the county level. County and year fixed effects are included in each specification. Significance levels are indicating as: *** p<0.01, ** p<0.05, * p<0.1.

Table 21: Effect of Electoral Outcome on Subsequent Political Participation across Racial Groups

	Nonwhite	White	Difference
Lose	25.7	16.2	9.5
Win	59.2	62.7	-3.5
Difference	-33.5	-46.5	13

Table 22: Likelihood of Running for Office Again by Electoral Outcome and Race

	(1)	(2)	(3)	(4)	(5)
	Polynomial order one				Polynomial order two
	Optimal bw	2xOptimal bw	0.5xOptimal bw	Full sample	Full sample
Lost	-0.465***	-0.467***	-0.426***	-0.477***	-0.462***
	(0.022)	(0.019)	(0.026)	(0.016)	(0.026)
Hispanic	-0.032	0.005	0.026	-0.010	0.002
	(0.034)	(0.035)	(0.058)	(0.025)	(0.033)
Lost*Hispanic	0.149**	0.109*	0.049	0.105**	0.137**
	(0.065)	(0.061)	(0.093)	(0.043)	(0.062)
White winner mean	0.629	0.623	0.612	0.639	0.614
Bandwidth	0.112	0.224	0.056	-	-
Observations	3,163	3,810	2,430	4,242	4,242
R-squared	0.241	0.239	0.247	0.240	0.241

Note: The outcome variable is whether a candidate runs again for office in 4 years. Standard errors are clustered at the county level. County and year fixed effects are included in each specification. Significance levels are indicating as: *** p<0.01, ** p<0.05, * p<0.1.

Table 23: Effect of Electoral Outcome on Subsequent Political Participation: Hispanic and White Alone

Appendix D Bibliography

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