Class Disparities and Discrimination in Traffic Stops and Searches

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Abstract

We document class disparities and discrimination in the incidence of police searches. Low-income motorists are more likely to be pursued in pretext stops and to be searched for contraband. Yet searches of low-income motorists are less likely to yield contraband. To isolate class-based discrimination, we show that motorists stopped in multiple vehicles are more likely to be searched when stopped in a vehicle that signals they are low-income. Overall contraband yield would increase if police did not engage in vehicle-based profiling. We provide suggestive evidence that lower hassle costs associated with arrests of low-income motorists help to explain trooper behavior.

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1 Introduction

Class may shape how police interact with potential offenders (Robison, 1936). Neighborhood disadvantage is associated with higher rates of police presence (Chen et al., forthcoming), contact (Fagan et al., 2010), and arrest (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 disparities and discrimination in traffic stops and searches using data on the universe of stops conducted by Texas Highway Patrol. We present new descriptive facts about income-based disparities in (1) whether troopers search motorists for contraband and in (2) whether these searches yield contraband. Guided by a simple model of trooper behavior, we measure class disparities in "pretext" stops—those based on minor infractions and conducted with the goal of identifying more serious crimes via search. We then exploit within-motorist variation in perceived class to test whether troopers engage in class discrimination when deciding whether to conduct a search or pursue a pretext stop.

We find that, among the motorists that they stop, troopers are more likely to search low-income motorists. Motorists in the bottom 20% by income are more than twice as likely to be searched than motorists in the top 20%. Conditioning on the location and time of the stop does not affect this disparity. By comparison, Black and Hispanic motorists are about 150% and 60% more likely to be searched than White motorists.

Though troopers are more likely to search low-income motorists, troopers are less likely to find contraband in these searches compared to 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. Disparate exposure to traffic stops has long been viewed as a central driver of inequitable treatment under the law and, more recently, as an important source of disparate exposure to police violence (Harris, 1997; Johnson and Johnson, 2023). However, testing for group-based disparities in stops is complicated by what is known in the literature as the "benchmarking problem" (Grogger and Ridgeway, 2006)—we only observe the stops that are made, not potential stops. To test for class differences in pretext stops, we first build a simple model of trooper stop and search behavior. We assume that a trooper decides whether to conduct a stop based on the severity of the infraction they observe and the option value of conducting a search. We define a stop as *pretextual* if a trooper would not conduct the stop in the absence of the search option value. The key prediction of the model is that, all else equal, troopers with lower search costs will conduct more pretext stops. At the extreme, troopers that find all searches prohibitively costly will never make a pretext stop.

We use this prediction to test for class disparities in pretext stops. We measure variation in implied search costs between troopers using their search propensities, adjusting for motorist characteristics. We find that, conditional on the location and time of the stop, low-income motorists are stopped by more search-intensive troopers. Through the lens of the model, this pattern indicates that low-income motorists are disproportionately subject to pretext stops.

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 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 data on a class signal that is salient to troopers and varies *within* motorists: the motorist's vehicle. We take advantage of the fact that the same motorist may be stopped in different vehicles, generating quasi-experimental variation in perceived class. Given that many motorists have access to multiple vehicles and that vehicle purchases are typically infrequent, motorists often change the vehicle they drive from trip to trip without any coincident change in their economic circumstances. Under the assumption that within-motorist variation in other relevant characteristics is unrelated to the vehicle a motorist is driving, we can identify the causal effect of vehicle attributes on stop outcomes. We find that when motorists are stopped in a low-status vehicle, they are more likely to be searched and are stopped by more search-intensive troopers. Placebo tests based on the timing and sequence of vehicle switches provide support for our identifying assumption.

We also use vehicle-induced variation in search rates to measure contraband yield for marginal searches. Consistent with observed income differences in contraband yield rates, we find that troopers are more likely to find contraband in marginal searches of high-income motorists than in marginal searches of low-income motorists. By engaging in vehicle-based profiling, troopers are reducing their contraband yield.

One explanation for troopers' behavior is that they are prejudiced or have inaccurate beliefs about those motorists most likely to carry contraband. An alternative possibility is that troopers value outcomes beyond contraband yield or 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. Consistent with prior research and officer testimonials (Newell et al., 2022; Boyce, 2006), we posit that these court appearances impose significant "hassle costs" given their stressful and acrimonious nature and the scheduling challenges they often pose to officers. To assess how these hassle costs vary with motorist income, we examine defendant pleading behavior and charge dispositions. Among motorists arrested after a search, we find that 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 subsequent court appearances by troopers, while dismissals and acquittals are more likely when troopers' actions or testimony are successfully challenged. We posit that class disparities in the court system may 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, due to local institutional factors, guilty and no contest pleas are more common.[1]

1.1 Related Literature

Our work relates to a literature on police profiling and discrimination in the criminal justice context more broadly. This literature has generally focused on race-based discrimination. Recent research has documented racial disparities in vehicle stops (Pierson et al., 2020), traffic citations (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), charging decisions (Rehavi and Starr, 2014), pre-trial detention (Arnold et al., 2018, 2022), and sentencing (Rehavi and Starr, 2014). We contribute to this literature by documenting large disparities along another social dimension: class. Prior work documents group-based differences in treatment, but does not establish that those differences reflect direct discrimination *per se*—police, prosecutors, or judges using a person's group identity in deciding how to treat them. Leveraging our quasi-experimental design that links within-motorist variation in stop outcomes to variation in perceived class based on the vehicle involved, we can credibly isolate the causal effect of motorists' perceived class on police behavior. The logic of our test is similar to that of correspondence studies, where the researcher experimentally manipulates the perceived group membership of a fictitious person (for example, a job applicant). Though our approach requires a stronger identifying assumption, we avoid common critiques of correspondence studies by studying organic social interactions (Heckman and Siegelman, 1993; Bertrand and Duflo, 2017).

We also contribute to research in criminology and sociology on "neighborhood stigma." This work assesses the degree to which neighborhood economic disadvantage is predictive of higher rates of police contact and arrest, conditional on a range of covariates capturing local racial composition, crime rates, and other potential predictors of policing outcomes (Fagan et al., 2010; MacDonald, 2021; Smith, 1986). While evidence from this literature is consistent with police profiling based on neighborhood disadvantage, these findings are ultimately challenging to interpret for two reasons. First, these correlational estimates are subject to standard omitted variable bias concerns to the extent that the characteristics and behaviors of local populations are not fully accounted for. Second, even if these estimates are causally interpretable, 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 such, 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 treat civilians differently as a function of class status *conditional on location.*

Economic disadvantage does not convey protections under current interpretations of anti-discrimination law, but there remains ongoing debate about whether the poor should be considered

[1]Such 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.

a protected class. Importantly, one proposed legal criterion to assess whether a given trait should serve as the basis for a protected class is whether social bias based on that 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 the context of traffic stops, the most common source of interactions between police and the public (Davis et al., 2018). Furthermore, our quasi-experimental evidence and analyses of how class disparities in search rates map to differences in contraband yield contribute to our understanding of the "legitimacy" of the disparities we document.

Although there is limited evidence on class discrimination in the policing context, our work contributes to an emerging literature on the regressive burden of criminal justice policies. This literature identifies a range of factors, including a reliance on indigent defense and the assignment of money bail, that contribute to higher rates of conviction and incarceration and that disproportionately affect 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 adds to a growing body of work that highlights discrimination on the basis of social class across a range of settings. People infer the social status of others based on a variety of cues, including material possessions, speech and accent patterns, 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 may also activate stereotypes and lead to discrimination. For example, resume studies indicate that signals of class status are predictive of lower callback rates among men (Rivera and Tilcsik, 2006) and that neighborhood disadvantage leads to lower response rates for sellers in an online marketplace (Besbris et al., 2015). Quasi-experimental evidence similarly points to penalties associated with class disadvantage, including work showing that poor dental health (which is highly correlated with socioeconomic status) has a negative causal impact on labor market earnings (Glied and Niedell, 2010).

2 Setting and Data

2.1 Institutional Setting

In Texas, the primary responsibility of highway patrol troopers is to enforce state traffic laws on highways and state roads, but they have authority to enforce state criminal law throughout the state. When conducting a traffic stop, a trooper will give a warning or citation for the original traffic violation. Troopers may also decide to further investigate if they suspect that a motorist may be carrying contraband, such as illicit drugs or weapons. As part of their investigation, troopers may search the motorist or vehicle for contraband. Troopers typically work alone, but may wait for support when conducting searches.

In our setting, there are four types of searches: consent, probable cause, incident to arrest, and inventory. Inventory searches are searches that occur after a vehicle is ordered impounded. In these instances, troopers are free to search the inventoried vehicle subject to departmental search policy. Incident to arrest searches are searches that occur following an arrest. After an arrest, troopers can search the arrested individual for contraband and, under broad conditions, search the vehicle. Alternatively, troopers have the right to conduct a search if they have probable cause to believe that a law has been broken. Finally, in a consent search, a trooper conducts a search only after receiving permission from the motorist to do so. In our sample, more than 80% of searches are consent and probable cause searches. When contraband is discovered following a search, the motorist may be arrested on charges related to the contraband discovered.

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

2.2 Administrative Traffic Stop Data

The primary data source we use is a comprehensive dataset of 16 million motor vehicle traffic stops conducted by the Texas Highway Patrol between 2009 and 2015. For each stop, the data include the date, time, location, motorist's race and ethnicity, motorist's gender, information on the motor vehicle (including make, model, and year), the associated violation(s), whether a search was conducted, the rationale for each search, whether contraband was found, and the ID number of the trooper who conducted the stop.[2] The data cover all stops, including both stops that result in warnings and citations. A unique feature of the data is that they include the 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 match multiple traffic stops to the same motorist, and (3) we merge in criminal histories for each motorist using data described below.

We make a few restrictions to form our analysis sample. We drop stops where information on the trooper involved, the location, or the outcome is missing. We limit our analysis to stops of motorists with valid Texas addresses. We also restrict the sample to stops where the vehicle involved is a passenger car, pick-up truck, or SUV. Appendix Table C.1 summarizes the number of observations we drop with each sample restriction. After applying these restrictions, our sample includes 11,022,012 stops.

We infer each motorist's household income as follows. Based on 5-year periods of American Community Survey (ACS) data, the Census Bureau reports estimates for the household income distribution at the level of the block group, a Census tract subdivision that generally includes between 600 and 3,000 people. We use ACS data from 2009–2013. The ACS reports income statistics for all households and separately for homeowners and renters. When reported separately for homeowners and renters, household income is partitioned into 7 intervals.[3] For motorists living

[2] 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).

[3] The 7 intervals are: less than \$10,000, \$10,000–\$19,999, \$20,000–\$34,999, \$35,000–\$49,999, \$50,000–\$74,999,

in single-family residences, we assign motorists to percentiles within block groups based on the assessed property value of their residence.[4] 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 (and those we are unable to match to a specific property), we assign the median household income category among renters in their block group. The data include a more disaggregated set of 16 income intervals when all households within a block group are pooled. 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.[5] Figure 1 plots the distribution of household 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.[6]

Our household income measure is imperfect for several reasons. The block group-level distribution of household income derived from the ACS is estimated with 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. 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.[7] The statewide rank correlation is 0.72. In addition, property assessments may not accurately reflect property values. Nonetheless, our household income measure should capture important dimensions of economic well-being.[8]

Table 1 presents descriptive statistics for all stops included in our analysis. We report statistics for motorists with below and above median income separately, and for all motorists pooled together.[9] Overall, motorists are searched in 1.9% of stops.

$75,000–$99,999, and more than $100,000.

[4]We use property-level records of assessments from ATTOM and assessments as of 2015.

[5]The 16 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.

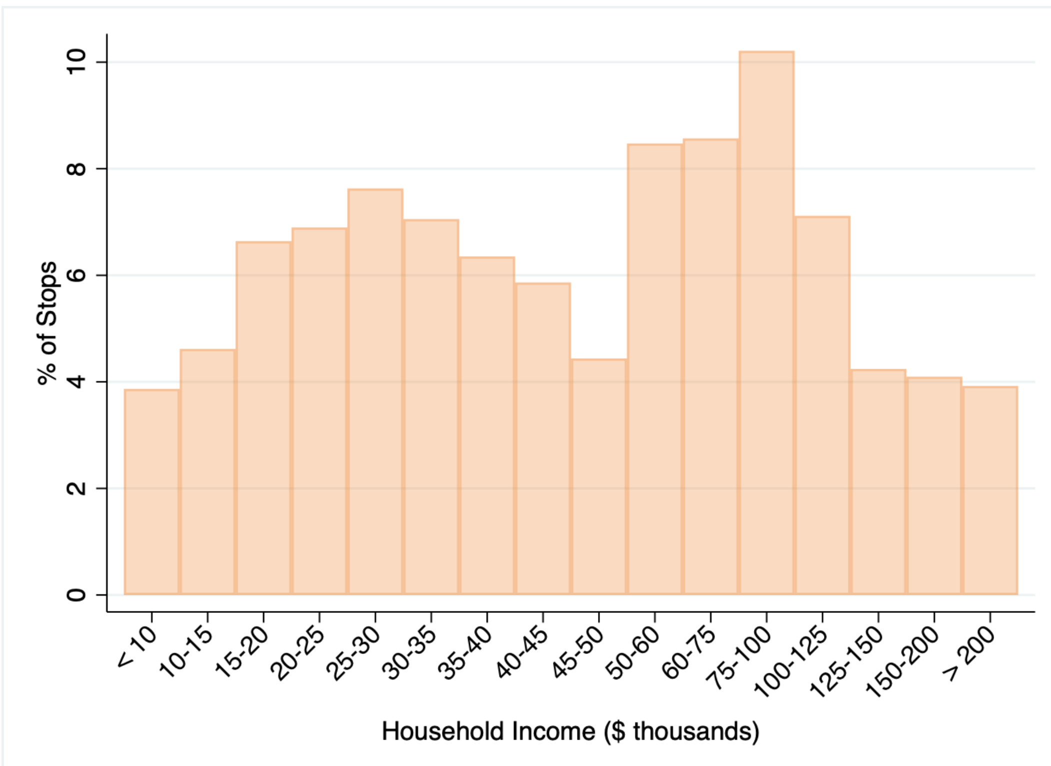
[6]Results throughout are not sensitive to using alternative strategies for income imputation, including using median household income in the block group for all motorists or restricting property-based imputation to households that are reported as homeowners in the address history data described in section 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.

[7]Note that tracts are collections of block groups.

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

[9]We refer to "household income" and "income" interchangeably throughout.

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.

Table 1
Traffic Stop Descriptive Statistics

	All Stops			All Searches		
	Below Median	Above Median	All	Below Median	Above Median	All
Black	10.14	8.685	9.462	16.79	15.04	16.18
Hispanic	37.70	24.63	31.60	39.39	29.72	36.01
White	49.84	63.18	56.07	42.00	52.61	45.71
Female	35.10	34.55	34.84	19.81	18.96	19.51
Log Household Income	9.938	11.34	10.59	9.908	11.23	10.37
	(0.606)	(0.489)	(0.891)	(0.608)	(0.445)	(0.842)
Search Rate	2.341	1.438	1.919	100	100	100
Unconditional Hit Rate	0.819	0.562	0.699	34.44	38.56	35.88
Moving	67.89	73.83	70.67	59.80	62.19	60.64
Driving while intoxicated	2.261	1.328	1.825	22.11	21.57	21.93
Speeding	55.38	63.43	59.14	28.27	32.94	29.90
Equipment	4.170	2.873	3.564	4.830	4.282	4.638
Regulatory	42.99	36.05	39.75	42.46	37.16	40.60
Observations	5,874,428	5,147,584	11,022,012	137,517	74,029	211,546

Sample restrictions are described in Section 2. All values, excluding log household income, 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.

2.3 Administrative Criminal History Data

We measure arrests and court outcomes using data from the Texas Computerized Criminal History System. These data are maintained by the Texas Department of Public Safety. State troopers have access to these same data when conducting stops. The data track state felony and misdemeanor criminal charges from arrest to sentencing up to 2015.[10] 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 an individual's full name, address, race and ethnicity, gender, and a unique individual ID.

2.4 Commercial Address History Data

One limitation of the traffic stop data is that it does not include a unique motorist ID. The problem this presents is that, for two traffic stops with the same associated motorist name but different addresses, we do not know whether these stops 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, if they have any criminal history at all.

To facilitate matching traffic stops and criminal history to a given motorist, we use commercial data on address history from Infogroup. These data are similar to address history data used in prior research, including Diamond et al. (2019) and Phillips (2020). For each individual, the data include their full name and street addresses at which the individual lived with estimated dates of residence. The data extract we use includes the address histories for all people in the database with a Texas residence between 2005 and 2016.

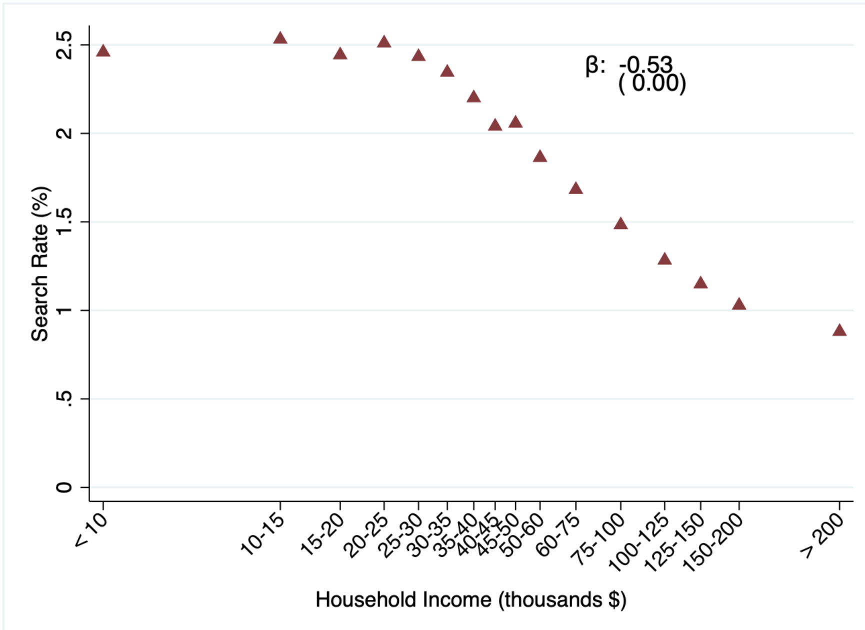
We merge traffic stops and criminal history to motorists using full name and address, incorporating address history data to account for address changes. Note that we do not require a match with the address history data to include a traffic stop in the analysis.

3 Class Disparities in Search Rates and Hit Rates

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

[10] In analyses based on court outcomes, we limit the sample to arrests records from 2010 and earlier as records are less complete in later years.

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.

3.1 Search Rates

We first examine search rates. Figure 2 plots the search rate as a function of income.[11] A 10 log point increase in household income is associated with a 0.05 percentage point decrease in the search rate. Motorists in the top quintile by income are searched in 1.1% of stops. Motorists in the bottom quintile are searched in 2.5% of stops, more than twice as often. To put the magnitude of this class disparity in perspective, note that 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 decreasing relationship between motorist income and search rates may reflect that the context of stops, including the location and time, varies with motorist income. An advantage of our study setting is that we can investigate the magnitude of class disparities holding contextual factors fixed. Class differences in search rates may also in part reflect previously documented racial and gender differences in search rates. To examine whether the pattern shown in Figure 2 is robust to conditioning on stop context and other motorist demographic characteristics, we estimate linear

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

Table 2
Search Rates and Hit Rates by Motorist Income

Outcome:	Search (×100)			Contraband Recovery (×100)		
	(1)	(2)	(3)	(4)	(5)	(6)
log Household Income	-0.53	-0.54	-0.48	3.25	1.56	1.36
	(0.00)	(0.00)	(0.00)	(0.12)	(0.12)	(0.12)
Sgt. Area × Time of Week × Year FEs		✓	✓			
Sgt. Area × Year FEs					✓	✓
Motorist Demographics			✓			✓
Mean of DV	1.92			35.88		
Observations	11,022,012			211,546		

This table reports regression coefficients from estimates of equation (1), where the outcome is an indicator (multiplied by 100) for whether a stop leads to a search (columns 1–3) or an indicator (multiplied by 100) for whether a search yields contraband (columns 4–6). Section 2.2 discusses the construction of motorist household income. Robust standard errors are provided in parentheses.

probability models of the form

$$Y_{it} = \alpha_{\ell_{i,t}\tau(t)y(t)} + \beta \log(\text{income})_{it} + X_{it}\Gamma + \epsilon_{it}, \tag{1}$$

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 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 race and gender.

Columns 1 through 3 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 -0.48, reflecting that Black and Hispanic motorists are both disproportionately stopped in lower-status vehicles and are more likely to be searched.[12]

Low- and high-income motorists are generally stopped for different violations. For example, high-income motorists are more likely to be stopped for moving violations, including speeding (see Table 1). It is possible that low-income motorists are searched at higher rates because they are stopped for 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 below in section 4.2, the violation that a trooper associates with a stop may itself be influenced by their perception

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

of the motorist's class, so it is arguably inappropriate to condition on the violation when measuring class disparities in search rates. Nonetheless, as a robustness check we repeat our analysis while limiting the sample to stops where the stop was likely initiated by a speeding violation, the most common type of stop. In particular, we limit to stops with an associated speeding violation (leading to a warning or citation) and no DWI violation. Although search rates are generally lower in this sample, (proportional) class disparities are larger (see Appendix Figure C.2).[13]

3.2 Hit Rates

Next, we examine how hit rates, a standard measure of search productivity, vary with motorist income.

Figure 3 plots hit rates as a function of motorist income. For every 10 log point increase in income, hit rates increase by 0.3 percentage points. Troopers detect contraband in 32.6% of searches of motorists in the bottom quintile by income; the hit rate for motorists in the top quintile is 41.1%.

Columns 4 through 6 of Table 2 present additional slope estimates where we add controls for the year and location of stops and motorist demographics as in equation (1). The structure mirrors columns 1 through 3 of the same table. Column 4 does not include additional controls and corresponds to the slope estimate provided in Figure 3, 3.25. Column 5 includes fixed effects for combinations of stop location and time. The slope decreases to 1.56. Column 6 adds fixed effects for motorist race and gender. The slope attenuates slightly to 1.36.

The fact that search rates are decreasing in motorist income is inconsistent with troopers' maximizing contraband yield and implies that troopers could increase contraband yield by reallocating searches from low-income motorists to high-income motorists (Feigenberg and Miller, 2022).[14] We return to this point in section 5.3. One potential explanation for trooper behavior is that, among motorists found with contraband, low-income motorists are found with more serious contraband. In practice, we find that low- and high-income motorists are found with similar forms of contraband (see Appendix Table C.3). We return to potential explanations for trooper behavior in section 6.

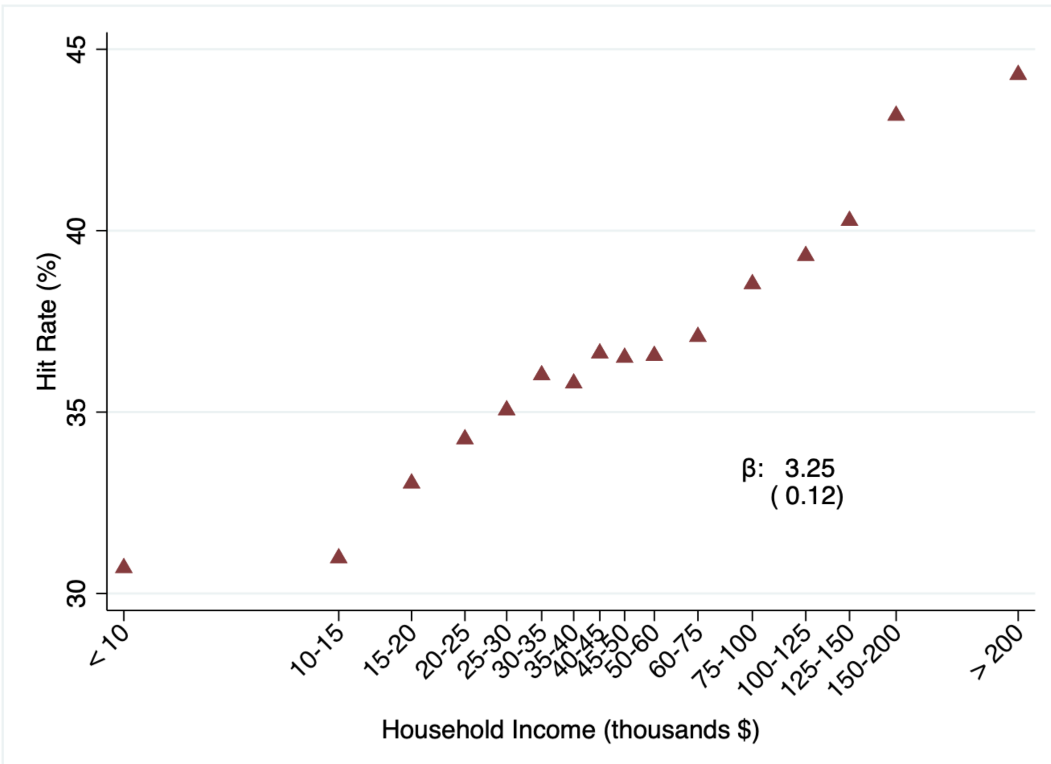
4 Class Differences in Pretext Stops

Although some stops are motivated solely by the need to enforce traffic laws, troopers make some "pretext" stops based on minor infractions to identify a more serious crime, including via search. The fact that troopers are more likely to search low-income motorists suggests that low-income motorists may be more likely to be subject to pretext stops. In this section we develop a simple model of trooper stop and search decisions. The model generates predictions that we use to test whether there are class disparities in exposure to pretext stops.

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

[14]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 there is no inframarginality problem in this context because average and marginal hit rates are similar.

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.

4.1 A Model of Troopers' Stop and Search Behavior

We build on the Anwar and Fang (2006) model of trooper search decisions and add a stop margin. In Anwar and Fang (2006) troopers decide whether to search a stopped motorist using a noisy signal for whether the motorist is carrying contraband. We further suppose that troopers decide whether to *stop* a motorist that has committed a potential traffic infraction based on the severity of the traffic infraction and an even noisier signal for whether the motorist is carrying contraband.

We begin with a continuum of motorists and we first consider the behavior of a single trooper. Suppose fraction π of motorists carry contraband. 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) $f_g(\cdot)$; if the motorist is not carrying contraband, θ is randomly drawn from a continuous PDF $f_n(\cdot)$. (The subscripts g and n stand for "guilty" and "not guilty," respectively.)

We assume that $f_g(\cdot)$ and $f_n(\cdot)$ satisfy a standard monotone likelihood ratio property (MLRP): $f_g(\theta)/f_n(\theta)$ 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 motorist with signal θ, the posterior probability that the motorist is guilty of carrying contraband, $Pr(G|\theta)$, is given by Bayes's rule:

$$P(G|\theta) = \frac{\pi f_g(\theta)}{\pi f_g(\theta) + (1-\pi) f_n(\theta)}.$$

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

Following the literature, we assume that the trooper's objective is to maximize the rate that traffic stops yield contraband, net of search costs, τ. Given this cost structure, troopers will choose some threshold θ^* where troopers will search any motorist with $\theta_i \geq \theta^*$. The trooper's problem is to choose θ^* that maximizes their objective function

$$\int_{\theta^*}^{1} \left[P(G|\theta^*) - \tau\right] f(\theta) d\theta,$$

where $f(\theta) = \pi f_g(\theta) + (1-\pi) f_n(\theta)$. Hence, the trooper will set a threshold θ^* to equalize the marginal cost and benefit of search for the marginal searched motorist:

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

The trooper's utility for a given signal θ is

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

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

Now we take a step back and consider the stop margin. The trooper observes the severity of the violation, ν, where ν represents the direct benefit of stopping a motorist that commits the violation. For example, speeding violations are generally considered greater threats to public safety than expired registration tags. The trooper also observes a signal for whether the motorist is carrying contraband, ω. This signal is coarser than the signal θ in the sense that once a trooper observes θ, ω is completely uninformative. Think of ω as summarizing a subset of information contained in θ. For example, troopers can observe the vehicle a motorist is driving before making a stop; they can also observe the vehicle during the stop.

Let $h(\omega|\theta)$ denote the pdf for ω given θ. We assume another MLRP condition where, for $\theta' > \theta$, $h(\omega|\theta')/h(\omega|\theta)$ increases strictly in ω. Therefore, a higher value of ω indicates that a higher value of θ is more likely. This implies that $E[U(\theta,\tau)|\omega]$ is increasing in ω.

Suppose that the cost of a stop is c. The trooper will conduct a stop if

$$\nu + E[U(\theta,\tau)|\omega] \geq c$$

We define a **pretext stop** as a stop where

$$\nu < c \leq \nu + E[U(\theta,\tau)|\omega] \tag{2}$$

This condition will hold for a wider range of ν values among troopers with low search costs, τ.

High search propensity troopers will conduct more pretext stops. Moreover, as we look at more search-intensive troopers, the violations associated with their stops become increasingly marginal. If all that's changing across troopers is their value of τ, then looking across troopers by search propensity identifies how motorist characteristics compare for marginal violations versus infra-marginal violations.

4.2 Testing for Class Differences in Pretext Stops

We use the model to test whether low-income motorists are more likely to be subject to pretext stops than high-income motorists. It is generally difficult to study how troopers make stop decisions because we typically do not observe information about motorists that are not stopped, but are *at risk* of being stopped. This is known in the racial profiling literature as the "benchmarking problem" (Grogger and Ridgeway, 2006). However, our model provides an indirect test. The key prediction of the model is that, holding the environment fixed, troopers with lower search costs conduct more pretext stops. If low-income motorists are more likely to be subject to pretext stops, then they will tend to be stopped by troopers with low search costs compared to high-income motorists. We test this prediction.

We infer trooper search costs using their search propensities, holding motorist characteristics fixed. The idea is that, for a given stop, troopers with low search costs are more likely to perform searches.

We measure trooper search propensities in three ways. First, we use each trooper's leave-out search rate. This is our baseline measure. Second, we construct a leave-out search rate that first partials out sergeant area by time of week by year fixed effects and a flexible spline in log household income. Third, we construct a leave-out search rate that partials out both sergeant area by time of week by year fixed effects and *motorist* fixed effects.[15] The raw standard deviations for each of the three measures are 2.3, 2.0, and 1.1 percentage points. We standardize each propensity measure to have mean zero and standard deviation one.

Figure 4 plots baseline trooper search propensities by motorist income. Low-income motorists are stopped by more search-intensive troopers. A 10 log point increase in motorist income is associated with a 0.002σ decrease in trooper search propensity. Motorists in the bottom 20% by income are stopped by troopers that are about 0.04σ more search-intensive than motorists in the top 20% by income.

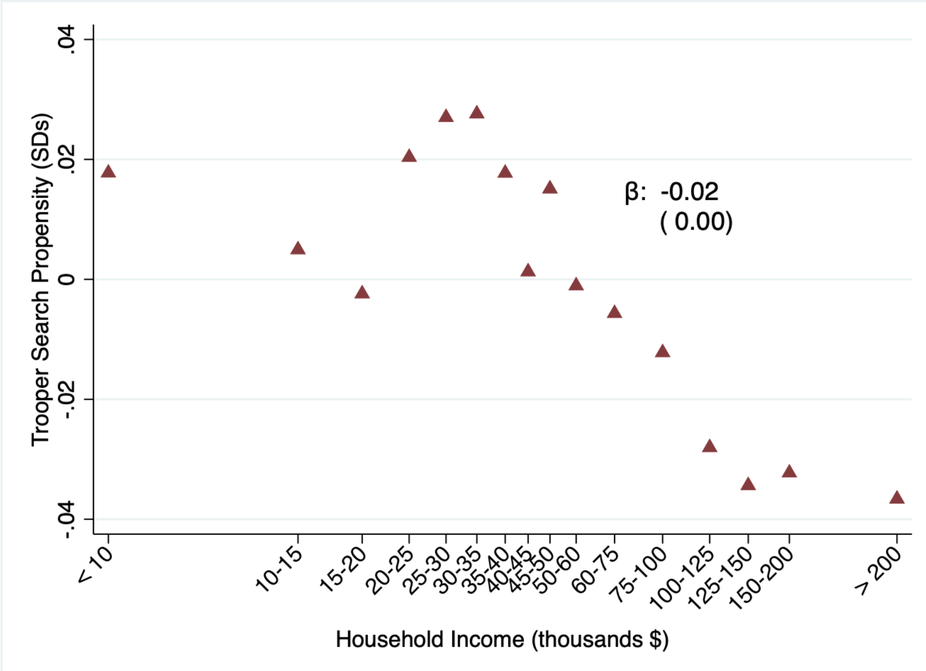
Table 3 presents additional slope estimates where we vary the measure of trooper search propensity and add controls for the year and location of stops as in equation (1). Columns 1 and 2 use the baseline trooper search propensity measure. Columns 3 and 4 use the second trooper search propensity measure, which nets out motorist income and fixed effects for stop location and time. Columns 5 and 6 use the third trooper search propensity measure, which nets out motorist fixed effects. Even columns include fixed effects for combinations of stop location and time. The coefficient hovers around -0.02 in the first four specifications. The coefficient halves to about -0.01 for the third trooper search propensity measure.

Low-income motorists are stopped by more search-intensive troopers and this pattern is consistent with the hypothesis that low-income motorists are more likely to be subject to pretext stops than high-income motorists. However, one alternative interpretation is that troopers vary in how they weigh the severity of different infractions, and low-income motorists are more likely to commit infractions that search-intensive troopers deem as serious. To assess this alternative explanation we limit the sample of stops to those likely initiated by speeding violations as a robustness check (as in section 3.1). Appendix Figure C.3 replicates Figure 4. The same pattern emerges.

The relationship between motorist income and trooper search propensities is statistically significant, but small in magnitude, likely because motorist income is difficult for troopers to infer prior to making a stop. In the next section we document a stronger relationship between a salient class signal, the motorist's vehicle, and trooper search propensities.

[15] The average stop involves a trooper with about 6,000 recorded stops.

Figure 4
Low-Income Motorists Are Stopped by Search-Intensive Troopers



Note: This figure plots the search propensity of the trooper conducting a stop 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 trooper search propensity used is the baseline measure described in section 4.2, the trooper's leave-out search rate. The reported slope coefficient (and standard error) is from a bivariate regression of trooper search propensity on log household income.

Table 3
Low-Income Motorists Are Stopped by Search-Intensive Troopers

	Outcome: Trooper Search Propensity (SDs)					
	(1)	(2)	(3)	(4)	(5)	(6)
log Household Income	-0.017 (0.000)	-0.020 (0.000)	-0.021 (0.000)	-0.019 (0.000)	-0.011 (0.000)	-0.012 (0.000)
Sgt. Area × Time of Week × Year FEs		✓		✓		✓
Propensity Measure	Baseline		Controls		Motorist FEs	
Observations			11,021,893			

This table reports regression coefficients from estimates of equation (1), where the outcome is the search propensity of the trooper conducting the stop. Section 2.2 discusses the construction of motorist household income. Section 4.2 describes the construction of trooper search propensities. Columns 1 and 2 use the baseline measure. Columns 3 and 4 use the second trooper search propensity measure, which nets out motorist income and fixed effects for stop location and time. Columns 5 and 6 use the third trooper search propensity measure, which nets out motorist fixed effects. Robust standard errors are provided in parentheses.

5 Testing for Class Discrimination

We have established that troopers are more likely to search low-income motorists and provided evidence that troopers are more likely to conduct pretext stops of low-income motorists. These findings alone do not establish that troopers engage in class *discrimination* or *profiling*—deciding whether to pursue or search motorists based on their perceived class. A central challenge to investigating disparities based on individual characteristics, such as race or gender, is distinguishing between discrimination and correlated unobservables (Charles and Guryan, 2011). In the ideal experiment structured to isolate class discrimination, we would vary the *perceived* class of a motorist while holding their economic circumstances or behavior fixed. In this section we test for trooper discrimination using a class signal that is salient to troopers and varies substantially *within* motorists: the motorist's vehicle. Specifically, we use the fact that many motorists are stopped in multiple vehicles conveying varying class signals.

Class profiling implies that troopers should be more likely to search the same motorist when they are driving a low-status vehicle than when they are driving a high-status vehicle. The key assumption that underpins our quasi-experimental design is that other search determinants—including motorist's demeanor and other signals that the motorist is in fact carrying contraband—are fixed within motorist or within-motorist deviations are independent of the vehicle the motorist is driving. This assumption is plausible because motorists often change the vehicle they drive from trip to trip without any coincident change in their economic circumstances. In particular, many motorists have access to multiple vehicles and vehicle purchases are typically infrequent. We provide more rigorous support for this identifying assumption below.

To construct our measure of *vehicle status* ($\text{VEHICLE STATUS}_{it}$), we use the stop data to predict log household income given the vehicle involved in the stop. 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 age.[16] The classification is intuitive. New vehicles are higher in status than old vehicles; luxury brand vehicles are higher in status than economy brand vehicles.

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). Second, the correlation between vehicle status and log household income is only 0.25. Figure 5 plots histograms of vehicle status for motorists in the bottom 20% and top 20% by income. There is substantial overlap.[17]

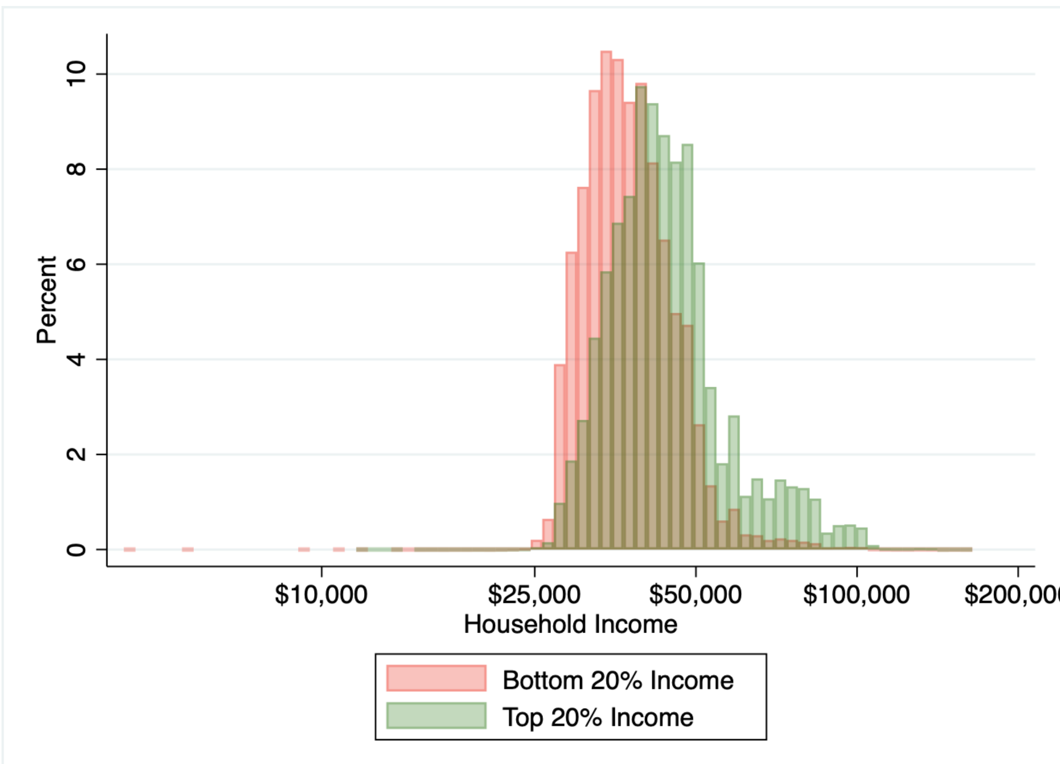
In addition to signalling motorist income, our vehicle status measure is also correlated with other motorist characteristics. Conditional on income, vehicle status is correlated with neighborhood education levels.[18] Prior research suggests that vehicle status is also an indicator of household

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

[17]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.

[18]Based on 2017 NHTS data, we find that vehicle status is correlated with education at the individual level,

Figure 5
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.

liquidity, particularly for low-income households (Adams et al., 2009; Aaronson et al., 2012; Mian et al., 2013).

We relate VEHICLE STATUS$_{it}$ to search rates, contraband yield, and trooper search propensities in Table 4. We estimate regression models analogous to equation (1), where we include VEHICLE STATUS$_{it}$ as an explanatory variable.

Columns 1 through 3 relate VEHICLE STATUS$_{it}$ to search. All columns include fixed effects for combinations of stop location and time. Column 1 does not include additional controls. The coefficient for VEHICLE STATUS$_{it}$ is -3.71, indicating that a 10 log point 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 -3.23. Column 3 adds income as an additional control. This further attenuates the coefficient to -2.95.

The coefficient for VEHICLE STATUS$_{it}$ is an order of magnitude higher 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.[19]

Columns 4 through 6 relate VEHICLE STATUS$_{it}$ to contraband yield, limiting to stops that led to searches. All columns include fixed effects for combinations of stop location and year. Column 4 does not include additional controls. The coefficient for VEHICLE STATUS$_{it}$ is 2.69, indicating that a 10 log point increase in VEHICLE STATUS$_{it}$ is associated with a 0.27 percentage point increase in the hit rate. Controlling for motorist race and gender (column 5) has little effect. Column 6 adds income as an additional control. This attenuates the coefficient to 1.80, comparable to the slope for income, 1.30. Note that the coefficient on income is estimated with significantly more precision. Overall, the evidence is consistent with vehicle status and household income conveying similar information about contraband risk.

Columns 7 and 8 relate VEHICLE STATUS$_{it}$ to the search propensity of the trooper conducting the stop. Here we use the baseline search propensity measure described in section 4.2. Both columns include fixed effects for combinations of stop location and time. Column 8 includes both vehicle status and income as explanatory variables. 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

conditional on household income.

[19]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 Trooper Search Propensities by Vehicle Status

Outcome:	Search (×100)			Contraband Recovery (×100)			Trooper Search Propensity (SDs)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Vehicle status	-3.71	-3.23	-2.95	2.69	2.63	1.80	-0.18	-0.18
	(0.02)	(0.02)	(0.02)	(0.56)	(0.56)	(0.57)	(0.00)	(0.00)
log household income			-0.33			1.30		-0.01
			(0.00)			(0.13)		(0.00)
Sgt. Area × Time of Week × Year FEs	✓	✓	✓				✓	✓
Sgt. Area × Year FEs				✓	✓	✓		
Motorist Demographics		✓	✓		✓	✓		
Observations	11,022,012			211,546			11,021,893	

This table reports regression coefficients from estimates of equation (1), where the outcome is an indicator (multiplied by 100) for whether a stop leads to a search (columns 1–3), an indicator (multiplied by 100) for whether a search yields contraband (columns 4–6), or the standardized search propensity of the trooper conducting the stop (columns 7–8). Section 4.2 describes the construction of trooper search propensities. We use the baseline measure. 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.

stop.

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

5.1 Discrimination in Search

To test for class discrimination, we limit our sample to motorists that we see stopped multiple times. Table 5 compares motorists who are stopped once to motorists who are stopped multiple times and compares sequential stops of the same vehicle to sequential stops of different vehicles. 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. 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.[20] 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. The average (absolute) change in vehicle age is 5 years.

We look at sequential pairs of stops for the same motorist and relate first differences in search rates to first differences in vehicle status, $\text{VEHICLE STATUS}_{it}$. Panel A of Figure 6 plots the results. For the same motorist, search rates are decreasing in $\text{VEHICLE STATUS}_{it}$. For every 10 log point increase in status, the search rate decreases by 0.08 percentage points. This pattern indicates that troopers are profiling motorists based on their perceived class.[21] 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 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 6 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 robustness checks to probe this concern.

First, we conduct a placebo test where we check whether *future* changes in vehicle status predict contemporaneous changes in search rates. We conduct this test for sequential stops of motorists in the same vehicle prior to a third stop in a different vehicle. Panel B of Figure 6 shows the results of this exercise. Unlike Panel A, the relationship is flat. Future changes in vehicle status do not have predictive power. Motorists are not on a downward (upward) trajectory in search risk before switching to a higher-status (lower-status) vehicle.

Second, we check whether the results vary with the time between sequential stops. Less time

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

[21] In Appendix Figure C.7, we again limit the sample of stops to those likely initiated by speeding violations as a robustness check. The results are similar.

Table 5
Descriptive Statistics for Sequential Stops

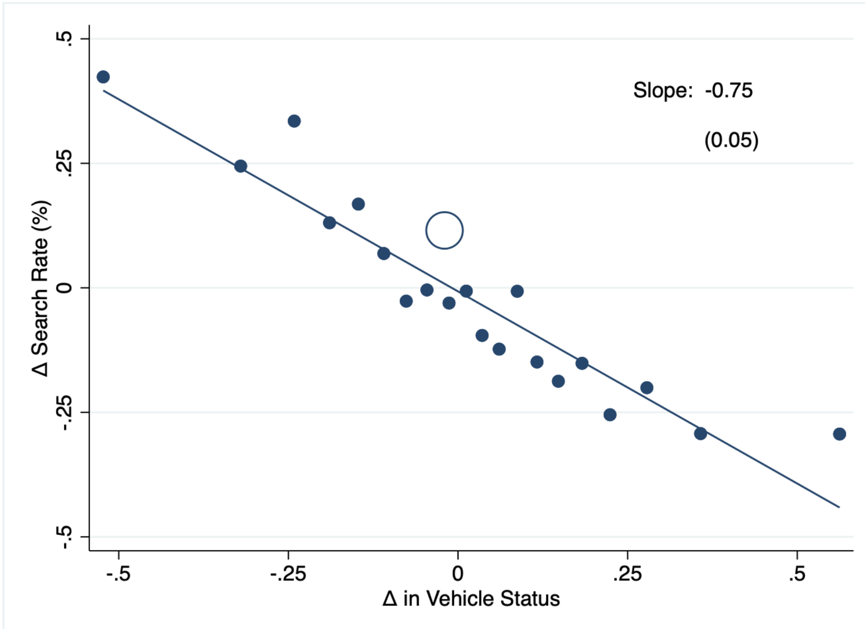
	Single Stop	Multiple Stops	
		Same Vehicle	Different Vehicle
Black	10.67	8.446	8.364
Hispanic	30.73	30.43	35.23
White	54.68	58.95	54.40
Female	40.98	33.65	25.83
Log Household Income	10.62	10.56	10.55
	(0.874)	(0.906)	(0.908)
Search Rate	2.022	1.808	1.925
Unconditional Hit Rate	0.749	0.684	0.684
Change in Vehicle Status	.	-0.0199	0.0197
	(.)	(0.0294)	(0.245)
Change in Vehicle Age	.	0.694	-0.461
	(.)	(0.924)	(6.510)
Months between Stops	.	8.683	16.69
	(.)	(10.37)	(15.26)
Absolute Change in Vehicle Status	.	0.0199	0.187
	(.)	(0.0294)	(0.160)
Absolute Change in Vehicle Age	.	0.694	4.865
	(.)	(0.924)	(4.350)
Observations	4,398,158	2,102,948	2,323,337

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).

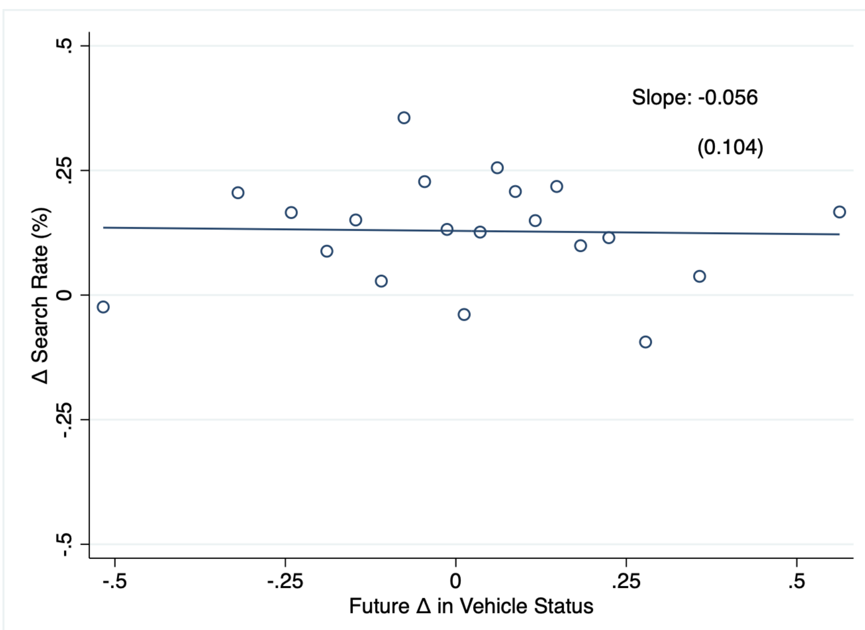
Figure 6

Troopers Profile Motorists at the Search Margin

(a) Contemporaneous Change in Vehicle



(b) Future 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.

between stops leaves less time for a motorist's economic circumstances to have changed between stops. Appendix Figure C.6 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 check whether changes in vehicle status coincide with changes in income as proxied by changes in residential address. We find that changes in vehicle status are not substantively associated with changes in income (see Appendix Figure C.4).

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, 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.5). 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 6. 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 essentially zero and unrelated to the status difference between vehicle A and vehicle B.

5.2 Discrimination in Pretext Stops

Paralleling our analysis in section 5.1, we look at sequential pairs of stops for the same motorist and relate first differences in the search propensity of the trooper to first differences in vehicle status. The interpretation of this exercise is complicated by the fact that we are conditioning on a stop occurring. If we think of stops by search-intensive troopers as marginal, then our findings suggest that, when driving low-status vehicles, motorists are stopped in cases where they would not be stopped in high-status vehicles.

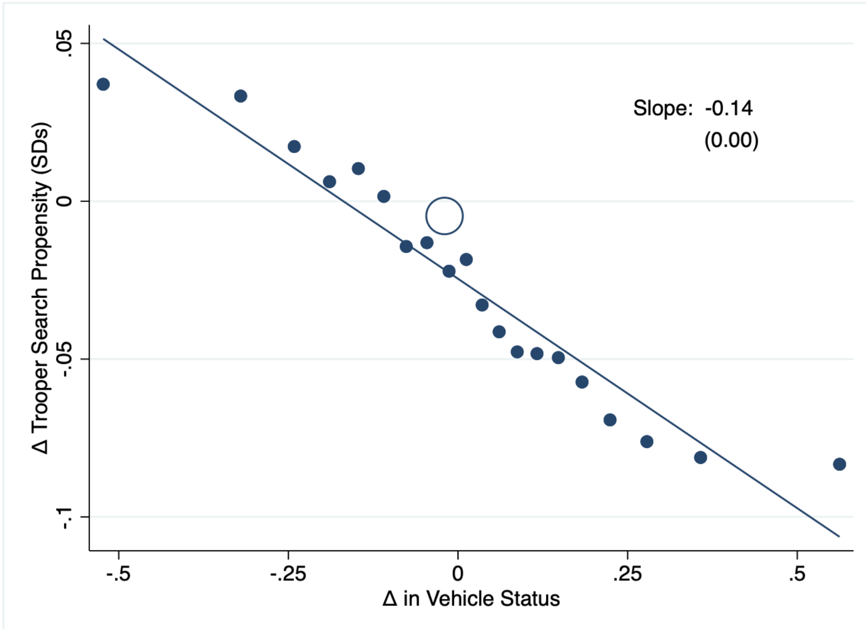
Panel A of Figure 7 shows the results using a binscatter plot with changes in the search propensity of the trooper conducting the stop on the vertical axis and changes in vehicle-based status on the horizontal axis. For the same motorist, the search propensity of the trooper conducting the stop is decreasing in vehicle status. The magnitude of the within-motorist relationship is about 80% as large as the overall relationship between VEHICLE STATUS$_{it}$ and trooper search propensity (see column 8 of Table 4).[22]

We repeat robustness checks analogous to those conducted in 5.1.

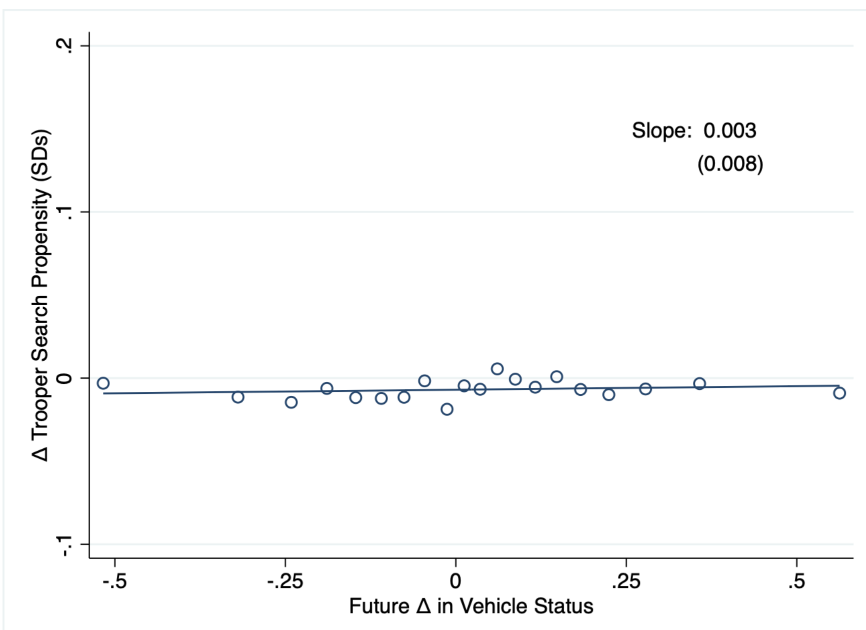
[22]In Appendix Figure C.9, we limit the sample of stops to those likely initiated by speeding violations. The results are similar.

Figure 7
Troopers Profile Motorists at the Stop Margin

(a) Contemporaneous



(b) Placebo



Note: These figures look at first differences in trooper search propensities for sequential pairs of stops of the same motorist as a function of changes in vehicle status. Panel A plots first differences in trooper search propensities against first differences in vehicle status. The open circle depicts the change in trooper search propensities 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 trooper search propensities.

First, we conduct a placebo test where we check whether *future* changes in vehicle status predict contemporaneous changes in trooper search propensity. We conduct this test for sequential stops of motorists in the same vehicle prior to a third stop in a different vehicle. Estimates are provided in Panel B of Figure 7. The relationship between *future* changes in vehicle status and contemporaneous changes in trooper search propensity is essentially flat.

Second, we check whether the results vary with the time between sequential stops. As above, we find a similar pattern for sequential pairs with more or less time between stops.

Third, we focus on a sample of motorists who are stopped multiple times and in alternating vehicles (see Appendix Figure C.8). Once again, we find a similar pattern for sequential stops that involve different vehicles (from vehicle A to vehicle B). When we compare trooper search propensities for stops involving the motorist's original vehicle (vehicle A) before and after stops involving a different vehicle (vehicle B), the change in trooper search propensities is essentially zero and unrelated to the status difference between vehicle A and vehicle B.

5.3 Identifying Hit Rates at the Margin

We have shown that troopers profile motorists based on their vehicle, a tactic that leads to more searches of low-income motorists. The fact that hit rates are increasing in motorist income suggests that profiling by vehicle reduces contraband yield. The within-motorist research design allows us to test this hypothesis directly. We measure the marginal hit rate for searches induced by variation in vehicle status and compare marginal hit rates for low- and high-income motorists.

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

$$\Delta_{it}\text{CONTRABAND} = \beta\Delta_{it}\text{SEARCH} + \epsilon_{it}, \tag{3}$$

where the first stage is

$$\Delta_{it}\text{SEARCH} = \pi\Delta_{it}\text{VEHICLE STATUS} + \zeta_{it}. \tag{4}$$

We report first stage, reduced form, and 2SLS coefficient estimates in Table 6. We split sequential pairs of stops into terciles based on motorist income, as measured for the first stop. We focus on the bottom and top terciles. The effect of vehicle status on search rates is similar for low- and high-income motorists. Yet the marginal hit rate for high-income motorists is more than twice as high.[23] Reallocating marginal searches of low-income motorists to high-income motorists would increase contraband yield. In other words, overall contraband yield would increase if all motorists were treated as if they were driving the average vehicle.

[23] The difference in coefficients across samples is statistically significant at the 1% level.

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	-0.75	-0.09	-0.27		
	(0.10)	(0.07)	(0.05)	(0.04)		
Δ Search					0.15	0.36
					(0.08)	(0.05)
Model	OLS	OLS	OLS	OLS	2SLS	2SLS
Observations	897,801	659,249	897,801	659,249	897,801	659,249

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.

6 Hassle Costs and Trooper Objectives

While researchers examining search behavior typically assume that troopers seek to maximize contraband yield, we posit that troopers may also respond to anticipated "hassle costs" associated with the adjudication process that follows contraband discovery and arrest. In Texas, criminal defense attorneys may seek trooper testimony during pre-trial pleadings, 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).[24]

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

[24] 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.

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 (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 will disproportionately rely on publicly-appointed counsel, these disparities indicate that troopers are indeed likely to face significantly 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.[25]

Panel A of Figure 8 plots guilty/no contest plea rates for motorists arrested after they are found with contraband as a function of motorist income. Guilty/no contest plea rates are decreasing in motorist income. Because our sample of motorists arrested after contraband discovery is relatively small, in Panel B of Figure 8 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.[26] The pattern in this

[25]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 judgement (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.

[26]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

much larger sample of arrests is similar to what we see for arrests resulting from motor vehicle searches in Panel A. A 10 log point increase in income is associated with a 0.24 percentage point decrease in the guilty or no contest plea rate.

In Appendix Figure C.10, we document that dismissal or acquittal rates are similarly increasing 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.[27] 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:

$$Y_{ict} = \alpha_{cth(i,t)} + X_i\Gamma^x + Z_{it}\Gamma^z + \Theta_{j(i,c,t)} + \epsilon_{ict}. \tag{5}$$

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.[28]

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:

$$Y_{ict} = \alpha_{\tau(t)y(t)} + X_i\Gamma^x + \Theta_{j(i,c,t)} + \epsilon_{ict}. \tag{6}$$

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

penalty group 1, between 4 and 200 grams; possession of controlled substance not in penalty group; prohibited substance in a correctional facility.

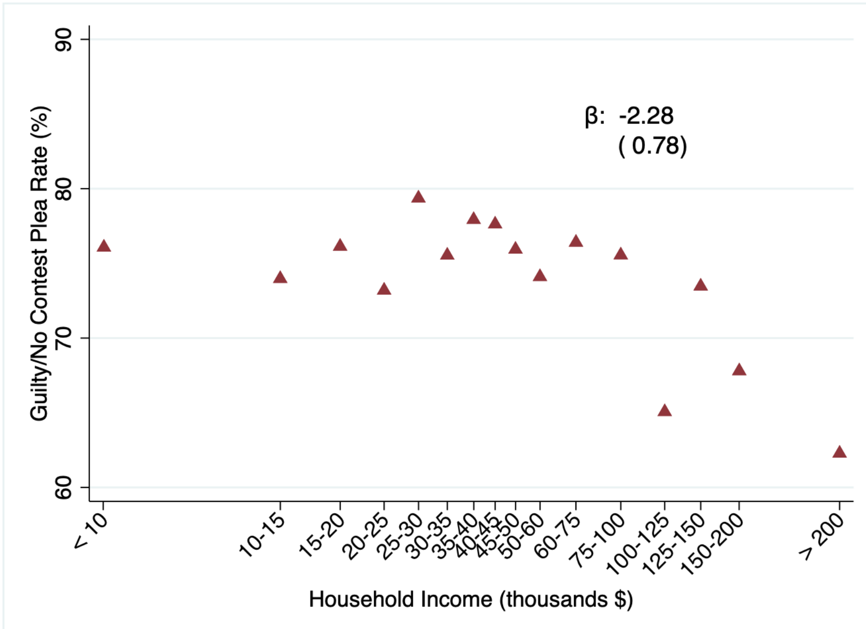
[27]In Texas, criminal cases are handled in District and County Courts, with most courts serving a single county.

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

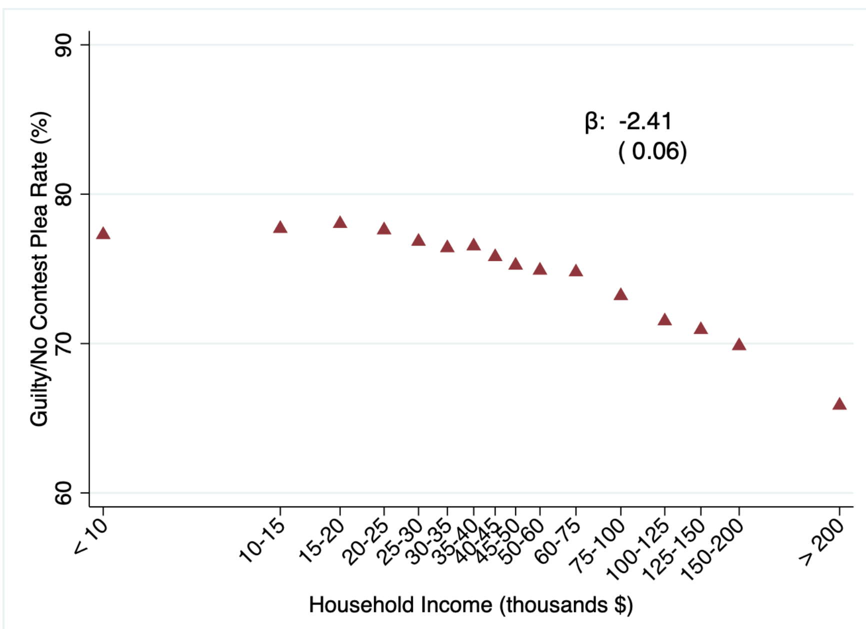
Figure 8

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 26 for details.

motorist fixed effects.

Table 7
County-level Search Rates and Hassle Costs

	Outcome: County-level Residual Search Rate					
	(1)	(2)	(3)	(4)	(5)	(6)
County-level Residual	0.016	0.014	0.014			
Guilty/No Contest Plea Rate	(0.005)	(0.004)	(0.004)			
County-level Residual				-0.017	-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 6) on county-level residual guilty/no contest plea rates and residual dismissal/acquittal rates (derived based on equation 5). 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 6. In columns 3 and 6, county-level measures are constructed conditional on defendant fixed effects in addition to the controls included in equation 5. 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 leads to a 0.14-0.16 percentage point (7-8 percent) increase in the residual search rate. Likewise, a 10 percentage point decrease in the dismissal/acquittal rate leads to a 0.16-0.17 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 large differences in how Texas state troopers interact with low-income and high-income motorists. Troopers are more likely to search low-income motorists, despite the fact that these searches are less likely to yield contraband than searches of high-income motorists. We also provide evidence that troopers are more likely to pursue low-income motorists for pretext stops.

To test whether class-based stop and search rate disparities we document reflect profiling on class *per se*, we develop a quasi-experimental research design that isolates how the same motorist fares in different vehicles that convey different class signals. We find that when motorists are stopped in a low-status vehicle, they are more likely to be searched and are stopped by more search-intensive troopers.

To the best of our knowledge, this is the first paper to characterize class disparities in traffic stops and searches and to identify the causal effect of perceived motorist class on these outcomes. One factor contributing to the dearth of prior evidence on class disparities is that many law enforcement agencies regularly report detailed information on stops, searches, and arrest counts by race, but do not collect or publish the data required to examine corresponding patterns as a function of economic class. We hope that our work helps to clarify the importance of such data collection efforts.

Even focusing on the search margin alone and ignoring disparities in exposure to pretext stops, our estimates indicate that low-income motorists are more likely to be searched and found with contraband during a stop despite the fact that they are less likely to be found with contraband conditional on being searched. This finding has important implications for fairness and equity in the criminal justice system and likely contributes to differences in subsequent exposure to criminal sanctions, which may limit future labor market opportunities and access to social programs. As such, a key question is what drives the sizable class disparities we observe. We have provided suggestive evidence that the hassle costs associated with class differences in court-mandated officer appearances may offer a partial answer. Future research that further unpacks why officer behavior changes with motorist income can illuminate effective policies to address the disparities we have found.

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

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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.

A.1 Imputing Income

We infer each motorist's household income as follows. Based on 5-year periods of American Community Survey (ACS) data, the Census Bureau reports estimates for the household income distribution at the level of the block group, a Census tract subdivision that generally includes between 600 and 3,000 people. We use ACS data from 2009–2013. The ACS reports income statistics for all households and separately for homeowners and renters. Household income is partitioned into 16 intervals. The 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. The ACS uses only 7 intervals when reporting information separately for homeowners and renters: 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. Within each of these coarser intervals, we assume for simplicity that homeowners and renters follow the same distribution across the more granular intervals.

For motorists living in single-family residences, we assign motorists to percentiles within block groups based on the assessed property value of their residence. We use property-level records of assessments from ATTOM and assessments as of 2015. 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 homes or apartment complexes (and those we are unable to match to a specific property), we assign the median household income category among renters in their block group.

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.

Our household income measure is imperfect for several reasons. The block group-level distribution of household income derived from the ACS is estimated with 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. 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.[29] The statewide rank correlation is 0.72. Moreover, property assessments may not accurately reflect property values. Nonetheless, our household income measure should capture important dimensions of economic well-being.

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.4. 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.

[29]Note that tracts are collections of block groups.

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

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	9.627	7.801	8.865	19.69	16.79	18.81
Hispanic	33.10	20.71	27.93	37.78	26.71	34.42
White	54.71	67.81	60.18	40.50	53.39	44.41
Female	37.62	36.00	36.94	18.61	17.02	18.13
Log Household Income	10.08	11.48	10.67	10.02	11.41	10.44
	(0.617)	(0.457)	(0.888)	(0.626)	(0.428)	(0.856)
Search Rate	0.964	0.586	0.806	100	100	100
Unconditional Hit Rate	0.259	0.194	0.232	26.60	32.85	28.49
Moving	100	100	100	100	100	100
Driving while intoxicated	0	0	0	0	0	0
Speeding	100	100	100	100	100	100
Equipment	1.062	0.732	0.924	2.091	1.951	2.048
Regulatory	24.86	18.76	22.31	42.29	31.98	39.16
Observations	3,772,069	2,703,131	6,475,200	36,352	15,834	52,186

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, 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	50.5	51.8	55.2
Weapon	43.0	45.3	44.8	43.9	40.3
Other	3.5	3.9	4.0	3.8	3.7
Observations	27,794	18,605	13,266	10,330	7,047
	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
Weapon	44.0	44.1	43.7	43.6	42.7
Other	3.6	4.0	3.6	3.7	3.5
Observations	19,901	19,508	15,111	12,581	9,941

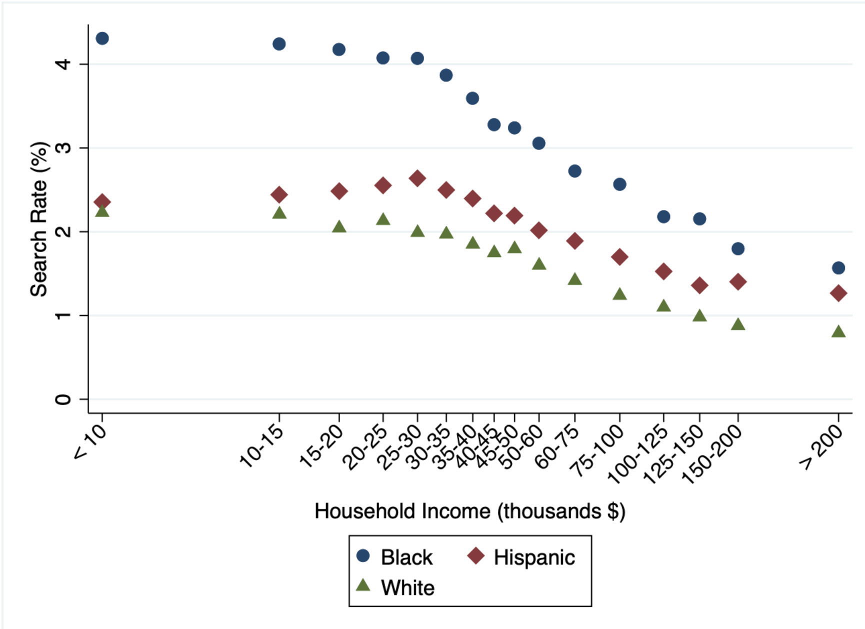
This table summarizes the distribution of contraband type by motorist economic status among motorists found with contraband. The top panel groups motorists into quintiles by vehicle status. The bottom panel groups motorists into quintiles by log household income.

Table C.4
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 Vehicle Group	0.810 (0.023)	0.792 (0.023)	1.472 (0.056)	0.784 (0.056)		
Log Average Income by Block Group Characteristics					0.790 (0.037)	0.252 (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.

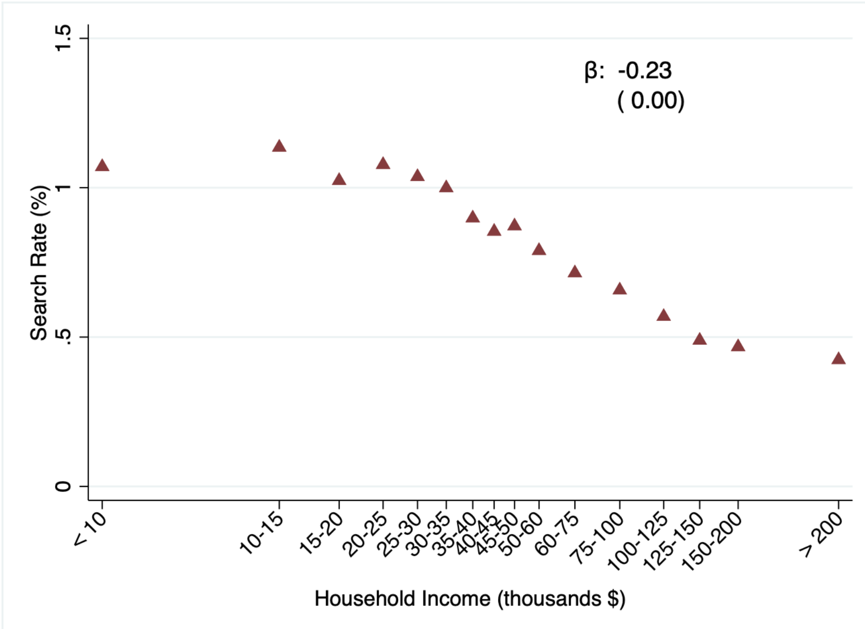
Figure C.1
Search Rates by Motorist Income and Race



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.

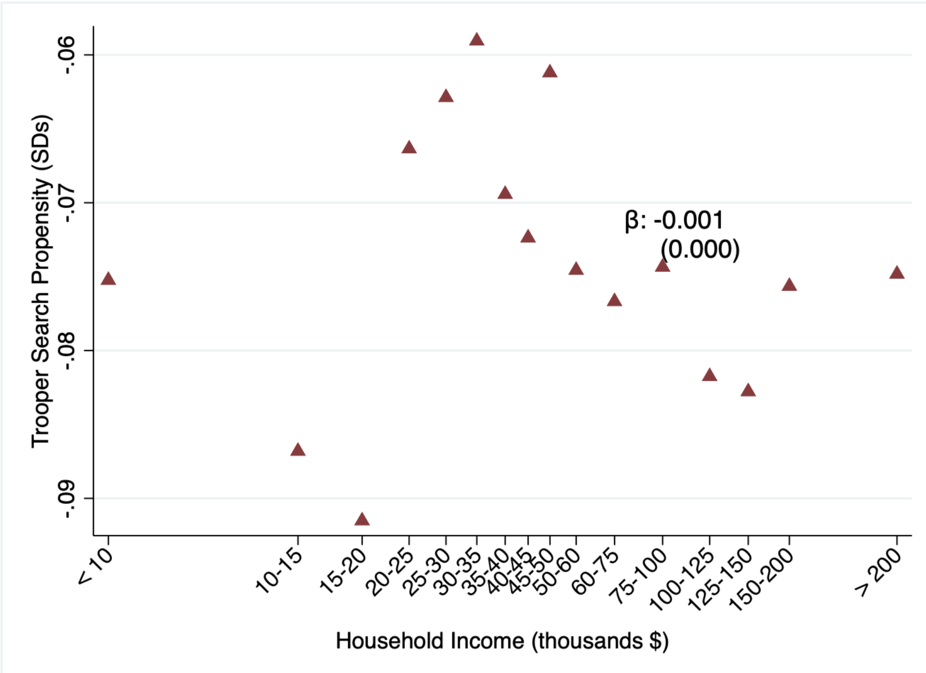
Figure C.2

Search Rates Are Decreasing in Motorist Income, Non-DWI Speeding Stops



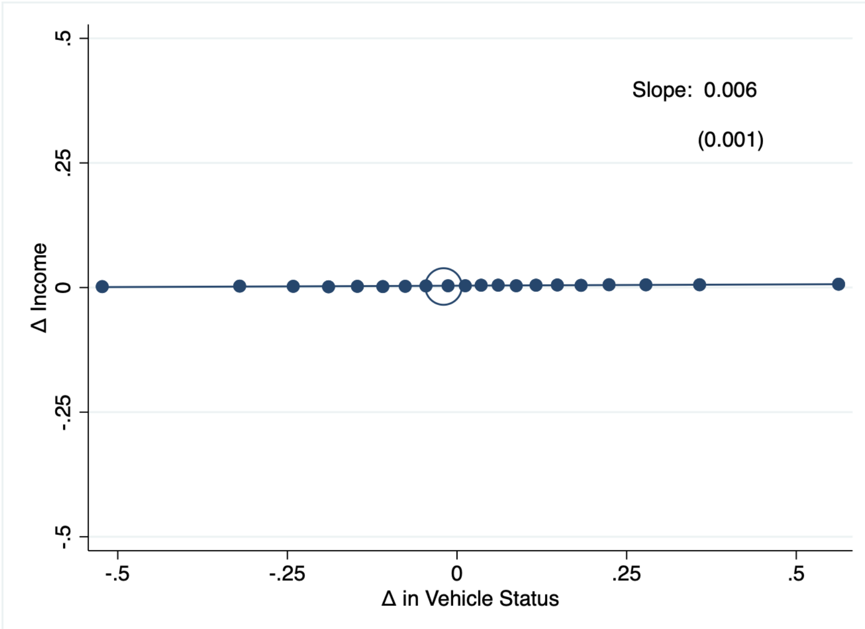
Note: This figure plots search rates as a function of motorist income. Household income is depicted on a log scale. Section 2.2 discusses the construction of the household income measure, which partitions household income into 16 intervals. We use the average household income for all Texas households 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
Low-Income Motorists Are Stopped by Search-Intensive Troopers, Non-DWI Speeding Stops



Note: This figure plots the search propensity of the trooper conducting a stop 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 trooper search propensity used is the baseline measure described in section 4.2, the trooper's leave-out search rate. The sample is limited to stops with a speeding warning or citation, and no DWI warning or citation.

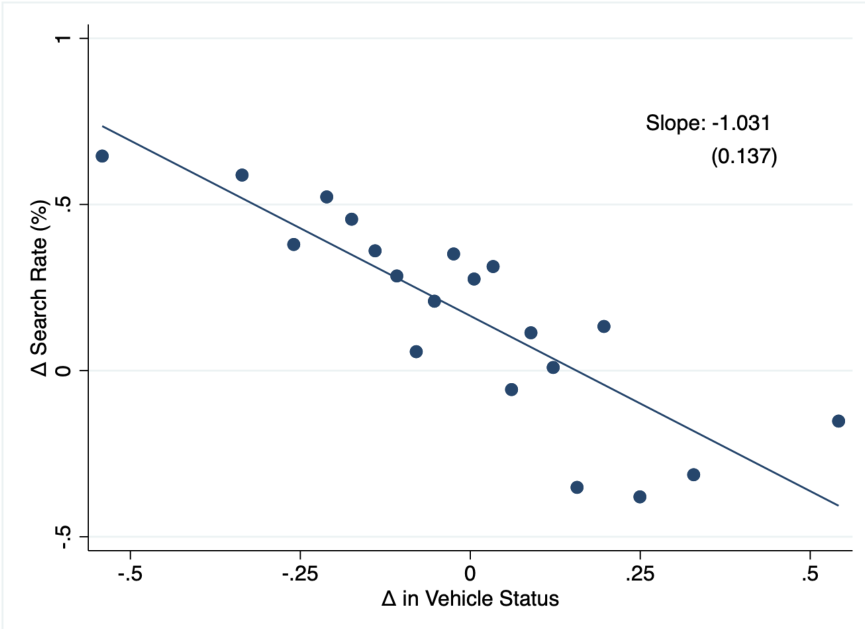
Figure C.4
Change in Motorist Income Between Stops in Within-Motorist Design



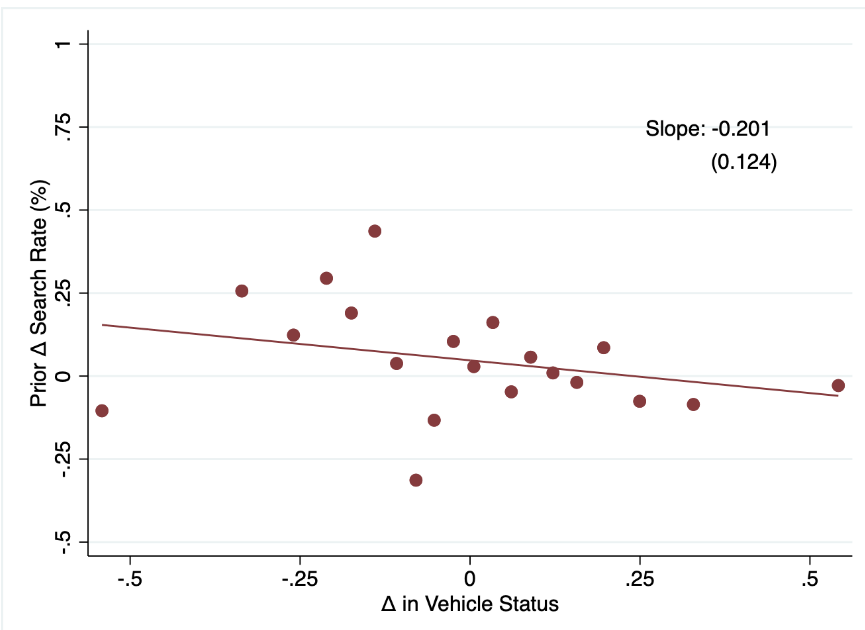
Note: This figure looks at the change in log household income between sequential stops (as described in section 5.1) as a function of the change in vehicle status between stops. A motorist's log household income may change if they change addresses between stops.

Figure C.5
Search Rates for Motorists That Alternate between Vehicles

(a) Contemporaneous Change in Vehicle

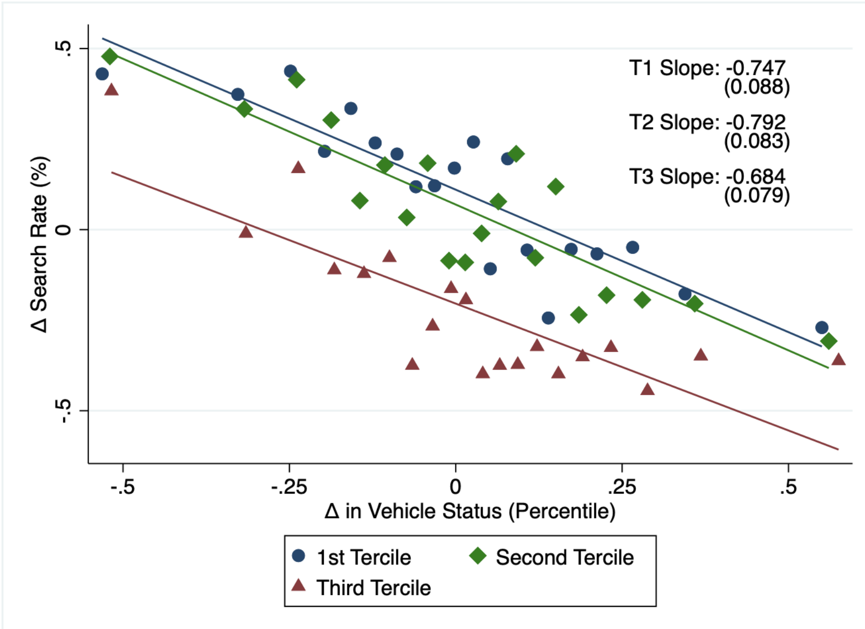


(b) Stops of Same Vehicle Bracketing Stops of Other Vehicles



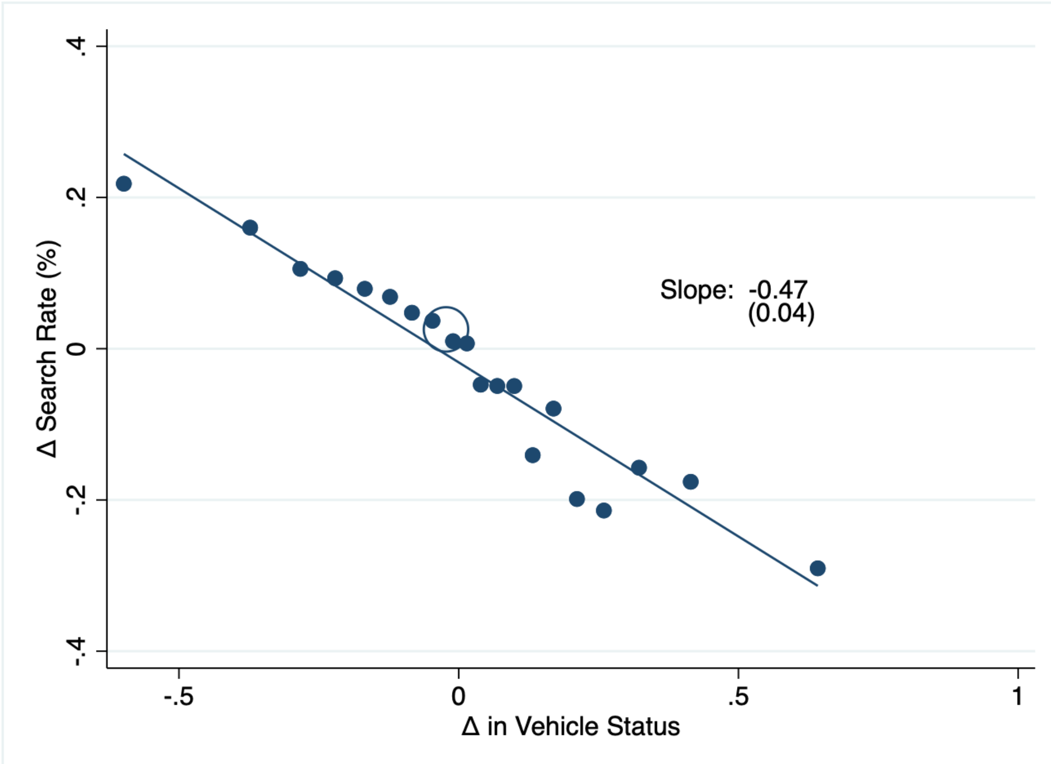
Note: This figure looks at differences in search rates for pairs of stops of the same motorist, limiting to motorists that alternate back and forth between vehicles. Panel A limits to sequential pairs of stops involving two vehicles (vehicle A then vehicle B). Panel B looks at the pairs of stops of the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

Figure C.6
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.7
Troopers Profile Motorists at the Search Margin, Non-DWI Speeding Stops

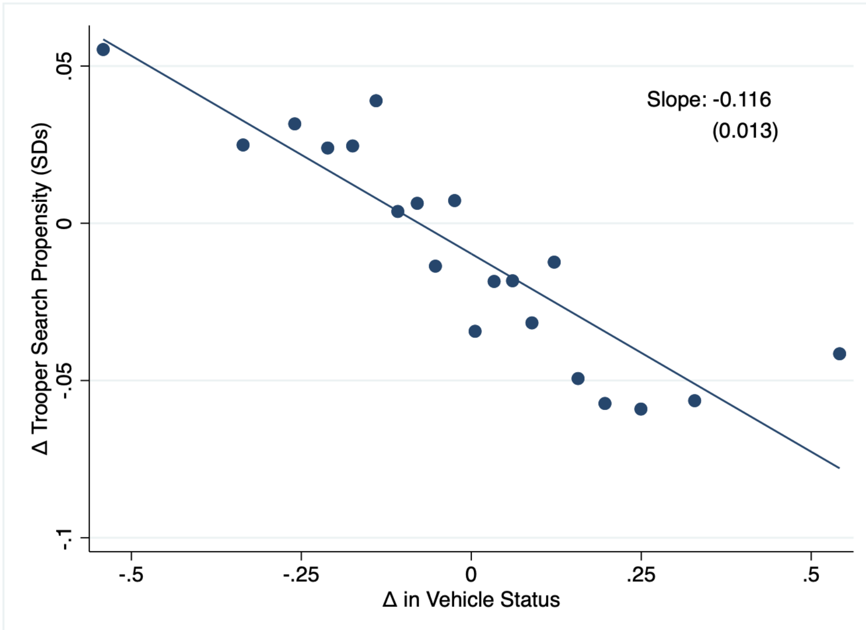


Note: This figure looks at differences in search rates for sequential pairs of stops of the same motorist. Stops are limited to those with a speeding warning or citation, and no DWI warning or citation. The figure plots differences in search rates against differences in vehicle status. The open circle depicts the change in search rates for sequential pairs of stops where the same vehicle is involved in both stops.

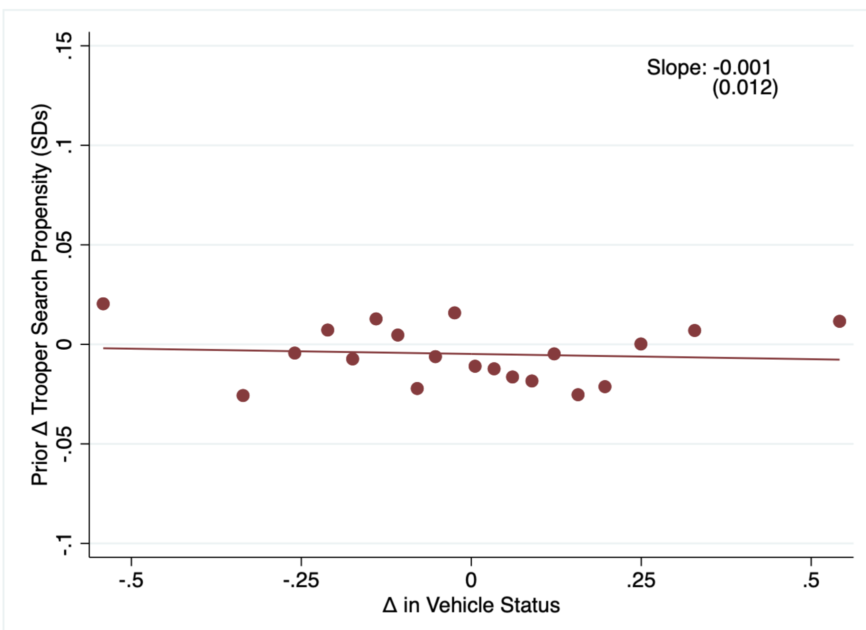
Figure C.8

Trooper Search Propensities for Motorists That Alternate between Vehicles

(a) Contemporaneous Change in Vehicle



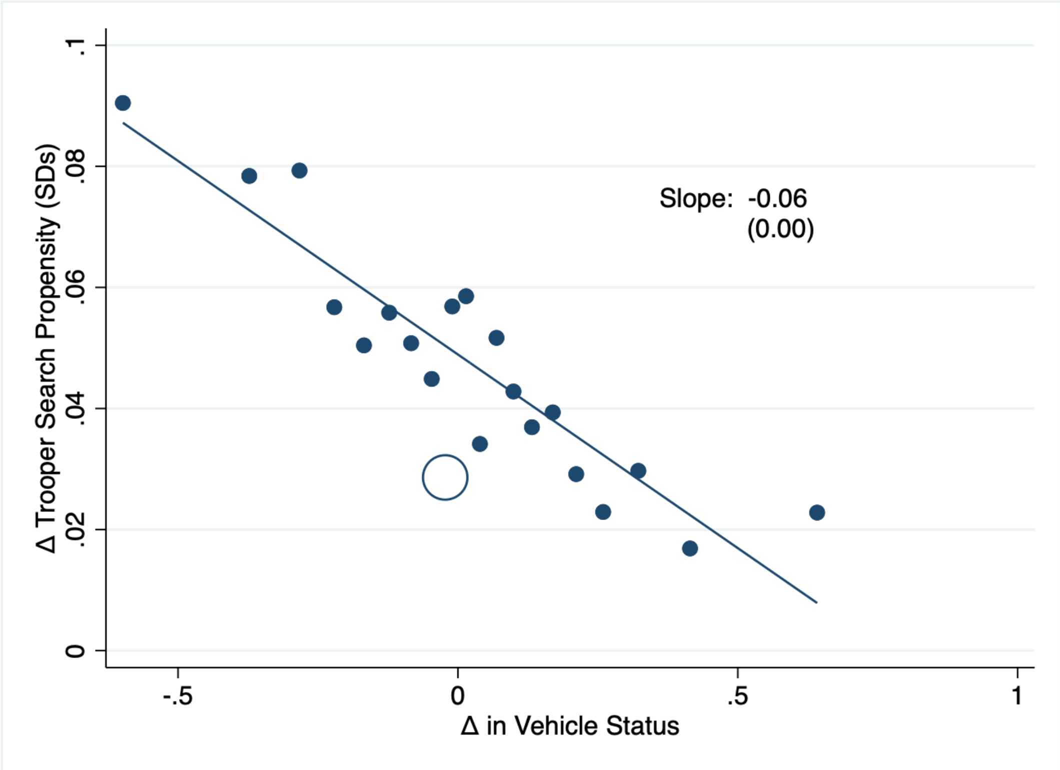
(b) Stops of Same Vehicle Bracketing Stops of Other Vehicles



Note: This figure looks at differences in trooper search propensities 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.9

Troopers Profile Motorists at the Stop Margin, Non-DWI Speeding Stops

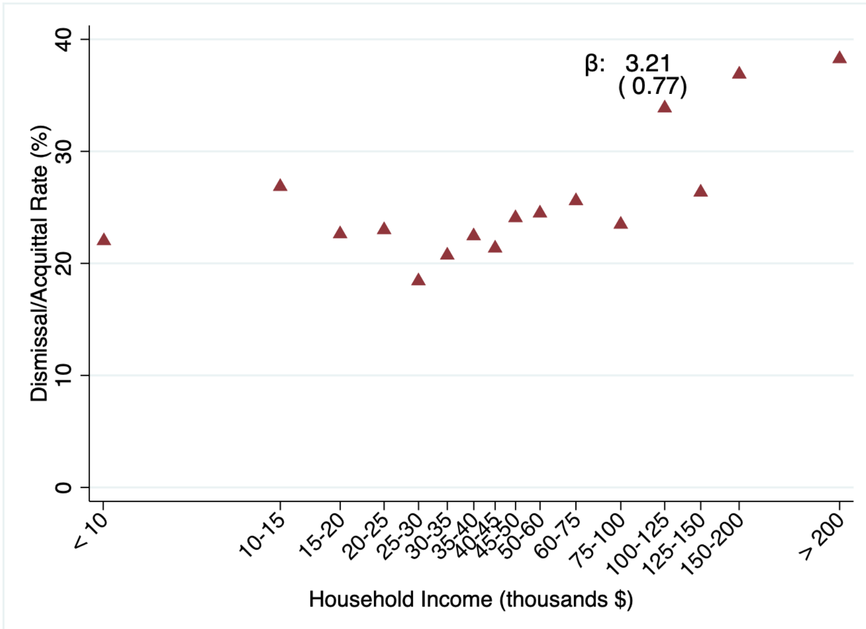


Note: This figure looks at differences in trooper search propensities for sequential pairs of stops of the same motorist. Stops are limited to those with a speeding warning or citation, and no DWI warning or citation. The figure plots differences in trooper search propensities against differences in vehicle status. The open circle depicts the change in trooper search propensities for sequential pairs of stops where the same vehicle is involved in both stops.

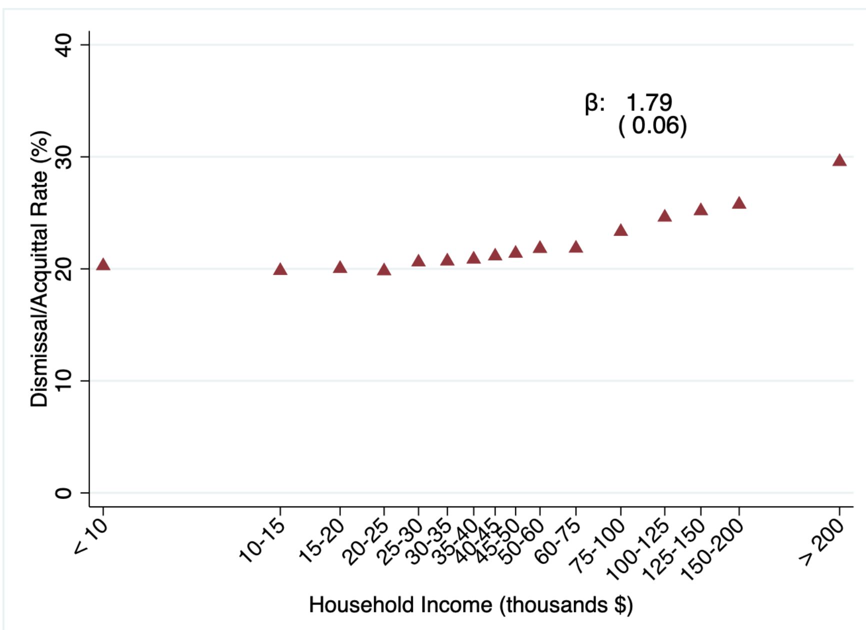
Figure C.10

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 26 for details.