NOT SO BLACK AND WHITE: UNCOVERING RACIAL BIAS FROM SYSTEMATICALLY MISREPORTED TROOPER REPORTS*

Elizabeth Luh†

December 10, 2021

Abstract

Highway police officers, or troopers, may misreport the race of motorists to evade detection of racial bias. I propose a new model of racial bias in policing that exploits systemic misreporting to identify racial bias. Using a unique event in Texas where troopers were caught misreporting minorities as White from 2010 to 2015, I find that a standard deviation in estimated bias leads to a 7-12 percentage point increase in reporting a failed search as White. Furthermore, I find suggestive evidence that misreporting is effective in shielding biased troopers from negative labor outcomes. These results suggest an important role for increased accountability in the veracity of data collection from law enforcement agents and the manipulation of outcome based tests of racial bias.

JEL Classification: J15, K42

Keywords: Racial Bias, Systemic Misreporting, Traffic Enforcement

*This research uses confidential information from the the Texas Department of Public Safety, graciously provided by the Stanford Open Policing Project. I pledge that the views expressed in this paper are my own and not of the State of Texas or of the Stanford Open Policing Project. I am grateful to Vikram Maheshri, Elaine Liu, and Gergely Ujhelyi, Aimee Chin, and Chinhui Juhn for their continual support and invaluable guidance throughout this project.

†CJARS, Univeristy of Michigan, 426 Thompson, Ann Arbor Michigan 48103. Email: eluh@umich.edu.

1 Introduction

In the United States, individuals of different race experience differential treatment by law enforcement officers at all steps of the criminal justice system. In 2017, 27% of Latinos and 50% of Blacks felt personally discriminated against by police compared to only 10% of White respondents.[1] This perception is supported by current research, which has identified racial bias in nearly all steps of the justice system from airport screening (Persico and Todd, 2005), ticketing (Anbarci and Lee, 2014; Goncalves and Mello, 2021), stop and frisk participation (Coviello and Persico, 2013), bail decisions (Arnold, Dobbie, and Yang, 2018), sentencing (Shayo and Zussman, 2011; Depew, Eren, and Mocan, 2017), parole (Anwar and Fang, 2015), and capital punishment (Alesina and Ferrara, 2014). Further, recent events following the death of George Floyd at the hands of law enforcement have led to widespread calls for action, making criminal justice reform a top priority for citizens and policymakers at all levels of government.

Despite the academic focus on testing for racial bias and discrimination in the criminal justice system, little attention has been given to the response of law enforcement officers to this heightened scrutiny. These behavioral responses are important as law enforcement officers oftentimes control how civilian interactions are recorded with little oversight on the accuracy of the record. Officers could deliberately misreport the interactions with civilians and could be more likely to do so to evade detection of bias. Thus, if the most discriminatory officers have the most incentive to misreport, any policies targeting discrimination may miss the officers who need the most discipline and training. Even more concerning, with widespread systematic misreporting, agencies and institutions could be underestimating the presence of discrimination within their department.

In this paper, I propose an new model of racial bias where discriminatory highway police officers (henceforth "troopers") have incentive to misreport their interactions with motorists to appear less biased. By misreporting a portion of their failed minority searches as failed White searches, troopers can improve their reported minority search success rate and appear less biased. I apply my model to a documented instance in Texas where highway troopers were found to be misreporting minority motorists' race as White (Collister, 2015b). Prior to 2015, Texas troopers were free to record motorist race based on their own best judgment, allowing for a discrepancy between motorists' actual race and the recorded race.

Studying racial bias in the presence of misreporting is advantageous for several reasons.

[1] See https://cdn1.sph.harvard.edu/wp-content/uploads/sites/94/2018/01/NPR-RWJF-HSPH-Discrimination-Final-Summary.pdf for details.

The major challenge in existing literature is disentangling statistical discrimination, or the difference in criminality by motorist race, from racial bias (Knowles, Persico, and Todd, 2001; Anwar and Fang, 2006; Antonovics and Knight, 2009). In the context of this paper, troopers' choice to misreport a portion of their searches creates an observable variation within minority searches that can be linked to racial bias. These repeated observations of misreporting within each trooper can be used to measure of racial bias that is on the individual, trooper level.

Using a statistical model of racial bias that builds on on Knowles et al. (2001) and Anwar and Fang (2006)'s seminal models of racial bias in highway searches, I demonstrate that misreporting is a direct measure of racial bias. Troopers have incentive misreport their failed searches in order to appear less racially biased and evade punishment for racial bias. Since misreporting itself is a punishable behavior and therefore has a cost, misreporting is only profitable when the following are true: 1) only racially biased troopers will find misreporting to be profitable; 2) biased troopers will only misreport their failed, minority searches. Thus, only biased troopers will find misreporting to be profitable and the more biased the trooper is, the more he will engage in this behavior.

To estimate trooper's bias, I use a restricted data set of Texas highway searches from the Stanford Open Policing Project (SOPP) from 2010 - 2015 combined with trooper employment data from the Texas Department of Public Safety (DPS). This data set contains driver's recorded races, full name, and home address for every recorded highway search in Texas during that time period. Leveraging the detailed driver's information, I can estimate driver's true race and estimate trooper's difference in misreporting across search outcome.

A major challenge in studying racial bias in the presence of misreporting is that intentional misreporting is unobservable. Troopers may misreport for reasons aside from racial bias (ex. poor visibility) and driver's race may be visually ambiguous. To address this issue, I estimate trooper's differential likelihood of misreporting conditional across search outcome as errors in race recording for reasons aside from bias should be independent of the search outcome. Specifically, biased troopers are more likely to misreport when minority motorists as white when searches end in failure.

Focusing on searches of Hispanic motorists, the group of drivers most likely to be misreported, I use each trooper's differential likelihood of misreporting across search outcome to estimate bias.[2] I show that on average, motorists with predicted Hispanic ethnicity are two percentage points more likely to be misreported when the search ends

[2]Black and Asian motorists were misreported at significantly lower rates (Collister, 2015a).

in failure compared to success off an average of 40%.

To determine the validity of my measure of bias, I exploit the discovery of the misreporting and the subsequent rule change at the end of 2015 requiring troopers to verbally ask motorists' for their race as a natural experiment. Using a triple difference in difference specification, I show that biased troopers were significantly more likely to misreport their minority searches compared to their unbiased peers prior to the rule change. Specifically, one standard deviation of estimated bias was correlated with a seven percentage point higher likelihood in reporting a failed search as White. After the rule change, this gap in reporting behavior between biased and unbiased troopers disappears. These results supports the interpretation of my measure as capturing bias and that troopers were indeed misreporting minority motorists who did not self-identify as White. Furthermore, from a policy standpoint, the new rule was effective in curbing the race misreporting behavior of biased troopers.

I also identify effects of bias on troopers' labor market outcomes using administrative, employment data. I find that prior to the discovery of misreporting by various media outlets starting in 2015, bias had no effect on labor market outcomes for troopers who remained in the force. After the discovery of misreporting, every standard deviation in bias, decreased the likelihood of promotion by 27 percent and decreased salary growth by 8 percent. Since these negative outcomes occur after the rule change, I interpret this as suggestive evidence that misreporting was effective in shielding biased troopers from punishment for bias before the change.

This paper makes several important contributions. First, I provide robust evidence that troopers were using misreporting to appear less biased, which may be attenuating current tests of racial bias. This may bring into question the validity of past findings of racial bias using marginal outcome tests, notably the hit rate test. Second, I provide a statistical model to show how misreporting can be used as a measure of bias. Lastly, I use a unique data set constructed from various administrative data sources to show that misreporting is an effective shield for biased troopers to evade punishment for racial bias.

1.1 Literature Review

This paper contributes to the literature on detecting racial bias in the criminal justice system, specifically in contexts where law enforcement officers record the interaction. Many earlier contributions to the literature, notably in motorist stops and racial bias, examine the role of motorist race and trooper race in stop interactions, notably Knowles et al. (2001) and Anwar and Fang (2006) along with Antonovics and Knight (2009). These papers use Becker's (1957) outcome test, which identifies racial bias by comparing the success rates across different groups. These tests cannot measure the magnitude of bias on the individual, trooper level. Relative to this literature, my paper is able to measure bias on the individual level and also is the first to address the possibility of the data being purposefully misreported to hide bias.

Another method of identifying racial bias in highway and traffic stops uses plausibly exogenous characteristics of the stop. One example is the 'veil of darkness,' which uses the diminished ability of trooper's to observe the motorist race after sunset. Grogger and Ridgeway (2006), Horrace and Rohlin (2016), and Kalinowski, Ross, and Ross (2017) use the differing speed distributions in daylight and darkness to test for bias against African Americans. Another method in the context of police ticketing decision, by West (2018), uses the plausibly exogenous assignment of police officers to traffic accidents to identify a causal relationship between the actions of police officers by driver race. The major drawback to both of these identification strategy is that the results are context specific and may not apply to a wider range of motorist and officer interactions.

My research is also related to empirical research in cheating behavior since misreporting in the context of trooper reports is a form of cheating (Jacob and Levitt, 2003; Dee, Dobbie, Jacob, and Rockoff, 2019). The most prominent of these papers, Jacob and Levitt (2003) used patterns in students' test scores to uncover cheating. Similar to my paper, the true measure of cheating was unobserved, thus Jacob and Levitt (2003) had to test for cheaters by measuring cheating rates across different thresholds. An advantage of my method for uncovering misreporting is that the measure is within trooper and does not rely on the existence of a comparison group to measure cheating. This paper also is able to examine trooper consequences before and after the cheating was possible, which Jacob and Levitt (2003) cannot.

A recent paper by Goncalves and Mello (2021) also identifies racial bias at the individual level using police officers' choice of leniency when giving speeding tickets. They find that officers are more likely to be lenient when ticketing speed violations with White drivers compared to non-White drivers, which they argue is proof of biased behavior. Using the individual officer's difference in leniency across motorist race, they identify discrimination by comparing these lenient officers to non-lenient officers. Compared to their paper, a strength of my approach is that my test does not rely on identifying a comparison group of unbiased officers to measure bias.

Given the severity of misreporting behavior in troopers observed in this paper, this motivates the need to carefully consider the accuracy of policing data when studying racial bias, especially when the agents themselves are responsible for recording the data. With misreporting, even the most robust tests of racial bias will be under-detecting the existence of bias. The results also motivate other law enforcement departments to require their officers to ask for driver's race in all interactions to prevent misreporting and biased behavior.

The rest of the paper is organized as follows. In Section 2, I outline the background of my research. Section 3 outlines my theoretical model of racial bias. In Section 4, I explain my data construction. Section 5 shows my empirical results and other testable implications of my model. I finally conclude in Section 6.

2 Background

2.1 Misreporting and Highway Troopers in Texas

Texas Highway Patrol is a division of the Texas Department of Public Safety, which is responsible for enforcing state traffic laws and commercial vehicle regulation on highways of Texas. They currently employ over 2,800 troopers in Texas divided across 6 regions in Texas, with a separate region for their headquarters in Austin. The department is responsible for licensing of drivers, vehicle inspections, and handgun licensing. Figure I shows the division map across Texas.

To become a trooper, a person must complete recruit school or transfer from prior law enforcement service. New hires spend some at least one year as probationary troopers before being permanent assignments. After the one year probationary period, troopers take their final exam and are promoted to trooper.

With every four years, troopers can be promoted to different level of trooper classes and to different ranks, which include salary increases. Salary amounts are determined by years in the force and rank. Ranks or classes of troopers are similar to military ranks and go from trooper, corporal, sergeant, lieutenant, captain, and major. In general, only troopers in good standing (no sustained complaints, no disciplinary actions, no demotions) are promoted. Unlike other state police agencies, Texas legislature sets the salary of troopers, rather than the individual agencies. With each salary promotion, troopers can be moved to different stations across the state to fill availability. Troopers are allowed to have some say in the choice of where they are stationed after significant changes in DPS in 2012. Prior to 2012, station assignment was based on availability and need.

In a motorist stop, troopers are allowed to investigate the passenger and the driver. While drivers are not required to answer questions, they are required to provide their driver's license and if arrested, they must also provide their name, residence address, and date of birth. Law enforcement officers may ask for consent to search the vehicle or person, which the driver can grant or deny. "... however, if an officer has probable cause to believe that your vehicle contains evidence of crime, it can be searched without your consent (DPS, DPS)." To search a vehicle without the driver's consent, the trooper must either have: probable cause, arrested the driver *prior* to searching the vehicle, reasonably believes the motorist has weapons, or has a warrant. If the officer believes that the driver or passenger has a weapon, he or she may pat down the person and search the vehicle and the surrounding immediate area. Motorists cannot physically resist a search but can notify the officer that he or she does not consent.

Drivers can report troopers if they feel that troopers behaved inappropriately during a stop and troopers can face repercussions if the claim is substantiated. Troopers badge numbers and names are normally provided and drivers can submit complaints to the department. Upon receipt of a complaint, the department assigns the complaint either he Personnel Complaint Investigations or Division Referrals to investigate the complaint. The investigation can have one of four outcomes: unfounded, exonerated, not sustained, or sustained. A sustained complaint can result one or more of the following: formal written reprimand, disciplinary probation, time off without pay, reduction of salary rate, demotion, and or discharge. A formal complaint "alleges one or more of either an infraction of Department rules, regulations, or polices, or an illegal act (TxDPS, 2018)." Racial profiling is considered an illegal act under Article 2.132 in the Code of Criminal Procedures and can be a legitimate reason to file a complaint against the trooper.

On November 8th, 2015, KXAN published the results of their investigation of DPS, which found that troopers were " inaccurately recording the race of large numbers of minority drivers, mostly Hispanic, as White" (Collister, 2015b). Texas troopers were already under scrutiny due to the death of Sandra Bland in jail after being pulled over for failing to signal a lane change (Sanchez, 2015). One week after the misreporting was uncovered, the House Committee on County Affairs held a hearing where DPS blamed the error on a computer glitch. As a result of the hearing, DPS changed its policies to require troopers to ask drivers to provide their race, rather than recording it based on the trooper's best judgment. This policy went in effect by November 23rd; as a result of the policy, the percent of White motorists being stopped fell from 18% to 4% by 2016 (Collister, 2015a).

An important result of the KXAN investigation was that misreporting was also found in other law enforcement departments in Texas, namely the Houston and Austin police departments. Thus, it is not out of the question to test for possible misreporting behavior in police or trooper forces in other state and law enforcement agencies. This raises the question if whether past reports and research of racial bias are possible under-measuring and under-detecting the existence of bias. Less than a month after the publication of the article, DPS changed its policies to require Texan troopers to now ask drivers for their race rather than using their own best judgment (Oyeniyi, 2015).

Misreporting is easy in motorist stops compared to other points of the criminal justice system. First, the trooper is not required to ask the driver for his or her race. Instead, the trooper is supposed to infer the race based on observable characteristics of the driver. Second, due to the high frequency of stops, troopers or police officers who participate in misreporting are not checked for accuracy and are less likely to be caught. Usually, only the driver focuses on the content of the ticket. Third, unless the trooper searches the driver and arrests the driver, it is unlikely another law enforcement officer will look at the recorded race.

3 Model

Motorists of race m travel on highways; a fraction π^m of them are carrying contraband. Trooper t may stop motorists without observing their race. Conditional on stopping a motorist, a trooper receives a signal θ that contains all available information on whether the motorist is carrying contraband.[3] θ is collapsed to a single index $\theta \in (0,1)$ and is drawn from distributions $f_g^m(.)$ if the driver does carry contraband and from $f_n^m(.)$ if the driver does not carry contraband. For ease of exposition, I assume that troopers and motorists are either White (W) or minority (M) in this section. In my empirical analysis, I allow for motorists to be W or H (Hispanic).

Similar to past papers on racial bias (notably, Alesina and Ferrara (2014); Anwar and Fang (2006)), I make the following assumption:

Assumption 1. $f_n^m(.)$ *and* $f_g^m(.)$ *are continuous and satisfy the strict monotone likelihood ratio property (MLRP). Specifically,* $\frac{f_g^m}{f_n^m}$ *is strictly increasing in* θ

This implies the following properties of the distribution. First, a higher index of θ implies a higher probability of driver guilt. Second, the cumulative distribution, $F_g^m(.)$ stochastically dominates $F_n^m(.)$. In other words, motorists who carry contraband are more

[3]Some examples of these characteristics are age, height, address, gender, the interior of the vehicle, the smell of the driver, whether the driver is under the influence, whether the license plate is in-state, the time and place of the stop, whether the vehicle is rented, and the attitude of the driver.

likely to appear more suspicious, or signal higher θ's. Lastly, $\frac{f_g^m}{f_n^m} \to +\infty$ as $\theta \to 1$ as some motorists may be obviously guilty.

3.1 Bias and Misreporting

Having observed (m, θ), a trooper decides whether to search the motorist in order to find contraband. Searching a driver incurs a cost of $c_{m,t} \in (0,1)$; troopers obtain a normalized benefit of 1 if drivers are guilty. The *ex ante* probability that a motorist is guilty is

$$\Pr(G = 1|m, \theta) = \frac{\pi_m f_g^m(\theta)}{\pi_m f_g^m(\theta) + (1 - \pi_m) f_n^m(\theta)} \tag{1}$$

Trooper t will search a race-m motorist if and only if

$$\Pr(G = 1|m, \theta) \geq c_{m,t} \tag{2}$$

This yields the search threshold, $\theta^*_{m,t}$.
Search thresholds that vary by m may reflect either statistical discrimination or bias on the part of troopers. A trooper may choose different thresholds purely because motorists θ's are drawn from different distributions or because π_m varies by race.

Definition 1. Trooper, with $c_{M,t} = c_{W,t}$, exhibits *statistical discrimination* against race M motorist if $\theta^*_{M,t} < \theta^*_{W,t}$.

Alternatively, a trooper may choose different thresholds because they incur different costs of failed searches. Following Knowles et al. (2001) and Anwar and Fang (2006), I define racial bias as

Definition 2. A trooper of race-t exhibits *racial bias* against motorist of race-M if $c_{M,t} < c_{W,t}$.

Given Definition 2, let $b = c_{W,t} - c_{M,t}$ be the magnitude of bias against race-M motorists for trooper-t. b is in terms of the trooper t's search cost across motorists' race and is unobservable. Thus, to be able to compare levels of bias across troopers, I need to transform b into measurable units.

Definition 3. v is a measure of bias if $b > b' \iff v(b) > v(b')$

v is a monotonic transformation of b. Since $f^m_{g,n}$ and π_m are unobservable, proving that the measure of v is driven by b (racial bias) and not $\theta^*_{M,t} - \theta^*_{W,t}$ (statistical discrimination) is key to identifying v as a measure of b.

Troopers may face punishment for biased policing with probability P, which is monotonically increasing in $|b|$. In order to evade detection, a trooper may intentionally misreport the race of a motorist following a search, which will reduce the appearance of bias and thereby the likelihood of detection. Troopers incur a cost of μ for misreporting, as it may open the door to greater punishment. I make the following assumptions on μ:

Assumption 2. *$\mu(\theta, G) > 0$ is increasing in θ.*

As θ, or the culpability of the motorist, increases, the cost of misreporting also rises. Therefore, motorists who appear less guilty are more likely to be misreported. Since troopers misreport to reduce the appearance of bias and because of Assumption 2, troopers will misreport the race of a motorist if and only if

$$c_{M,t} + \mu_{M,t}(\theta, G) \leq c_{W,t} \tag{3}$$

Therefore, only troopers who are biased against race M motorists will misreport motorists of race M as W. If a trooper is unbiased, there exists no θ such that Equation (3) will hold.

Assumption 3. *$0 < \mu(\theta, G = 0) < 1$, $\mu(\theta, G = 1) > 1$ for all $\theta \in (0, 1)$.*

Guilty searches are more likely to end up in court where another person (i.e. a judge) will view the search report with the incorrect driver's race. Thus, misreporting searches is only profitable when the search ends in failure.
Assumption 2 and 3 implies that troopers will misreport the race of a motorist if and only if

$$c_{M,t} + \mu_{M,t}(\theta, G = 0) \leq c_{W,t} \tag{4}$$

This yields the misreporting ceiling, $\theta^{\mu}_{M,t}$.

Given this set up, I obtain the following result:

Proposition 1. *Under Assumption 1, 2, and 3, troopers will misreport motorists with characteristics (M, θ) if and only if $\theta \in (\theta^*_{M,t}, \theta^{\mu}_{M,t})$ and the search ends in failure.*[4]

Troopers will only misreport their failed searches. Because the misreporting decision is conditional on search, any misreported motorists must have $\theta > \theta^*$. Troopers also will not misreport motorists over a certain threshold, specifically $\theta > \theta^{\mu}$. That is, motorists who appear more guilty than the search threshold will not be misreported.[5]

[4]The proof of Proposition 1 is in the appendix.

[5]One intuitive reason for this is that the searching motorists who appear more guilty (have higher θ) are more justifiable if the trooper is accused of discrimination.

The fact that only biased troopers will misreport their searches provides an attractive criterion to identify bias. In particular, biased troopers will only misreport their unsuccessful searches and correctly report the motorists' race in successful searches, creating an observable difference in search behavior across motorists race between biased troopers and unbiased troopers:

Proposition 2. *Under Assumption 1,2, and 3, the difference in the average misreporting rate of race M motorists for trooper t across search outcome G,*

$$v_{M,t} = (1 - \pi_M)[F_n^M(\theta_{M,t}^{\mu}) - F_n^M(\theta_{M,t}^{*})] \tag{5}$$

is a measure of bias against race M motorists for trooper t.

For unbiased troopers, $v = 0$. For biased troopers, $v > 0$.[6] The magnitude of $v_{M,t}$ itself will also be trooper t's measure of bias against race M motorists. This forms the basis of my measure of racial bias for trooper t against race M motorist that I use throughout the rest of the paper.

4 Data

4.1 Stop Data

The Stanford Open Policing Project (SOPP) has a restricted version of highways stops conducted from 2005 to 2015 from the Texas Department of Public Safety. The restricted version contains personally identifiable information of the driver such as full name, home address, owner's full name, and license plate of the stopped vehicle. **Pierson, Simoiu, Overgoor, Corbett-Davies, Ramachandran, Phillips, and Goel (2020) courteously provided the raw version of the data.**[7] As DPS did not record the driver's last name prior to 2010, only stops from 2010 onward are included in the study. For reasons I explain in the next section, female drivers are excluded from the data set leaving 9 million total stops in the data set.

The data also has rich stop information such as the latitude, longitude of the stop, the badge number of the officer who recorded the stop, the race of the driver, the state in which the driver's license was issued, and the make and model of the vehicle. The data

[6]The proof of this and Proposition 2 is in the appendix.

[7]SOPP collected over 130 million records from 31 state police agencies (Pierson et al., 2020). The goal of the project is to analyze detailing interactions between police and the public. This data is freely available on the website.

also has information on the violation such as reason for the stop, the outcome of the stop (citation, warning), whether a search was conducted, the search reason, and the outcome of the search. The highway stop data is publicly available on the TX DPS website from 2013 - 2019.[8]

I also augment the SOPP data with 2016–2017 highway stop data from the Texas Department of Public Safety. This data has identical information to the SOPP data, but does not have the driver's full name or addresses in order to protect the privacy of the driver's in the data set. I also drop the female driver's from this data set to maintain consistency with the SOPP data. Since the stops occurred after the misreporting was revealed in November 2015, I take the driver's races as given.[9] My primary purpose for including the publicly available data is to measure each trooper's change in misreporting behavior after the publication of the article.

In Texas, troopers can legally search a vehicle for many reasons aside from probable cause or driver consent. Some of these situations, such as search incident to arrest, after the car is impounded, or with a warrant, do not fit the framework of the model. One of the assumptions in my model is that motorists are only guilty through finding contraband. Including searches where the motorist is arrested prior to searching the vehicle violate the assumptions of my model. Because of this, I restrict my definition of search success to only include searches due to probable cause or driver consent.

4.2 Trooper Employment Data

The employment data is from the Texas Department of Public Safety, which I obtained using a Freedom of Information Act (FOIA). Unfortunately, DPS only has this information for employees after 2013. If a trooper left DPS prior to 2013, I do not have his or her employment information. For troopers in the data, I have the year the trooper was hired, if he or she left the position and why, the salary for each year, which work city he or she was stationed at, the work position for each year, race/ethnicity of the trooper, the full name of the trooper, and the badge number. I have approximately 2,789 unique troopers of which I can match 2,578 to the stop data.

I merge the stop data to the trooper data together using the badge number of the trooper. I can match all but 10% of the stop data to the trooper so I only have 11,819,236 observations. My final number of observations is 7,685,007 after dropping observations after June 2015 for reasons listed in the prior section.

[8] The SOPP data is originally from the TX DPS. I have verified that the data is the same for overlapping years.

[9] I test this assumption in a later section.

I further the time period of my trooper employment data by adding 2019 trooper employment data, which is publicly available on the Texas Tribune Salary website. I link the 2019 employment data to my trooper data using the full name of the trooper. I include this data as a measure of a trooper's long-term employment outcomes.

I also include trooper complaint data from 2010 to 2015, which I obtained using a FOIA, as an objective measure of trooper quality. The complaint data contains information on the date the incident occurred, the date the complaint was received, the allegation of the complaint, the trooper's badge number (if applicable), and the investigator of the complaint. The badge number is not always included due to Texas' privacy laws. Specifically, "Employee names and ID numbers are not releasable unless the complaint resulted in disciplinary action such as discharge, suspension, or demotion (Government Code 411.00755)." Out of the original 1,873 complaints, only 334 had the trooper's badge number in the complaint.

4.3 Race Estimation

I use two main methods supported by past literature on using observable characteristics to determine race. These methods are predominantly used in social science and health research to infer patient race (Fiscella and Fremont, 2006; Freedman, Owens, and Bohn, 2018). The first method is to use surname analysis, which works well for Hispanic and Asian surnames. I match the driver surnames in my data to the U.S. Census Surnames data set. If the probability of the last name is Hispanic is greater than a certain threshold (75%), I impute the 'estimated' race as Hispanic.[10] For example, Figure III shows an actual ticket from a stop. The driver, with last name Mendez, is pulled over for speeding by Officer Salinas and is recorded as a White, male driver. Since, the probability this driver is Hispanic, conditional on his last name, Mendez, is 92%, I estimate his actual race to be Hispanic. The advantage of this method is that the correction is fairly quick and simple. But, the main drawback is that this method results in errors in the race estimation. However, even if the race estimate is incorrect, as long as the errors are independent of the search outcome, my measure of bias will be unaffected. Another minor drawback is that surname analysis is only suitable for Asian and Hispanic names and is less effective with females since married women have a higher likelihood of changing their last names. Thus, I only keep male drivers in my sample.

The second method I employ is geocoding analysis, which I only use on to uncover Black drivers "because at least half of Black Americans continue to live in predominantly

[10] As a robustness check, I raise the threshold to higher levels in later sections.

Black neighborhoods (Fiscella and Fremont, 2006; Glaeser and Vigdor, 2001)." I map the home address of the driver to a Block FIPS code using geocoder.us and the FCC block finder.[11] Using the 2010 American Community Survey, I estimate a driver as black if the percentage of Black population in the Block FIPS of the home address is greater than a certain threshold (67%).[12] This method also has a few disadvantages. First, it is impossible to geocode without a recorded address, which occurs in half of the stops. Second, the address is recorded by the trooper, which is prone to spelling and typing errors. For example, I found 116 different spellings of the city "Houston," the most populous city in Texas. Third, this method is also computationally expensive so I restrict this analysis to only drivers who live in Texas, which is approximately 90% of the stop data.[13]

Lastly, I show the how the misreporting affected the observed stop rates of motorists by race by comparing stop rates before and after the DPS rule change. Figure II shows the raw, time trend of the stop rate by driver race using the recorded races for Asian, Black, Hispanic, and White motorists from 2010 to 2017.[14] The vertical line denotes year-month of the publication of the article and DPS' rule change. After November 2015, the Hispanic stop rate rises to over 40% while the White stop rate falls to nearly equal levels. I observe no noticeable changes for Black motorist stop rate before and after November 2015. Since only Hispanic motorists have enough observations that are misreported, I focus the rest of my analysis on bias on Hispanic motorists.

4.4 Descriptive Statistics

I present summary statistics of motorist characteristics in Table I using the estimated races. On average, I find that Hispanics are over-represented in searches when compared to the stop rate. Specifically, conditional on being stopped, Hispanics motorists are searched the most at nearly 40% followed by White motorists at nearly 39%. Black

[11] I first geocode the address to a latitude and longitude and then use the latitue and longitude to map to a Block FIPS code.

[12] I use 67% since Fiscella and Fremont (2006) found that with "block groups where more than two-thirds of the residents were Black... 89 percent were classified correctly." I also raise this threshold later as a robustness check.

[13] Note: I find no significant difference in the order of race estimation (geocoding analysis first or surname analysis first) since there is very little overlap between driver's with a Hispanic surname living in a predominantly Black neighborhood who were recorded as White in the data.

[14] I exclude stops that occurred from July 2015–November 2015. In July 2015, Sandra Bland was stopped by a Texas highway patrol officer and died in jail as a result of the stop. During the time period after her stop and the publication of the article, Texas troopers appeared to significantly increase their misreporting behavior. Since I also cannot discern the reason for this change in behavior, I only use stops from January 2010 to June 2015 for the rest of my analysis.

motorists also show a higher search rate compared to stop rate with a difference of 9.5%. The only non-White race of motorist that is stopped at a higher rate than the search rate are Asian motorists. I also find that certain stop characteristics, such as midnight stop, an older car and a luxury brand cars have a higher search rate compared to the stop rate.

Table II shows summary statistics of troopers. Of the 2,701 troopers I was able to match to the data, approximately 60% are White, 30% are Hispanic, and almost 9% are Black. The last one percent is composed of Asian, American Indian, and other race troopers. The force is predominantly male at 94%. By trooper race, I find that White troopers make up most of the searches at 64%, followed by Hispanic troopers at 21%. I find that only White troopers search at a higher rate compared to the stop rate while Black and Hispanic troopers search at a lower rate. I also find that troopers less experienced troopers searched more than more experienced troopers since the average hire year for searches was greater than the average hire year for stops.

In the bottom part of the table, I break down the stop and search statistics by trooper position, with rank listed in decreasing order.[15] Ranked officers make up only 20% of the highway patrol. I find as rank increases, troopers are less likely to search. Using the rank of Captain as an example, the interpretation of the probabilities is "If the trooper is a captain, then captains conduct 0% of total searches." I find that troopers make up most of the stops and searches at 70% and 72% respectively.

5 Empirical Results

5.1 Test for racial prejudice

From Eq. (5), troopers' decision to misreport the motorists' race as White will vary by search outcome. Specifically, misreporting is only profitable for biased troopers when the search ends in failure. But, a trooper's decision to purposefully misreport in a search is unobservable for the following reasons: 1) driver's true race is unobservable; 2) trooper's may misreport driver's for reasons aside from hiding bias (poor visibility, driver may self-identify as another race). Following the intuition from the model of racial bias developed in the prior section to circumvent this issue, I rely on the differential misreporting behavior across search outcome to measure bias. The identifying assumption is that biased troopers are more likely to misreport their failed searches. Thus, any driver race mis-identification from any reason aside from troopers' bias will occur at equal rates across search failure

[15] I excluded the rank of major as only two troopers were majors and they conducted no searches and only four stops during 2010 to 2015.

or search success.

To estimate trooper level bias, I focus the rest of the analysis on Hispanic motorists as I do not have a sufficient number of searches for Asian or Black drivers to identify misreporting at the individual trooper level. Specifically, I have nearly 60,000 searches with Hispanic motorists and only 29,000 and 1,500 searches with Black or Asian motorists respectively.

To measure the magnitude of Hispanic bias for each officer, I allow for each trooper to have his own misreporting rate depending on the search outcome. For every stop i by trooper$j$ at time t:

$$I(Mismatch_{i,c,j,t}) = \beta_0 + \beta_1^j I(Failure_{i,c,j,t}) + \delta_j + X_{i,c}\gamma + \alpha_t + \epsilon_{i,c,j,t} \tag{6}$$

β_1^j measures officer j's differential misreporting behavior based on search outcome. A positive estimate indicates that trooper j is more likely to have mismatch between the observed and estimated race when the search ends in failure, which implies bias against Hispanics.[16] I include controls for county characteristics in $X_{i,c}$. These characteristics include median income, percentage Hispanic, percentage Black, employment rate, percentage with high school diploma, and population size.[17] I also include month specific fixed effects.

From prior work using these value-added models (see Daniel Aaronson, Barrow, and Sanders (2007); Goncalves and Mello (2021); Koedel, Mihaly, and Rockoff (2015), the distribution of $\hat{\beta}_1^j$ will have a higher variance relative to the true distribution due to estimation error. Compounding on this, the few number of searches the trooper-level estimate of bias introduces potential measurement error, further attenuating the estimates. To correct for this, I follow the Bayes shrinkage procedure from Daniel Aaronson et al. (2007); Goncalves and Mello (2021) to estimate the distribution of bias accounting for the estimation error in each $\hat{\beta}_1^j$.

Figure IV shows the kernel density plot of raw bias estimates (solid line) plotted with the shrunken estimates of bias from Daniel Aaronson et al. (2007) (dashed line). The further right the trooper is in the distribution of bias, the higher his level of bias. The measurement of bias is the difference in likelihood of misreporting between search failure and search success. For example, a trooper with bias of 0.5 if 50 percentage points more

[16] Since troopers with more searches will have a more precise estimate of bias than troopers with few searches, I exclude troopers with less than 5 searches of Hispanic or White motorists using the observed races.

[17] I use county fixed effects and have similar results, but less troopers since some troopers only search within a single county.

likely to misreport his failed searches of estimated Hispanic motorists compared to his successful searches.

The symmetry and the significant shrinkage of the distribution show that the estimates of bias are noisy. While we estimate an average 0.02 percentage point higher likelihood of misreporting when search ends in failure compared to success, this is small given the reported scale of the misreporting. Given the high shrinkage, the imprecision of the race prediction is potentially biasing the results towards zero.

Fortunately, the discovery of the trooper misreporting behavior and the subsequent rule change will provide a robust way of testing the estimated measure of bias from Equation (6).

5.2 Testing validity of bias measure using race reporting rule change

The publication of the news article by KXAN revealing the misreporting sparked a rule change in DPS that required troopers to always verbally ask drivers for their race when recording stop information. Thus, troopers who were correctly reporting race prior to the rule change will likely be less affected compared to troopers who were purposefully misreporting race.

The rule went into effect in November 2015, but I consider stops after July 2015 as post-rule change data since DPS was already under investigation beginning in July 2015 after Sandra Bland's death in DPS custody. Since the SOPP version of the highway stop data ends in 2015, I augment my data using the publicly available data, which has the recorded driver's races but no driver's names or addresses.[18] If β_i^j is a valid measure of bias, then troopers who were using misreporting to hide their bias will respond the most to the rule change. Specifically, troopers with estimated high rates of bias will be more likely to report their failed searches as White compared to their un-biased peers.

To test this, I use the following specification. For every stop i by trooper j in county

[18]The SOPP data originated from DPS but contains restricted, personally identifiable information, that is not available in the public version on their website. I have verified that both data sets are the same aside from the publicly available data lacking the PII of driver's.

c at time t:

$$
\begin{aligned}
I(RecRace = White_{i,c,j,t}) = &\alpha + \sum_{t=2010}^{2017.5} \Big[\beta_1^t Hisp\ Bias_j \times I(Year\ Half = t) + \\
& \beta_2^t I(Failure_{i,c,j,t}) \times I(Year\ Half = t) \\
& + \beta_3^t Hisp\ Bias_j \times I(Year\ Half = t) \times I(Failure_{i,c,j,t}) \Big] + \\
& \boldsymbol{X}_{i,c,t}\gamma + \theta_{c,t} + \phi_c + \alpha_m + \epsilon_{i,c,j,t}
\end{aligned} \tag{7}
$$

where $Hisp\ Bias_j$ is the standardized estimates of β_i^j derived from Equation (6). $I(Failure_{i,c,j,t})$ is an indicator variable if stop i ends in failure.[19] I also control for county, year, and month fixed effects along with county specific time trends. The primary coefficient of interest is β_3^t for $t > 2015$, which is the triple interaction between officer j's estimate of bias measured using stops before 2015, the search outcome, and the years after the changes were implemented. β_3^t will reflect the differential probability in being recorded as White when the search ends in failure for biased officers.

Figure V shows the results for motorists recorded as Hispanic and motorists recorded as White. Specifically, a standard deviation away from the mean level of bias leads to a 7-12 percentage point increase in the probability of being recorded White when the search ends in failure from 2010 to the June 2015. For Hispanic motorists, I find the opposite pattern. Motorists are 6-12 percentage points less likely to be recorded as Hispanic if the search ended in failure.

The pattern of the estimates over time reveals three interesting behaviors. The first is that officers with higher levels of bias are more likely to report their failed searches as White prior to the rule change. The pattern of coefficients supports the interpretation that $Hisp\ Bias_j$ is a valid measure of bias.

The second is that the differences in race reporting behavior across search outcome between biased troopers and unbiased troopers disappears after the rule change with β_3^t going to zero for $t > 2015$. This implies that biased troopers complied to the rule change and began correctly reporting motorists race after 2015 at rates similar to that of their unbiased peers. While DPS was not clear as to how they would enforce their new policy, it appears effective in changing the biased troopers' race reporting behavior.

[19] This robustness test uses a different specification than with Equation (6) since the publicly available data doesn't contain driver's names or driver's home addresses. But, even without the full names and addresses, DPS' rule change to require troopers to verbally ask for driver's race, thus the recorded race should be the estimated race after November 2015.

Lastly, the misreporting was concentrated on motorists who self-identified as Hispanic. This is important since Hispanic is technically an ethnicity and many driver's may self-identify their race as White rather than as Hispanic despite having a Hispanic last name (Lopez, Gonzalez-Barrera, and López, 2017). If, on the other hand, troopers appeared to misreport Hispanic motorists because the motorists self-identified as White while the data estimated the race to be Hispanic, I would observe no change in behavior as a result of the rule change. Thus, troopers would not have been misreporting in biased manner.

Overall, the pattern in Figure V show that trooper's used misreporting to make their search success rate for Hispanic motorists appear higher. This enabled biased troopers to escape detection by appearing less biased. The newly instituted race recording rule change in 2015 appears to have curbed this behavior such that biased troopers race reporting pattern conditional on search outcome was similar to their unbiased peers.

5.3 Robustness Checks

To ensure that the relationship between my measure of bias is not dependent on my census surname cutoff. I vary the threshold I use in the surname analysis at 50%, 75% (the measure I use throughout my analysis), and 85%, and re-estimate my trooper level measure of bias.[20] Figure **??** shows the distribution for each cutoff. These distributions show a similar shape; it also shows that even with tighter thresholds, the distribution of officer level bias is robust.

Since my main estimation of Hispanic motorists relies on the distribution of Hispanic surnames in the United States from the 2000 census, I test if my results are driven by the unequal distribution of last names across search outcome. I randomly assign the probability a last name is Hispanic based off a normal distribution with mean 0.5 and standard deviation of 0.25. Using different Z-score cutoffs, I re-estimate bias using Equation (**??**). I repeat this procedure a thousand times to get the distribution of average bias in Figure VI. The average level of bias is -0.04, which is far less than my estimate in Table **??**. In fact, that natural distribution implies troopers on average are *less* likely to misreport unsuccessful searches by 4 percentage points. Thus, I can reject the null hypothesis that my results are merely driven by the distribution of last names.

[20] I also use 95% as a threshold, but at 95%, there are only 1,242 Hispanic surnames compared to the 4,647 surnames at 85%. Most of the bias estimates were concentrated at no bias, making the other densities hard to see on the graph.

5.4 Bias and Trooper Characteristics

One contribution of this paper is to be able to generate trooper level estimates of discrimination and to identify effects of bias on labor outcomes. In this section, I will address how discrimination varies with other employment characteristics such as promotions, salary, and officer transfers. I will also test how troopers' employment outcomes were affected by the change in driver race identification method in 2015.

Next, I test if employment outcomes, such as salary and experience, are correlated to bias where experience is measured using the hire year of the trooper. Table III shows a positive correlations of trooper salary and experience to Hispanic bias, but these results are not significant. For trooper rank, I find that troopers with rank probationary troopers are 0.2 standard deviations less biased than troopers. No other rank has significant different levels of bias compared to troopers.

Column (4) reports the average differences in bias across trooper race. I find no significant differences in average bias across race, which is contrary to past tests of bias (Goncalves and Mello, 2021; Antonovics and Knight, 2009). One explanation of this could be that Hispanic troopers are better at visually determining whether motorists are Hispanic or not compared to their white peers. Thus, since my estimate of bias is measured using misreporting, bias estimates for troopers better at race identification will be less attenuated.

To examine the effect of trooper bias on the trooper's career across time, I divide the trooper's career into two sections: pre-2013, and 2014–2015. This has a few advantages; first, DPS does not have trooper employment data available prior to 2013 so 2013 is the earliest possible year. Second, combining years increases the number of searches used to measure bias, which increases the precision of the estimate. Lastly, with the panel-like structure, I can test if changes in employment outcomes are related to bias, specifically outcomes such as increasing in rank, moving cities, and leaving the force. If bias in 2013 has no effect on employment outcomes from 2014 to 2015, this could imply that misreporting is effective in making biased troopers appear unbiased thereby avoiding punishment of bias.

This is indeed what I find, as shown in Table IV. Specifically, I find that bias measured using stops from 2010–2013 has no significant effect on employment outcomes from 2014 to 2015. Not only are the point estimates insignificant with large standard errors, but the estimates are also close to zero indicating a null effect. I interpret this as evidence that misreporting allows biased officers to evade punishment for bias since I observe no difference in the likelihood of leaving the force, increasing in rank, or changes in salary with respect to the trooper's level of bias.

I next test if biased troopers also perform worst in other aspects of their job by using complaint data obtained from DPS. While misreporting may help troopers evade negative employment outcomes, drivers may find cause to report the trooper. The results in Table V show a positive relationship between trooper level bias and the probability of receiving a complaint. One standard deviation of bias is associated with a one percentage point higher likelihood in having a complaint filed against the trooper (relative to mean of 5.4%). This estimate is likely an underestimate of the actual association of bias and complaints since unsustained or unfounded complaints repressed the trooper's badge number. From the 1,873 complaints, only 334 included the trooper's badge number.

Lastly, I test to see how the employment outcomes of troopers were affected by the publication of the article relative to their level of bias. I use publicly available 2019 salary data published by the Texas Tribune. My results in Table VI show that bias had no effect on the probability of leaving the force by 2019. However, bias is negatively correlated with salary growth and rank promotion. Specifically, each standard deviation of bias is associated with a decreased likelihood of increasing in rank by 16 percent and lower salary growth of 7 percent. This provides suggestive evidence that without misreporting, biased troopers had worse employment outcomes and that employers may negatively

6 Conclusion

Recent events have highlighted the disparate treatment by race in the criminal justice system by law enforcement officers. In this paper, I show how these officers misreport the race of Hispanic drivers in potentially racially biased searches in order to appear less biased. Crucially, the findings of the paper bring into question outcome based tests, notably by Knowles et al. (2001) and Anwar and Fang (2006) that are at risk of manipulation by law enforcement officers.

Using a statistical model of racial bias that accounts misreporting, I show that biased troopers have incentive to misreport their searches to appear less biased. Further, this misreporting can be used to measure racial bias.

Leveraging a documented event in Texas where troopers were caught misreporting Hispanic drivers' race as White in 2015, I show how a portion of troopers were using misreporting to appear less biased. Using the misreporting rule change in 2015 as a natural experiment and as a robustness check for my estimates of racial bias, I confirm that the rule was effective in curbing misreporting behavior and that biased troopers were indeed more likely to record their failed, Hispanic searches as White when compared to their unbiased counterparts. Overall, one standard deviation of estimated racial bias was associated with a 7 to 11% higher likelihood of recording a failed search as White.

Using my estimates of racial bias, I also test whether bias is correlated to labor outcomes for troopers before and after a rule change in motorists' race recording policy in response to the misreporting. I find that prior to the rule change, troopers' racial bias did not affect their labor outcomes. I also find that biased troopers were objectively worse troopers with one standard deviation of bias correlated to an 18% higher likelihood of receiving a complaint. But after the rule change, one standard deviation in bias led to a 27% decrease in the likelihood of increasing in rank and salary growth also fell by 8%. Thus, I conclude that misreporting was an effective way of evading detection and punishment for racial bias. I also find that this rule change was highly cost effective in reducing misreporting.

While my paper is the first to find a relationship between misreporting and racial bias, the geographic scope of this paper is limited and further study of misreporting in other levels of policing and varying geographic contexts will require further study. Inputs such as trooper peers and supervisors, can explain the distribution of trooper behavior and raise important policy implications, which are beyond the scope of this paper. Lastly, evidence for what other factors, aside from punishment, may induce misreporting are important for future policies and research.

7 Appendix

7.1 Discussion of the Model

Proof of Proposition 1

Suppose some motorist, z, with characteristics (M, θ) with $\theta_z > \theta^{\mu}_{M,t}$ is pulled over by trooper t. Then this implies

$$c_{M,t} + \mu_{M,t}(\theta_z) > c_{W,t}$$

Therefore, the trooper will not misreport motorists with $\theta > \theta^{\mu}$, regardless of the search outcome, G. If $G = 1$, under Assumption 3, $\mu_{\theta,G=1} > 1$. This implies

$$c_{M,t} + \mu_{M,t}(\theta, G = 1) > c_{W,t}$$

Therefore the trooper will not misreport motorists if the search ends in success ($G = 1$) regardless of the characteristics, (M, θ), of the motorist.

For motorists with $\theta < \theta^*$, this is not sufficient for search, therefore the trooper will also never misreport motorists with $\theta < \theta^*$.

Proof of Proposition 2

Suppose trooper i and trooper j are biased against race M motorist, but trooper i is more biased such that $c_{M,i} < c_{M,j}, c_{W,i} = c_{W,j}$, and $c_{M,t} < c_{W,t}$ for $t \in \{i, j\}$. Since both troopers face the same population of race-M motorist and race-W motorist, then this implies that $\theta^{\mu}_{M,i} > \theta^{\mu}_{M,j}$, $\theta^*_{M,i} < \theta^*_{M,j}$, and $\theta^*_{W,i} = \theta^*_{W,j}$. From Assumptions 1, 2, and 3, this implies that:

$$\begin{aligned}
&\Rightarrow \theta^{\mu}_{M,i} - \theta^*_{M,i} > \theta^{\mu}_{M,j} - \theta^*_{M,j} \\
&\Rightarrow (1 - \pi_M)[F^M_n(\theta^{\mu}_{M,i}) - F^M_n(\theta^*_{M,i})] > (1 - \pi_M)[F^M_n(\theta^{\mu}_{M,j} - \theta^*_{M,j})] \\
&\Rightarrow v_{M,i} > v_{M,j} > 0
\end{aligned}$$

Thus, since trooper i is more biased than trooper j, trooper i also misreports a higher portion of race M searches than trooper j.

Relaxing Assumption 3

While Assumption 3 is fairly intuitive, specifically that misreporting is only profitable when the search ends in failure, it is not a necessary condition for using misreporting as a measure of bias. From Assumption 1 and 2, the average, misreporting rate for trooper t is:

$$\phi_{M,t} = \frac{\pi_M[F_g^M(\theta^{\mu}_{M,t}) - F_G^M(\theta^*_{M,t})] + (1-\pi_M)[F_n^M(\theta^{\mu}_{M,t}) - F_n^M(\theta^*_{M,t})]}{\pi_M[1 - F_g^M(\theta^*_{M,t})] + (1-\pi_M)(1 - F_n^M(\theta^*_{M,t})]} \tag{8}$$

Proposition 3. *From Assumptions 1 and 2, if a trooper exhibits racial bias against race M motorists, then $\phi_{M,t} > 0$.*

Further,

Corollary 1. *The misreporting rate, $\phi_{M,t}$, is the magnitude of bias against race M motorist.*

The proof is in the section below.

Since the distributions, f_g^m and f_n^m, and the true proportion of guilty motorists, π_m are unobservable, the misreporting rate cannot be directly measured. Instead, the misreporting rate can be derived from the observed, average search rate and the true, average search rate. The difference between the observed and true search rate vary depending on whether the trooper is racially biased or not.

The true, average search rate $\gamma_{m,t}$ for race m motorists is as follows:

$$\gamma_{m,t} = \pi_m[1 - F_g^m(\theta^*_{m,t})] + (1-\pi_m)[1 - F_n^m(\theta^*_{m,t})] \tag{9}$$

Let $\gamma^O_{m,t}$ denote trooper t's observed, average search rate of race m motorist.

From Proposition 1, only a portion of race M motorists are misreported, specifically, unsuccessful searches of race M motorists of $\theta \in (\theta^*, \theta^{\mu})$ if the trooper is biased. Thus the observed search rate, composed of the correctly recorded race M motorists, is:

$$\gamma^O_{M,t} = \pi_M[1 - F_g^M(\theta^{\mu}_{M,t})] + (1-\pi_M)[1 - F_n^M(\theta^{\mu}_{M,t})] \tag{10}$$

Since motorists of characteristics (M, θ) where $\theta \in (\theta^*, \theta^{\mu})$ are misreported, the observed search rate for race M motorists is lower than the true search rate for race M motorists.

Misreporting also affects the search rate for race W motorists. The inclusion of race M motorists miscategorized as race W will affect the search rate for race W motorists in the following way:

$$\begin{aligned} \gamma^O_{W,t} &= \pi_W[1 - F_g^W(\theta^*_{W,t})] + (1-\pi_W)(1 - F_n^W(\theta^*_{W,t})] \\ &+\pi_M[F_g^M(\theta^{\mu}_{M,t}) - F_G^M(\theta^*_{M,t})] + (1-\pi_M)[F_n^M(\theta^{\mu}_{M,t}) - F_n^M(\theta^*_{M,t})] \end{aligned} \tag{11}$$

Therefore, the misreporting rate, $\phi_{M,t}$, can be rewritten in terms of the observed search rates:

$$\phi_{M,t} = \frac{\gamma^{O}_{W,t} - \gamma_{W,t}}{\gamma_{M,t}} \tag{12}$$

While this test and measure of racial bias relies on fewer assumptions, it requires knowing the true search rate which is often unobservable. Thus, I will include my results using this measure of racial bias once I better my race estimation methods.

Proof of Proposition 2 and Corollary 3

The magnitude of this misreporting rate also yields a measure of bias. For example, suppose trooper i and trooper j are biased against race M motorist, but trooper i is more biased such that $c_{M,i} < c_{M,j}, c_{W,i} = c_{W,j}$, and $c_{M,t} < c_{W,t}$ for $t \in \{i, j\}$. Since both troopers face the same population of race-M motorist and race-W motorist, then this implies that $\theta^{\mu}_{M,i} > \theta^{\mu}_{M,j}$ and $\theta^{*}_{M,i} < \theta^{*}_{M,j}$. From Proposition 1, this implies that:

$$\Rightarrow \theta^{\mu}_{M,i} - \theta^{*}_{M,i} > \theta^{\mu}_{M,j} - \theta^{*}_{M,j}$$
$$\Rightarrow \phi_{M,i} > \phi_{M,j}$$

Thus, since trooper i is more biased than trooper j, trooper i also misreports a higher portion of race M searches than trooper j.

References

Alesina, A. and E. L. Ferrara (2014). A test of racial bias in capital sentencing. *American Economic Review 104*(11), 3397–3433.

Anbarci, N. and J. Lee (2014). Detecting racial bias in speed discounting: evidence from speeding tickets in Boston. *International Review of Law and Economics 38*, 11–24.

Antonovics, K. and B. G. Knight (2009). A new look at racial profiling: evidence from the Boston police department. *The Review of Economics and Statistics 91*(1), 163–177.

Anwar, S. and H. Fang (2006). An alternative test of racial prejudice in motor vehicle searches: theory and evidence. *American Economic Review 96*(1), 127–151.

Anwar, S. and H. Fang (2015). Testing for racial prejudice in the parole board release process: theory and evidence. *Journal of Legal Studies 44*(1).

Arnold, D., W. Dobbie, and C. S. Yang (2018). Racial bias in bail decisions. *Quarterly Journal of Economics 133*(4), 1017–1055.

Collister, B. (2015a). DPS troopers getting race right after KXAN investigation. *KXAN*. March 1. https://www.kxan.com/news/dps-troopers-getting-race-right-after-kxan-investigation/.

Collister, B. (2015b). Texas troopers ticketing Hispanic drivers as white. *KXAN*. July 21. https://www.kxan.com/investigations/texas-troopers-ticketing-hispanic-drivers-as-white/.

Coviello, D. and N. Persico (2013). An economic analysis of black-white disparities in NYPD's stop and frisk program. Working Paper 18803, NBER, https://www.nber.org/papers/w18803.

Daniel Aaronson, L. Barrow, and W. Sanders (2007). Teachers and student achievement in the chicago public high schools. *Journal of Labor Economics 25*, 95–135.

Dee, T. S., W. Dobbie, B. A. Jacob, and J. Rockoff (2019). The causes and consequences of test score manipulation: evidence from New York Regents Examination. *American Economic Journal: Applied Economics 11*(3), 382–423.

Depew, B., O. Eren, and N. Mocan (2017). Judges, juveniles, and in-group bias. *Journal of Law and Economics 60*(2).

DPS. *When stopped by law enforcement.* Texas DPS.

Fiscella, K. and A. M. Fremont (2006). Use of geocoding and surname analysis to estimate race and ethnicity. *HSR: Health Services Research 41*(4), 1482–1500.

Freedman, M., E. Owens, and S. Bohn (2018). Immigration, employment opportunities, and criminal behavior. *American Economic Journal: Economic Policy 10*(2), 117–151.

Glaeser, E. L. and J. L. Vigdor (2001). Racial segregation in the Census 2000: Promising news. *Brookings Institute Center on Urban & Metropolitan Policy*.

Goncalves, F. and S. Mello (2021). A few bad apples? racial bias in policing. *American Economic Review 111*(5), 1406–41.

Grogger, J. and G. Ridgeway (2006). Testing for racial profiling in traffic stops from behind a veil of darkness. *101*(475), 878–887.

Horrace, W. C. and S. Rohlin (2016). How dark is dark? Bright lights, big city, racial profiling. *The Review of Economics and Statistics 98*(2), 226–232.

Jacob, B. A. and S. D. Levitt (2003). Rotten apples: an investigation of the prevalence and predictors of teacher cheating. *Quarterly Journal of Economics 118*(3), 843–877.

Kalinowski, J., S. L. Ross, and M. B. Ross (2017). Endogenous driving behavior in veil of darkness tests for racial profiling. Working Papers 2017-017, Human Capital and Economic Opportunity Working Group, https://ideas.repec.org/p/hka/wpaper/2017-017.html.

Knowles, J., N. Persico, and P. Todd (2001). Racial bias in motor vehicle searches: theory and evidence. *Journal of Political Economy 109*(1), 203–229.

Koedel, C., K. Mihaly, and J. E. Rockoff (2015). Value-added modeling: A review. *Economics of Education Review 47*, 180–195.

Lopez, M. H., A. Gonzalez-Barrera, and G. López (2017). Hispanic identity fades across generations as immigrant connections fall away. *Pew Research Center*.

Oyeniyi, D. (2015). State troopers will now just ask drivers their race. *Texas Monthly*. November 23. https://www.texasmonthly.com/the-daily-post/state-troopers-will-now-just-ask-drivers-their-race/.

Persico, N. and P. E. Todd (2005). Passenger profiling, imperfect screening, and airport security. *American Economic Association Papers and Proceedings 95*(2), 127–131.

Pierson, E., C. Simoiu, J. Overgoor, S. Corbett-Davies, V. Ramachandran, C. Phillips, and S. Goel (2020). A large scale analysis of racial disparities in police stops across the United States. *Nature Human Behavior 4*.

Sanchez, R. (2015). Who was Sandra Bland? *CNN*. July 23. https://www.cnn.com/2015/07/22/us/sandra-bland/index.html.

Shayo, M. and A. Zussman (2011). Judicial ingroup bias in the shadow of terrorism. *Quarterly Journal of Economics 126*(3), 1447–1484.

TxDPS (2018). Complaint investigation and resolution.

West, J. (2018). Racial bias in police investigations.

Tables

TABLE I. Mean of Variables Related to Drivers

Driver Characteristics	All	Searches Only	Δ
Reported Black	.108	.201	-.093
	(.311)	(.401)	(.001)
Reported Hispanic	.341	.402	-.061
	(.474)	(.49)	(.001)
Reported White	.549	.396	.153
	(.498)	(.489)	(.001)
Midnight	.088	.131	-.043
	(.283)	(.337)	(.001)
Owner is Driver	.201	.142	.059
	(.401)	(.35)	(.001)
In-state Driver	.896	.848	.048
	(.305)	(.359)	(.001)
Car model <2000	.307	.427	-.12
	(.461)	(.495)	(.001)
Car model > 2005	.331	.177	.154
	(.471)	(.382)	(.001)
Luxury Car	.083	.101	-.018
	(.276)	(.301)	(.001)
Observations	7,244,972	145,726	

Standard deviations are in parentheses. Unweighted means are shown.

TABLE II. Mean of Variables Related to Troopers

Troopers' Characteristics	(1) All Stops	(2) Searches Only
Black	.087	.04
	(.282)	(.207)
Hispanic	.287	.205
	(.453)	(.404)
White	.606	.633
	(.489)	(.482)
Male	.946	.979
	(.226)	(.142)
Hire Year	2004	2006
	(7.244)	(4.676)
Trooper Rank		
Captain	.007	0
	(.084)	(.018)
Lieutenant	.023	.004
	(.15)	(.059)
Sergeant	.125	.063
	(.33)	(.242
Corporal	.1	.104
	(.3)	(.305)
Trooper	.697	.723
	(.46)	(.447)
Probationary Trooper	.018	.004
	(.133)	(.063)
No Rank	.031	.102
	(.173)	(.303)
Average Total Searches (2010-2015)		211.5161
		(216.746)
Total Troopers	2,701	

Notes: Only merged observations are shown. Trooper rank uses the highest rank the trooper obtained during 2010 - 2015. Stops from July 2015 to December 2015 were omitted. Stops are considered mismatched if the recorded race does not equal the corrected race. 10.5% of the troopers in the employment data were not matched to the stop data. 22% of the troopers in the stop data were not matched to the trooper employment data.

TABLE III. Correlates of Hispanic Bias

	(1)	(2)	(3)
	Dependent Variable: Hispanic Bias		
Experience	−0.003		
	(0.008)		
Salary	0.033		
	(0.058)		
Prob. Troop		−0.609**	
		(0.313)	
Corporal		−0.095	
		(0.077)	
Sergeant		−0.067	
		(0.098)	
Lieutenant or higher		−0.353	
		(0.339)	
Black			−0.013
			(0.118)
Hispanic			0.038
			(0.058)
Constant	−0.136	.041	.002
Observations	1,446	1,446	1,446

Notes: Regression includes controls for the work city and is clustered at the work city level. Troopers with negative levels of bias are omitted from the regression. Dependent variable is the officer level measure of bias from Equation (6). Troopers with rank equal to or higher than sergeant (lieutenant, major, captain) were grouped into "Sergeant +". Salary is monthly salary measured in thousands of dollars. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

TABLE IV. Hispanic Bias on Labor Outcomes - Panel Results

	(1) I(Left Force)	(2) I(Moved Cities)	(3) I(Rank Up)
Hispanic Bias	0.0213	-0.0081	-0.0018
	(0.0147)	(0.0136)	(0.0158)
Prob. Troop	-0.1835	0.0867	0.8358^{***}
	(0.1219)	(0.1496)	(0.1053)
Corporal	0.0536	-0.0523	0.0275
	(0.0595)	(0.0426)	(0.0568)
Sergeant	-0.0726	0.0471	-0.1034^{*}
	(0.0583)	(0.0584)	(0.0605)
Lieutenant	-0.3086^{***}	-0.0023	0.0001
	(0.0391)	(0.0429)	(0.0462)
Black Trooper	0.0091	-0.0519	-0.1216^{*}
	(0.0821)	(0.0687)	(0.0722)
Hispanic Trooper	-0.0024	-0.0033	-0.0038
	(0.0468)	(0.0437)	(0.0451)
Observations	837	724	724

Notes: Troopers with negative levels of bias are omitted from the regression. Regression has robust standard errors. Dependent variable is the officer level measure of bias from Equation (6) using only stops from 2010 to 2013. Employment outcomes are from 2013 and 2014. Includes controls for the trooper's gender, trooper's rank in 2013, and trooper race. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

TABLE V. Hispanic Bias on Complaints

	(1) Complained	(2) Sustained
Hisp Bias	0.0103^{*}	0.011^{**}
	(0.005)	(0.005)
Constant	0.054^{***}	0.051^{***}
	(0.005)	(0.005)
Observations	1,728	1,728
R^2	0.001	0.001
F	1.931	1.996

Notes: Dependent variable is an indicator variable equal to one if bthe trooper had a sustained complaint from 2010 to 2015. Regression has robust standard errors. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$.

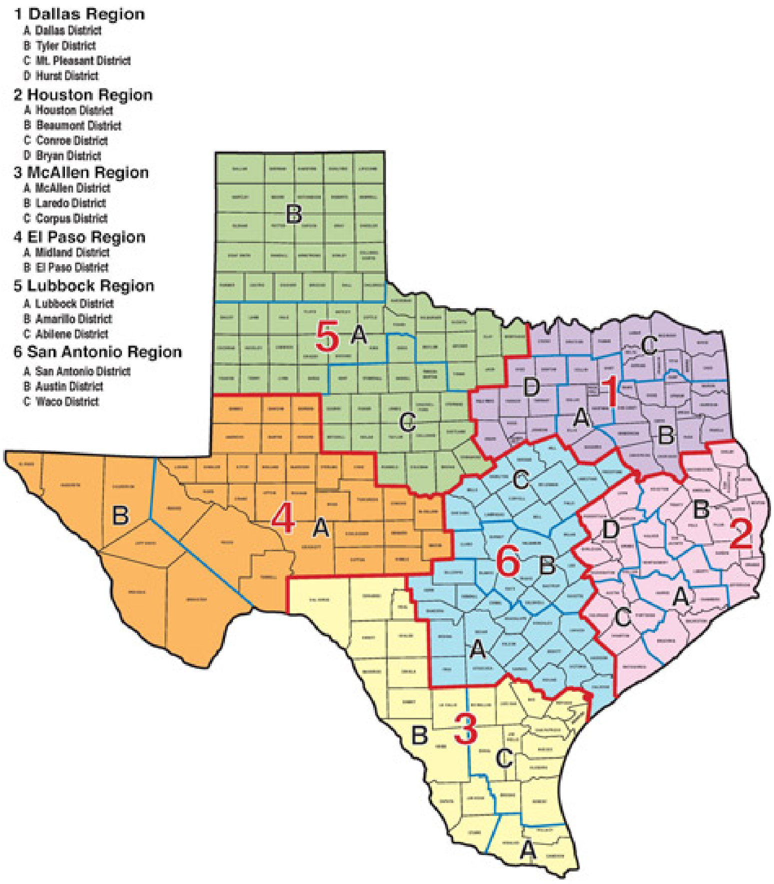
TABLE VI. Hispanic Bias on Labor Outcomes - after 2015

	(1) I(Left Force)	(2) Δ Salary	(3) I(Rank Up)
Hispanic Bias	0.0240	-0.035*	-0.036**
	(0.017)	(0.021)	(0.0182)
Constant	0.236***	0.529***	0.224
	(0.087)	(0.139)	(0.204)
Observations	766	594	594

Notes: Regression has robust standard errors show in parentheses and uses 2019 employment data posted publicly by the Texas Tribune. Includes controls for the trooper's gender, trooper's maximum rank from 2010 to 2015, and trooper race. Each trooper is weighted by their total number of searches conducted from January 2010 to June 2015. Black and Hispanic are indicator variables equal to one if the trooper is Black or Hispanic, respectively, and equal to one otherwise. * $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

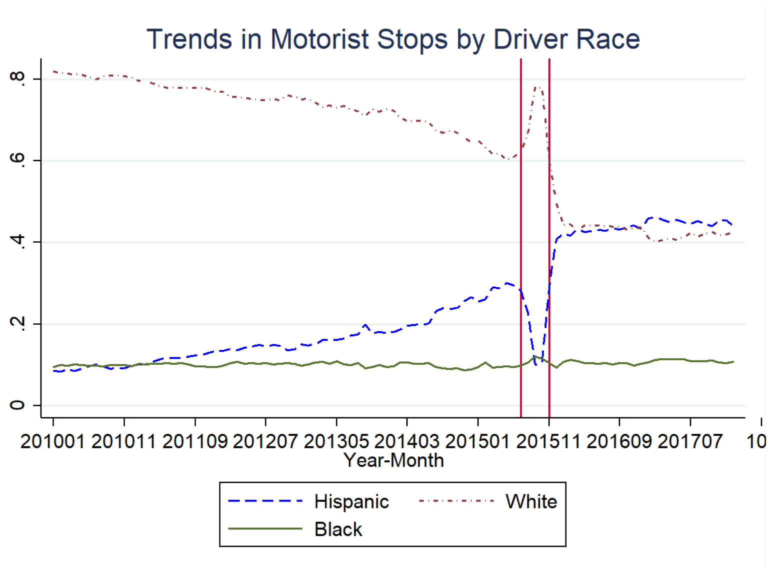
Figures

Figure I. Trooper Division Map



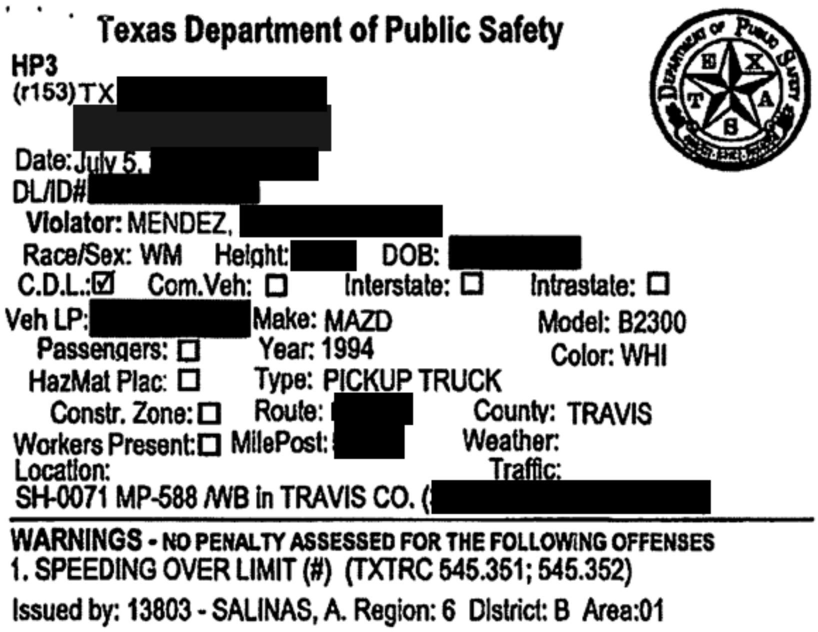
Notes: Source: Texas Department of Public Safety. The 7th region is not shown on the map, but its jurisidiction is limited to only Austin, TX.

Figure II. Monthly Stop Rate by Driver's Race using Recorded Races



Notes: Dot-dash line shows the recorded Asian stop rate, solid line shows the recorded Black motorist stop rate; dashed line shows the Hispanic motorist stop rate using the recorded races, and the dotted line shows the stop rate for White motorists using the recorded races. Average, unweighted stop rates for a given month-year from January 2010 to December 2017 are shown. The vertical red line indicates the year-month the article was released. The dashed red line indicates the year-month Sandra Bland died after a trooper stop.

Figure III. Example of misreported Highway Ticket



Texas Department of Public Safety

HP3

(r153)TX [redacted]

Date: July 5, [redacted]

DL/ID# [redacted]

Violator: MENDEZ, [redacted]

Race/Sex: WM Height: [redacted] DOB: [redacted]

C.D.L.: ☑ Com.Veh: ☐ Interstate: ☐ Intrastate: ☐

Veh LP: [redacted] Make: MAZD Model: B2300

Passengers: ☐ Year: 1994 Color: WHI

HazMat Plac: ☐ Type: PICKUP TRUCK

Constr. Zone: ☐ Route: [redacted] County: TRAVIS

Workers Present: ☐ MilePost: [redacted] Weather:

Location: Traffic:

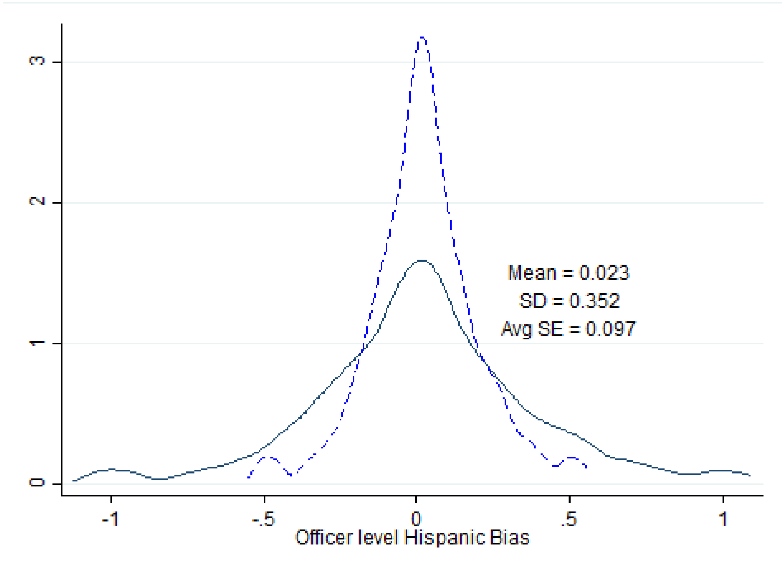
SH-0071 MP-588 /WB in TRAVIS CO. ([redacted]

WARNINGS - NO PENALTY ASSESSED FOR THE FOLLOWING OFFENSES

1. SPEEDING OVER LIMIT (#) (TXTRC 545.351; 545.352)

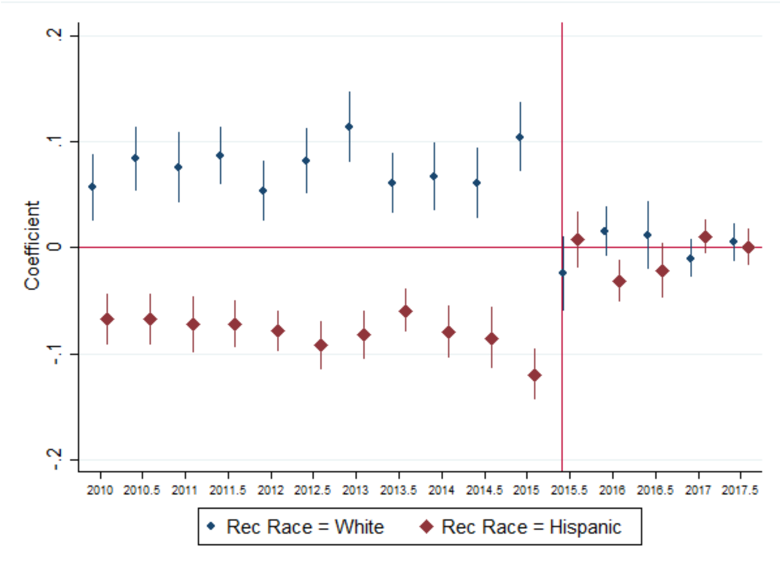
Issued by: 13803 - SALINAS, A. Region: 6 District: B Area:01

Figure IV. Distribution of officer level measure of Hispanic Bias



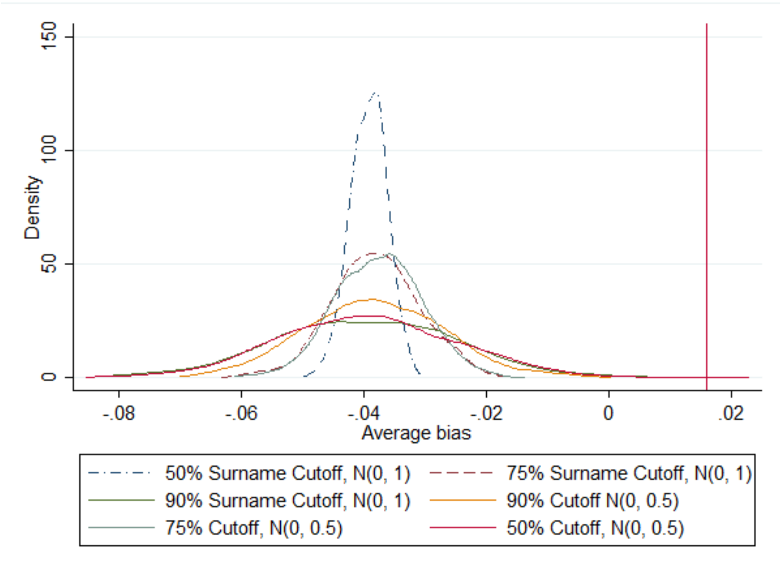
Notes: Kernel density distribution of officer-level Hispanic bias. The figure plots each officer's β^j from the regression $I(Mismatch_{i,t}) = \alpha + \beta^j I(Failure)_{i,t} + \delta_j + X_{i,c,t}\gamma + \epsilon_{j,t}$. Mean reports the average β_1^j and Avg S.E. reports the average standard error for each β^j.

Figure V. Change in race reporting behavior across search outcome – White and Hispanic Motorists



Notes: Figure plots the coefficient of interaction $I(Failure_i) \times Hisp\ Bias_j \times I(Year\ Half = t)$, β_5^t, and with 5% confidence intervals. $Hisp\ Bias_j$ is standardized. Points to the right of the vertical line are after the article publication. Diamond points are when the dependent variable is $I(RecRace = White)$ and circle points are when the dependent variable is $I(RecRace = Hispanic)$. Dashed line shows the kernel density distribution of the Bayes shrunk estimates of bias.

Figure VI. Monte Carlo Simulation of Bias



Notes: The graph plots the density of estimates of β_1 from the regression $I(Mismatch_{i,t} = \alpha + \beta_1 I(Failure_{it}) + X_i\gamma + c + t + c * T + m + \epsilon_{i,t}$ where rates of mismatch are simulated using different normal distributions. Horizontal intercept at 0.16 is the bias estimate from the regression using the actual data.