Class Disparities and Discrimination in Traffic Stops and Searches

Benjamin Feigenberg*

Conrad Miller†

March 2025

Abstract

We examine class disparities and discrimination in police searches and stops using data on traffic stops conducted by Texas Highway Patrol. Low-income motorists are more likely to be searched for contraband, less likely to be found with contraband when searched, and more likely to be stopped for infractions associated with pretext stops. We measure class-based discrimination in searches per potential stop, accounting for both the search and stop margins. Our research design leverages motorists stopped in multiple vehicles conveying different class signals. Motorists are more likely to be searched when stopped in a low-status vehicle, and evidence suggests that they are also more likely to be stopped when driving one. Marginal searches triggered by vehicle status are also less likely to yield contraband when the motorist is low-income. We argue that lower hassle costs associated with arrests of low-income motorists help explain trooper behavior.

*University of Illinois, Chicago and NBER (email: bfeigenb@uic.edu).

†Haas School of Business, University of California, Berkeley and NBER (email: ccmiller@berkeley.edu). We thank Jesse Nelson and Konso Mbakire for excellent research assistance. We thank Mackenzie Alston, Bocar Ba, Panka Bencsik, Luke Brinkman, Andrew Jordan, Steven Mello, Aurélie Ouss, Felix Owusu, and participants at the 2023 Texas Economics of Crime Workshop, BYU, UBC, University of Nebraska, Syracuse University, the 2023 Transatlantic Workshop on the Economics of Crime, the 2023 Conference on Discrimination in the 21st Century, Harvard Kennedy School, Columbia, the 2024 Criminal Justice Conference at WashU, the 2024 NBER Law and Economics Program Meeting, and the 2024 NBER Economics of Crime Working Group Meeting for valuable comments. We thank researchers at the Stanford Open Policing Project for providing data on Texas Highway Patrol stops.

1 Introduction

Class may shape how police interact with civilians (Robison, 1936). More disadvantaged neighborhoods have greater police presence (Chen et al., forthcoming), more frequent stops (Fagan et al., 2010), and higher arrest rates (MacDonald, 2021). Class disparities in policing may have important implications for trust in criminal justice institutions, economic inequality and mobility, and the effectiveness of policing. Yet there is little evidence on whether police treat low- and high-status civilians differently under otherwise similar circumstances.

In this paper we study class[1] disparities and discrimination in traffic stops and searches using data on the universe of stops conducted by Texas Highway Patrol. First, we document new descriptive facts about income-based disparities in (1) the rate at which troopers search motorists for contraband, (2) the rate at which these searches yield contraband, and (3) the types of infractions associated with stops. Then, we test whether troopers engage in class-based discrimination by exploiting within-motorist variation in perceived class.

To guide the analysis, we develop a simple model of trooper stop and search decisions, extending Anwar and Fang (2006). Troopers aim to enforce traffic laws and recover contraband, net of stop and search costs. Before initiating a stop, troopers observe the motorist's traffic infraction and, perhaps imperfectly, the motorist's economic status. Troopers may decide to initiate stops based solely on the observed infraction or may also consider the option value of conducting a follow-on search. During a stop, troopers observe a noisy signal indicating whether the motorist is likely to possess contraband, which informs their search decision. Since troopers may discriminate on both the stop and search margins, we define class discrimination as the ratio of *unconditional* search rates—that is, searches per potential stop rather than realized stop—for otherwise identical motorists perceived as low-status versus high-status. This ratio can be decomposed into the product of two components: (1) the ratio of (conditional) search rates and (2) the ratio of stop rates.

Empirically, we find that troopers are more likely to search low-income motorists. Overall, troopers search motorists in 1.9% of stops. A 100% increase in income is associated with a 0.37 percentage point decrease in the search rate. Motorists in the bottom 20% of the income distribution are more than twice as likely to be searched as motorists in the top 20%. Conditioning on the location and time of the stop does not reduce this disparity. By comparison, Black and Hispanic motorists are about 150% and 60% more likely to be searched than White motorists.

Despite searching low-income motorists more often, troopers are less likely to find contraband in these searches than in searches of high-income motorists. Our findings are inconsistent with troopers engaging in accurate statistical discrimination with contraband yield maximization as their sole objective (Feigenberg and Miller, 2022).

Given large class disparities in search rates, we posit that troopers are more likely to pursue low-income motorists in "pretext" stops—stops based on minor infractions and conducted with the goal of identifying more serious crimes via search. However, pretext stops are not explicitly

[1] Throughout the paper, we use "class" as shorthand for household income, recognizing that this captures only part of the broader concept of social class.

identified in the data, making it difficult to measure class differences in their prevalence. To address this challenge, we use the model to classify how *discretionary* different types of infractions are—the likelihood that a given violation category (e.g., illegally tinted windows) serves as the basis for a pretext stop. The key implication of the model is that troopers without a search motive are disproportionately responsible for stops based on less discretionary infractions.[2] Consistent with conventional wisdom, our approach indicates that speeding violations are the least discretionary type of infraction (Epp et al., 2014). We find that low-income motorists are stopped for more discretionary infractions. However, class differences in infraction type alone do not account for the search disparity we document.

Class disparities in search rates and pretext stops could be driven by troopers engaging in class *discrimination*—using perceived class as one factor when deciding whether to search or stop a motorist. But they could also reflect other stop or search determinants—including the motorist's conduct and contextual factors surrounding the stop—that are correlated with motorist status. To test for class discrimination, we use *within-motorist* variation in a key class signal: the motorist's vehicle. Many motorists have access to multiple vehicles, and vehicle purchases are typically infrequent. As a result, motorists often change the vehicle they drive from trip to trip without any coincident change in their socioeconomic circumstances. Our identifying assumption is that within-motorist variation in other relevant characteristics and behavior, including driving behavior, is unrelated to the vehicle a motorist is driving. Under this assumption, vehicle switches generate quasi-experimental variation in perceived class. We directly measure the effect of perceived class on the conditional search rate and infer its effect on the stop rate by comparing the infractions for which motorists are stopped in different vehicles.

Motorists are searched more often when stopped in a low-status vehicle. Switching to a vehicle that doubles signaled income reduces the search rate by 0.53 percentage points, 27% of the sample mean. Placebo tests based on the timing and sequence of vehicle switches provide support for our identifying assumption. These patterns hold if we limit the analysis to stops associated with speeding violations, the most common type of stop and the type we identify as least likely to be pretextual.

Determining how vehicle status affects stop rates is challenging due to the "benchmarking problem"—while we observe realized stops, we do not observe all *potential* stops (Grogger and Ridgeway, 2006; Knox et al., 2020). However, if motorists indeed commit the same infractions regardless of the vehicle they are driving, then differences in reported infractions by vehicle status reveal differences in trooper stop decisions. We find that when motorists are stopped in a low-status vehicle, they are stopped for more discretionary infractions. This pattern suggests that troopers disproportionately target low-status vehicles in pretext stops and that the stop ratio exceeds one.[3] Switching to a vehicle that doubles signaled income reduces the implied stop rate by about 9%.

[2] These troopers serve an identification role similar to that of the "supremely lenient" judges in Arnold et al. (2022).

[3] While some equipment or regulatory violations may be more common in low-status vehicles (e.g., a broken tail light), this pattern remains if we restrict the sample to stops associated with a moving infraction.

We apply the same within-motorist research design to estimate contraband yield from marginal searches induced by changes in vehicle status, using vehicle status as an instrument for search. If troopers were accurately targeting searches to maximize contraband recovery, marginal searches of low-income motorists would be as productive as those of high-income motorists. Instead, we find that troopers are more likely to recover contraband in marginal searches of high-income motorists. This is consistent with the income differences in contraband yield described above. Reallocating marginal searches from low-income motorists to high-income motorists would increase overall contraband yield.

One explanation for troopers' behavior is that they are prejudiced or have inaccurate beliefs about which motorists are most likely to carry contraband. An alternative possibility is that troopers consider outcomes beyond contraband yield, or that the expected costs of search vary with motorist income. In Texas, as elsewhere, troopers may be required to testify during criminal proceedings following contraband discovery and arrest. Prior research and officer testimonials (Newell et al., 2022; Boyce, 2006) suggest that these court appearances impose significant "hassle costs" due to their stressful and acrimonious nature, as well as the scheduling challenges they often pose to officers.[4] To assess how these hassle costs vary with motorist income, we examine defendant plea behavior and case outcomes. Among motorists arrested after a search, low-income motorists are more likely to plead guilty or no contest to associated charges and are less likely to be acquitted or to have their charges dismissed. Guilty and no contest pleas preclude the need for troopers to appear in court, while dismissals and acquittals are more likely when troopers' actions or testimony are successfully challenged. We posit that class disparities in the court system discourage troopers from stopping and searching high-income motorists in the first place. Consistent with this mechanism, we find that search rates are higher in jurisdictions where guilty and no contest pleas are more common due to local institutional factors.[5]

1.1 Related Literature

Our work contributes to the literature on police profiling and discrimination in the criminal justice system. This literature has primarily focused on race-based disparities, including in vehicle stops (Grogger and Ridgeway, 2006; Horrace and Rohlin, 2016; Pierson et al., 2020; Cai et al., 2022; Aggarwal et al., 2022), traffic citations (Anbarci and Lee, 2014; Goncalves and Mello, 2021), searches (Knowles et al., 2001; Anwar and Fang, 2006; Close and Mason, 2007; Antonovics and Knight, 2009; Marx, 2022; Feigenberg and Miller, 2022), police use of force (Fryer, 2019; Hoekstra and Sloan, 2022), charging decisions (Rehavi and Starr, 2014; Tuttle, 2023), pre-trial detention (Arnold et al.,

[4] Graef et al. (2023) show that officer failure to appear rates in Philadelphia courts are highest for driving while intoxicated (DWI) and drug-related cases, which frequently originate in traffic stops. They note that officers' expectations regarding whether they will be challenged on the legality of a stop or search may contribute to their relatively high failure to appear rates.

[5] Finlay et al. (2023) provide evidence that higher-income Texas defendants are also more successful in avoiding a 2003 fine increase. Class disparities in the presence of hassle costs are by no means unique to the criminal justice setting. For instance, Nathan et al. (2020) document that wealthier households in Dallas County, Texas are more likely to file tax protests to reduce the amount they owe in property taxes.

2018, 2022), and sentencing (Mustard, 2001; Abrams et al., 2012; Fischman and Schanzenbach, 2012; Rehavi and Starr, 2014; Yang, 2015; Tuttle, 2023). We extend this literature by examining disparities along another social dimension: class.[6]

While prior work documents group-based differences in treatment, it has not established that those differences reflect direct discrimination *per se*—police, prosecutors, or judges using a person's group identity in deciding how to treat them. Our quasi-experimental design isolates the causal effect of perceived class on police behavior by leveraging within-motorist variation in perceived class across stops based on the vehicle involved. The logic of our test is similar to that of correspondence studies, where researchers experimentally manipulate the perceived group membership of a fictitious person (e.g., a job applicant). Although our approach requires a stronger identifying assumption, it avoids common critiques of correspondence studies by examining organic interactions (Heckman and Siegelman, 1993; Bertrand and Duflo, 2017). We also leverage our research design to estimate and compare marginal returns across groups, using an instrumental variables strategy as in Arnold et al. (2018).

We contribute to research in criminology and sociology on "neighborhood stigma." This literature measures the degree to which neighborhood economic disadvantage predicts higher rates of police contact and arrest, conditional on local racial composition, crime rates, and other relevant factors (Fagan et al., 2010; MacDonald, 2021; Smith, 1986). While evidence from this literature suggests that police profile based on economic class, these findings are difficult to interpret for two reasons. First, these correlational estimates are subject to standard omitted variable bias concerns, as they may not fully account for the characteristics and behaviors of local populations. Second, even if these estimates can be interpreted as causal, the "high crime area" doctrine allows for police to consider neighborhood-based contextual factors when determining if it is reasonable to suspect criminal activity (Fagan et al., 2010). As a result, differences in treatment based on neighborhood disadvantage may reflect legally permissible cross-neighborhood differences in evidentiary standards. Such neighborhood-based disparities may persist even if police do not consider individual class *conditional on location.*

Economic disadvantage is not currently recognized as a protected class under anti-discrimination law, but debate continues over whether it should be. One proposed legal criterion for protected status is whether "social bias" against a given trait is both (1) pervasive and (2) illegitimate, in the sense of being economically irrational (Peterman, 2018). We study the pervasiveness of class-based discrimination in traffic stops, the most common form of police-public interaction (Davis et al., 2018). Furthermore, our analysis of how class disparities in search rates affect contraband yield informs the debate over the "legitimacy" of these disparities.

Although evidence on class discrimination in policing is limited, our work contributes to the growing literature on the regressive burden of criminal justice policies. This research highlights institutional features, such as reliance on indigent defense, money bail, and court fees, that dispro-

[6] A related sociology literature documents class disparities in incarceration rates, though discrimination is not its focus (Pettit and Western, 2004; Western, 2006; Muller and Roehrkasse, 2022, 2025).

portionately burden economically disadvantaged defendants (Agan et al., 2021; Gupta et al., 2016; Makowsky, 2019; Clair, 2020; Mello, 2021; Finlay et al., 2023; Lieberman et al., 2023).

Lastly, our study contributes to a growing body of research on class-based discrimination across various settings. People infer social status from a range of cues, including material possessions, speech and accent, physical appearance and behavior, leisure activities, residential neighborhoods, and names (Kraus and Keltner, 2009; Nelissen and Meijers, 2011; Bjornsdottir and Rule, 2017; Kraus et al., 2017, 2019). These cues can activate stereotypes and lead to discrimination. For example, Rivera and Tilcsik (2006) show that class signals affect callback rates for men in a correspondence study, Besbris et al. (2015) find lower response rates for sellers from disadvantaged neighborhoods in an online marketplace, and Glied and Niedell (2010) show that poor dental health—highly correlated with socioeconomic status—reduces labor market earnings.

2 Setting and Data

2.1 Institutional Setting

In Texas, highway patrol troopers are primarily responsible for enforcing state traffic laws on highways and state roads, though they have statewide authority to enforce criminal law. During a traffic stop, a trooper issues a warning or citation for the original infraction(s). If they suspect a motorist is carrying contraband, such as illicit drugs, they may initiate a further investigation, which can include searching the motorist, vehicle, or passengers. When contraband is discovered during a search, the motorist may be arrested on related charges. Troopers typically work alone but may wait for backup before conducting searches.

There are four types of searches in our setting: probable cause, consent, inventory, and incident to arrest. Probable cause searches occur when a trooper has sufficient grounds to believe that a law has been violated. Consent searches require the motorist's permission before a search can proceed. In our sample, roughly three-quarters of searches are probable cause or consent searches. Inventory searches occur when a vehicle is impounded, allowing troopers to search the vehicle subject to departmental policy. Finally, incident to arrest searches take place after an arrest, permitting troopers to search the detained person and, under broad conditions, the vehicle. We include all search types in our analysis.

Within these constraints, troopers have broad discretion in deciding whether to pursue or conduct a search.

2.2 Administrative Traffic Stop Data

Our primary dataset consists of 16 million traffic stops conducted by Texas Highway Patrol between 2009 and 2015. The data include detailed information for each stop, such as the date, time, location, motorist demographics (race, ethnicity, and gender), vehicle characteristics (make, model, and year), the reported infraction(s), whether a search was conducted, the rationale for each search, whether contraband was found, and the ID number of the trooper conducting the stop.[7] The data cover all stops, including both stops that result in warnings and citations.

A unique feature of these data is that they include each motorist's full name and address. This identifying information allows us to augment the data in three ways: (1) we use each motorist's address to measure household income, (2) we link multiple traffic stops to the same motorist, and (3) we merge in criminal histories for each motorist using data described below.

To construct our analysis sample, we impose the following restrictions. We exclude stops missing data on the trooper, location, or stop outcome. We limit the sample to stops of motorists with valid Texas addresses. We restrict the sample to stops involving passenger cars, pick-up trucks, or SUVs. Appendix Table C.1 summarizes the number of observations dropped with each restriction. After applying these restrictions, our sample includes 11,006,538 stops.[8]

We infer household income using a combination of block group-level data from the American Community Survey (ACS) (covering 2009–2013) and residence-level property value assessments from ATTOM (from 2015).[9] The ACS provides income distributions for all households and separately for homeowners and renters. Our approach depends on the motorist's residence type.

For motorists living in single-family residences, we use property values to refine our income predictions within block groups. We assign motorists to percentiles within block groups based on the assessed property value of their residence. For a motorist living in a property that falls in the p^{th} percentile of all single-family residential properties in their block group, we impute household income using the p^{th} percentile of the household income distribution among homeowners in their block group.

For motorists living in multifamily housing or apartment complexes (or those we are unable to match to a specific property), we assign the median household income category among renters in their block group.

The ACS reports household income separately for homeowners and renters using seven intervals and reports a more granular set of 16 income intervals when pooling all households in a block group. We allocate households across these 16 intervals based on the simplifying assumption that, within the coarser intervals to which they are assigned, homeowners and renters follow the same distribution across these more granular intervals.[10] Figure 1 plots the distribution of household

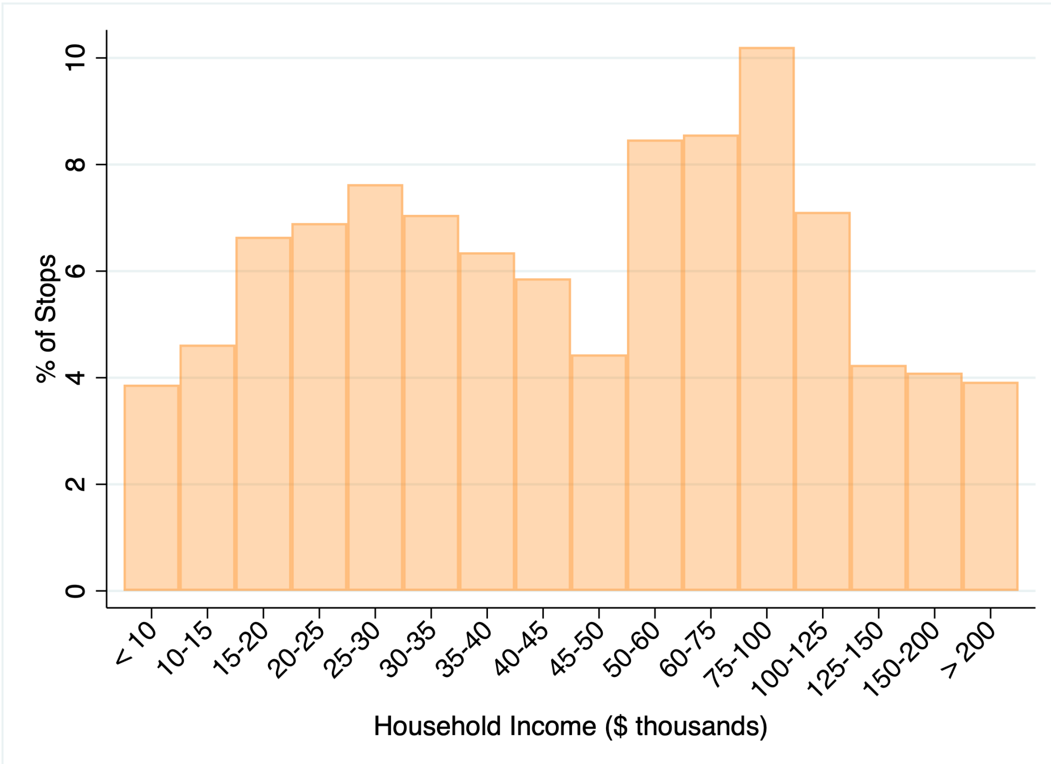
[7] A prior investigation found that Texas state troopers incorrectly recorded many Hispanic motorists as White, at least prior to 2016 (Collister (2015); see also Luh (2020)). Following Pierson et al. (2020), we categorize motorists as Hispanic if they have a surname such that at least 75% of people with that surname identify as Hispanic in the 2010 census. For the subsample of motorists with arrest records, the correlation between this constructed measure of Hispanic ethnicity and the measure included in Texas administrative criminal history data is 0.74 (0.75 for men and 0.70 for women).

[8] Among stops that satisfy all other sample restrictions, fewer than 5% are excluded due to invalid Texas addresses.

[9] The Census Bureau reports household income distributions at the block group level, a subdivision of Census tracts typically including 600–3,000 people.

[10] The seven income intervals are: less than \$10,000, \$10,000–\$19,999, \$20,000–\$34,999, \$35,000–\$49,999, \$50,000–\$74,999, \$75,000–\$99,999, and more than \$100,000. The 16 income intervals are: less than \$10,000, \$10,000–\$14,999, \$15,000–\$19,999, \$20,000–\$24,999, \$25,000–\$29,999, \$30,000–\$34,999, \$35,000–\$39,999, \$40,000–\$44,999, \$45,000–\$49,999, \$50,000–\$59,999, \$60,000–\$74,999, \$75,000–\$99,999, \$100,000–\$124,999, \$125,000–\$149,999, \$150,000–\$199,999, and more than \$200,000.

Figure 1
Distribution of Household Income across Stops



Note: In this figure we present a histogram of household income across stops. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. Household income is inferred from the motorist's exact address.

income across stops. After assigning each stop to a household income category, we impute log household income using the average log household income for all Texas residents in that category in the 2009–2013 ACS data.[11]

Our household income measure is imperfect for several reasons. The block group-level ACS estimates contain sampling error. Some motorists living in single-family homes are in fact renters. The rank correlation between property value and household income within a block group is less than one in practice.[12] In addition, property assessments may not accurately reflect property values. Despite these limitations, our household income measure captures key dimensions of economic status.[13]

[11] Results throughout are not sensitive to using alternative strategies for income imputation, including using median household income in the block group for all motorists or restricting property-based imputation to households that are reported as homeowners in the address history data described in section 2.4. About 80% of households living in single-family residences are reported as homeowners or likely homeowners in those data. Only 5% of households living in multifamily housing and apartment complexes are reported as homeowners or likely homeowners.

[12] For reference, in Home Mortgage Disclosure Act (HMDA) records from 2018–2020, the average within-tract rank correlation between reported income and home prices among home buyers in Texas is 0.55. The statewide rank correlation is 0.72.

[13] We also use the block group-level distribution of household income derived from the ACS to investigate the

Table 1 presents descriptive statistics for all stops in our analysis. We report results separately for motorists with below and above median income, and for all motorists pooled together.[14] Overall, motorists are searched in 1.9% of stops.

2.3 Administrative Criminal History Data

We measure arrests and court outcomes using data from the Texas Computerized Criminal History System, maintained by the Texas Department of Public Safety. State troopers have access to these same records when conducting traffic stops. The data track state felony and misdemeanor criminal charges from arrest to sentencing through 2015.[15] Agencies are required to report data for all offenses that are Class B misdemeanors or greater, including all offenses that would potentially lead to a confinement sentence. The data include information on each criminal charge, including the original arrest charge, date of arrest, final court charge, final court pleading, charge disposition, and, if the charge results in conviction, the final sentence. The data include arrest charges that are ultimately dropped. The data also include each person's full name, address, race, ethnicity, gender, and a unique person ID.

2.4 Commercial Address History Data

One limitation of the traffic stop data is that it does not include a unique motorist ID. This creates a challenge when two stops share the same motorist name but list different addresses, because we cannot immediately determine whether they correspond to the same person. The criminal history data include an individual identifier and allow us to construct a partial address history for a given person. But the addresses we observe in those data only correspond to the points in time when that person is arrested, and only for people with any criminal history.

To improve the matching of traffic stops and criminal history to unique individuals, we use commercial data on address history from Infogroup.[16] These data provide full names and residential addresses with estimated dates of residence. Our data include the address histories for all people in the database with a Texas residence between 2005 and 2016.

We map traffic stops and criminal history data to individuals using full name and address, incorporating address history data to account for address changes. We do not require traffic stops to match the address history data to be included in the analysis.

---

predictive power of block group median income. We generate a simulated dataset with household income levels assigned to observations based on block group-level distributions, and we calculate a rank correlation of 0.50 between this simulated income measure and block group median income. The median income of the block group is itself a robust predictor of household income, and the adjustments we make based on the income distribution of the block group and property values serve to further strengthen our prediction.

[14] Throughout the paper we refer to "household income" and "income" interchangeably.

[15] For analyses based on court pleadings and dispositions, we limit the sample to arrest records from 2010 and earlier, as records are less complete in later years.

[16] These data are similar to address history data used in prior research, including Diamond et al. (2019) and Phillips (2020).

Table 1
Traffic Stop Descriptive Statistics

	All Stops			All Searches		
	Below Median	Above Median	All	Below Median	Above Median	All
Black	10.11	8.64	9.42	16.79	15.04	16.18
Hispanic	37.72	24.64	31.61	39.39	29.72	36.01
White	49.85	63.21	56.09	42.00	52.61	45.71
Female	35.08	34.53	34.82	19.81	18.96	19.51
Log household income	9.94	11.34	10.59	9.91	11.23	10.37
	(0.61)	(0.49)	(0.89)	(0.61)	(0.45)	(0.84)
Search rate	2.34	1.44	1.92	100	100	100
Unconditional hit rate	0.82	0.56	0.70	34.44	38.56	35.88
Moving	68.22	74.18	71.00	59.97	62.39	60.82
Driving while intoxicated	2.26	1.33	1.82	22.04	21.50	21.85
Speeding	55.21	63.32	59.00	27.98	32.61	29.60
Equipment	21.09	15.84	18.64	18.53	16.83	17.93
Regulatory	34.36	28.39	31.58	35.95	30.39	34.00
Prior felony arrests	0.145	0.0818	0.115	0.570	0.472	0.536
	(0.771)	(0.564)	(0.683)	(1.554)	(1.403)	(1.503)
Prior misdemeanor arrests	0.346	0.219	0.286	1.265	1.130	1.218
	(1.370)	(1.041)	(1.229)	(2.708)	(2.469)	(2.628)
Observations	5,868,149	5,138,389	11,006,538	137,507	74,025	211,532

Sample restrictions are described in section 2. All values, excluding log household income and prior arrests, are expressed as percentage points. 'Below Median' and 'Above Median' refer to stops where household income is below and above the median value. Section 2.2 discusses the construction of the household income measure, which divides household income into 16 intervals. The unconditional "hit" rate refers to the unconditional contraband discovery rate.

3 A Model of Troopers' Stop and Search Behavior

In this section, we develop a simple model of trooper stop and search decisions. The model serves three main purposes: (1) clarifying how the two decisions are linked; (2) precisely defining the notion of discrimination that we study; and (3) motivating our approach to classifying how *discretionary* each infraction type is—that is, the likelihood that a given category of violation serves as the basis for a pretext stop.

We extend the Anwar and Fang (2006) model of trooper search decisions by incorporating a stop margin. In Anwar and Fang (2006), troopers decide whether to search a stopped motorist based on a noisy signal for whether the motorist is carrying contraband and the motorist's group membership. In our extension, troopers also decide whether to *stop* a motorist based on: (1) the observed traffic infraction and (2) the motorist's group membership.

We consider a unit continuum of motorists and focus initially on a single trooper's decision-making. Each motorist i has perceived economic status $\gamma_i \in \{L, H\}$. Suppose fraction π_γ of motorists carry contraband. For each potential stop, the trooper first decides whether to stop the motorist, and if so, whether to conduct a search for contraband. We first examine the search decision and then return to the stop decision.

3.1 The Search Decision

For each stopped motorist i, the trooper observes a noisy signal for the motorist's guilt, $\theta_i \in [0, 1]$. If the motorist is carrying contraband, the index θ is randomly drawn from a distribution with continuous probability density function (PDF) $f_g^\gamma(\cdot)$; if the motorist is not carrying contraband, θ is randomly drawn from a continuous PDF $f_n^\gamma(\cdot)$. (The subscripts g and n stand for "guilty" and "not guilty," respectively.)

We assume that $f_g^\gamma(\cdot)$ and $f_n^\gamma(\cdot)$ satisfy a standard monotone likelihood ratio property (MLRP): $f_g^\gamma(\theta)/f_n^\gamma(\theta)$ is strictly increasing in θ. The MLRP assumption on the signal distributions provides that a higher index θ signals that a motorist is more likely to be guilty.

Let G denote the event that a motorist is found with contraband if searched. When a trooper observes a type γ motorist with signal θ, the posterior probability that the motorist is guilty of carrying contraband, $Pr(G|\theta, \gamma)$, is given by Bayes's rule:

$$P(G|\theta, \gamma) = \frac{\pi_\gamma f_g^\gamma(\theta)}{\pi_\gamma f_g^\gamma(\theta) + (1 - \pi_\gamma) f_n^\gamma(\theta)}.$$

From the MLRP, we have that $P(G|\theta, \gamma)$ is strictly increasing in θ.

Following the literature, we assume that the trooper's objective is to maximize contraband yield, net of search costs, τ^γ. These costs may vary by motorist status due to taste-based discrimination (as in Anwar and Fang, 2006) or other considerations, including the risks of receiving civilian complaints or the effort associated with later court charges, which we examine in section 6.

Given this cost structure, troopers will choose some threshold θ_γ^* where troopers will search any type γ motorist with $\theta_i \geq \theta^*_\gamma$. The threshold equalizes the marginal cost and benefit of search for the marginal searched motorist:

$$P(G|\theta^*_\gamma, \gamma) = \tau^\gamma.$$

Note that if search costs do not vary with motorist characteristics, then the trooper will set a common $P(G)$ threshold when deciding whom to search.

With respect to the search decision, the trooper's utility for a given type γ motorist with signal θ is

$$\begin{aligned} U(\theta, \gamma, \tau^\gamma) &= \max\{P(G|\theta, \gamma) - \tau^\gamma; 0\} \\ &= \begin{cases} P(G|\theta, \gamma) - \tau^\gamma & \theta \geq \theta^*_\gamma \\ 0 & \theta < \theta^*_\gamma \end{cases} \end{aligned}$$

Note that $U(\theta, \gamma, \tau^\gamma)$ decreases (weakly) in τ^γ. For a trooper with a search cost so high that they never search, $U(\theta, \gamma, \tau^\gamma) = 0$.

3.2 The Stop Decision

Motorists commit different types of infractions, which vary in both category (e.g., speeding) and severity (e.g., the recorded speed over the limit). Let $v_i \in \mathbb{V}$ denote the specific violation. Before making a stop, the trooper observes the infraction v_i and the motorist type γ.[17] When deciding whether to conduct a stop, the trooper trades off the cost of the stop, c, with two (potential) benefits.

First, there is the direct benefit of enforcing the law, $B(v_i)$. This may vary by infraction: for example, speeding violations are generally considered greater threats to public safety than illegally tinted windows, so the direct benefit of enforcing speeding laws is likely greater.

Second, there is an option value of search, $\mathbb{E}[U(\theta_i, \gamma_i, \tau^{\gamma_i})|v_i, \gamma_i]$. This option value depends on trooper search costs, motorist type, and potentially the violation, which could be correlated with θ_i.

The trooper will stop motorist i if

$$B(v_i) + \beta\mathbb{E}[U(\theta_i, \gamma_i, \tau^{\gamma_i})|v_i, \gamma_i] \geq c,$$

where $\beta \leq 1$ is a discount factor. We define a *pretext stop* as a stop where

$$B(v_i) < c \leq B(v_i) + \beta\mathbb{E}[U(\theta_i, \gamma_i, \tau^{\gamma_i})|v_i, \gamma_i]. \tag{1}$$

In other words, a trooper would not initiate a pretext stop if not for the search motive. If a trooper's search costs are prohibitively high, they do not conduct pretext stops. Below, we use this set of

[17] We assume the trooper observes γ rather than a noisy signal for γ. This simplifies the model without affecting its implications. We could also allow the trooper to observe a noisy signal for θ.

troopers for identification.

Finally, we introduce variation across troopers, indexed by j. Specifically, we allow troopers to differ in their search costs, τ_j^γ. Troopers draw potential stops from the same distribution. We assume that: (i) troopers share a common stop cost, c, and valuation for the direct benefit of enforcement, $B(v)$, and (ii) a common interpretation of signals, θ.[18]

3.2.1 Class Differences and Discrimination

We next define class differences and discrimination in search.

Let $T(v_i, \gamma, \tau_j^\gamma)$ denote an indicator function for whether trooper j would stop motorist i in a potential stop.

Let $S(\theta_i, \gamma, \tau_j^\gamma)$ denote an indicator function for whether, *during a stop*, trooper j would conduct a search of motorist i.

The observed search rate for type γ motorists is

$$\bar{S}^\gamma = \mathbb{E}_\gamma[S(\theta_i, \gamma, \tau_j^\gamma) | T(v_i, \gamma, \tau_j^\gamma) = 1] \tag{2}$$

where the subscript γ for the expectation operator refers to the fact that the expectation is taken over the distribution of (v_i, θ_i) values for type γ motorists. The observed search rate conditions on the set of potential stops that lead to a stop.

The observed search disparity between type L and type H motorists can be written as

$$\frac{\bar{S}^L}{\bar{S}^H} = \frac{\mathbb{E}_L[S(\theta_i, L, \tau_j^L) | T(v_i, L, \tau_j^L) = 1]}{\mathbb{E}_H[S(\theta_i, H, \tau_j^H) | T(v_i, H, \tau_j^H) = 1]}. \tag{3}$$

We show below that this ratio exceeds one, meaning L motorists are searched at higher rates.

We define class-based discrimination in search using two *unconditional* search rates—that is, searches per potential stop rather than realized stop. We focus on unconditional search rates because troopers potentially discriminate on both the stop and search margins. We take all potential stops $(v_i, \theta_i, \gamma_i, \tau_j^{\gamma_i})$ and ask how the unconditional search rate would change if all motorists were perceived as type L versus type H.

$$\Delta = \underbrace{\frac{\mathbb{P}[S(\theta_i, L, \tau_j^L) = 1 | T(v_i, L, \tau_j^L) = 1]}{\mathbb{P}[S(\theta_i, H, \tau_j^H) = 1 | T(v_i, H, \tau_j^H) = 1]}}_{\text{(conditional) search ratio}} \times \underbrace{\frac{\mathbb{P}(T(v_i, L, \tau_j^L) = 1)}{\mathbb{P}(T(v_i, H, \tau_j^H) = 1)}}_{\text{stop ratio}}. \tag{4}$$

In other words, troopers discriminate against L motorists if, for the pooled distribution of motorist conduct (v_i, θ_i), troopers would stop and search L motorists more often than H motorists. This definition has two components: the ratio of (conditional) search rates (the *search ratio*) and the ratio of stop rates (the *stop ratio*).

[18] Alternatively, we could allow troopers to vary in their beliefs over how perceived class predicts guilt. This would have similar implications to and would be difficult to distinguish from differences in search costs.

The former term captures both within-trooper and between-trooper differences in treatment. Some troopers may stop a motorist of either type for violation v_i, but their search decision hinges on the type. This is a within-trooper difference in treatment. The set of troopers that stop L motorists may also differ from the set of troopers that stop H motorists, holding motorist conduct fixed, and these two sets of troopers may differ in their search costs. For example, L motorists may face higher search rates in part because they tend to be stopped by more search-intensive troopers. This is a between-trooper difference in treatment.

The latter term captures class-based discrimination on the stop margin. Though discrimination in search does not imply discrimination at the stop margin, the two margins are naturally connected. For example, if $P(G|\theta,\gamma) = P(G|\theta)$ is common across motorist types and v_i and θ_i are independent, then $\tau_j^H > \tau_j^L$ implies discrimination both on the search margin and the stop margin.

4 Class Differences in Search Rates, Hit Rates, and Infraction Type

In this section we examine how search rates, contraband discovery ("hit") rates, and infraction type vary with motorist income.

4.1 Search Rates

We first examine search rates. Figure 2 shows how search rates vary with income.[19] A 10% increase in household income is associated with a 0.05 percentage point decrease in the search rate, while a 100% (69 log point) increase in income corresponds to a 0.37 percentage point decrease. Motorists in the top income quintile are searched in 1.1% of stops, while those in the bottom quintile are searched in 2.5% of stops, over 125% more often. For comparison, Black and Hispanic motorists are about 150% and 60% more likely to be searched than White motorists in our data (Feigenberg and Miller, 2022).

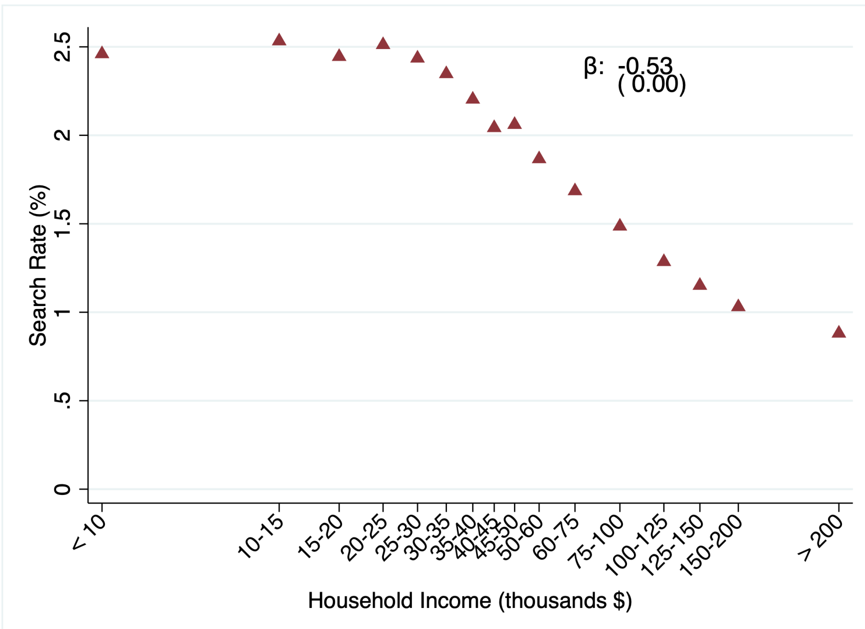
The negative relationship between motorist income and search rates may reflect differences in stop context, including the location and time of day. An advantage of our study setting is that we can measure class disparities holding these factors constant. Class differences in search rates may also in part reflect differences in other motorist characteristics, including race, gender, and criminal history. To examine whether the pattern shown in Figure 2 is robust to conditioning on stop context and other motorist characteristics, we estimate linear probability models of the form

$$Y_{it} = \alpha_{\ell_{i,t}\tau(t)y(t)} + \beta \log(\text{income})_{it} + X_{it}\Gamma + \phi_{p(i,t)} + \epsilon_{it}, \tag{5}$$

where Y_{it} is an indicator for whether the stop of motorist i at time t leads to a search, $\alpha_{\ell_{i,t}\tau(t)y(t)}$ are fixed effects for the combination of the trooper patrol area ("sergeant area") corresponding

[19] Household income is partitioned into 16 intervals. We plot the search rate for each interval, using a logarithmic scale on the horizontal axis. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate.

Figure 2
Search Rates Are Decreasing in Motorist Income



Note: This figure plots search rates as a function of motorist income. Household income is depicted on a log scale. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. The reported slope coefficient (and standard error) is from a bivariate regression of an indicator for whether the stop leads to a search on log household income.

Table 2
Search Rates and Hit Rates by Motorist Income

Outcome:	Search (×100)					Contraband Recovery (×100)				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
log household income	-0.53	-0.54	-0.49	-0.38	-0.35	3.25	1.56	1.36	1.39	1.30
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.12)	(0.12)	(0.12)	(0.12)	(0.12)
Sgt. Area × Time of Week × Year FEs		✓	✓	✓	✓					
Sgt. Area × Year FEs							✓	✓	✓	✓
Motorist Demographics			✓	✓	✓			✓	✓	✓
Motorist Criminal History				✓	✓				✓	✓
Trooper FE					✓					✓
Mean of DV			1.92					35.88		
Observations			11,006,538					211,532		

This table reports regression coefficients from estimates of equation (5), where the outcome is an indicator (multiplied by 100) for whether a stop leads to a search (columns 1–5) or an indicator (multiplied by 100) for whether a search yields contraband (columns 6–10). Section 2.2 discusses the construction of motorist household income. Robust standard errors are provided in parentheses.

to the stop location, time of week (quarter of day, weekday or weekend), and year. X_{it} is a vector of motorist demographic characteristics, including some combination of race, gender, and criminal history.[20] We also estimate models with trooper fixed effects, $\phi_{p(i,t)}$, to determine whether differences in which troopers stop low- and high-income motorists contribute to class disparities in search.

Columns 1 through 5 of Table 2 provide β coefficient estimates. Column 1 does not include additional controls and corresponds to the slope estimate provided in Figure 2, -0.53. Column 2 includes fixed effects for combinations of stop location and time. The slope is essentially unchanged. Column 3 adds fixed effects for motorist race and gender. The slope attenuates slightly to -0.49, reflecting that Black and Hispanic motorists have lower incomes and are also more likely to be searched.[21] Column 4 adds separate fixed effects for the motorist's number of prior misdemeanor and felony arrests. Conditioning on criminal history reduces the coefficient to -0.38. Those with prior arrests are more likely to be searched, and low-income motorists are more likely to have prior arrests. Finally, column 5 adds trooper fixed effects. Their inclusion has little effect.

Low- and high-income motorists are generally stopped for different violations. For example, high-income motorists are more likely to be stopped for speeding (see Table 1). It is possible that low-income motorists are searched at higher rates because they commit violations where searches are more common. For example, stops associated with driving while intoxicated (DWI) violations are much more likely to lead to searches than other stops, and low-income motorists are more likely to be involved in DWI stops. As we argue in section 3, the violations associated with stops of low-income versus high-income motorists are likely to be influenced by troopers' search intentions.

[20]Unfortunately, we do not have data on motorist age.

[21]Interestingly, we find that race and class effects for search rates are roughly multiplicatively separable (see Appendix Figure C.1).

Then, conditioning on the violation when measuring class disparities in search rates may lead us to understate those disparities. Nonetheless, as a robustness check, we restrict our analysis to stops initiated by a speeding violation, the most common type of stop.[22] Although search rates are lower in this sample, (proportional) class disparities are larger (see Appendix Figure C.2).[23] In section 4.3, we further investigate how class differences in the infractions associated with stops contribute to search disparities.

4.2 Hit Rates

Next, we examine hit rates—the percentage of searches that yield contraband and the standard measure of search productivity—and how they vary with motorist income.

Figure 3 shows that hit rates increase with motorist income. For every 10% increase in income, hit rates increase by 0.3 percentage points, while a 100% increase in income corresponds to a 2.2 percentage point increase. Troopers detect contraband in 32.6% of searches of motorists in the bottom income quintile, compared to 41.1% in the top quintile.

Columns 6 through 10 of Table 2 present slope estimates that account for the year and location of stops, motorist characteristics, and trooper fixed effects, following equation (5). The structure mirrors columns 1 through 5. Column 6 does not include additional controls and corresponds to the slope estimate provided in Figure 3, 3.25. Column 7 adds fixed effects for combinations of stop location and time. The slope decreases to 1.56, a drop that primarily reflects that high-income motorists tend to be stopped in areas with higher hit rates. Column 8 adds fixed effects for motorist race and gender, slightly reducing the slope to 1.36. Column 9 adds fixed effects for the motorist's number of prior misdemeanor and felony arrests, to little effect. Finally, column 10 adds trooper fixed effects. The coefficient attenuates slightly to 1.30.

The decreasing relationship between search rates and motorist income indicates that troopers are not maximizing contraband yield. Troopers could increase contraband yield by reallocating searches from low-income motorists to high-income motorists (Feigenberg and Miller, 2022).[24] We return to this point in section 5.3.

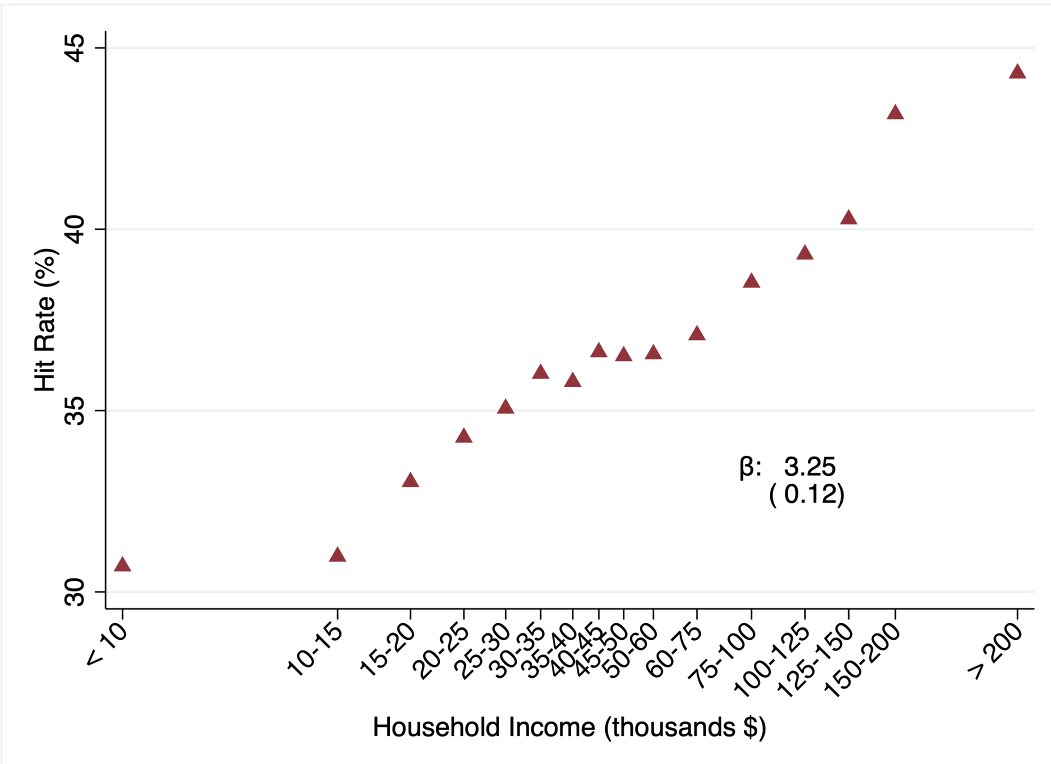
One potential explanation for trooper behavior is that low-income motorists are found with more serious contraband. However, we find that low- and high-income motorists are found with similar forms of contraband based on the coarse categorization provided in the traffic stop data (see Appendix Table C.3). In section 6, we use arrests data to test for class differences in the significance

[22]Specifically, we limit to stops with an associated speeding violation (leading to a warning or citation) and no DWI violation.

[23]See Appendix Table C.2 for descriptive statistics on this subsample of stops.

[24]In theory, this type of reallocation may not be feasible given the "inframarginality problem"—if troopers face diminishing returns to search, the hit rate for the average and marginal search may differ significantly, and the hit rate for low-income motorists at the margin could, in principle, be higher than the marginal hit rate for high-income motorists (Ayres, 2002). In practice, Feigenberg and Miller (2022) document that average and marginal hit rates are similar within each motorist racial group, which indicates that there is no inframarginality problem in this context. In Appendix Figure C.3, we employ the methodology detailed in Feigenberg and Miller (2022) to show that the relationship between trooper search rates and unconditional hit rates is also linear within subgroups defined by motorist income tercile.

Figure 3
Hit Rates are Increasing in Motorist Income



Note: This figure plots hit rates—the percentage of searches that yield contraband—as a function of motorist income. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. The reported slope coefficient (and standard error) is from a bivariate regression of an indicator for whether the search yields contraband on log household income.

of recovered contraband before considering alternative potential explanations for trooper behavior.

4.3 Infraction Type

Although some stops are conducted solely to enforce traffic laws, others are "pretext" stops—stops based on minor infractions that troopers use to identify a more serious crime, often through searches. The higher search rate for low-income motorists suggests that they may be disproportionately subject to pretext stops. Since pretext stops are not explicitly labeled in the data, assessing class differences in their prevalence requires an indirect approach. We apply the model described in section 3 to classify how *discretionary* each infraction type is—that is, the likelihood that a given category of violation serves as the basis for a pretext stop. We then test whether low-income motorists are disproportionately stopped for more discretionary infractions.

We apply the model as follows. Suppose that proportion λ of troopers have prohibitively high search costs τ^γ such that they have no search motive. Then proportion λ of non-pretext stops are conducted by troopers with no search motive. Deviations from this benchmark reveal the proportion of stops that are pretextual. If proportion η of stops are conducted by troopers without a search motive, then proportion $1 - \frac{\eta}{\lambda}$ are pretext stops. We measure this share separately by infraction type. Even if we do not know λ, we can use the relative share of stops conducted by troopers without a search motive to compare infraction types.

We identify troopers with no search motive by selecting those who have conducted 1,000 or more stops without ever conducting a search.[25] $\tilde{\eta}_k$ is then the share of stops for infraction k conducted by those troopers. We estimate $\tilde{\lambda}$, the proportion of all non-pretext stops conducted by troopers with no search motive, using only speeding stops, the violation with the highest proportion of stops made by troopers without a search motive.[26] To the extent that a share of stops for speeding are themselves pretextual, the infraction-specific pretext shares we calculate will be attenuated, but the relative rankings across infractions will be unchanged.[27]

Panel A of Figure 4 presents the implied percent of stops that are pretextual for the 10 most common infraction types. Consistent with conventional wisdom, our framework classifies speeding violations as the least discretionary infraction, while violations such as illegally tinted windows are classified as highly discretionary (Epp et al., 2014). At the same time, we identify several (potentially more subjective) moving violations, such as tailgating, as having high pretext shares. Panel B shows the relative distribution of infraction types for motorists in the bottom versus top income quintiles. Low-income motorists are disproportionately stopped for more discretionary infractions.[28]

[25] We show that results are robust to a less conservative approach that includes all troopers in the bottom search rate decile. In practice, this corresponds to troopers who search roughly one or fewer of every 1,000 stopped motorists.

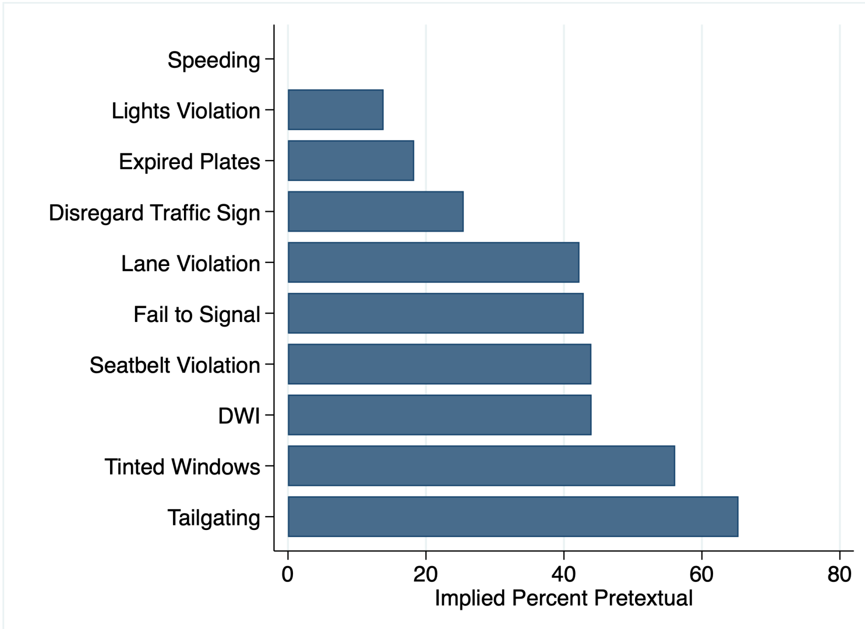
[26] Before calculating these proportions, we re-weight each stop based on the number of stops conducted in the corresponding sergeant area by troopers with no search motive. We restrict the sample to stops with only one associated infraction.

[27] In any case, as we detail below, results are unchanged if we simply use relative $\tilde{\eta}_k$ values to characterize stops that are more versus less likely to be pretextual.

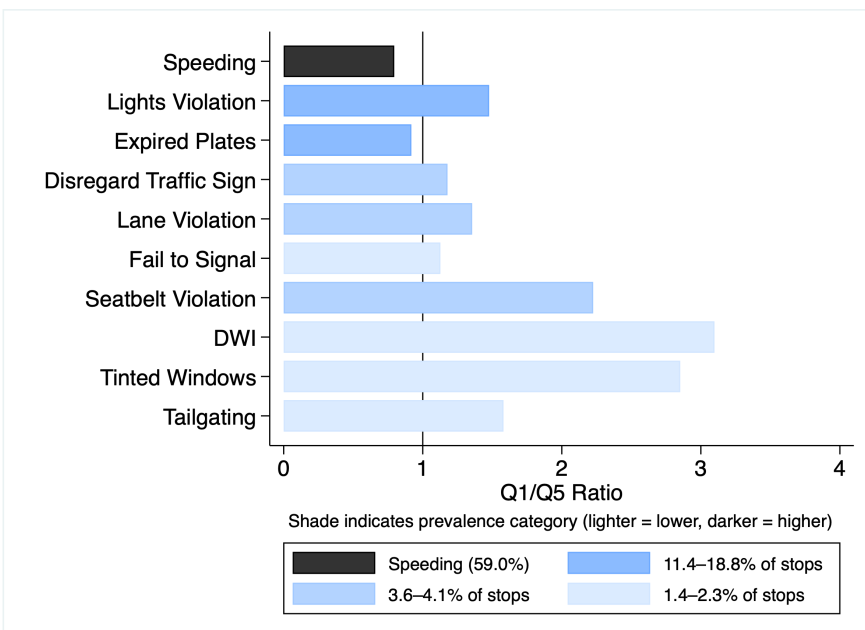
[28] DWIs are generally considered to be a serious offense but we find that a relatively high percent of DWI stops are pretextual. One potential explanation for this is that some stops that lead to a DWI citation are initiated by

Figure 4
Distribution of Pretext Stops and Household Income by Infraction Type

(a) Implied Percent Pretextual by Infraction

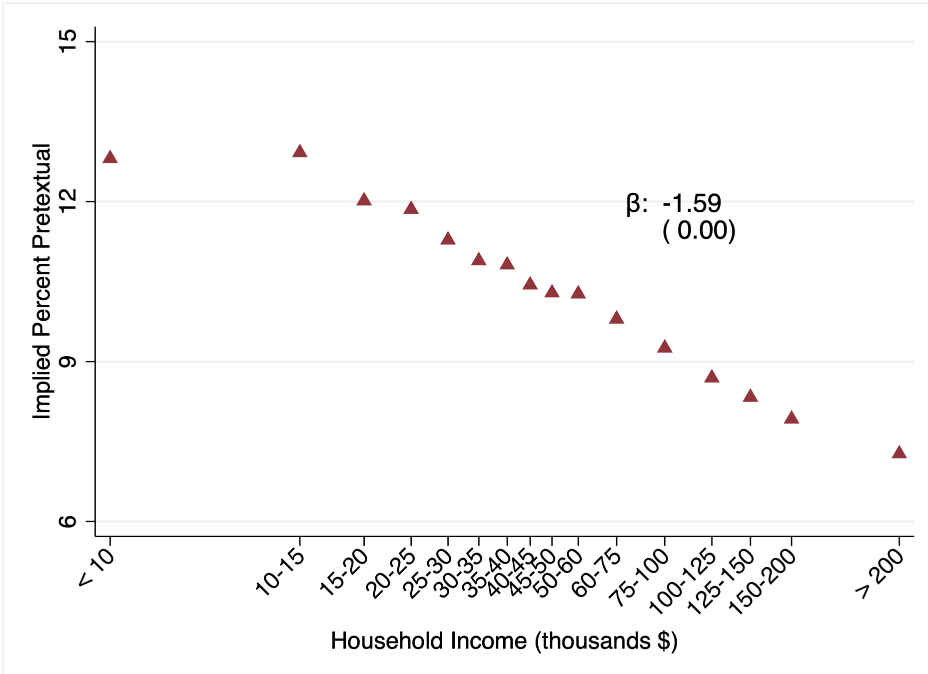


(b) Percent of Stops for Bottom versus Top Income Quintile Motorists by Infraction



Note: Panel A presents the implied percent pretextual for the 10 most common infraction types. Panel B presents the ratio of the percent of stops associated with the given infraction for motorists in the bottom income quintile (Q1) relative to the top income quintile (Q5). The sample includes stops that are associated with exactly one infraction type. The shading of each bar in Panel B reflects the percent of all stops associated with the given infraction. These percentages are based on the full sample of stops, including those with more than one associated infraction type.

Figure 5
Low-Income Motorists Are Stopped for More Discretionary Infractions



Note: This figure plots the implied percent of pretext stops as a function of motorist income. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. The implied percent pretextual measure used is described in section 4.3. The reported slope coefficient (and standard error) is from a bivariate regression of implied percent pretextual on log household income.

As another test for whether low-income motorists are stopped for more discretionary infractions, Figure 5 plots the implied percent of stops that are pretextual by motorist income. A 10% increase in motorist income is associated with a 0.15 percentage point decrease in the implied percent of stops that are pretextual, while a 100% increase in income corresponds to a 1.10 percentage point decrease. Stops of motorists in the bottom income quintile are for infractions with nearly 50% higher pretext shares than stops of motorists in the top income quintile.

Table 3 parallels Table 2, presenting additional slope estimates under different model specifications. Column 1 reproduces the slope estimate provided in Figure 5, -1.59. In Column 2, which includes fixed effects for combinations of stop location and time, the coefficient falls to -1.26. The inclusion of additional covariates in each of Columns 3–5 marginally decreases the slope. In the most saturated specification (Column 5), the estimated coefficient is -0.97. Across specifications, we find that low-income motorists are stopped for more discretionary infractions.[29] We identify

minor traffic violations, and troopers may only identify that the motorist is potentially intoxicated during the stop. On this point, about 50% of DWI citations are associated with another infraction. While our classification includes only stops with a single recorded infraction type, the included DWI citations may be associated with other (minor) infractions that were not recorded.

[29] In Appendix Table C.4 we present results where we either (1) define troopers without a search motive to include all those in the bottom search rate decile or (2) replace the percent pretextual dependent variable with the infraction-

Table 3
Pretext Stop Shares by Motorist Income

Outcome:	Pretextual Share (×100)				
	(1)	(2)	(3)	(4)	(5)
log household income	-1.59	-1.26	-1.13	-1.07	-0.97
	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)
Sgt. Area × Time of Week × Year FEs		✓	✓	✓	✓
Motorist Demographics			✓	✓	✓
Motorist Criminal History				✓	✓
Trooper FE					✓
Mean of DV	10.33				
Observations	11,006,538				

This table reports regression coefficients from estimates of equation (5), where the outcome is the implied pretext share associated with a given stop (multiplied by 100). Section 2.2 discusses the construction of motorist household income. The sample includes all stops, which are assigned to either one of the ten most common infraction types or a residual "Other" category. For stops with multiple associated infractions, the assigned pretext share is the minimum value across infractions. Robust standard errors are provided in parentheses.

this robust relationship despite the fact that motorist income may be difficult for troopers to infer prior to making a stop. In the next section, we identify a sharper relationship between a salient class signal, the motorist's vehicle, and infraction type.

Before proceeding, we investigate the potential contribution of differential exposure to discretionary stops in explaining search disparities. In Appendix Table C.5, we reproduce columns 1–5 of Table 2 while controlling for the implied pretext share associated with each stop. Estimates decline by roughly 40% in magnitude, suggesting that a substantial share of the search disparity may manifest even before a trooper-motorist interaction takes place.

5 Testing for Class Discrimination

We have shown that troopers search low-income motorists more often and stop low-income motorists for more discretionary infractions. However, these findings alone do not establish that troopers engage in class *discrimination*—the practice of stopping or searching motorists based on their perceived class. A central challenge to investigating group disparities is distinguishing between discrimination and correlated unobservables (Charles and Guryan, 2011). The ideal experiment to isolate class discrimination would vary the *perceived* class of a motorist while holding their economic circumstances or behavior constant.

In this section we test for class discrimination using a salient class signal that varies substantially *within* the same motorist: the vehicle they drive. Many motorists are stopped in multiple vehicles

specific percent of stops conducted by troopers without a search motive. In either case, we find that low-income motorists are differentially stopped for more discretionary infractions.

conveying varying class signals. If troopers engage in class discrimination, they should be more likely to search the same motorist when they are driving a low-status vehicle rather than a high-status vehicle.

Our quasi-experimental design relies on the assumption that, for the same motorist, other stop (v) and search (θ) determinants—such as the infractions they commit, their demeanor, and willingness to consent to a search—are independent of the vehicle they are driving. This assumption is reasonable because motorists often change the vehicle they drive from trip to trip without any coincident change in their socioeconomic circumstances. Many motorists have access to multiple vehicles: among Texas households with at least one car, about 65% possess multiple cars.[30] Vehicle changes are relatively infrequent and typically not prompted by immediate changes in income. We provide more detailed support for and probe our identifying assumption below.

Under this assumption, we can (1) measure the search ratio by comparing search rates for stops in different vehicles and (2) infer the stop ratio by comparing the infractions associated with stops in different vehicles.

We define *vehicle status* ($\text{VEHICLE STATUS}_{it}$), as the predicted log household income associated with a given vehicle in the stop data. We classify vehicles by regressing log income on vehicle make and type (passenger car, pick-up truck, or SUV), both interacted with a quadratic in vehicle age, and we winsorize values at 0.5% and 99.5%.[31] The classification is intuitive. New vehicles are higher in status than old vehicles; luxury brand vehicles are higher in status than economy brand vehicles. Using the NHTS, we verify that results are similar if we alternatively measure vehicle status based on the log household income of vehicle owners in that sample. The correlation between the two vehicle status measures is 0.86.

There are several features of our vehicle status measure to note. First, vehicle status varies significantly across stops. The standard deviation is 22 log points (compared to 89 log points for income).[32] Second, the correlation between vehicle status and log household income is only 0.25. Figure 6 plots histograms of vehicle status for motorists in the bottom 20% and top 20% by income. There is substantial overlap.[33]

In addition to signaling motorist income, our vehicle status measure is also correlated with other motorist characteristics. Conditional on income, vehicle status is correlated with neighborhood education levels.[34] Prior research suggests that vehicle status is also an indicator of household liquidity, particularly for low-income households (Adams et al., 2009; Aaronson et al., 2012; Mian et al., 2013).

[30] This statistic is derived from 2009 and 2017 National Household Travel Survey (NHTS) data.

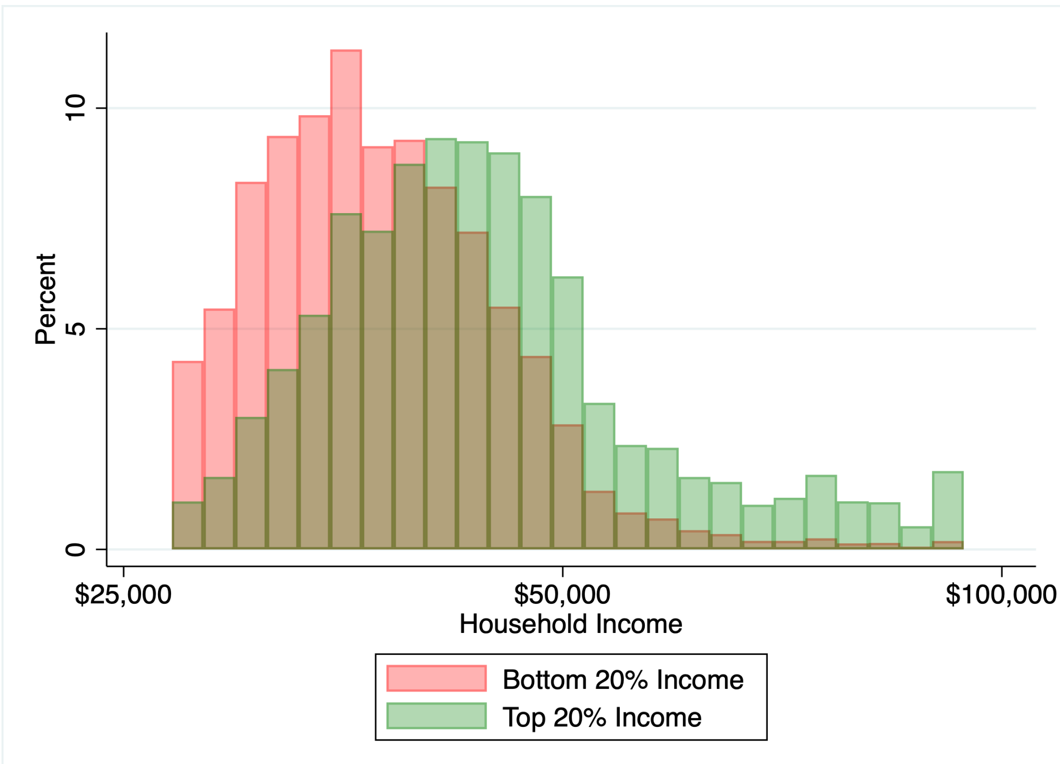
[31] This measure explains about 60% of the variation in search rates across vehicle classes.

[32] For reference, in 2009 and 2017 NHTS data, the range in vehicle status within households that possess multiple cars is about 33 log points.

[33] Using data from the NHTS, we provide external validation in the Appendix that vehicle status is nonetheless a robust predictor of household income. At the same time, we find that vehicle status is less predictive of household income than location of residence. In the NHTS, we calculate a rank correlation of 0.35 between reported household income and the leave-out average household income of those who own vehicles of the same make, type, and age.

[34] Based on 2017 NHTS data, we find that vehicle status is correlated with education at the individual level, conditional on household income (see Appendix Table C.6).

Figure 6
Distribution of Vehicle Status by Income



Note: In this figure we present histograms of vehicle status by household income quintile. The histograms are log scale. The vehicle status measure is the expected log household income for people driving vehicles of the same make, type, and age.

We relate VEHICLE STATUS_{it} to search rates (Panel A), contraband yield (Panel B), and implied pretextual stop shares (Panel C) in Table 4. We estimate regression models analogous to equation (5), where we include VEHICLE STATUS_{it} as an explanatory variable.

We first describe the results for search, which are shown in Panel A. All specifications include fixed effects for combinations of stop location and time. Column 1 does not include additional controls. The coefficient for VEHICLE STATUS_{it} is -3.77, indicating that a 10% increase in VEHICLE STATUS_{it} is associated with a 0.4 percentage point decrease in the search rate. The top quintile of motorists by vehicle status are searched in 0.8% of stops. The bottom quintile of motorists by vehicle status are searched in 3.5% of stops, over 4 times as often. Column 2 adds fixed effects for motorist race and gender, attenuating the coefficient to -3.28. Column 3 adds fixed effects for the motorist's number of prior misdemeanor and felony arrests. This reduces the slope to -2.68. Column 4 adds income as an additional control. This slightly attenuates the coefficient to -2.47.

The coefficient for VEHICLE STATUS_{it} is an order of magnitude larger than the corresponding coefficient for income. This pattern reflects at least two factors. First, from the trooper's perspective, the vehicle may be the most salient indicator of the motorist's economic class. It would be significantly more difficult for troopers to infer a motorist's income from their address in real-time, for example. Other signals, including those based on a trooper's face-to-face interaction with the motorist, may be noisier. Hence, if troopers profile motorists based on perceived class, we should expect vehicle status to receive significant weight in their decision-making process. Second, as noted above, VEHICLE STATUS_{it} may include additional information about a motorist's economic circumstances beyond their household income.[35]

Finally, column 5 adds trooper fixed effects. The same general pattern holds within trooper. This clarifies that differences in the set of troopers who stop low-status versus high-status motorists do not drive the estimated relationships between income and search.

Panel B examines the relationship between VEHICLE STATUS_{it} and contraband yield, using only stops that led to searches. The specifications match Panel A, except that we condition on stop location and year rather than stop location, year, and time of week. In column 1, the coefficient for VEHICLE STATUS_{it} is 2.73, indicating that a 10% increase in VEHICLE STATUS_{it} is associated with a 0.26 percentage point increase in the hit rate. Controlling for motorist race and gender (column 2) has little effect. Column 3 adds fixed effects for the motorist's number of prior misdemeanor and felony arrests. This increases the slope to 3.16; motorists with low status vehicles are more likely to have prior arrests, and those with prior arrests have higher hit rates. Column 4 adds income as an additional control. This attenuates the coefficient to 2.32, while the coefficient for income is 1.32. Finally, column 5 includes trooper fixed effects. Overall, the evidence is consistent with vehicle status and household income conveying comparable information about contraband risk.

[35]Moreover, given that we measure household income with error, VEHICLE STATUS_{it} may also provide additional signal for the motorist's true household income.

Table 4
Search Rates, Hit Rates, and Infraction Type by Vehicle Status

Panel A	Search (×100)				
	(1)	(2)	(3)	(4)	(5)
Vehicle status	-3.77	-3.28	-2.68	-2.47	-2.21
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
log household income				-0.25	-0.24
				(0.00)	(0.00)
Observations	11,006,538				
Panel B	Contraband Recovery (×100)				
Vehicle status	2.73	2.68	3.16	2.32	3.23
	(0.56)	(0.56)	(0.57)	(0.57)	(0.55)
log household income				1.32	1.20
				(0.13)	(0.12)
Observations	211,532				
Panel C	Pretextual Share (×100)				
Vehicle status	-11.61	-11.10	-10.79	-10.35	-9.49
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
log household income				-0.53	-0.49
				(0.01)	(0.00)
Observations	11,006,538				
Sgt. Area × Time of Week × Year FEs*	✓	✓	✓	✓	✓
Motorist demographics		✓	✓	✓	✓
Motorist criminal history			✓	✓	✓
Trooper FEs					✓

This table reports regression coefficients from estimates of equation (5), where the outcome is an indicator (multiplied by 100) for whether a stop leads to a search (Panel A), an indicator (multiplied by 100) for whether a search yields contraband (Panel B), or the implied pretext share (multiplied by 100) corresponding to the infraction(s) associated with the stop (Panel C). In Panel B we condition on combinations of stop location and year rather than stop location, year, and time of week. Section 4.3 describes the construction of pretext shares. Section 2.2 discusses the construction of motorist household income. Vehicle status measures expected log household income for people driving vehicles of the same make, type, and age. Robust standard errors are provided in parentheses.

Panel C relates VEHICLE STATUS_{it} to the pretext share associated with the stop. The covariates included in each column mirror those included in Panel A. The coefficient in column 1 is -11.61, indicating that a 10% increase in VEHICLE STATUS_{it} is associated with a 1.10 percentage point decrease in the pretext share associated with the stop. The magnitude of this relationship attenuates only marginally (by about 5%) in each subsequent column. In the most saturated specification (column 5), the estimate is -9.49. As with search, the coefficient on vehicle status is more than an order of magnitude larger than the coefficient on income. This is sensible because the vehicle is likely the most salient class signal that a trooper can observe prior to conducting the stop.

Next, we use our vehicle status measure to test for class discrimination.

5.1 Discrimination in Search

To test for class discrimination, we focus on motorists that we see stopped multiple times and in different vehicles. This selection may complicate the interpretation of the exercise because these motorists may differ from the general population of stopped motorists. They will tend to have driven many miles in different vehicles or commit infractions at elevated rates. This may limit external validity: it is possible that the nature of discrimination in this subsample may differ from that experienced by the broader population.

Table 5 compares motorists who are stopped once to motorists who are stopped multiple times and distinguishes repeat stops in the same vehicle from those in different vehicles. Forty percent of stops involve motorists that we only observe in one stop, 19% involve motorists who were previously stopped in the same vehicle, and 21% involve motorists who were previously stopped in a different vehicle.[36] While these groups of motorists and stops differ, these differences are generally small or moderate in magnitude. Those stopped more than once are less likely to be Black and less likely to be female. Among motorists stopped multiple times, motorists stopped in different vehicles are more likely to be Hispanic and less likely to be female. The starkest difference across groups of stops and motorists is criminal history: motorists that are stopped multiple times, and those stopped in different vehicles in particular, have more prior felony and misdemeanor arrests.

In sequential stops of the same vehicle, the average time between stops is 9 months. In sequential stops of different vehicles, the average time between stops is 17 months. The average (absolute) change in vehicle status is 19 log points. This corresponds, for example, to the difference between a two-year-old Nissan SUV and a fourteen-year-old Toyota pick-up truck. The average (absolute) change in vehicle age is 5 years.

One concern with our sample selection is that the outcome of a stop may determine whether a motorist is stopped in the future, either due to deterrence or incapacitation. In particular, we might worry that motorists that are searched in a stop are unlikely to be stopped again and that, in sequential stops, search rates in the initial stop are relatively low. Reassuringly, search rates and unconditional hit rates are in fact similar across stops.

[36] The remaining 20% of stops are the first stops for motorists that are stopped multiple times.

Table 5
Descriptive Statistics for Sequential Stops

	Single Stop	Multiple Stops Same Vehicle	Multiple Stops Different Vehicle
Black	10.62	8.42	8.33
Hispanic	30.75	30.44	35.25
White	54.71	58.97	54.41
Female	40.95	33.63	25.82
Log household income	10.62	10.56	10.55
	(0.87)	(0.91)	(0.91)
Search rate	2.03	1.81	1.93
Unconditional hit rate	0.75	0.68	0.69
Search rate in prior stop	.	1.69	1.95
Unconditional hit rate in prior stop	.	0.63	0.63
Moving	71.09	69.01	71.38
Driving while intoxicated	2.18	1.45	1.73
Speeding	57.82	58.12	59.81
Equipment	18.11	19.71	18.82
Regulatory	33.17	30.71	30.85
Prior felony arrests	0.072	0.130	0.191
	(0.536)	(0.725)	(0.885)
Prior misdemeanor arrests	0.178	0.333	0.465
	(0.958)	(1.313)	(1.592)
Vehicle status	10.60	10.58	10.59
	(0.23)	(0.21)	(0.22)
Change in vehicle status	.	-0.0197	0.0196
	(.)	(0.0293)	(0.243)
Change in vehicle age	.	0.694	-0.462
	(.)	(0.924)	(6.510)
Months between stops	.	8.678	16.68
	(.)	(10.36)	(15.26)
Absolute change in vehicle status	.	0.0198	0.185
	(.)	(0.0293)	(0.158)
Absolute change in vehicle age	.	0.694	4.865
	(.)	(0.924)	(4.350)
Observations	4,390,956	2,100,915	2,320,268

This table presents descriptive statistics for three sets of stops: stops that involve motorists that we only observe in one stop (column 1); stops that involve motorists that were previously stopped in the same vehicle (column 2); and stops that involve motorists that were previously stopped in a different vehicle (column 3).

We analyze sequential stops for the same motorist and examine how changes in search rates correspond to changes in vehicle status, VEHICLE STATUS_{it}. We estimate the bivariate regression model

$$\Delta_{it}\text{SEARCH} = \alpha + \beta\Delta_{it}\text{VEHICLE STATUS} + \epsilon_{it}. \tag{6}$$

Panel A of Figure 7 plots the results. For the same motorist, search rates are decreasing in VEHICLE STATUS_{it}. A 10% increase in status decreases the search rate by 0.07 percentage points; switching to a vehicle that doubles signaled income reduces it by 0.53 percentage points. This pattern indicates that troopers are profiling motorists based on their perceived class. The magnitude of the within-motorist relationship between vehicle status and the search rate is about a quarter of the overall relationship (corresponding to column 3 in Panel A of Table 4). We interpret this percentage as a lower bound on the share of the overall relationship explained by class discrimination given that troopers may incorporate other correlated status signals in their search decision.

One challenge in interpreting the pattern documented in Panel A of Figure 7 is that changes in search rates associated with changes in vehicle status may not be driven by vehicle characteristics, but rather by some common shock to the motorist. Motorists who buy new vehicles may experience other simultaneous changes such that their search rate would change even in the absence of a car change. We conduct four sets of robustness checks to probe this concern.

First, we conduct placebo tests that assess whether *future* or *past* changes in vehicle status predict contemporaneous changes in search rates. We conduct the first test using sequential stops of motorists in the same vehicle prior to a third stop in a different vehicle. We conduct the second test using sequential stops of motorists in the same vehicle subsequent to an initial stop in a different vehicle. Panels B and C of Figure 7 show the results of these exercises. Unlike Panel A, the relationships in Panels B and C are flat. Neither future nor past changes in vehicle status have predictive power. Motorists are not on a downward (upward) trajectory in search risk either before or after switching to a higher-status (lower-status) vehicle.

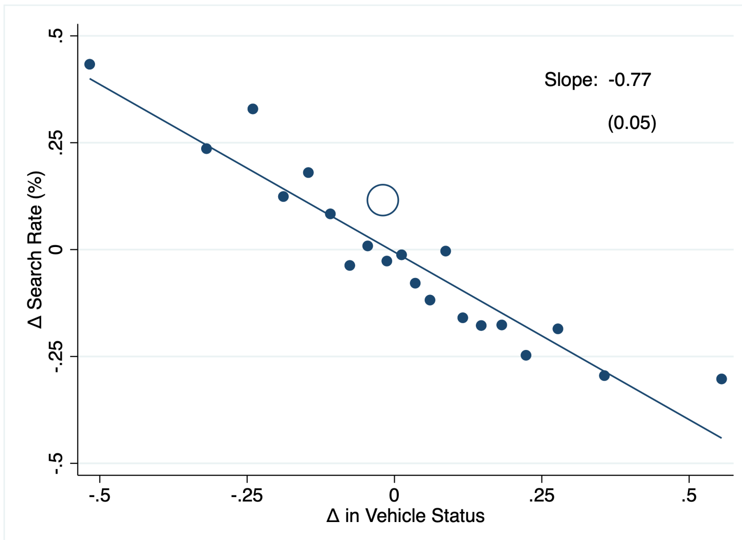
Second, we check whether results vary with the time between stops. Less time between stops leaves less time for a motorist's economic circumstances to have changed between stops. Appendix Figure C.4 partitions the results by time between stops, grouping sequential stops into terciles. For the first tercile, there is less than 7 months between stops. For the second tercile, there is between 7 and 19 months between stops. For the third tercile, there is at least 19 months between stops. The pattern and slope coefficient is essentially identical across terciles.

Third, we test whether vehicle status changes coincide with income changes, using address changes as a proxy. We find that changes in vehicle status are not substantively associated with changes in income (see Appendix Figure C.5).

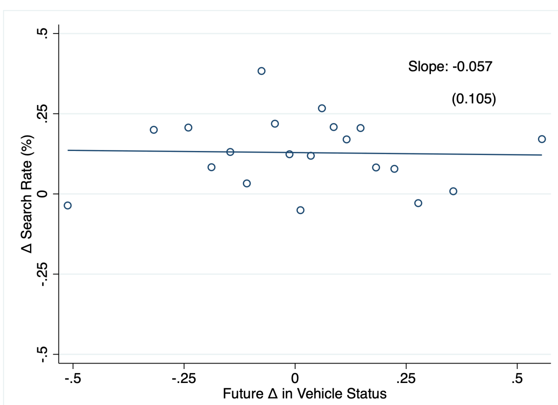
Fourth, we focus on a sample of motorists who are stopped multiple times and in alternating vehicles. We look at sequential pairs of stops in vehicle A and vehicle B where the motorist is eventually stopped again in vehicle A. The appeal of this sample is twofold. First, motorists stopped in alternating vehicles are particularly likely to be switching between household vehicles,

Figure 7
Troopers Profile Motorists at the Search Margin

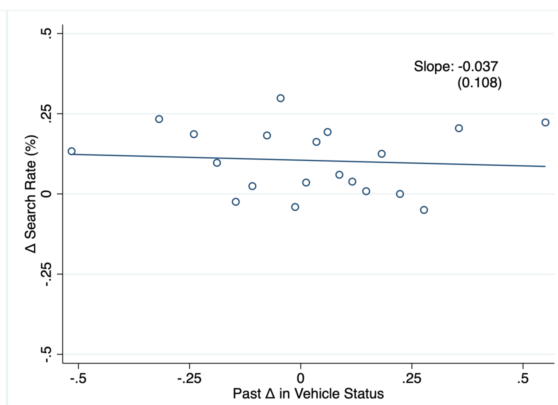
(a) Contemporaneous Change in Vehicle



(b) Future Change in Vehicle



(c) Past Change in Vehicle



Note: These figures look at first differences in search rates for sequential pairs of stops of the same motorist as a function of changes in vehicle status. Panel A plots first differences in search rates against first differences in vehicle status. The open circle depicts the change in search rates for sequential pairs of stops where the same vehicle is involved in both stops. Panel B looks at whether future changes in vehicle status predict contemporaneous changes in search rates. Panel C looks at whether past changes in vehicle status predict contemporaneous changes in search rates.

in which case the vehicle changes would not correspond to any changes in economic circumstances. Second, if changes in vehicle do coincide with some common shock that also influences search rates, then it is plausible that this shock would persist once the motorist switches back to their original vehicle. Yet if the changes in search rates are driven by the change in vehicle *per se*, those changes should be reversed if the motorist is stopped again in their original vehicle. We find evidence consistent with vehicle-based profiling (see Appendix Figure C.6). For sequential stops that involve different vehicles (from vehicle A to vehicle B), the pattern is similar to that documented in Panel A of Figure 7. Yet when we compare search rates of the motorist's original vehicle (vehicle A) before and after stops involving a different vehicle (vehicle B), the change in search rates is only weakly related to the status difference between vehicle A and vehicle B ($p = 0.09$).

A final concern is that, as we document below, motorists are stopped for different violations when driving low-status versus high-status vehicles. We interpret this as evidence of discrimination on the stop margin, consistent with the assumption that motorists do not systematically alter their behavior based on the vehicle they drive. Alternatively, if motorists commit different infractions depending on the vehicle, and troopers base their search decisions partly on the infraction itself, then variation in search rates by vehicle status could reflect differences in infractions rather than class discrimination. To address this concern, we restrict the sample to stops associated with (and likely initiated by) a non-DWI speeding violation in Appendix Figure C.7, holding the context of the stop (relatively) fixed. Speeding violations are the infraction type least associated with pretext stops (see section 4.3). As in the full sample, we find a strong negative relationship between search rates and vehicle status, reinforcing the conclusion that troopers discriminate on the search margin.

5.2 Discrimination in Pretext Stops

Following our analysis in section 5.1, we examine sequential stops of the same motorist and analyze how changes in pretext stop shares correspond to changes in vehicle status. If motorists indeed commit the same violations in their low-status and high-status vehicles, then if motorists are stopped for more discretionary infractions in their low-status vehicles, that implies they are stopped more frequently in those vehicles. In particular, if shares ρ_L and ρ_H of stops are pretextual in low-status vehicles and high-status vehicles, respectively, then the implied stop ratio is $\frac{1-\rho_H}{1-\rho_L}$.[37]

Panel A of Figure 8 presents a binscatter plot showing the relationship between changes in pretext stop shares and changes in vehicle status. For the same motorist, the pretext share is decreasing in vehicle status. This suggests that low-status vehicles are stopped in cases where high-status vehicles would not be. A 10% increase in status decreases the pretext share by 0.86 percentage points; switching to a vehicle that doubles signaled income reduces it by 6.3 percentage points. The within-motorist relationship is 85% as large as the overall relationship between

[37]Suppose R is the rate of non-pretext stops for both motorist types and T_γ is the overall stop rate for type γ motorists. Then $T_\gamma = \frac{R}{1-\rho_\gamma}$ and

$$\frac{T_L}{T_H} = \frac{\frac{R}{1-\rho_L}}{\frac{R}{1-\rho_H}} = \frac{1-\rho_H}{1-\rho_L}.$$

VEHICLE STATUS$_{it}$ and pretext share (see column 3 in Panel C of Table 4). Given an average pretext share of 10% in the sample, switching to a vehicle that doubles signaled income reduces the implied stop rate by about 9%.[38]

We conduct robustness checks similar to those in section 5.1.

First, we conduct placebo tests that assess whether future or past changes in vehicle status predict contemporaneous changes in pretext share. To test the predictive power of future changes, we analyze sequential stops of motorists in the same vehicle prior to a third stop in a different vehicle. To test the predictive power of past changes, we analyze sequential stops of motorists in the same vehicle subsequent to an initial stop in a different vehicle. Panels B and C of Figure 8 present the results. The two relationships are nearly flat and flat. The slope coefficient in Panel B is statistically significant but only about 5% as large as in Panel A. This modest relationship between contemporaneous changes in pretext share and future changes in vehicle status is driven by a small number of infractions—such as DWI offenses—that are classified as highly discretionary and carry penalties severe enough to affect subsequent vehicle status.

Our analysis assumes that motorists commit the same infractions regardless of vehicle status. However, this assumption may not hold if certain infractions are more relevant for specific vehicle types, regardless of motorist behavior. In particular, some equipment or regulatory infractions (e.g., broken tail lights or an expired license plate) may be more common for low-status vehicles, which tend to be older.[39]

As an additional robustness check, we focus on a subsample of stops associated with moving violations, which are more directly tied to driving behavior rather than vehicle characteristics.[40] We also redefine the implied pretext share using only these moving violations. In Appendix Figure C.8, we reproduce Figure 8 using this restricted sample and modified pretext share measure. The strong negative relationship between vehicle status and the pretext share persists (Panel A), while the placebo estimate (Panel B) shrinks in magnitude and is no longer statistically significant at the 5% level.[41]

Next, we test whether results vary with the time between stops. As above, we find a similar pattern for sequential pairs with more or less time between stops (see Appendix Figure C.10).

Lastly, we focus on a sample of motorists who are stopped in alternating vehicles (see Appendix Figure C.11). Once again, we find a similar pattern for sequential stops that involve different

[38]In Appendix Table C.7, we regress first differences in indicators for each violation category on first differences in vehicle status for the same motorist. Estimates suggest that increasing vehicle status leads to an increase in speeding stops and proportionate declines in stops for all other infraction categories. Appendix Table C.8 relates first differences in MPH over the speed limit for sequential speeding stops of the same motorist to changes in vehicle status. The finding that increases in vehicle status are associated with higher speeds in the stops sample is consistent with troopers setting lower infraction severity thresholds for stopping low-status motorists.

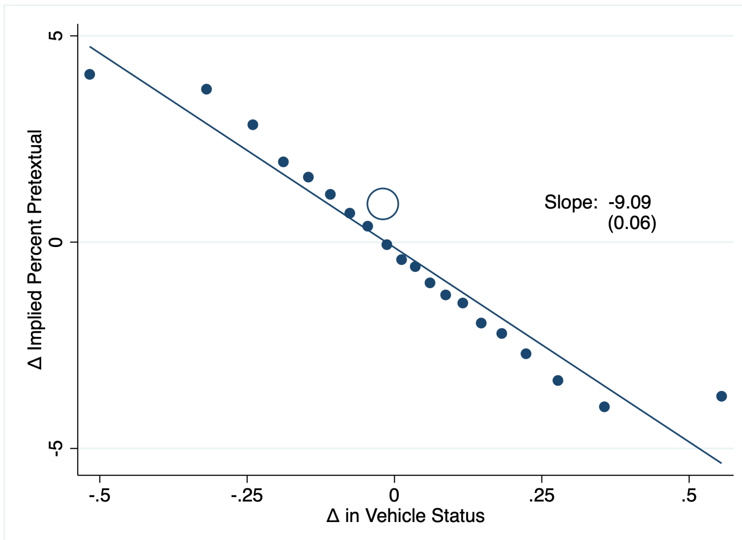
[39]These infractions may also influence future vehicle switches if, for example, motorists are more likely to change vehicles following an equipment failure or registration expiration.

[40]Appendix Table C.9 provides descriptive statistics for this sample. Motorists stopped for moving violations are more likely to be white, have higher incomes, and have fewer prior arrests. However, differences by stop history closely parallel those presented in Table 5.

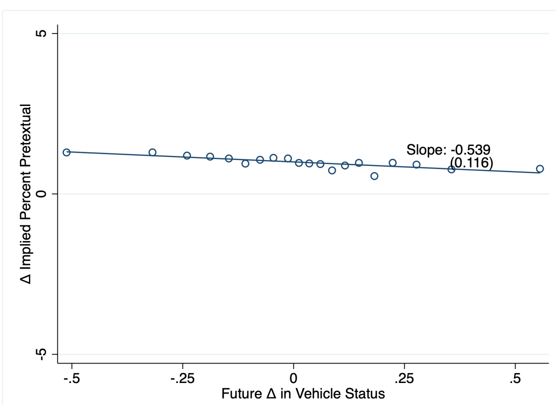
[41]In Appendix Figure C.9, we similarly restrict the sample to sequential pairs of stops with associated moving violations and reproduce Figure 7. Results remain essentially unchanged.

Figure 8
Troopers Profile Motorists at the Stop Margin

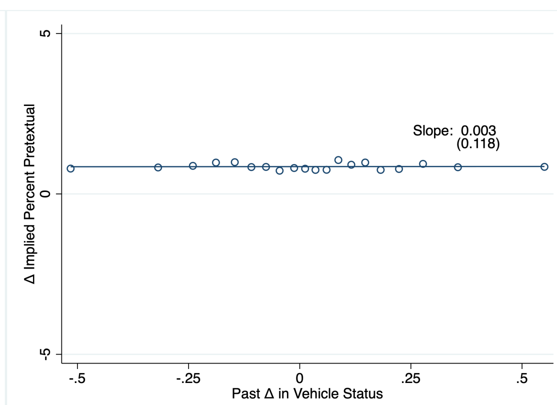
(a) Contemporaneous Change in Vehicle



(b) Future Change in Vehicle



(c) Past Change in Vehicle



Note: These figures look at first differences in the implied pretextual stop percentage for sequential pairs of stops of the same motorist as a function of changes in vehicle status. Panel A plots first differences in the implied pretext share against first differences in vehicle status. The open circle depicts the change in pretext share for sequential pairs of stops where the same vehicle is involved in both stops. Panel B looks at whether future changes in vehicle status predict contemporaneous changes in pretext share. Panel C looks at whether past changes in vehicle status predict contemporaneous changes in pretext share.

vehicles (from vehicle A to vehicle B). When we compare pretext shares for stops involving the motorist's original vehicle (vehicle A) before and after stops involving a different vehicle (vehicle B), the change in pretext share is close to zero and its relationship to the status difference between vehicle A and vehicle B is statistically insignificant at the 5% level.

5.3 Identifying Hit Rates at the Margin

Our analysis demonstrates that troopers profile motorists based on vehicle status, leading them to search low-income motorists more frequently. Since hit rates increase with income, this profiling likely reduces contraband yield. To test this hypothesis directly, we use our within-motorist research design and estimate marginal hit rates using changes in vehicle status as an instrument for search. We then compare marginal hit rates between low- and high-income motorists.[42] Our approach relies on two assumptions (Imbens and Angrist, 1994): (1) a motorist's decision to carry contraband does not depend on vehicle status (*independence*) and (2) for every motorist, an increase in vehicle status weakly decreases the probability of search (*monotonicity*).

We estimate the following model using just-identified two-stage least squares (2SLS), separately for low- and high-income motorists:

$$\Delta_{it}\text{CONTRABAND} = \beta\Delta_{it}\text{SEARCH} + \epsilon_{it}, \tag{7}$$

where the first stage is

$$\Delta_{it}\text{SEARCH} = \pi\Delta_{it}\text{VEHICLE STATUS} + \zeta_{it}. \tag{8}$$

We divide sequential stop pairs by income tercile, based on the motorist's income in the first stop, and focus on the bottom and top terciles. Appendix Table C.10 presents descriptive statistics separately for these groups. We confirm that search and hit rates remain stable between prior and subsequent stops within each income group. Differences across income bins in vehicle status are an order of magnitude smaller than differences in log household income.

Table 6 presents first stage, reduced form, and 2SLS estimates. The effect of vehicle status on search rates is similar for low- and high-income motorists. However, the marginal hit rate for high-income motorists is more than twice as high (36% vs. 15%).[43] Reallocating marginal searches of low-income motorists to high-income motorists would increase contraband yield in this population of motorists who are stopped multiple times.[44] Given that this population is comparable to the general population of stopped motorists, and average hit rates also decline with household income, our findings suggest that marginal hit rates are likely decreasing with income in the broader population.

[42] Our strategy builds on Arnold et al. (2018), who use quasi-random assignment of bail judges with varying release propensities to estimate pre-trial misconduct rates for white and black defendants at the margin of release.

[43] This difference is statistically significant at the 5% level.

[44] Even if troopers lack precise information on income, the fact that income predicts search rates, even conditional on vehicle status, suggests that reallocating searches is feasible.

Table 6
Marginal Hit Rate Is Increasing in Motorist Income

Outcome:	Δ Search (×100)		Δ Contraband Recovery (×100)			
	Bottom Tercile	Top Tercile	Bottom Tercile	Top Tercile	Bottom Tercile	Top Tercile
Δ Vehicle status	-0.64	-0.77	-0.09	-0.28		
	(0.10)	(0.07)	(0.06)	(0.05)		
Δ Search					0.15	0.36
					(0.08)	(0.05)
Model	OLS	OLS	OLS	OLS	2SLS	2SLS
Observations	897,223	657,914	897,223	657,914	897,223	657,914

This table relates first differences in search rates and unconditional hit rates for sequential pairs of stops of the same motorist to changes in vehicle status. Sequential pairs of stops are divided into tercile groups based on the motorist's household income in the initial stop. The table reports estimates for the bottom and top terciles. Standard errors are clustered at the motorist level.

6 Trooper Objectives and Hassle Costs

Researchers studying search behavior typically assume that troopers seek to maximize contraband yield. However, we have shown that current search patterns do not align with this objective. While trooper behavior could stem from prejudice or inaccurate beliefs about which motorists are most likely to carry contraband, it may also reflect objectives beyond contraband yield or income-based differences in search costs.

The standard measure of hit rates—whether any contraband is discovered—may obscure differences in the significance of the contraband found. To explore this, Appendix Table C.11 presents two alternative measures: (1) the expected sentence length (in days) associated with arrest charges following contraband discovery and (2) an indicator for whether this expected sentence length exceeds the median among stops that result in contraband discovery and arrest.[45] For each alternative outcome, we identify a negative relationship between the hit rate and vehicle status when we condition on combinations of stop location and year (column 1) and add controls for motorist race and gender (column 2). However, adding fixed effects for the motorist's number of prior misdemeanor and felony arrests in column 3 reverses the estimated relationship between vehicle status and each alternative hit rate measure; we continue to identify a significant positive relationship between vehicle status and each hit rate measure when we include income as an additional control (column 4) and add trooper fixed effects (column 5). This pattern of findings reflects the fact that motorists with lower status vehicles are more likely to have prior arrests, and those with prior

[45] To construct the expected sentence length associated with an arrest, we first calculate the average sentence length associated with a given charge (conditional on conviction) and then take the sum across all charges associated with the given arrest. The mean (median) expected sentence length conditional on arrest is 396 days (132 days). It is worth noting that the arrest and charging decisions involve some degree of trooper discretion and that the reliance on external CCH data to construct these alternative hit rate measures means that not all arrests associated with stops will be correctly matched in practice.

arrests have higher hit rates across the alternative hit rate measures used. Notably, these same arrest history data are available to troopers who are conducting traffic stops. As such, the findings that motorists with lower vehicle status and lower income are searched more frequently conditional on arrest history (see Table 4) indicate that troopers' search behavior cannot be rationalized by an objective function that simply places more weight on more significant contraband.

Another possibility is that asset forfeiture laws create financial incentives influencing trooper search decisions. While the evidentiary bar for successful forfeiture prosecution in Texas is low—criminal conviction is not required—Texas Highway Patrol troopers' private financial incentives are limited in practice. In particular, a substantial majority of forfeited funds are obtained through federal prosecutions and the Department of Public Safety (DPS) does not typically benefit financially from cases that are federally prosecuted (Slayton, 2014).[46] Even when forfeitures are successfully prosecuted by the state, the use of seized assets is determined by central DPS administrators rather than local or regional Texas Highway Patrol leadership.

We propose an alternative explanation: troopers may adjust their search decisions based on anticipated "hassle costs" associated with adjudicating contraband-related arrests. In Texas, criminal defense attorneys may seek trooper testimony during pre-trial proceedings, including motions to suppress evidence, quash charges, or request an examining trial to establish probable cause when the accused is charged with a felony (Texas Code of Criminal Procedure, Chs. 16, 28). Although we are not aware of any systematic data on the frequency with which peace officers in Texas are required to testify in court proceedings, procedural guidelines in local police officer manuals and archived case records frequently make reference to officer testimony before and at trial (see, for example, San Antonio Police Department: General Manual, 2021, Court of Criminal Appeals of Texas, 2012). Although officers are typically paid overtime for off-duty court appearances, Chalfin and Goncalves (2020) find evidence that Dallas police officers are, on average, averse to working overtime. Officers' aversion to court appearances may be greater both because these appearances will typically be more disruptive (i.e., less likely to immediately precede or follow a shift) and because associated interactions are often adversarial in nature. Indeed, prior research and officer testimonials emphasize that court appearances worsen officer mental health (Newell et al., 2022, Boyce, 2006).[47]

To the extent that troopers anticipate and respond proactively to expected hassle costs, one reason troopers may be less aggressive in searching high-income motorists is that, if the trooper finds contraband, high-income motorists may be more likely to contest any associated charges. A defining feature of the criminal justice system is courts' provision of assigned counsel to defendants classified as indigent, typically based on defendant net income. In Texas, most indigent defense is provided by private attorneys who are hired on a case-by-case basis, as opposed to public defenders

[46] Texas Highway Patrol is a division of the Texas Department of Public Safety.

[47] A related search cost that troopers face is the risk of a civilian complaint for malfeasance. Although we have not been able to obtain data on such complaints, it is plausible that the rate at which complaints are sustained is increasing in motorist status. Ba (2020) finds that, for civilian complaints filed against officers of the Chicago Police Department, Black complainants are less likely to have their complaints sustained than Hispanic and White complainants.

(Satija, 2019). Prior research has demonstrated that publicly-appointed defense attorneys achieve worse case outcomes for defendants than privately-retained attorneys across a number of margins (Agan et al., 2021, Cohen, 2014). Survey data drawn from a sample of defense attorneys in Bexar County, Texas further highlight that felony cases in which defendants privately retain attorneys involve significantly more pre-trial motions, require significantly more hearings, and involve nearly two times as many attorney hours as cases handled by publicly-appointed attorneys (Agan et al., 2021). Most relevant to our study, Agan et al. (2021) find that Bexar County cases involving privately-retained attorneys are nearly 20 percentage points less likely to result in conviction via guilty or no contest plea.

Given that low-income motorists are more likely to rely on publicly-appointed counsel, troopers likely face higher hassle costs after arresting high-income motorists. To more rigorously probe how hassle costs vary with motorist income in our sample of motorists arrested after contraband discovery, we relate motorist income to two courts-based measures that proxy for hassle costs. First, we examine the rate at which defendants plead "guilty" or "no contest" to associated charges. While Texas Department of Public Safety data do not allow us to directly investigate how trooper court appearance rates vary with motorist income, we expect that guilty and no contest pleas will reduce the demand for trooper testimony by precluding the need for trial proceedings and pre-trial hearings. Second, we examine the rate at which charges are dismissed or result in an acquittal. Dismissals and acquittals are more likely when troopers' actions or testimony are successfully challenged (for instance, related to the legality of a stop or search), suggesting increased hassle costs.[48]

Panel A of Figure 9 shows that guilty/no contest plea rates for motorists arrested after they are found with contraband decline as motorist income increases. Because our sample of motorists arrested after contraband discovery is relatively small, in Panel B of Figure 9 we also compare plea rates for all drug arrests in the criminal history data, not just those related to traffic stops. We limit the sample to the 11 most common drug offenses associated with contraband-related arrests in the traffic stop data.[49] The pattern in this much larger sample of arrests is similar to what we see for arrests resulting from motor vehicle searches in Panel A. A 10% increase in income is associated with a 0.23 percentage point decrease in the guilty or no contest plea rate.

In Appendix Figure C.12, we document that dismissal or acquittal rates are similarly increasing

[48]We employ this secondary outcome because it captures additional information on the disposition margin to the extent that "not guilty" pleas ultimately result in conviction or deferred judgment (the correlation between the guilty/no contest plea outcome and the dismissal/acquittal outcome is -0.83). Moreover, twenty percent of pleas are recorded as "unknown." We treat these pleas as equivalent to "not guilty" pleas because disposition outcomes are nearly identical (85.9% of not guilty pleas are associated with dismissal or acquittal as compared to 84.3% of unknown pleas). The fact that we arrive at similar conclusions regardless of whether we focus on plea- or disposition-based outcomes is reassuring.

[49]These offenses are: possession of 2 ounces or less of marijuana; possession of 1 gram or less of a controlled substance penalty group 1; possession of 1 gram or less of a controlled substance penalty group 2; possession of 2 ounces or less of a controlled substance penalty group 2-A; possession of 28 grams or less of a controlled substance penalty group 3; possession of 28 grams or less of a controlled substance penalty group 4; possession of between 5 and 50 pounds of marijuana; possession of a dangerous drug; possession with intent to deliver a controlled substance penalty group 1, between 4 and 200 grams; possession of controlled substance not in penalty group; prohibited substance in a correctional facility.

in income in both the motorist sample and the sample of all those arrested for the most common contraband-related drug offenses.

If troopers are responding to anticipated hassle costs when making search decisions, search rates should be falling in expected hassle costs, all else equal. To test this prediction, we focus on cross-county variation in the same two courts-based measures.[50] The idea is that troopers should conduct more searches in jurisdictions where, due to local institutional factors, expected hassle costs are lower. We first isolate the contributions of counties to court outcomes conditional on charge and defendant characteristics. To do so, we closely follow the approach employed in Feigenberg and Miller (2021), estimating models of the following form:

$$Y_{ict} = \alpha_{cth(i,t)} + X_i\Gamma^x + Z_{it}\Gamma^z + \Theta_{j(i,c,t)} + \epsilon_{ict}. \tag{9}$$

Here, i indexes individuals, c indexes the specific contraband-related charge, t indexes year, $h(i,t)$ is a measure of criminal history at time t for individual i defined based on Texas criminal statutes, and $j(i,c,t)$ represents the county in which charges were filed. Y_{ict} represent one of our two alternative proxies for charge-related hassle costs: (1) whether the charge results in a guilty or no contest plea and (2) whether the charge is ultimately dismissed or results in an acquittal. $\alpha_{cth(i,t)}$ are specific charge by defendant criminal history by year fixed effects; X_i represents controls for defendant race, ethnicity and gender; Z_{it} represents defendant age and age squared. $\Theta_{j(i,c,t)}$ is the set of county fixed effects of interest. In alternative models, we replace time-invariant demographic controls with individual defendant fixed effects. We construct these county-level courts-based measures again using all arrests in the criminal history data for the 11 most common drug offenses associated with contraband-related arrests in the traffic stop data.[51]

To relate these county-level measures of anticipated hassle costs to search rates, we estimate analogous models in the traffic stop data that residualize our search outcome using motorist and contextual characteristics:

$$Y_{ict} = \alpha_{\tau(t)y(t)} + X_i\Gamma^x + \Theta_{j(i,c,t)} + \epsilon_{ict}. \tag{10}$$

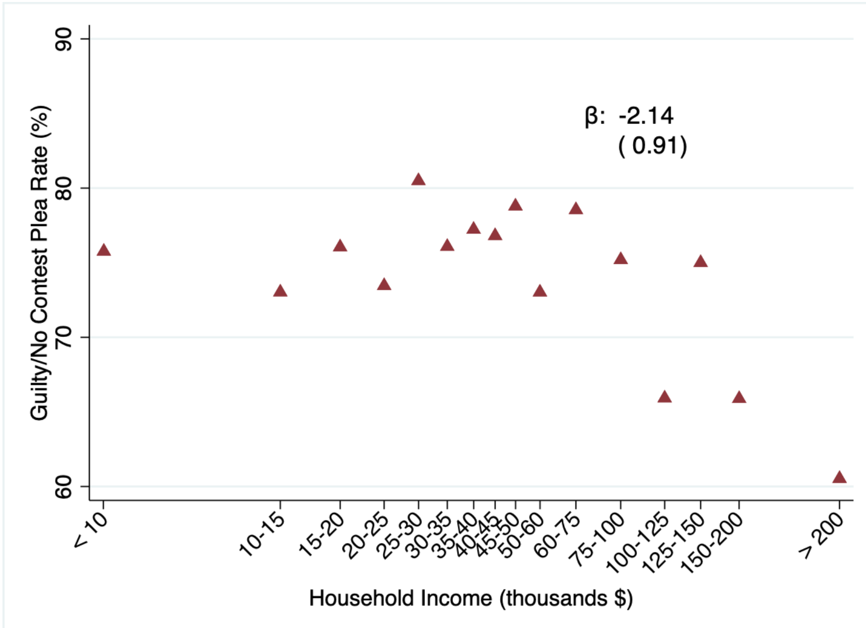
Terms are defined as above, with Y_{ict} now representing an indicator for whether a stop results in a search and $\alpha_{\tau(t)y(t)}$ characterizing year-by-stop time (quarter of day, weekday or weekend) fixed effects. In alternative models, we replace time-invariant demographic controls with individual motorist fixed effects.

[50] In Texas, criminal cases are handled in District and County Courts, with most courts serving a single county.

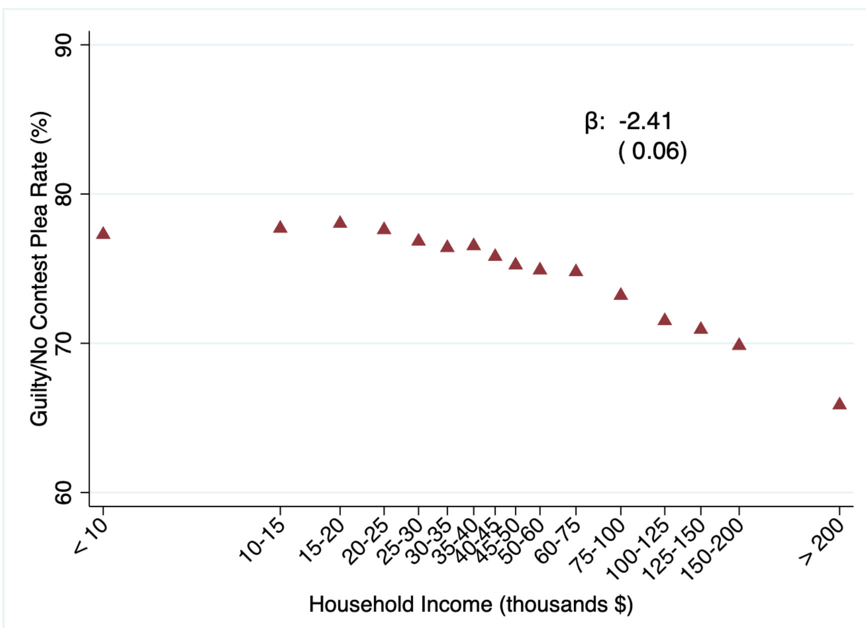
[51] The sample of motorists arrested after contraband discovery is relatively small and the number of counties in Texas (254) is large.

Figure 9
Guilty/No Contest Plea Rates are Decreasing in Motorist Income

(a) DPS Searches



(b) All Drug Arrests



Note: These figures plot guilty or no contest plea rates as a function of motorist income. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. In Panel A the sample includes traffic stops that lead to a search, contraband recovery, and arrest. In Panel B the sample is all arrests in the CCH data for those drug charges most commonly associated with contraband-related arrests in the traffic stop data. See footnote 49 for details.

Table 7
County-level Search Rates and Hassle Costs

	Outcome: County-level Residual Search Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
County-level Residual	0.015	0.014	0.014			
Guilty/No Contest Plea Rate	(0.005)	(0.004)	(0.004)			
County-level Residual				-0.018	-0.017	-0.016
Dismissal/Acquittal Rate				(0.005)	(0.004)	(0.004)
Residualized on Motorist FEs		✓	✓		✓	✓
Residualized on Defendant FEs			✓			✓
Dependent Variable Mean	0.019	0.020	0.020	0.019	0.020	0.020
Observations	225					

This table reports results from regressing county-level residual search rates (derived based on equation 10) on county-level residual guilty/no contest plea rates and residual dismissal/acquittal rates (derived based on equation 9). In columns 2-3 and 5-6, the county-level search rate is constructed conditional on motorist fixed effects in addition to the controls included in equation 10. In columns 3 and 6, county-level measures are constructed conditional on defendant fixed effects in addition to the controls included in equation 9. The sample excludes counties with fewer than 100 observations in the CCH data. Robust standard errors are provided in parentheses.

In Table 7 we relate county-level search rates to county-level guilty/no contest plea rates (or dismissal/acquittal rates) in simple bivariate regressions. Point estimates indicate that a 10 percentage point increase in the guilty/no contest plea rate is associated with a 0.14–0.15 percentage point (7–8 percent) increase in the residual search rate. Likewise, a 10 percentage point decrease in the dismissal/acquittal rate corresponds to a 0.16–0.18 percentage point (8–9 percent) increase in the residual search rate. In sum, residual search rates are lower where expected hassle costs are higher. This finding, along with the evidence presented above that low-income motorists are searched more frequently and are expected to impose lower hassle costs as measured by our pleading- and disposition-based proxies, is consistent with troopers responding to differences in hassle costs faced after successful searches of low-income versus high-income motorists.

7 Discussion

We document substantial differences in how Texas state troopers interact with low- and high-income motorists. Troopers are more likely to search low-income motorists, even though these searches yield contraband less often than searches of high-income motorists. We also find that low-income motorists are stopped for more discretionary infractions, those often associated with pretext stops. To test whether these disparities reflect class-based discrimination, we develop a quasi-experimental research design that isolates how the same motorist is treated when driving vehicles that convey different class signals. Motorists are more likely to be searched when stopped in a low-status vehicle and are also stopped for more discretionary infractions, suggesting they are more likely to be stopped when driving one.

To our knowledge, this is the first study to systematically document class disparities in traffic stops and searches and to identify the causal effect of perceived motorist class on trooper behavior. One reason for the lack of prior evidence is that many law enforcement agencies track and report data on stops, searches, and arrests by race but do not collect similar information based on economic class. Our findings highlight the need for more systematic data collection on class disparities in policing.

A key question is what drives the sizable class disparities we observe. Our findings suggest that hassle costs associated with court-mandated officer appearances may play a role. Future research that further unpacks why trooper behavior varies with motorist income could inform how to address these disparities.

References

Aaronson, Daniel, Sumit Agarwal, and Eric French, "The spending and debt response to minimum wage hikes," *American Economic Review*, December 2012, *102* (7), 3111–3139.

Abrams, David S, Marianne Bertrand, and Sendhil Mullainathan, "Do judges vary in their treatment of race?," *The Journal of Legal Studies*, 2012, *41* (2), 347–383.

Adams, William, Liran Einav, and Jonathan Levin, "Liquidity Constraints and Imperfect Information in Subprime Lending," *American Economic Review*, March 2009, *99* (1), 49–87.

Agan, Amanda, Matthew Freedman, and Emily Owens, "Is Your Lawyer a Lemon? Incentives and Selection in the Public Provision of Criminal Defense," *Review of Economics and Statistics*, May 2021, *98* (2), 294–309.

Aggarwal, Pradhi, Alec Brandon, Ariel Goldszmidt, Justin Holz, John A List, Ian Muir, Greg Sun, and Thomas Yu, "High-frequency location data shows that race affects the likelihood of being stopped and fined for speeding," *University of Chicago, Becker Friedman Institute for Economics Working Paper*, 2022, (2022-160).

Anbarci, Nejat and Jungmin Lee, "Detecting racial bias in speed discounting: Evidence from speeding tickets in Boston," *International Review of Law and Economics*, 2014, *38*, 11–24.

Antonovics, Kate and Brian G. Knight, "A New Look at Racial Profiling: Evidence fom the Boston Police Department," *Review of Economics and Statistics*, February 2009, *91* (1), 163–177.

Anwar, Shamena and Hanming Fang, "An Alternative Test of Racial Prejudice in Motor Vehicle Searches: Theory and Evidence," *American Economic Review*, March 2006, *96* (1), 127–151.

Arnold, David, Will Dobbie, and Crystal S. Yang, "Racial Bias in Bail Decisions," *Quarterly Journal of Economics*, November 2018, *133* (4), 1885–1932.

_ , _ , and Peter Hull, "Measuring Racial Discrimination in Bail Decisions," *American Economic Review*, September 2022, *112* (9), 2992–3038.

Ayres, Ian, "Outcome Tests of Racial Disparities in Police Practices," *Justice Research and Policy*, 2002, *4*, 131–142.

Ba, Bocar A., "Going the Extra Mile: the Cost of Complaint Filing, Accountability, and Law Enforcement Outcomes in Chicago," November 2020. Working paper.

Bertrand, Marriane and Esther Duflo, "Chapter 8 - Field Experiments in Discrimination," in Abhijit Vinayak Banerjee and Esther Duflo, eds., *Handbook of Economic Field Experiments*, Vol. 1, Elsevier, 2017, pp. 309–393.

Besbris, Max, Jacob William Faber, Peter Rich, and Patrick Sharkey, "Effect of Neighborhood Stigma on Economic Transactions," *PNAS*, 2015, *112* (16), 4994–4998.

Bjornsdottir, R. Thora and Nicholas O. Rule, "The Visibility of Social Class From Facial Cues," *Journal of Personality and Social Psychology*, 2017, *113* (4), 530–546.

Boyce, James, "Police Officers Under Stress," 2006. Criminal Justice Institute, University of Arkansas System, School of Law Enforcement Supervision.

Cai, William, Johann Gaebler, Justin Kaashoek, Lisa Pinals, Samuel Madden, and Sharad Goel, "Measuring racial and ethnic disparities in traffic enforcement with large-scale telematics data," *PNAS nexus*, 2022, *1* (4), pgac144.

Cattaneo, Matias, Richard Crump, Max Farrell, and Yingjie Feng, "On Binscatter," 2019. Unpublished manuscript.

Chalfin, Aaron and Felipe Goncalves, "The Pro-Social Motivations of Police Officers," Working Paper, National Bureau of Economic Research November 2020.

Charles, Kerwin Kofi and Jonathan Guryan, "Studying Discrimination: Fundamental Challegences and Recent Progress," *Annual Review of Economics*, September 2011, *3*, 479–511.

Chen, M. Keith, Katherine L. Christensen, Elicia John, Emily Owens, and Yilin Zhuo, "Smartphone Data Reveal Neighborhood-Level Racial Disparities in Police Presence," *Review of Economics and Statistics*, forthcoming.

Clair, Matthew, *Privilege and Punishment: How Race and Class Matter in Criminal Court*, Princeton University Press, 2020.

Close, Billy R. and Patrick Leon Mason, "Searching for Efficienct Enforcement: Officer Characteristics and Racially Biased Policing," *Review of Law and Economics*, 2007, *3* (2), 263–321.

Cohen, Thomas, "Who Is Better at Defending Criminals? Does Type of Defense Attorney Matter in Terms of Producing Favorable Case Outcomes?," *Criminal Justice Policy Review*, 2014, *25*, 29–58.

Collister, Brian, "Texas Troopers Ticketing Hispanic Drivers as White," Kxan Investigates, November 6, 2015. `http://www.kxan.com/news/investigations/texas-troopers-ticketing-hispanics-motorists-as-white/1156475533` [Accessed: 2021-04-06].

Court of Criminal Appeals of Texas, "Vanessa M. Mendoza v. The State of Texas," May 2012. [Accessed: 2023-03-09].

Davis, Elizabeth, Anthony Whyde, and Lynn Langton, "Contacts Between Police and the Public, 2015," Special Report, Bureau of Justice Statistics, October 2018.

Diamond, Rebecca, Tim McQuade, and Franklin Qian, "The Effects of Rent Control Expansion on Tenants, Landlords, and Inequality: Evidence from San Francisco," *American Economic Review*, September 2019, *109* (9), 3365–3394.

Epp, Charles R., Steven Maynard-Moody, and Donald P. Haider-Markel, *Pulled Over: How Police Stops Define Race and Citizenship*, Chicago: University of Chicago Press, 2014.

Fagan, Jeffrey, Amanda Geller, Garth Davies, and Valerie West, "Street Stops and Broken Windows Revisited: The Demography and Logic of Proactive Policing in a Safe and Changing City," in Stephen Rice and Michael White, eds., *Race, Ethnicity, and Policing: New and Essential Readings*, New York University Press, 2010.

Feigenberg, Benjamin and Conrad Miller, "Racial Divisions and Criminal Justice: Evidence from Southern State Courts," *American Economic Journal: Economic Policy*, 2021, *2* (13), 49–113.

_ and _ , "Would Eliminating Racial Disparities in Motor Vehicle Searches Have Efficiency Costs?," *Quarterly Journal of Economics*, 2022, *137* (1), 49–113.

Finlay, Keith, Matthew Gross, Elizabeth Luh, and Michael Mueller-Smith, "The Impact of Financial Sanctions: Regression Discontinuity Evidence from Driver Responsibility Fee Programs in Michigan and Texas," January 2023. Unpublished manuscript.

Fischman, Joshua B and Max M Schanzenbach, "Racial disparities under the federal sentencing guidelines: The role of judicial discretion and mandatory minimums," *Journal of Empirical Legal Studies*, 2012, *9* (4), 729–764.

Fryer, Roland, "An Empirical Analysis of Racial Differences in Police Use of Force," *Journal of Political Economy*, June 2019, *127* (3), 1210–1261.

Glied, Sherry and Matthew Niedell, "The Economic Value of Teeth," *Journal of Human Resources*, 2010, *45* (2), 468–496.

Goncalves, Felipe and Steve Mello, "A Few Bad Apples? Racial Bias in Policing," *American Economic Review*, May 2021, *111* (5), 1406–41.

Graef, Lindsay, Sandra Gabriel Mayson, Aurelie Ouss, and Megan Stevenson, "Systemic Failure To Appear in Court," *University of Pennsylvania Law Review*, 2023, *172*, 1–60.

Grogger, Jeffrey and Greg Ridgeway, "Testing for Racial Profiling in Traffic Stops From Behind a Veil of Darkness," *Journal of the American Statistical Association*, 2006, *101* (475), 878–887.

Gupta, Arpit, Christopher Hansman, and Ethan Frenchman, "The Heavy Costs of High Bail: Evidence from Judge Randomization," *Journal of Legal Studies*, 2016, *45*, 471–505.

Heckman, James J. and Peter Siegelman, "The Urban Institute audit studies: Their methods and findings," in "Clear and convincing evidence: Measurement of discrimination in America," Urban Institute Press, 1993, pp. 187–258.

Hoekstra, Mark and CarlyWill Sloan, "Does race matter for police use of force? Evidence from 911 calls," *American economic review*, 2022, *112* (3), 827–860.

Horrace, William C. and Shawn M. Rohlin, "How Dark Is Dark? Bright Lights, Big City, Racial Profiling," *Review of Economics and Statistics*, May 2016, *98* (2), 226–232.

Imbens, Guido W. and Joshua D. Angrist, "Identification and Estimation of Local Average Treatment Effects," *Econometrica*, 1994, *62* (2), 467–475.

Knowles, John, Nicola Persico, and Petra Todd, "Racial Bias in Motor Vehicle Searches: Theory and Evidence," *Journal of Political Economy*, February 2001, *109* (1), 203–229.

Knox, Dean, Will Lowe, and Jonathan Mummolo, "Administrative Records Mask Racially Biased Policing," *American Political Science Review*, 2020, *114* (3), 619–637.

Kraus, Michael W. and Daeher Keltner, "Signs of Socioeconomic Status," *Psychological Science*, 2009, *20* (1), 99–106.

_ , Brittany Torrez, Jun Won Park, and Fariba Ghayebi, "Evidence from the reproduction of social class in brief speech," *Proceedings of the National Academy of Sciences*, 2019, *116* (46), 22998–23003.

Kraus, Michael W, Jun Won Park, and Jacinth J.X. Tan, "Signs of social class: the experience of economic inequality in everyday life," *Perspectives on Psychological Science*, 2017, *12* (3), 422–435.

Lieberman, Carl, Elizabeth Luh, and Michael Mueller-Smith, "Criminal court fees, earnings, and expenditures: A multi-state RD analysis of survey and administrative data," January 2023. Unpublished manuscript.

Luh, Elizabeth, "Not So Black and White: Uncovering Racial Bias from Systematically Misreported Trooper Reports," 2020. Unpublished manuscript.

MacDonald, John, "Race, Crime, and Police Interaction," September 26, 2021. Conference Paper: Federal Reserve Bank of Boston Economic Research Conference Series: Racial Disparities in Today's Economy, 64th Economic Conference.

Makowsky, Michael D., "A Proposal to End Regressive Taxation Through Law Enforcement," Policy Proposal 2019-06, The Hamiliton Project 2019.

Marx, Philip, "An Absolute Test of Racial Prejudice," *Journal of Law, Economics, and Organization*, March 2022, *38* (1), 42–91.

Mello, Steven, "Fines and Financial Wellbeing," March 2021. Unpublished manuscript.

Mian, Atif, Kamalesh Rao, and Amir Sufi, "Household Balance Sheets, Consumption, and Economic Slump," *Quarterly Journal of Economics*, 2013, *128* (4), 1687–1726.

Muller, Christopher and Alexander F. Roehrkasse, "Racial and Class Inequality in U.S. Incarceration in the Early Twenty-First Century," *Social Forces*, 2022, *101* (2), 803–828.

_ **and Alexander F Roehrkasse**, "Falling racial inequality and rising educational inequality in US prison admissions for drug, violent, and property crimes," *Proceedings of the National Academy of Sciences*, 2025, *122* (4).

Mustard, David B, "Racial, ethnic, and gender disparities in sentencing: Evidence from the US federal courts," *The Journal of Law and Economics*, 2001, *44* (1), 285–314.

Nathan, Brad, Ricardo Perez-Truglia, and Alejandro Zentner, "My Taxes Are Too Darn High: Why Do Households Protest Their Taxes?," Working Paper, National Bureau of Economic Research September 2020.

Nelissen, Rob M.A. and Marijn H.C. Meijers, "Social benefits of luxury brands as costly signals of wealth and status," *Evolution and Human Behavior*, 2011, *32* (5), 343–355.

Newell, Caitlin J., Rosemary Ricciardelli, Stephen M. Czarnuch, and Krystle Martin, "Police staff and mental health: barriers and recommendations for improving help-seeking," *Police Practice and Research*, 2022, *23* (1), 111–124.

Peterman, Danieli Evans, "Socioeconomic Status Discrimination," *Virginia Law Review*, 2018, *104*, 1283–1359.

Pettit, Becky and Bruce Western, "Mass Imprisonment and the Life Course: Race and Class Inequality in U.S. Incarceration," *American Sociological Review*, 2004, *69* (2), 151–169.

Phillips, David, "Measuring Housing Stability with Consumer Reference Data," *Demography*, 2020, *57* (4), 1323–1344.

Pierson, Emma, Camelia Simoiu, Jan Overgoor, Sam Corbett-Davies, Daniel Jenson, Amy Shoemaker, Vignesh Ramachandran, Phoebe Barghouty, Cheryl Phillips, Ravi Shroff, and Sharad Goel, "A Large-scale Analysis of Racial Disparities in Police Stops Across the United States," *Nature Human Behaviour*, 2020, *4*, 736–745.

Rehavi, M. Marit and Sonja B. Starr, "Racial Disparity in Federal Criminal Sentences," *Journal of Political Economy*, 2014, *122* (6).

Rivera, Lauren and András Tilcsik, "Class Advantage, Commitment Penalty: The Gendered Effect of Social Class Signals in an Elite Labor Market," *American Sociological Review*, 2006, *81* (6), 1097–1131.

Robison, Sophia, *Can Delinquency Be Measured?*, Chicago: Columbia University Press, 1936.

San Antonio Police Department: General Manual, "Procedure 311- Court Appearances," November 2021. `https://www.sanantonio.gov/Portals/0/Files/SAPD/GeneralManual/311.pdf` [Accessed: 2023-03-09].

Satija, Neena, "How judicial conflicts of interest are denying poor Texans their right to an effective lawyer," The Texas Tribune, August 19, 2019. `https://www.texastribune.org/2019/08/19/unchecked-power-texas-judges-indigent-defense/` [Accessed: 2023-03-09].

Slayton, David, "Asset Forfeiture in Texas: DPS and County Interactions," Technical Report, Office of Court Administration 2014.

Smith, Douglas, "The Neighborhood Context of Police Behavior," *Crime and Justice: Review of Research*, 1986, *8*, 313–342.

Texas Code of Criminal Procedure, "Chapter 16. The Commitment or Discharge of the Accused." `https://statutes.capitol.texas.gov/Docs/CR/htm/CR.16.htm` [Accessed: 2023-03-09].

_ , "Chapter 28. Motions, Pleadings, and Exceptions." `https://statutes.capitol.texas.gov/Docs/CR/htm/CR.28.htm` [Accessed: 2023-03-09].

Tuttle, Cody, "Racial Disparities in Federal Sentencing: Evidence from Drug Mandatory Minimums," 2023. Unpublished manuscript.

Western, Bruce, *Punishment and Inequality in America*, New York: Russell Sage Foundation, 2006.

Yang, Crystal S, "Free at last? Judicial discretion and racial disparities in federal sentencing," *The journal of legal studies*, 2015, *44* (1), 75–111.

Online Appendix: Class Disparities and Discrimination in Traffic Stops and Searches

Ben Feigenberg

Conrad Miller

March 2025

A Data Appendix

We merge traffic stop data to commercial address history data from Infogroup using full name and address. We first use an address standardization algorithm, the Stata function `stnd_address`, to ensure that addresses are structured analogously across the two data sets, with separate fields for street address, unit number, etc. We also extract the address number. In addition, we manually standardize Texas city and town names in the traffic stop data. We standardize full names and extract suffixes. We then use the Stata command `reclink2` to perform a probabilistic linkage across the two data sources. We fuzzy match using the following fields: last name, first name, middle name, suffix, address number, street name, city, and zip code. We require that observations match exactly on the first letter of the first name and the first letter of the last name. For zip code, we define agreement discretely based on whether the fields match exactly. For all other fields, we utilize the bigram string comparator to assess the degree of agreement. The address history data includes an identifier that matches the same individual to multiple addresses. We use this identifier to match multiple stops to the same person. We are able to match 75% of stops to the address history data. For stops that we are unable to match, we create identifiers based on full name, street address, and zip code.

We then match the criminal history data to traffic stops using the full set of addresses associated with each person. We apply the same address and name standardization to the criminal history data, and apply the same fuzzy match.

Though Diamond et al. (2019) and Phillips (2020) find that similar address history data from Infutor are of high quality, we are unable to match every stop to the address history data and these data may be incomplete. Hence, we may not correctly associate all stops and criminal history with the corresponding motorist.

To match geocoded stops to sergeant patrol areas, we use the sergeant area boundaries shapefile received in response to a Texas Public Information Act request. This shapefile includes two sergeant area identifiers: *sgt_area* and *sgt_area_n*. In practice, the *sgt_area* identifier includes a significant number of unique values corresponding to identical geographies and the same value of *sgt_area_n*. For example, the boundaries for *sgt_area* 1B03 and *sgt_area* 1B05 are identical; both objects are assigned to the same value of *sgt_area_n* ($1B03\_1B05$). As such, we rely on the *sgt_area_n* identifier to map stops to sergeant areas, and we reassign stops associated with the small number of remaining *sgt_area_n* values that are themselves unique but correspond to identical geographies. There are also instances in which distinct *sgt_area_n* objects are partially overlapping. In cases in which a stop is associated with multiple distinct but partially overlapping *sgt_area_n* values, we include one observation for each unique *sgt_area_n* value associated with the stop. The sergeant area(s) associated with each geocoded stop were identified using the `Spatial Join` analysis tool in ArcGIS.

B National Household Travel Survey Analyses

We use data from the 2017 NHTS survey wave in order to examine the associations between reported household income, educational attainment, vehicle group, and location of residence.

In the NHTS, household income is partitioned into 11 intervals in the 2017 survey and five educational attainment levels are reported. To characterize economic status based on vehicle grouping, we follow the approach employed in the stop data and classify vehicles based on make, car type (passenger car, pick up truck, or SUV), and vehicle age. While the NHTS does not provide residential location at a more disaggregated level than core-based statistical area (CBSA), to analyze the association between respondent years of schooling and area-level income, we group Texas respondents based on response values for the following variables: (1) Category of the percent of renter-occupied housing in the census block group of the household's home location, (2) Category of population density (persons per square mile) in the census block group of the household's home location, (3) Category of housing units per square mile in the census block group of the household's home location, (4) Block group urban/rural status, (5) Urban area size where home address is located, and (6) CBSA for the respondent's home address. Note that a unique set of response values will typically correspond to multiple block groups, limiting the predictive power of our measure of average income based on block group characteristics.

In the text, we summarize results based on several analyses. First, we report the 0.35 rank correlation between reported income and vehicle group average income (using the grouping approach described above). Next, we reference the results shown in Appendix Table C.6. In columns 1 and 2 of that table, we report results from regressions of log household income on log average household income by vehicle group, with and without demographic controls. In columns 3 through 6, we report results from regressions of years of schooling on log average household income by vehicle group and by block group characteristics, with and without demographic controls and a control for log household income.

C Additional Exhibits

Table C.1
Sample Selection

Sample step	Observations Dropped	Observations Remaining
1. All stops conducted by Texas Highway Patrol between 2009 and 2015		15,761,299
2. Drop stops with missing trooper ID or stop outcomes	2,114	15,759,185
3. Retain stops of motorists with Texas addresses	1,872,413	13,886,772
4. Retain stops of motorists with valid addresses	1,958,380	11,928,392
5. Retain stops of valid passenger cars, pick-up trucks, and SUVs	577,141	11,351,251
6. Drop stops with missing location information	329,239	11,022,012
7. Drop stops with miscoded or toll violations	15,474	11,006,538

Table C.2
Traffic Stop Descriptive Statistics, Non-DWI Speeding Stops

	All Stops			All Searches		
	Below Median	Above Median	All	Below Median	Above Median	All
Black	9.62	7.79	8.86	19.68	16.82	18.82
Hispanic	33.09	20.70	27.92	37.79	26.64	34.41
White	54.73	67.82	60.20	40.48	53.43	44.41
Female	37.63	36.01	36.96	18.59	17.02	18.11
Log Household Income	10.08	11.48	10.67	10.02	11.41	10.44
	(0.62)	(0.46)	(0.89)	(0.63)	(0.43)	(0.86)
Search Rate	0.960	0.583	0.802	100	100	100
Unconditional Hit Rate	0.258	0.193	0.231	26.57	32.83	28.47
Moving	100	100	100	100	100	100
Driving while intoxicated	0	0	0	0	0	0
Speeding	100	100	100	100	100	100
Equipment	3.943	2.796	3.464	5.716	5.088	5.526
Regulatory	23.13	17.22	20.66	40.01	29.54	36.84
Prior felony arrests	0.105	0.0551	0.0842	0.545	0.426	0.509
	(0.651)	(0.455)	(0.578)	(1.540)	(1.335)	(1.482)
Prior misdemeanor arrests	0.254	0.154	0.212	1.141	0.957	1.085
	(1.130)	(0.834)	(1.018)	(2.606)	(2.249)	(2.505)
Observations	3,756,694	2,694,498	6,451,192	36,054	15,703	51,757

Sample restrictions are described in section 2. We further restrict to stops with a speeding warning or citation, and no DWI warning or citation. All values, excluding log household income and prior arrests, are expressed as percentage points. ‘Below Median’ and ‘Above Median’ refer to stops where household income is below and above the median value. Section 2.2 discusses the construction of the household income measure, which divides household income into 16 intervals.

Table C.3
Contraband Type by Motorist Income and Vehicle Status

	Vehicle Status Quintile				
	Q1	Q2	Q3	Q4	Q5
Contraband Type (%)					
Currency	0.28	0.4	0.7	0.6	0.8
Drugs	53.2	50.5	50.6	51.8	55.2
Other	43.0	45.3	44.8	43.9	40.3
Weapons	3.5	3.9	4.0	3.8	3.7
Observations	27,791	18,605	13,266	10,329	7,046
	Log Income Quintile				
	Q1	Q2	Q3	Q4	Q5
Contraband Type (%)					
Currency	0.7	0.4	0.4	0.4	0.3
Drugs	51.6	51.6	52.3	52.3	53.5
Other	44.0	44.1	43.7	43.6	42.7
Weapons	3.7	4.0	3.6	3.7	3.5
Observations	19,899	19,508	15,109	12,581	9,940

This table summarizes the distribution of contraband type by motorist economic status among motorists found with contraband. The top panel groups motorists into quintiles by vehicle status. The bottom panel groups motorists into quintiles by log household income.

TABLE C.4
PRETEXT STOP SHARES BY MOTORIST INCOME: ALTERNATIVE DEFINITIONS

Panel A	Pretextual Share Based on Bottom Decile Search Rate Troopers($\times$100)				
	(1)	(2)	(3)	(4)	(5)
log household income	-1.03	-0.80	-0.73	-0.69	-0.63
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Mean of DV	7.46				
Observations	11,006,538				
Panel B	Infraction-Specific Share of Stops by No Search Troopers ($\times$100)				
log household income	0.015	0.012	0.010	0.0092	0.0087
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Mean of DV	1.71				
Observations	11,006,538				
Sgt. Area $\times$ Time of Week $\times$ Year FEs		✓	✓	✓	✓
Motorist Demographics			✓	✓	✓
Motorist Criminal History				✓	✓
Trooper FE					✓

This table reports regression coefficients from estimates of equation (5). In Panel A, the outcome is the infraction-specific pretext share associated with a given stop (multiplied by 100), where all troopers in the bottom search rate decile are considered troopers without a search motive. In Panel B, the outcome is the infraction-specific share of stops conducted by troopers without a search motive. Section 2.2 discusses the construction of motorist household income. The sample includes all stops, which are assigned to either one of the ten most common infraction types or a residual “Other” category. For stops with multiple associated infractions, the assigned pretext share (share of troopers without a search motive) is the minimum (maximum) value across infractions. Robust standard errors are provided in parentheses.

Table C.5
Search Rates by Motorist Income, Controlling for Implied Pretext Share

Outcome:	Search (×100)				
	(1)	(2)	(3)	(4)	(5)
log household income	-0.31	-0.35	-0.32	-0.24	-0.22
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Sgt. Area × Time of Week × Year FEs		✓	✓	✓	✓
Motorist Demographics			✓	✓	✓
Motorist Criminal History				✓	✓
Trooper FE					✓
Pretextual Share FEs	✓	✓	✓	✓	✓
Mean of DV	1.92				
Observations	11,006,538				

This table reports regression coefficients from estimates of equation (5), where the outcome is an indicator (multiplied by 100) for whether a stop leads to a search. Section 2.2 discusses the construction of motorist household income. The sample includes all stops, which are assigned to either one of the ten most common infraction types or a residual "Other" category. For stops with multiple associated infractions, the assigned pretext share is the minimum value across infractions. Robust standard errors are provided in parentheses.

Table C.6
National Household Travel Survey (NHTS) Correlational Analyses

	Outcome: Log HH Income		Outcome: Years of Schooling			
	(1)	(2)	(3)	(4)	(4)	(6)
Log Average Income by	0.810	0.792	1.472	0.784		
Vehicle Group	(0.023)	(0.023)	(0.056)	(0.056)		
Log Average Income by					0.790	0.252
Block Group Characteristics					(0.037)	(0.037)
Race and Gender Controls		✓		✓		✓
Log Household Income Control				✓		✓
Observations			40,106			

This table reports results from regressing respondent log household income/years of schooling on leave-out log average household income by vehicle group (make, car type, and age) and leave-out log average household income by block group characteristics. Data is from the 2017 NHTS sample of Texas respondents. Average income measures are constructed as described in Appendix B. Robust standard errors are provided in parentheses.

Table C.7
Within-Motorist Vehicle Status Gradient by Infraction (All Stops)

	Outcome: Δ Infraction										
	Speeding	Fail to Signal	Expired Plates	Lane Viol.	Seatbelt Viol.	Lights Viol.	Disregard Sign	Tinted Windows	DWI	Tailgating	Other Viol.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Δ Vehicle Status	0.380 (0.002)	-0.015 (0.001)	-0.149 (0.002)	-0.014 (0.001)	-0.068 (0.001)	-0.137 (0.001)	-0.014 (0.001)	-0.022 (0.001)	-0.002 (0.000)	-0.004 (0.000)	-0.084 (0.001)
Observations	2,320,268										

This table relates first differences in indicators for the given infraction type for sequential pairs of stops of the same motorist to changes in vehicle status. The sample includes all stops, including those with multiple associated infractions. Robust standard errors are provided in parentheses.

Table C.8
Speeding Violation Severity by Vehicle Status
(Within-Motorist Analysis)

Outcome:	Δ MPH Over Limit			
	(1)	(2)	(3)	(4)
Δ Vehicle Status	1.05	0.87	1.48	1.06
	(0.09)	(0.12)	(0.09)	(0.29)
Restricted to Speeding-Only Violations		✓		
Speed Limit FEs			✓	
Restricted to Same Road Segment				✓
Mean of DV	-0.15	-0.15	-0.15	-0.51
Observations	103,808	56,622	103,808	11,023

This table relates first differences in MPH over the speed limit for sequential pairs of speeding stops of the same motorist to changes in vehicle status. The sample includes all speeding stops in column (1) and speeding-only stops in column (2). Column (3) controls for the speed limit associated with each within-pair stop and Column (4) restricts the sample to pairs of stops taking place on the same highway and in the same Sergeant Area. Robust standard errors are provided in parentheses.

Table C.9
Descriptive Statistics for Sequential Stops with Moving Infractions

	Single Stop	Multiple Stops Same Vehicle	Multiple Stops Different Vehicle
Black	10.21	7.98	8.13
Hispanic	28.38	26.71	31.11
White	57.20	63.05	58.59
Female	42.43	37.63	28.75
Log household income	10.68	10.63	10.64
	(0.873)	(0.904)	(0.908)
Search rate	1.340	0.951	1.074
Unconditional hit rate	0.405	0.291	0.312
Search rate in prior stop	.	0.863	1.084
Unconditional hit rate in prior stop	.	0.240	0.275
Moving	100	100	100
Driving while intoxicated	1.172	0.678	0.779
Speeding	84.95	88.50	87.89
Equipment	3.540	3.701	3.657
Regulatory	22.78	17.47	18.80
Prior felony arrests	0.0555	0.0895	0.133
	(0.467)	(0.591)	(0.729)
Prior misdemeanor arrests	0.139	0.231	0.327
	(0.821)	(1.034)	(1.287)
Vehicle status	10.63	10.62	10.64
	(0.227)	(0.201)	(0.221)
Change in vehicle status	.	-0.0214	0.0257
	(.)	(0.0305)	(0.245)
Change in vehicle age	.	0.729	-0.660
	(.)	(0.928)	(5.941)
Months between stops	.	9.132	17.03
	(.)	(10.38)	(15.34)
Absolute change in vehicle status	.	0.0214	0.188
	(.)	(0.0305)	(0.160)
Absolute change in vehicle age	.	0.729	4.405
	(.)	(0.928)	(4.040)
Observations	2,988,784	1,111,170	1,148,587

This table presents descriptive statistics for three sets of stops: stops that involve motorists that we only observe in one stop (column 1); stops that involve motorists that were previously stopped in the same vehicle (columns 2); and stops that involve motorists that were previously stopped in a different vehicle (column 3). We restrict the sample to stops (and prior stops) associated with moving violations.

Table C.10
Descriptive Statistics for Sequential Stops, By Income Tercile

	Bottom Tercile			Top Tercile		
	Single Stop	Multiple Stops		Single Stop	Multiple Stops	
		Same Vehicle	Different Vehicle		Same Vehicle	Different Vehicle
Black	11.93	8.81	8.60	8.70	7.50	7.73
Hispanic	39.81	39.25	43.80	20.00	20.00	24.35
White	45.24	50.26	46.08	66.27	69.59	65.12
Female	40.38	34.57	26.67	41.18	31.99	24.52
Log household income	9.72	9.66	9.66	11.61	11.61	11.61
	(0.56)	(0.59)	(0.59)	(0.41)	(0.41)	(0.41)
Search rate	2.753	2.149	2.347	1.181	1.334	1.323
Unconditional hit rate	0.971	0.760	0.770	0.480	0.561	0.537
Search rate in prior stop	.	2.041	2.381	.	1.208	1.344
Unconditional hit rate in prior stop	.	0.708	0.709	.	0.518	0.493
Moving	66.87	66.04	67.94	76.07	73.02	76.03
Driving while intoxicated	3.068	1.681	2.091	1.189	1.037	1.166
Speeding	51.95	53.87	55.08	64.64	63.66	66.08
Equipment	21.69	22.52	21.90	13.74	16.02	14.52
Regulatory	38.99	31.91	33.31	26.66	28.88	27.34
Prior felony arrests	0.0978	0.170	0.247	0.0409	0.0761	0.112
	(0.634)	(0.841)	(1.006)	(0.393)	(0.532)	(0.661)
Prior misdemeanor arrests	0.230	0.408	0.569	0.112	0.223	0.305
	(1.120)	(1.485)	(1.782)	(0.714)	(1.013)	(1.246)
Vehicle status	10.54	10.54	10.55	10.69	10.64	10.67
	(0.198)	(0.185)	(0.200)	(0.252)	(0.226)	(0.246)
Change in vehicle status	.	-0.0171	0.0168	.	-0.0233	0.0230
	(.)	(0.0266)	(0.227)	(.)	(0.0328)	(0.269)
Change in vehicle age	.	0.624	-0.401	.	0.783	-0.533
	(.)	(0.876)	(6.802)	(.)	(0.975)	(5.982)
Months between stops	.	7.828	15.53	.	9.758	18.14
	(.)	(9.755)	(14.76)	(.)	(11.02)	(15.83)
Absolute change in vehicle status	.	0.0172	0.175	.	0.0234	0.204
	(.)	(0.0266)	(0.146)	(.)	(0.0328)	(0.177)
Absolute change in vehicle age	.	0.624	5.126	.	0.783	4.416
	(.)	(0.876)	(4.489)	(.)	(0.975)	(4.070)
Observations	1,544,342	798,129	897,223	1,332,165	607,844	657,914

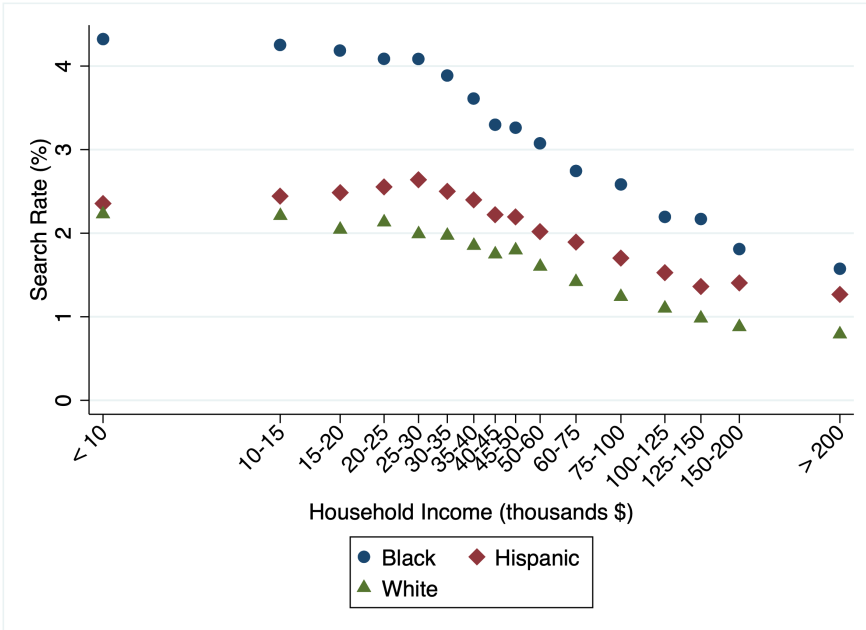
This table presents descriptive statistics for motorists in the bottom income tercile (columns 1-3) and in the top income tercile (columns 4-6). Descriptive statistics are shown for three sets of stops: stops that involve motorists that we only observe in one stop (columns 1 and 4); stops that involve motorists that were previously stopped in the same vehicle (columns 2 and 5); and stops that involve motorists that were previously stopped in a different vehicle (columns 3 and 6).

Table C.11
Alternative Hit Rate Measures by Vehicle Status

Panel A	Expected Sentence Length				
	(1)	(2)	(3)	(4)	(5)
Vehicle status	-3.88	-2.77	5.71	5.44	5.80
	(1.87)	(1.88)	(1.87)	(1.90)	(1.93)
log household income				0.44	0.44
				(0.40)	(0.40)
Observations	211,532				
Panel B	Charges of Above Median Severity				
Vehicle status	-0.62	-0.55	0.45	0.39	0.45
	(0.19)	(0.20)	(0.19)	(0.20)	(0.20)
log household income				0.09	0.10
				(0.04)	(0.04)
Observations	211,532				
Sgt. Area × Year FEs	✓	✓	✓	✓	✓
Motorist demographics		✓	✓	✓	✓
Motorist criminal history			✓	✓	✓
Trooper FEs					✓

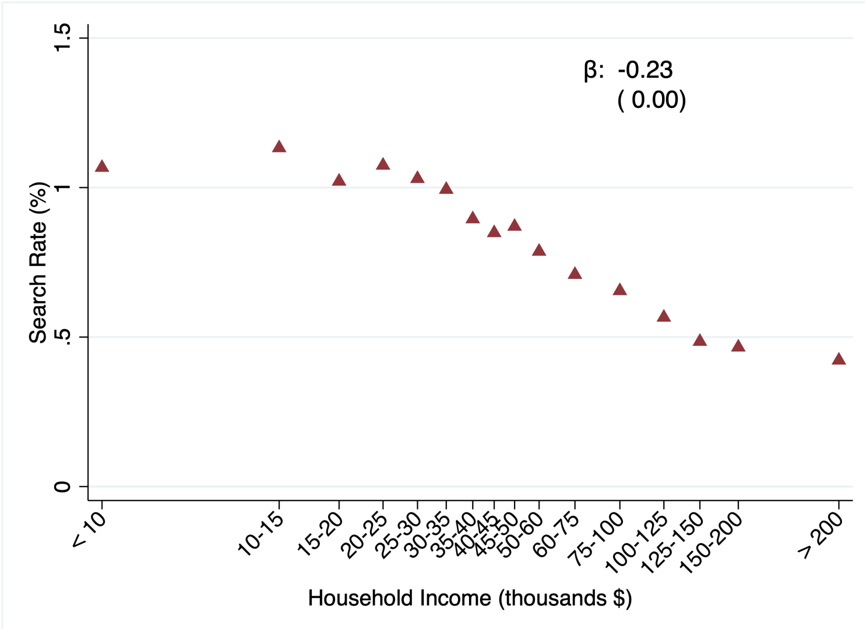
This table reports regression coefficients from estimates of equation (5), where the outcome is expected incarceration sentence (Panel A) or an indicator (multiplied by 100) for whether a motorist is arrested for criminal charges of above median severity (Panel B). Section 2.2 discusses the construction of motorist household income. Vehicle status measures expected log household income for people driving vehicles of the same make, type, and age. Robust standard errors are provided in parentheses.

Figure C.1
Search Rates by Motorist Income and Race



Note: This figure plots search rates as a function of motorist income, separately by motorist race. Household income is depicted on a log scale. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals.

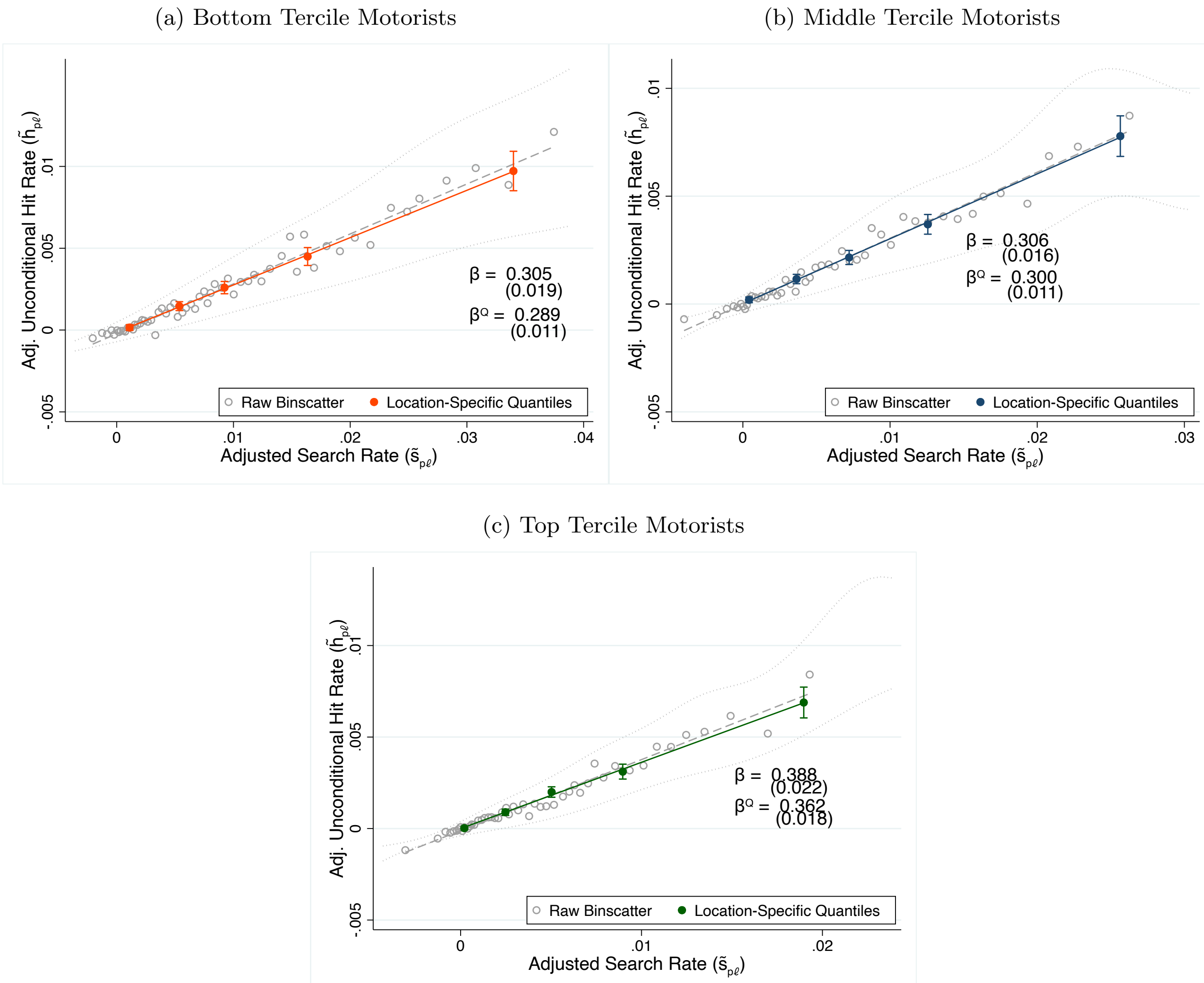
Figure C.2
Search Rates Are Decreasing in Motorist Income, Non-DWI Speeding Stops



Note: This figure plots search rates as a function of motorist income. Household income is depicted on a log scale. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. The sample is limited to stops with a speeding warning or citation, and no DWI warning or citation.

Figure C.3

Between-Trooper Search Productivity Curve, by Motorist Income Tercile



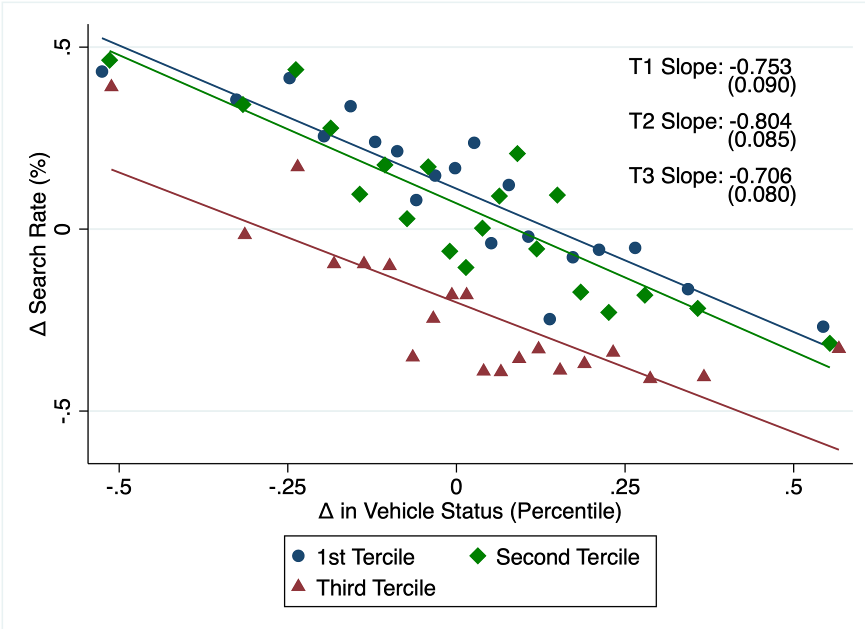
(a) Bottom Tercile Motorists

(b) Middle Tercile Motorists

(c) Top Tercile Motorists

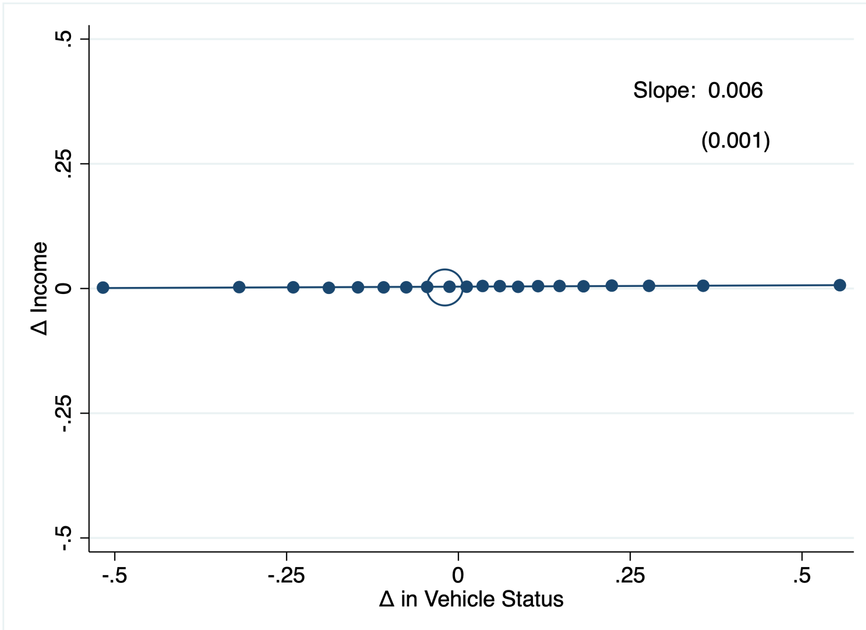
Note: In this figure we plot adjusted trooper unconditional hit rates ($\tilde{h}_{p\ell}$) against trooper search rates ($\tilde{s}_{p\ell}$) using two approaches, where $\tilde{s}_{p\ell}$ and $\tilde{h}_{p\ell}$ take on values between zero and one (before each is residualized). The first approach is a simple binscatter, where we choose the integrated mean square error-optimal number of bins as in Cattaneo et al. (2019) (using the Stata package `binsreg`). The figure includes 95% confidence bands for the local linear relationship between adjusted trooper search rates and unconditional hit rates and the best linear fit and its slope. The local linear fit is derived using a Gaussian kernel with a rule-of-thumb bandwidth. Bootstrap standard errors for the estimated slopes, where we stratify resampling by trooper and location, are provided in parentheses. In the second approach we divide troopers into quantiles by search rate within locations, group quantiles across locations, and then plot the relationship between search rates and unconditional hit rates across quantiles. From this approach, the figure includes the mean values for each quintile and the best linear fit and its slope. Bootstrap standard errors for the estimated slopes are provided in parentheses. Panel A, Panel B, and Panel C plot the search productivity curve (SPC) for bottom income tercile motorists, middle income tercile motorists, and top income tercile motorists, respectively.

Figure C.4
Search Rates for Motorists in Sequential Stops by Time between Stops



Note: This figure looks at differences in search rates for pairs of stops of the same motorist in different vehicles. Sequential pairs are partitioned by the time between the stops. For the first tercile, there is less than 7 months between stops. For the second tercile, there is between 7 and 19 months between stops. For the third tercile, there is at least 19 months between stops.

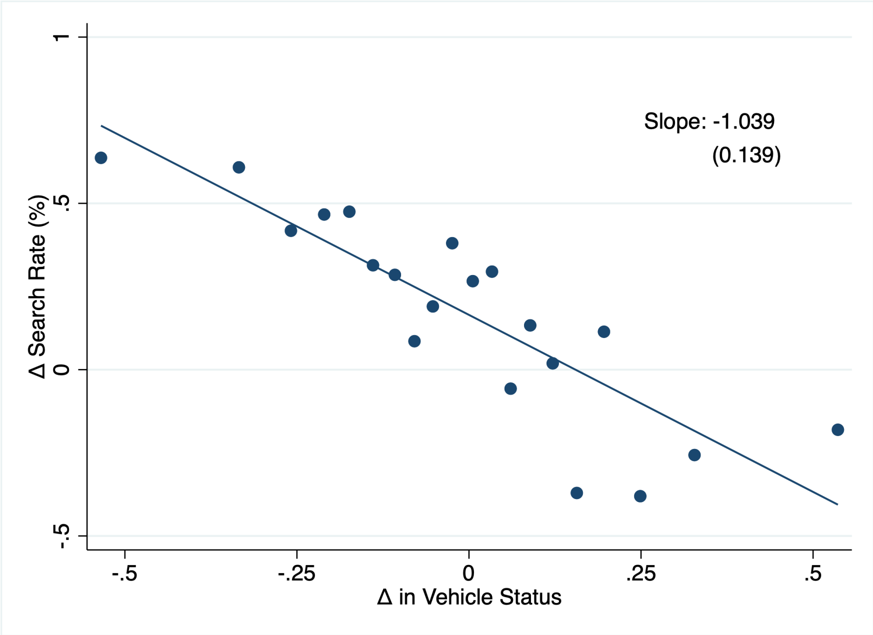
Figure C.5
Change in Motorist Income Between Stops in Within-Motorist Design



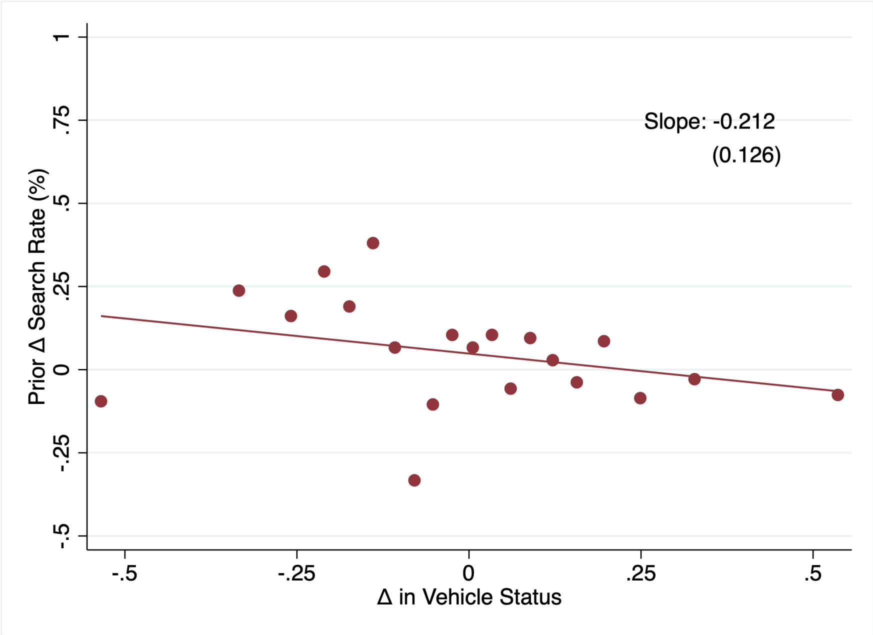
Note: This figure looks at the change in log household income between sequential stops (as described in section 5.1) as a function of the change in vehicle status between stops. A motorist's log household income may change if they change addresses between stops.

Figure C.6
Search Rates for Motorists That Alternate between Vehicles

(a) Contemporaneous Change in Vehicle



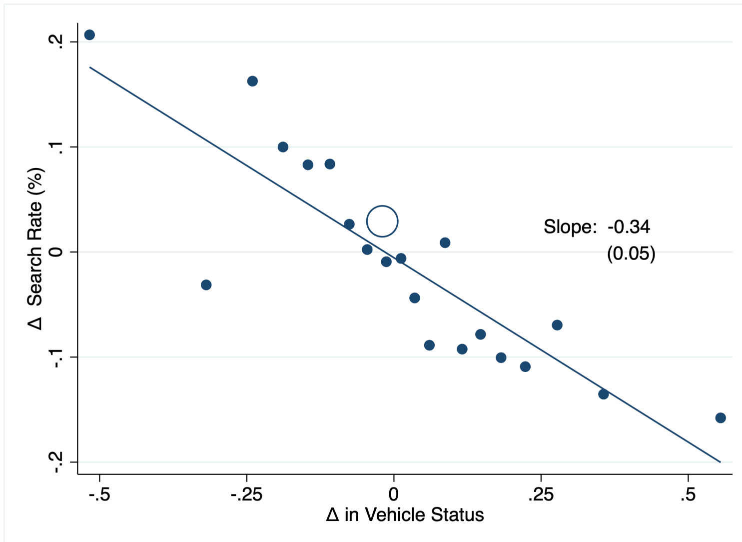
(b) Stops of Same Vehicle Bracketing Stops of Other Vehicles



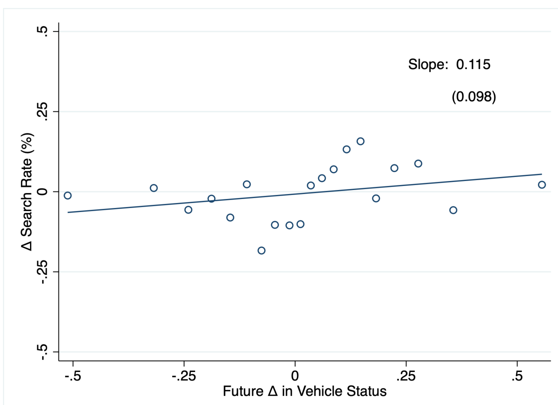
Note: This figure looks at differences in search rates for pairs of stops of the same motorist, limiting to motorists that alternate back and forth between vehicles. Panel A limits to sequential pairs of stops involving two vehicles (vehicle A then vehicle B). Panel B looks at the pairs of stops of the original vehicle (vehicle A) that immediately bracket stops of other vehicles.

Figure C.7
Troopers Profile Motorists at the Search Margin, Non-DWI Speeding Stops

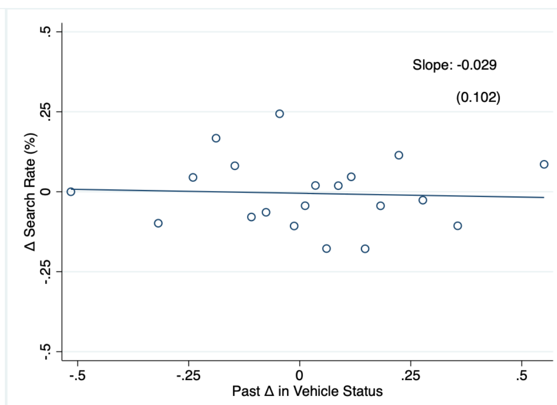
(a) Contemporaneous Change in Vehicle



(b) Future Change in Vehicle



(c) Past Change in Vehicle

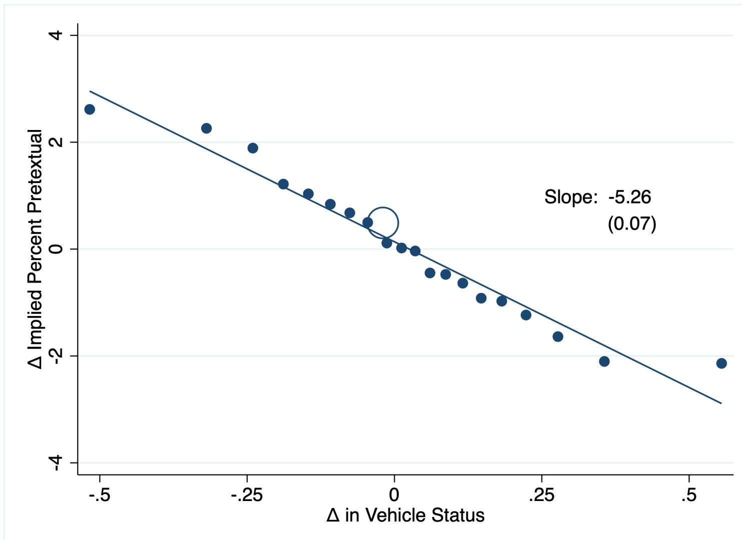


Note: These figures look at first differences in search rates for sequential pairs of stops of the same motorist as a function of changes in vehicle status. Stops are limited to those with a speeding warning or citation, and no DWI warning or citation. Panel A plots first differences in search rates against first differences in vehicle status. The open circle depicts the change in search rates for sequential pairs of stops where the same vehicle is involved in both stops. Panel B looks at whether future changes in vehicle status predict contemporaneous changes in search rates. Panel C looks at whether past changes in vehicle status predict contemporaneous changes in search rates.

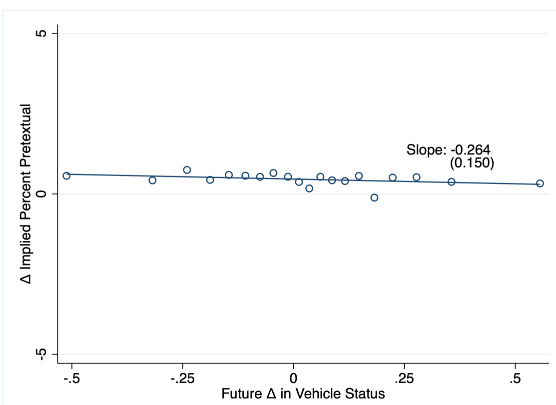
Figure C.8

Troopers Profile Motorists at the Stop Margin, Moving Violations

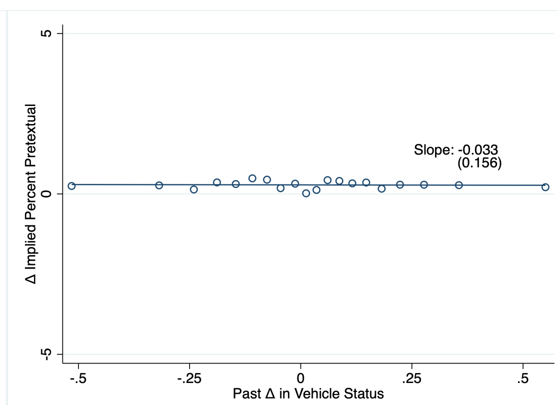
(a) Contemporaneous Change in Vehicle



(b) Future Change in Vehicle



(c) Past Change in Vehicle

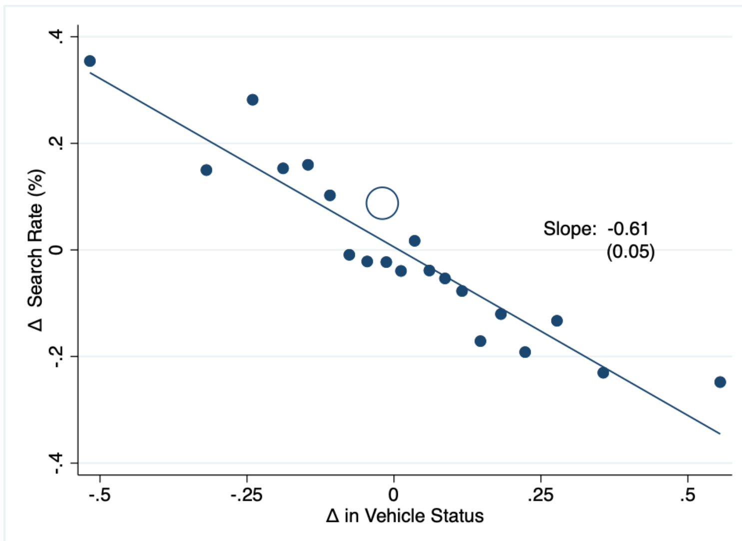


Note: These figures look at first differences in the implied pretextual stop percentage for sequential pairs of stops of the same motorist as a function of changes in vehicle status. The sample is restricted to sequential pairs of stops with associated moving violations, and we define the implied pretextual stop percentage based only on these moving violations. Panel A plots first differences in the implied pretext share against first differences in vehicle status. The open circle depicts the change in pretext share for sequential pairs of stops where the same vehicle is involved in both stops. Panel B looks at whether future changes in vehicle status predict contemporaneous changes in pretext share. Panel C looks at whether past changes in vehicle status predict contemporaneous changes in pretext share.

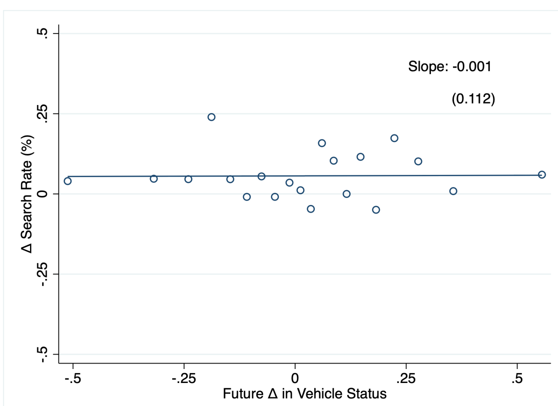
Figure C.9

Troopers Profile Motorists at the Search Margin, Moving Violations

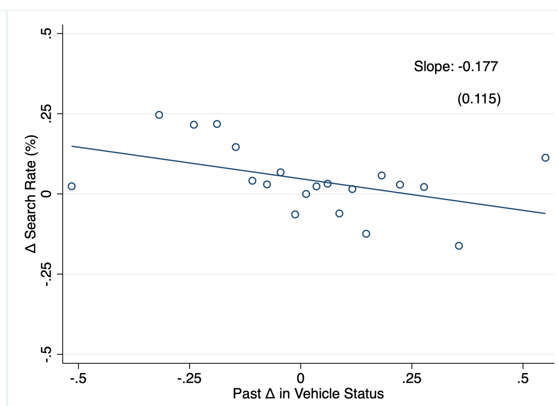
(a) Contemporaneous Change in Vehicle



(b) Future Change in Vehicle

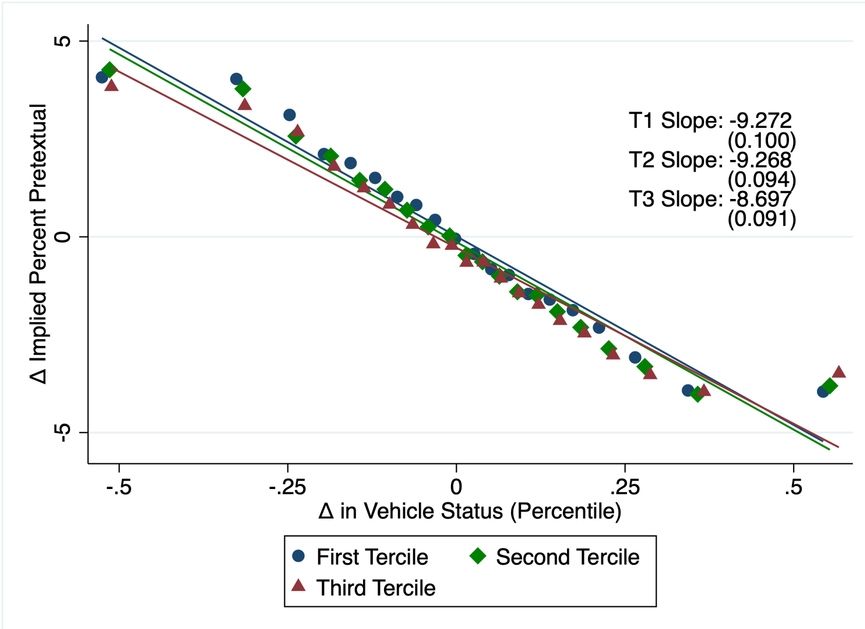


(c) Past Change in Vehicle



Note: These figures look at first differences in search rates for sequential pairs of stops of the same motorist as a function of changes in vehicle status. The sample is restricted to sequential pairs of stops with associated moving violations. Panel A plots first differences in search rates against first differences in vehicle status. The open circle depicts the change in search rates for sequential pairs of stops where the same vehicle is involved in both stops. Panel B looks at whether future changes in vehicle status predict contemporaneous changes in search rates. Panel C looks at whether past changes in vehicle status predict contemporaneous changes in search rates.

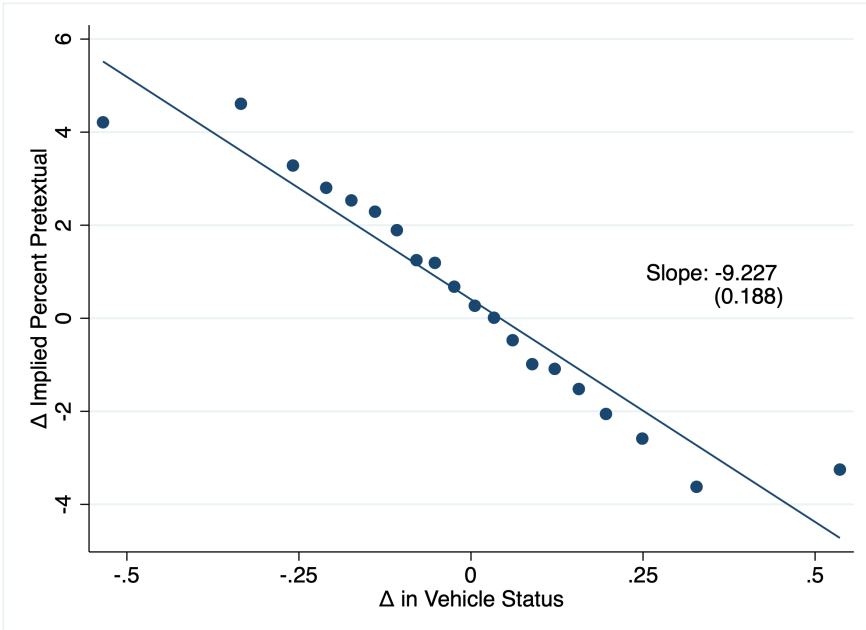
Figure C.10
Pretext Stop Shares for Motorists in Sequential Stops by Time between Stops



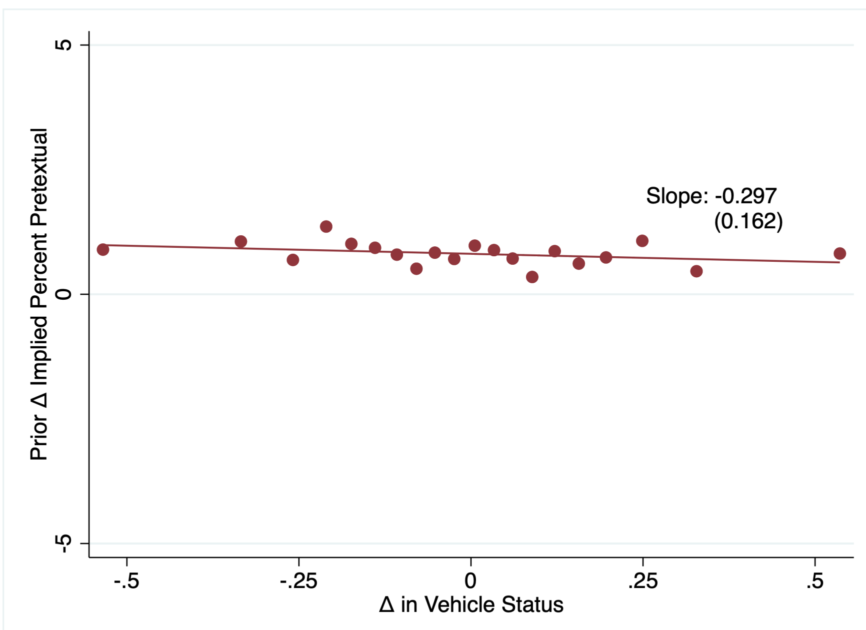
Note: This figure looks at differences in pretext stop shares for pairs of stops of the same motorist in different vehicles. Sequential pairs are partitioned by the time between the stops. For the first tercile, there is less than 7 months between stops. For the second tercile, there is between 7 and 19 months between stops. For the third tercile, there is at least 19 months between stops.

Figure C.11
Pretext Stop Shares for Motorists That Alternate between Vehicles

(a) Contemporaneous Change in Vehicle



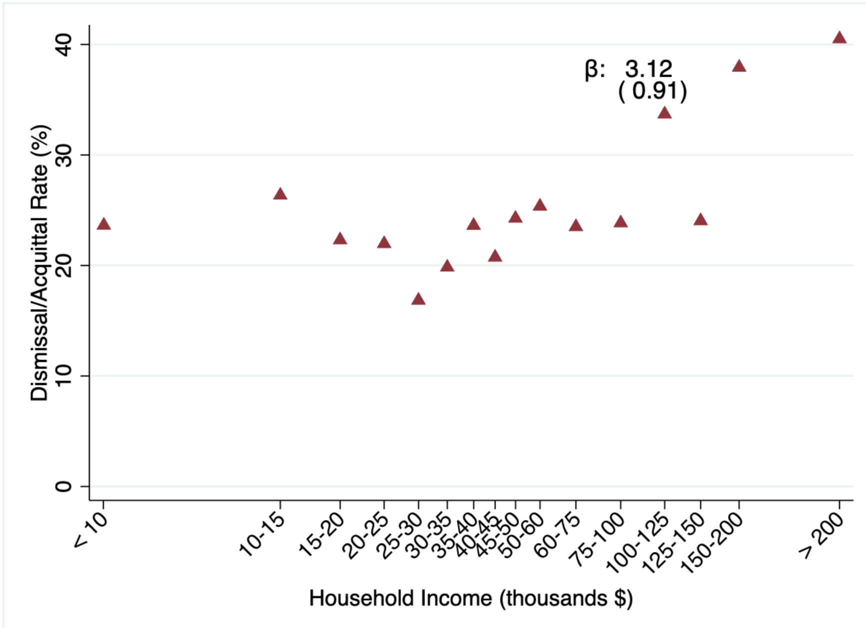
(b) Stops of Same Vehicle Bracketing Stops of Other Vehicles



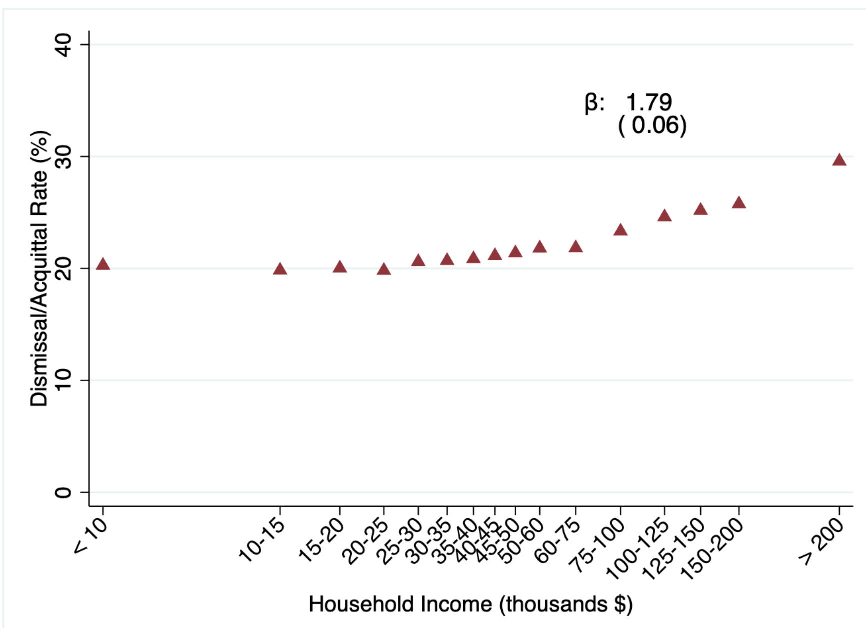
Note: This figure looks at differences in pretext stop shares for pairs of stops of the same motorist, limiting to motorists that alternate back and forth between vehicles. Panel A limits to sequential pairs of stops involving two vehicles (vehicle A then vehicle B). Panel B looks at the pairs of stops of the original vehicle (vehicle A) that immediately bracket stops of other vehicles.

Figure C.12
Dismissal/Acquittal Rates are Increasing in Motorist Income

(a) DPS Searches



(b) All Drug Arrests



Note: These figures plot dismissal or acquittal rates as a function of motorist income. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households in a given interval as the horizontal axis coordinate. In Panel A the sample includes traffic stops that lead to a search, contraband recovery, and arrest. In Panel B the sample is all arrests in the CCH data for those drug charges most commonly associated with contraband-related arrests in the traffic stop data. See footnote 49 for details.