# Learning by Doing in Law Enforcement

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**Abstract**

This study examines task-specific learning by police officers. Using data on twenty million traffic stops, I show that rookie officers become significantly more likely to find contraband as they accumulate experience, despite searching the same fraction of stopped vehicles. This indicates that officers learn to more accurately select drivers to search. I find these productivity improvements accrue primarily from changes to searching behavior within rather than across driver demographic groups, supporting an explanation of cognitive skill development instead of increasing statistical discrimination. This evidence suggests that, while not eliminating racial bias, policing experience partially corrects inefficiencies from searching innocent people.

JEL: D24, D83, J18, K42

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# 1 Introduction

At the turn of the nineteenth century, psychologists William Bryan and Noble Harter (1899) demonstrated that the productivity of telegraph operators improves significantly with practice. Since this pioneering work, researchers have explored how occupational experience forms human capital in settings ranging from subsistence farming to Uber driving (Foster and Rosenzweig, 1995; Cook, Diamond, Hall, List, and Oyer, 2018). Even among academic scholars, "a byproduct of producing research is learning" (Levin and Stephan, 1991). Broadly, two findings emerge from this literature. The first is that "task-specific human capital" is at least as important to worker productivity as is general knowledge and training (Gibbons and Waldman, 2004; Gathmann and Schönberg, 2010; Ost, 2014). The second is that, provided that workers are given frequent feedback regarding their performance, occupational learning curves tend to be steep initially before soon leveling off (e.g. Shaw and Lazear, 2008; Harris and Sass, 2011; Levitt, List, and Syverson, 2013; Haggag, McManus, and Paci, 2017).

In this study, I examine the productivity of police officers as they make contraband search decisions during traffic stops. This is a compelling context in which to evaluate learning by doing for several reasons. Most importantly, these decisions carry significant economic welfare consequence. The U.S. Bureau of Justice Statistics estimates that police officers find contraband in only 8.4 percent of the 870,000 traffic stop searches conducted nationally, equating to nearly 800,000 failed searches each year (Eith and Durose, 2011). Beyond the direct inefficiency from this misallocation of policing resources, fruitless searches create substantial time and stress cost for innocent civilians, and they invoke significant equity concerns regarding disparate treatment of racial and ethnic minorities (Engel and Calnon, 2004; Gelman, Fagan, and Kiss, 2007; Pierson, Simoiu, Overgoor, Corbett-Davies, Jenson, Shoemaker, Ramachandran, Barghouty, Phillips, Shroff, and Goel, 2019). A second feature of the setting is that police officers' search productivity is directly observable, in contrast to the indirect measures of performance such as wages or teachers' "value added" to student achievement that are typically examined in studies of occupational learning (e.g. Murnane and Phillips, 1981; Dustmann and Meghir, 2005; Lazear, 2006; Gathmann and Schönberg, 2010; Harris and Sass, 2011). Finally, because police officers typically find contraband only several times per year, their traffic search efficiency can yield insights about task-specific learning when workers are provided with sparse feedback from which to learn.

Building on Becker (1968) and a subsequent literature's longstanding framework for policing strategy, I construct a simple behavioral model of officer learning in traffic stops. Conceptually, my model centers around an officer using information gleaned from interacting with previously stopped drivers to update, à la Bayes and Price (1763), how the officer forms beliefs about whether a currently stopped driver is carrying contraband. As an officer accumulates traffic stop experience, learning by doing implies that the officer's accuracy at identifying the presence of contraband should improve. The empirically testable implication is that, for a given (opportunity) cost of conducting a search and a given propensity for a stopped driver to be carrying contraband, an officer's "hit-rate" likelihood of finding contraband during a search will increase with experience. My model then provides a method to determine whether the mechanism(s) underlying any learning is officer cognitive skill development – improved ability to map driver behaviors such as nervousness or evasiveness into likelihood of guilt – or increasing use of statistical discrimination such as demographic profiling. Specifically, the empirical test is whether the composition of searched drivers changes primarily within or across driver attributes such as demographic categorization that could be used to facilitate increased statistical discrimination.

Rather than focusing on a single police department, I empirically test the model's predictions using data on police stops during 2001-2016 across eight states, which comprise over twenty percent of the total U.S. population and span all four Census Regions. The data, obtained via public records requests made by Pierson et al. (2019), include nearly twenty million traffic stops made by about 5000 involved state police officers who are first observed as a rookie. Methodologically, my empirical identification uses within-officer variation, examining how search behavior and productivity change as an officer accumulates stop experience. These officers' average rookie hit-rate is only 21 percent, with significant heterogeneity in hit-rates across drivers by race and gender, providing substantial scope for productivity improvements both from skill development and from increased statistical discrimination.

I find robust evidence of task-specific learning in traffic stops. Consistent with the prediction of the model, I show that rookie officers significantly increase their productivity at finding contraband as they accumulate experience. Quantitatively, the average rookie will obtain a 56 percent improvement (11.74 percentage points) in contraband search hit-rate with ten years of experience. This finding is not attributable to officers' changing search frequencies or the composition or locations of stopped drivers. Moreover, it is invariant to controlling for the composition of when an officer makes stops within the week or calendar year, as well as for broader nonlinear time trends. I also rule out explanations based on survivor bias, such as if police supervisors systematically deployed officers who are innately more successful at finding contraband to conduct higher volumes of traffic stops. Instead, I find that the primary mechanism underlying officers' search productivity improvements is changes to the search composition within driver demographic groups, supporting that the learning occurs predominantly via cognitive skill improvements rather than increased use of statistical discrimination.

This study makes several contributions. Most directly, my findings illustrate that task-specific human capital partially corrects for policing inefficiencies in law enforcement and reduces some of the burden placed on innocent people from fruitless police searches. A growing literature shows that a significant share of the variation in arrests and other treatment by police is attributable to heterogeneity in individual officers' behavior (Goncalves and Mello, 2017; Bulman, 2018; Weisburst, 2018; West, 2018). In recent closely-related work, DeAngelo and Owens (2017) demonstrate that more experienced state troopers tend to issue more frequent traffic citations, and concurrent work by Horrace, Jung, and Rohlin (2018) also finds that police contraband search productivity improves with officer experience. Neither of these studies, however, identifies whether the underlying mechanism for these productivity gains from experience is skill improvements or changing use of discrimination.

My findings also add to the discussion on reforming public sector compensation policies (Ebbinghaus, 2006). Because more experienced personnel add a larger wage burden to government budgets, policymakers increasingly offer early retirement incentives as a means of reducing labor costs (Fitzpatrick and Lovenheim, 2014). Using the *Texas Tribune*'s Government Salaries Explorer, I find that the wages of state police officers increase on average by about 1.82 percent in current dollars per year of experience.[1] Provided that my finding of ongoing productivity improvements generalizes to other policing responsibilities, this evidence supports that police officers are paid an efficiency wage for their service.

Finally, my study contributes to the larger literature on demographic bias in policing (e.g. Knowles, Persico, and Todd, 2001; Anwar and Fang, 2006; Antonovics and Knight, 2009; Fryer, 2016; West, 2018). Much of this literature focuses on determining whether observed disparities in police treatment result from statistical or preference-based discrimination. My evidence that police officers appear not to alter their use of statistical discrimination with experience provides insights, at least indirectly, to this significant literature. Given the large welfare consequences noted earlier from fruitless police searches, my findings also hold direct policy implications. For instance, the findings support that implementing two-officer patrol – particularly that of pairing a junior officer with a more senior one – could facilitate valuable spillovers between officers of skills that are learned through experience. More

[1] I computed this average salary increase using cross-sectional data available from the *Texas Tribune* at https://salaries.texastribune.org. Additional discussion and regression results are presented in Appendix B.

moderately, policing agencies could also consider increasing the frequency of deployment of more experienced officers to field work. Policy changes such as these could yield substantial efficiency gains in law enforcement and reduce the large and disparate burden imposed by police searches of innocent people.

# 2 Model

Police officers make traffic stops for various reasons, ranging from observed violations (e.g. speeding) or suspicion of criminal activity, to reports of dangerous driving provided by other motorists (Engel and Calnon, 2004; Eith and Durose, 2011). Following the literature on motor vehicle searches, I take the sample of stopped cars as the population and do not model officers' stopping decisions (e.g. Knowles et al., 2001; Dharmapala and Ross, 2004; Anwar and Fang, 2006; Persico and Todd, 2006; Bjerk, 2007; Persico and Todd, 2008; Antonovics and Knight, 2009; Simoiu et al., 2017; Horrace et al., 2018).[2] After signaling to a driver to pull over and stop, the officer initiates a record of the stop using the police radio before personally interacting with the driver. Although "there is no such thing as a routine traffic stop," typically the officer informs the driver of the reason for the stop and records some administrative information such as driver license and insurance details.[3] Next, the officer may decide to conduct a vehicle search in order to seize any contraband such as stolen goods or illicit drugs or weapons.[4] I model this search process.

In the model, an officer's search decisions result from a constrained optimization problem. I model the officer's objective as maximizing the total amount of contraband found, subject to a limit on the total volume of searches. This resource constraint could be due to external factors such as supervisor restrictions or simply a limit to the officer's available time. Formally, let $d_i \in \{0, 1\}$ be an indicator for whether driver $i$ is carrying contraband and $\tilde{d}_i \in [0, 1]$ denote the officer's belief about the probability that $d_i = 1$ when making the decision of whether or not to conduct a search, $s_i \in \{0, 1\}$. For some given volume of stops $N \in \mathbb{Z}^+$ and search rate limit $R \in (0, 1)$, the officer makes search decisions to solve:

$$\underset{s_i \in \{0,1\}}{\text{Max}} \sum_{i=1}^{N} s_i \tilde{d}_i \qquad s.t. \qquad \frac{\sum_i s_i}{N} < R$$

[2] Of course, the composition of drivers that an officer stops could be a dimension of learning, I empirically explore this possibility and find evidence refuting this as a mechanism for search productivity improvements.

[3] c.f. http://www.policemag.com/channel/careers-training/articles/2012/11/traffic-stops.aspx

[4] Although many police searches require a search warrant, an exception for motor vehicles was established by the U.S. Supreme Court in *Carroll v. United States* (1925).

From the officer's perspective, however, the problem is sequential rather than simultaneous. In a particular traffic stop, the officer's search decision solves: $\text{Max}_{s_i \in \{0,1\}}\, s_i \cdot (\tilde{d}_i - r)$. Here, the search cost $r$ can be interpreted as the officer's opportunity cost of searching. To avoid trivial cases, I assume $r \in (0, 1)$, so that the officer will search driver $i$ if and only if $\tilde{d}_i > r$. Thus far, the model is very similar to those in the broader literature on police searches.

Next, I incorporate the potential for learning into the model. One clear implication of learning is that an officer's accuracy at predicting the presence of contraband should improve with experience. From the analyst's perspective, learning implies that the expected prediction error for an officer should decrease in magnitude as the officer's stop experience increases: $\mathbb{E}\Big[\mid d_i - \tilde{d}_i \mid\Big] < \mathbb{E}\Big[\mid d_{i-1} - \tilde{d}_{i-1} \mid\Big] \;\; \forall i$, in which the driver subscript sequentially indexes stops made by the same officer. As officers' internal beliefs $\tilde{d}_i$ are unobservable, to facilitate empirical tests for learning I assume that each officer faces a constant opportunity cost of searching over time, i.e. that $r$ is constant within-officer. Although models in the literature on police searches typically allow for heterogeneity in search costs across officers (as do I), they similarly assume that search costs remain constant within-officer (e.g. Anwar and Fang, 2006; Persico and Todd, 2006; Antonovics and Knight, 2009; Horrace et al., 2018). Given this assumption of constant search costs, for which I provide empirical support below in Section 4, a straightforward empirical test for learning is to evaluate whether an officer's rate of finding contraband (per stop or search) increases with experience. That is, I test whether unconditionally $\mathbb{E}\Big[d_i\Big] > \mathbb{E}\Big[d_{i-1}\Big] \;\; \forall i$ and whether $\mathbb{E}\Big[d_i \mid s_i = 1\Big] > \mathbb{E}\Big[d_{i-1} \mid s_{i-1} = 1\Big] \;\; \forall i$.[5]

The model described so far provides testable predictions for whether officers learn to more accurately select which drivers to search. I next focus on identifying the underlying mechanism(s) for any search productivity improvements. In particular, I consider two categorical mechanisms for officers' improving their prediction errors with experience. First, officers might increase their use of statistical discrimination based on readily quantifiable driver attributes such as demographics. Second, officers might develop skill in interpreting difficult to quantify behavioral aspects such as drivers' levels of nervousness, aggression, and evasiveness. If present, the latter would undoubtedly be a form of learning by doing, whereas the former need not involve learning *by doing*, as officers can in principle study data or be trained to improve their statistical discrimination and thereby reduce prediction error.[6]

[5] An implicit assumption in these tests is that there is no change over time in stopped drivers' propensity to be carrying contraband. Empirically, I can control for this possibility using time period fixed effects. More generally, my empirical study also provides direct support for the implicit assumption that an officer faces a similar composition of drivers across the experience profile.

[6] This framework is also similar in spirit to that presented in Altonji and Pierret's (2001) study of statistical discrimination based on demographics by employers in labor markets.

To distinguish between these potential mechanisms, let the officer's contraband prediction $\tilde{d}_i = f(Z_i, X_i; \tilde{\alpha}, \tilde{\beta})$. Here, $Z_i$ is a vector of readily quantifiable driver attributes such as demographics and $X_i$ is a vector of difficult to quantify behavioral attributes particular to the driver. Let $\tilde{\alpha}$ be an officer-specific vector of parameters that the officer uses to map $Z_i$ into the contraband prediction $\tilde{d}_i$ and $\tilde{\beta}$ be an officer-specific vector of parameters that the officer uses to map $X_i$ into the contraband prediction $\tilde{d}_i$.

Plugging $\tilde{d}_i = f(Z_i, X_i; \tilde{\alpha}, \tilde{\beta})$ into the definition of learning to reduce prediction error,

$$\mathbb{E}\Big[ \mid d_i - f(Z_i, X_i; \tilde{\alpha}, \tilde{\beta}) \mid \Big] < \mathbb{E}\Big[ \mid d_{i-1} - f(Z_{i-1}, X_{i-1}; \tilde{\alpha}, \tilde{\beta}) \mid \Big] \;\; \forall i.$$

It is clear from this formulation that at least some of the elements of either $\tilde{\alpha}$ or $\tilde{\beta}$ must be converging towards the true parameters $\alpha^\star$ and $\beta^\star$ as the officer accrues stop experience. As noted above, $\tilde{\alpha}$ converging to $\alpha^\star$ indicates increasing use of statistical discrimination in forming contraband predictions $\tilde{d}_i$, whereas $\tilde{\beta}$ converging to $\beta^\star$ indicates cognitive skill development in mapping driver behavioral tells into likelihood of guilt.[7]

Thus, a compelling empirical test for the mechanism(s) underlying search productivity improvements is to evaluate whether the composition of searched drivers is changing primarily within or across (observable) components of $Z_i$ that could be used to facilitate increased statistical discrimination. If productivity improvements result from changing the composition of searched drivers *across* driver demographics, this supports that officers are effectively increasing their use of statistical discrimination in making search decisions. If instead, the improvements load primarily from changing the composition of searched drivers *within* driver demographic groups, then this supports a primary mechanism of cognitive skill development.

# 3 Data

My empirical study employs a large panel of state police stops across eight states, sourcing data from Pierson et al. (2019). One advantage of this approach is generalizability. In addition to spanning all four Census Regions and time zones, the included states had a combined population of 68 million people in 2017, comprising 21 percent of the total U.S.

[7]This possible mechanism of skill improvement is strongly conveyed in *Police* magazine's guidance on contraband searches in traffic stops: "An inexperienced [police officer] focuses on the violation, whereas an experienced person focuses on indicators displayed by the [driver] and observed throughout the vehicle. [I]nterview techniques, curiosity, and a sense of urgency will prove productive... We need to learn to talk to, and not at, violators... No detail is too small when you are looking for things that stand out from the norm." (c.f. https://www.policemag.com/340820/drug-interdiction-for-patrol).

population.[8] A second advantage is statistical power. Although few details about each traffic stop are needed to test the predictions derived in Section 2, the empirical tests require a large quantity of data. Identifying learning uses within-officer variation, so statistical power scales with the number of officers rather than with the number of observed stops. Moreover, because police officers locate contraband in only a small fraction of stops – about one in 200 stops in this study – a substantial number of observations per officer are also necessary to evaluate learning in contraband searches.

The data initially include the population of electronic records of traffic stops made by the state police in these eight states. The panel is unbalanced across officers, but ranges overall from 2001-2016. Because my study focuses on officer learning, I restrict my analysis to officers who are first observed as a "rookie," which I define to be an officer whose first observed stop occurs at least 365 days after the earliest observed stop made in the state.[9] After minor data cleaning, described in Appendix A, the analysis dataset includes 19.4 million traffic stops made by 4728 officers. All stops are made by only a single officer, and I include in this study only officers that conducted at least one search of a stopped driver.

Whereas the data include an average of more than 4000 traffic stop observations per officer, the level of detail is fairly limited. For each stop, I observe the unique officer identifier; the state, date, and time of day; the driver's gender and race; and binary indicators for whether a search was conducted and whether any contraband (drugs, weapons, or stolen property) was found. For some stops in four of the states, I am also able to determine the specific geographic location of the stop. The data do not include any demographic information about the officers, nor do they indicate how much or what kind(s) of contraband was seized.[10]

Table 1 provides summary statistics on these traffic stops. The first two columns show the fraction of stops and searches, respectively, for which each indicator covariate equals one. Officers search a driver's vehicle for contraband in 2.14 percent of stops, and they find contraband in 0.53 percent of stops. This yields an average "hit-rate" search efficacy for contraband of 24.81 percent (in the data, officers never locate contraband when the record

[8]Figure 1 shows a map of the included states: Arizona, Connecticut, North Carolina, South Carolina, Texas, Vermont, Washington, and Wisconsin. State population data are from the Census Bureau at https://www.census.gov/data/tables/2017/demo/popest/state-total.html.

[9]In principle, the study could also examine learning among more senior officers. However, the experience level for non-rookie officers is truncated and thus uncertain.

[10]The detail of the stop records varies extensively. For example, Arizona, Texas, and Wisconsin do not report driver age, which is reported by the other five states included in this study. Even within-state, some records include the county or geocoordinates of the stop, whereas others do not.

shows that no search was conducted). The remainder of Columns (1) and (2) shows the fraction of stops and searches that include a driver of a given demographic characteristic. As these statistics mainly reflect the composition of drivers on the road, a more informative comparison is provided in Columns (3) and (4), which respectively show the average search rate and (conditional on search) contraband finding hit-rate by driver demographic attribute.

These statistics reveal two intriguing insights. First, the significant variation in both search rates and hit-rates across drivers of differing demographic characteristics indicates that substantial efficiency gains might be expected from officers reallocating searches across driver groups. For instance, intensified statistical discrimination would support officers increasing search frequencies for both black and white drivers, while decreasing searches of Hispanics. More strikingly, even for the demographic groups that officers are most effective at searching, more than 70 percent of searches yield no contraband; overall, three out of four searches are fruitless.[11] This illustrates that (by far) the bigger potential for search efficiency gains is from changing search behavior within rather than across demographic groups. In the following section, I evaluate both potential channels for improvements in officer contraband search decisions, testing predictions derived above in Section 2. Altogether, Table 1 indicates there is substantial potential for policing productivity gains from officer learning by doing.

# 4 Results

In this section, I use the data described in Section 3 to test the predictions of the model presented in Section 2. After demonstrating that officers' contraband search efficacy increases significantly with experience, I evaluate the potential mechanisms underlying this productivity improvement. I find that the primary channel is through changing search behavior within rather than across driver demographic groups, supporting an explanation of officer cognitive skill development rather than increased statistical discrimination.

## 4.1 Vehicle search productivity improves with experience

Before testing the predictions of the model econometrically, I explore the basic relationship between officer experience and contraband search efficacy. Figure 2 plots the likelihood that an officer finds contraband in a vehicle search across different levels of accumulated

[11] Notably, this average search productivity is three times the national average, which may be attributable to my focus only on state police (Eith and Durose, 2011). Other studies find similarly low average hit-rates (e.g. Anwar and Fang, 2006; Persico and Todd, 2006; Simoiu et al., 2017).

experience. The experience term includes all stops made by the officer prior to that stop, regardless of whether a search was conducted. All included officers are initially rookies, and the same officers are shown across multiple levels of experience along the horizontal axis. The marker sizes indicate the number of officers observed at each experience level, using bins of 100 stops. In addition to plotting local averages, I plot a linear regression fit line, estimated using the underlying more granular microdata. This regression has an intercept of 21.12 percent and a slope of 1.081 percentage points per thousand stops, indicating that officers substantially improve their contraband hit-rates with experience.

This motivating visual evidence from the plot of the raw data strongly supports the presence of learning by doing. Next, I more formally examine the role of accumulated traffic stop experience using specifications of the following form.

$$\text{outcome}_{ij} = \text{experience}_{ij} \cdot \beta + \mu_j + \text{controls}'_{ij}\Gamma + \epsilon_{ij} \tag{1}$$

In Equation 1, $\text{outcome}_{ij}$ is some outcome pertaining to the traffic stop of driver $i$ made by officer $j$, such as whether a vehicle search was conducted; $\text{experience}_{ij}$ is a linear term counting officer $j$'s accumulated stop experience prior to stop $i$; $\mu_j$ is an officer fixed effect; and $\text{controls}_{ij}$ include various fixed effects and covariates particular to the stop, such as the hour within the week and the date of the calendar year. Table 2 presents estimates using Equation 1. Each cell shows an estimate of $\beta$ from a separate linear model, using different outcomes across the panels. Standard errors in parentheses are clustered by officer.

In Panel [A], I first evaluate how officers' monthly stop volume changes with experience. Across Columns (1)-(4), which incorporate various fixed effects (discussed below), the estimates indicate that each 1000 stops of prior experience is associated with the officer making about four fewer stops per month. In Column (5), which adds fixed effects for the 16 years of the sample, the estimated magnitude shrinks by about half, suggesting that some of this decline reflects a broader systematic reduction in traffic stop volume (per officer), but there remains a strong negative relationship between officer experience and stop frequency. Given that officers' average monthly volume is about 84 stops (just over 1000 per year), these estimates equate to about a two to five percent reduction in traffic stop volume per year of police experience. Although the model presented in Section 2 neither assumes nor predicts a relationship between officer experience and stop frequency, this finding is not surprising. State police officers have numerous responsibilities, and it seems quite plausible that more senior officers could be relatively more likely to be assigned to other tasks such as vehicle crash investigations or sobriety checkpoints. However, this finding has two important implications. First, any learning by doing occurs despite officers having reduced opportunity to practice as they accumulate experience. Second, the most experienced officers – who are more productive at finding contraband – are also less likely to be making traffic stops.

In Panel [B] of Table 2, I test the key assumption of the model that each officer faces a constant search cost over time. Specifically, this panel assesses how the likelihood of conducting a vehicle search varies with an officer's stop experience. The evidence is clear that officers do not systematically change their search rates of stopped drivers as they accumulate experience. Across all columns, the estimates are statistically and economically insignificant. If officers' search costs were declining over time – or if the reduction in stop frequency shown in Panel [A] was due to officers systematically stopping fewer drivers who have high search costs or low expected contraband propensity – then officers would be increasing their search rates as they accrue experience. The null effects for changing search rates are consistent with officers' facing constant search costs despite their level of experience, as is assumed in the model in Section 2, as well as by models in the literature more broadly (e.g. Anwar and Fang, 2006; Persico and Todd, 2006; Antonovics and Knight, 2009; Horrace et al., 2018).[12]

Panel [C] shows tests for learning, presenting estimates for the likelihood that an officer finds contraband during a stop in reduced-form, unconditional on whether a search is conducted. In Column (1), which includes officer fixed effects and no controls, the estimated coefficient indicates that officers are 0.022 percentage points (s.e.= 0.003) more likely to find contraband in a stop for each thousand stops of prior experience they have accumulated. On a mean contraband-finding rate of 0.53 percent, this estimate is economically significant, especially when considering that officers on average make about 1000 stops per year. The estimate is barely changed in Column (2), which adds fixed effects for the 168 hours of the week, or in Column (3), which also adds fixed effects for the 366 dates of the calendar year.

The final two columns of Table 2 address alternative explanations. One consideration is that, rather than learning, more experienced officers may be assigned more often to time shifts that have systematically better opportunities to find contraband. Indirectly, this explanation is largely ruled out by the null effects for search rates. As further evidence, Column

[12] The null result for changing search rates with experience could instead result from officers facing higher search costs as they accrue experience along with improved predictions about whom to search, such that the change in expected benefit and the change in search cost perfectly offset. The result is also consistent with officers stopping relatively fewer drivers whom they intend to possibly search and searching a larger share of drivers whom they stop with the intention of searching, i.e. making relatively more of the search decisions prior to even making a stop. Both of these alternative interpretations of the results in Panel [B] of Table 2 would still support the empirical tests below for the role of changing searched driver composition as the mechanism(s) underlying the search productivity improvements.

(4) uses officer by hour-of-week fixed effects, assessing how contraband finding efficacy varies within the same officer during the same hour of the week. The estimated coefficient of 0.024 percentage points (s.e.= 0.003) is identical to that in the previous columns. A related consideration is that more experienced officers may patrol locations with systematically better opportunities for finding contraband. State police officers conduct almost exclusively highway stops, but there could be selection or assignment onto "back roads" versus Interstates, for example. Such a practice is also largely ruled out by the null effects for changing search rates. Additionally, in Section 4.3 below I show directly that officers do not systematically change their stop locations with respect to the proximity of contraband as they accrue experience, supporting that stop locations are not driving the productivity improvements shown in Table 2. Finally, I evaluate whether these findings simply capture some broader change(s) in drivers' propensity to carry contraband. The estimate in Column (5) of Table 2 Panel [C] remains unchanged as I add fixed effects for the sixteen years of the sample, using within-officer by hour-of-week and within-year variation, while controlling for the calendar date.

Given the above results, it is unsurprising in Panel [D] of Table 2 to see that officers' search productivity significantly increases with stop experience. Quantitatively, each thousand stops' worth of experience is associated with about a 1.23 percentage points (s.e.= 0.091) improvement in an officer's expected hit-rate. On a rookie baseline hit-rate of 21.12 percent (from Figure 2), this predicts that the average rookie will obtain a 56 percent increase (11.74 percentage points) in the rate of finding contraband per search after ten years of experience – quite an economically significant improvement. Altogether, Table 2 provides ample evidence in support of the model of learning by doing in traffic stops. Having established that officer experience significantly impacts contraband search performance, I next focus on identifying the underlying mechanism(s) for these productivity improvements.

## 4.2 No evidence of survivor bias in traffic stop volumes

State police officers have numerous tasks, and the task assignment of officers is not arbitrary. Even before accruing much traffic stop experience, police officers differ substantially in their contraband search productivity. For officers during their rookie year, the 25th percentile hit-rate is zero percent whereas the 75th percentile is 34 percent. Some of this variation is attributable to heterogeneous search volumes; but, even restricting to officers who made at least ten searches during their rookie year, the 25th percentile hit-rate is seven percent and the 75th percentile is 31 percent. Thus, police supervisors have scope to assign officers who had comparatively better rookie contraband search productivity to conduct higher traffic stops volumes in later years. This would bias the tests presented earlier towards showing learning. I evaluate whether such survivor bias is a concern using tests of the following form.

$$\text{total\_stops}_j = \text{rookie\_stops}_j \cdot \phi + \text{rookie\_contraband}_j \cdot \delta + \text{controls}_j' \Lambda + \epsilon_j \tag{2}$$

In the specification of Equation 2, $\text{total\_stops}_j$ is the total number of observed stops in the data that are made by officer $j$; $\text{rookie\_stops}_j$ is the number of stops that the officer made during his or her rookie year; $\text{rookie\_contraband}_j$ is the total number of searches the officer conducted during the rookie year that yielded contraband; and $\text{controls}_j$ are time-invariant officer covariates such as the officer's state and starting date. The coefficient of interest is $\delta$, which would be large and positive if there were significant survivor bias.

Table 3 presents the results of these tests for survivor bias. Officers make on average 4106 observed stops in total (961 annualized), averaging 962 stops during their rookie year and 1788 stops during their first two years. Consistent with intuition about officer deployment to heterogeneous tasks, the estimates for $\phi$ are statistically and economically significant: officers who made more stops during their rookie year(s) are observed making more stops in total. Quantitatively, an officer who made 100 additional stops (about 10 percent) as a rookie will make about 292 more observed stops in total. The magnitude and statistical significance of this relationship continues to hold across Columns (2)-(4), which add a covariate for rookie contraband finding as well as fixed effects for the officer's starting date and state of employment. The estimated coefficient for $\phi$ is smaller in Column (5), which uses each officer's rookie 2-years, but this attenuation is a mechanical artifact of the predictor variable absorbing a larger share of the total stops.

In contrast to the strong predictive power of officers' rookie stop volumes, there is no relationship between officers' rookie search efficacy and total volume of traffic stops. Taking the point estimates for $\delta$ at face value, the coefficients suggest that, at most, an officer with *double* the average first-year contraband finding productivity will make 18 additional observed stops in total, 0.4 percent of the mean. This null effect could be explained by the relatively low information content of rookie search efficacy. Alternatively, it could reflect the multifaceted objectives of traffic stops, such as minimizing traffic fatalities in addition to seizing contraband (Bates et al., 2012; DeAngelo and Hansen, 2014). In any case, these tests convincingly rule out survivor bias as a mechanism for the productivity gains from experience demonstrated earlier.

### 4.3 Evidence supports mechanism of officer skill improvement

The findings presented so far show that police officers do not alter their overall search rates as they accrue traffic stop experience and that officers with higher baseline search productivity do not systematically make more stops. Thus, the demonstrated productivity gains associated with experience indicate that officers learn to more accurately select which drivers to search.[13] In the behavioral model presented in Section 2, there are two broad mechanisms by which officers might select for a more accurate composition of drivers to search: they could increase their use of statistical discrimination based on readily-quantifiable driver characteristics such as demographics, or they could improve their cognitive skill at evaluating drivers' idiosyncratic attributes and behavioral tells. Before testing for both of these potential mechanisms, I provide additional support that the productivity improvements shown in Section 4.1 are not attributable to (1) spillovers from relatively more to relatively less innately productive officers, nor to (2) officers systematically changing the location of the traffic stops that they make as their experience increases.

To address potential spillovers across officers – a form of peer effect – in which relatively more innately productive officers share their "best practices" with their less productive colleagues, I replicate Figure 2 to explore heterogeneity in productivity improvements by baseline rookie search productivity. Specifically, Figure 3 shows the linear regression fit lines separately for each quartile of rookie 2-years baseline contraband search productivity, including the same officers and stops as in Figure 2. Although the most productive rookies at finding contraband do exhibit a somewhat flatter (but still significantly upward-sloping) experience curve, the search productivity learning curves for the initially lowest three quartiles of officers are essentially parallel in slope. This pattern of results is not consistent with a peer effects explanation of the innately more productive officers sharing best practices with the innately least productive officers, at least as a primary mechanism for the broader search productivity improvements shown above.[14]

Next, I evaluate whether the search productivity improvements associated with experience result from more experienced officers being assigned (or choosing) to make traffic stops in locations that have systematically better opportunities to find contraband. As noted

[13] An alternative explanation is that officers improve in the search procedure itself. Given the magnitude of the hit-rate improvements and the long duration of the learning curve, it is difficult to hold that these productivity gains are attributable to officers learning to more thoroughly search vehicles. Moreover, such an explanation implies that officers should be increasing their search rates with experience as well.

[14] Note that this evidence does not imply that there are no peer effects among police officers in this context, only that peer effects are not driving the search productivity improvements shown in Section 4.1.

above in Section 4.1, such a mechanism would also support that officers should be increasing – rather than holding constant – their overall search rate with experience. To more directly address this potential mechanism, I leverage the available stop location information in the data. For this analysis, only stops with available geocoordinates are included, which are a subset of the stops made in South Carolina, Texas, Washington, and Wisconsin only – 47 percent of all in-sample stops included in this study.

Table 4 presents regression estimates of Equation 1 for an outcome of the proximity of the stop location to the nearest (other) stop location where contraband was found in the data. Across the columns, replicating the specifications of Table 2, the evidence is clear that more experienced officers do not make traffic stops in locations that have systematically better opportunity to yield contraband hits. To verify that proximity to the nearest found contraband is a meaningful predictor of likelihood of finding contraband, I show in Figure 4 that stops in which contraband was found are indeed systematically closer to other stops at which contraband was found, relative to stops during which contraband was not found. That is, the results in Table 4 serve as convincing evidence that movement with experience towards different stop locations is not the mechanism underlying the search productivity improvements shown in Section 4.1.

Turning to the predictions of the behavioral model in Section 2, I test for both mechanisms in Table 5, presenting estimates of Equation 1 separately for various subsets of drivers based on selected demographic characteristics. Panel [A] evaluates how monthly stop frequencies change with officer experience, Panel [B] assesses the likelihood that an officer conducts a search during a stop, and Panel [C] examines the likelihood that the officer finds contraband during the stop. All specifications in the table use the same fixed effects as in Column (4) of Table 2, and the officer experience term is defined using all prior stop experience.

Broadly, Table 5 does not support that traffic stop experience substantially changes officers' use of statistical discrimination. Across Panel [A], drivers of all demographic groups are stopped less frequently by more experienced officers. As discussed above, this is more likely to be attributable to changing task assignments rather than intentional officer discretion in stop volumes. Consistent with the statistical guidance provided in Table 1 (and discussed near the end of Section 3), Panel [B] shows that officers decrease search frequencies of Hispanic drivers as they accrue experience. However, they also increase search rates of female drivers, in contrast to the pattern supported by heightened statistical discrimination. Accumulated experience is not associated with any significant changes to the search rates of male, black, or white drivers.[15] In Panel [C], by contrast, it is clear that significant search productivity improvements are present within all demographic groups. Across the columns of Panel [C], the increases in contraband-finding efficacy range from 2.9 to six percent of the respective subgroup means, and all of the point estimates lie within the 95 percent confidence interval for the full-sample estimate in Column (1), and vice versa.

More tellingly, comparing the estimates in Panel [B] to those in Panel [C] shows that the search productivity gains are much larger in magnitude than the changes to the demographic search composition. Proportionally, the biggest observed change in search rates (for Hispanics) is 1.5 percent in magnitude per 1000 stops of experience, whereas the increase in contraband finding is 4.5 percent of the mean for the same demographic group. Even more directly, the results show that, with increased experience, officers both significantly decrease the search rate of Hispanics *per stop* while significantly increasing contraband finding rates *per stop* among the same group. Collectively, Table 5 illustrates that the productivity gains from experience are primarily attributable to officers changing search behavior within demographic groups, supporting a mechanism of task-specific skill improvements rather than intensified statistical discrimination.

# 5 Conclusions

This study examines task-specific human capital development by police officers. Layering a Bayesian updating framework onto a model of officer policing strategy, I develop several tests for learning by doing in contraband searches, which I then empirically evaluate using data on nearly twenty million traffic stops made by about 5000 police officers.

Consistent with the model, I find that police officers' productivity in searching for contraband significantly increases as they accumulate stop experience. Supporting exercises show that this finding is not being driven by selection bias, nor by any obvious channels attributable to police agencies or broader secular patterns. Instead, I find that the primary mechanism is officers' choices about the composition of searched drivers within demographic categories, supporting that learning occurs predominantly through cognitive skill improvements in officers' ability to map driver behavior such as nervousness and evasiveness into likelihood of guilt.

[15] Officers might be increasing their use of statistical discrimination along other dimensions, such as based on the age of a driver's vehicle. However, that they do not substantially change their use of statistical discrimination along observable demographic driver attributes makes it less plausible that they are significantly changing their use of statistical discrimination along unobservable (to the analyst) dimensions.

This study yields several contributions. Most directly, my findings show that task-specific human capital partially corrects for policing inefficiencies in law enforcement. In addition, by providing evidence that policing experience appears not to alter officers' use of statistical discrimination, my study contributes to the broader literature on demographic bias in policing. Finally, my findings hold direct policy implications. For instance, the findings support that implementing two-officer patrol – particularly that of pairing a junior officer with a more senior one – could facilitate valuable spillovers between officers of skills that are learned through experience. Given the large welfare consequences and social justice concerns associated with fruitless police searches, this study's evidence supports that policies to revise the training and task assignments of police officers can substantially improve the efficiency of law enforcement and reduce the large burden imposed by police searches of innocent people.

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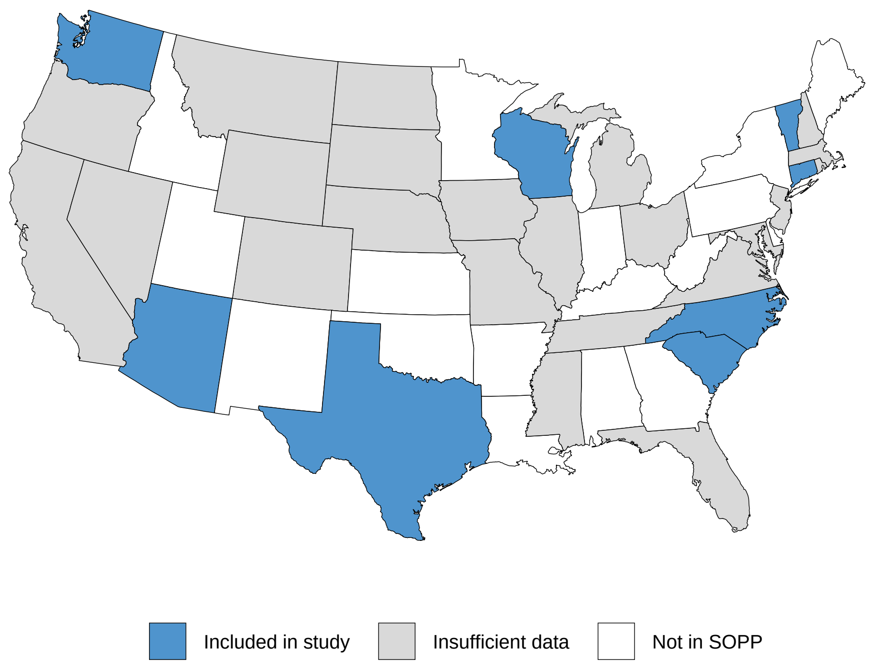
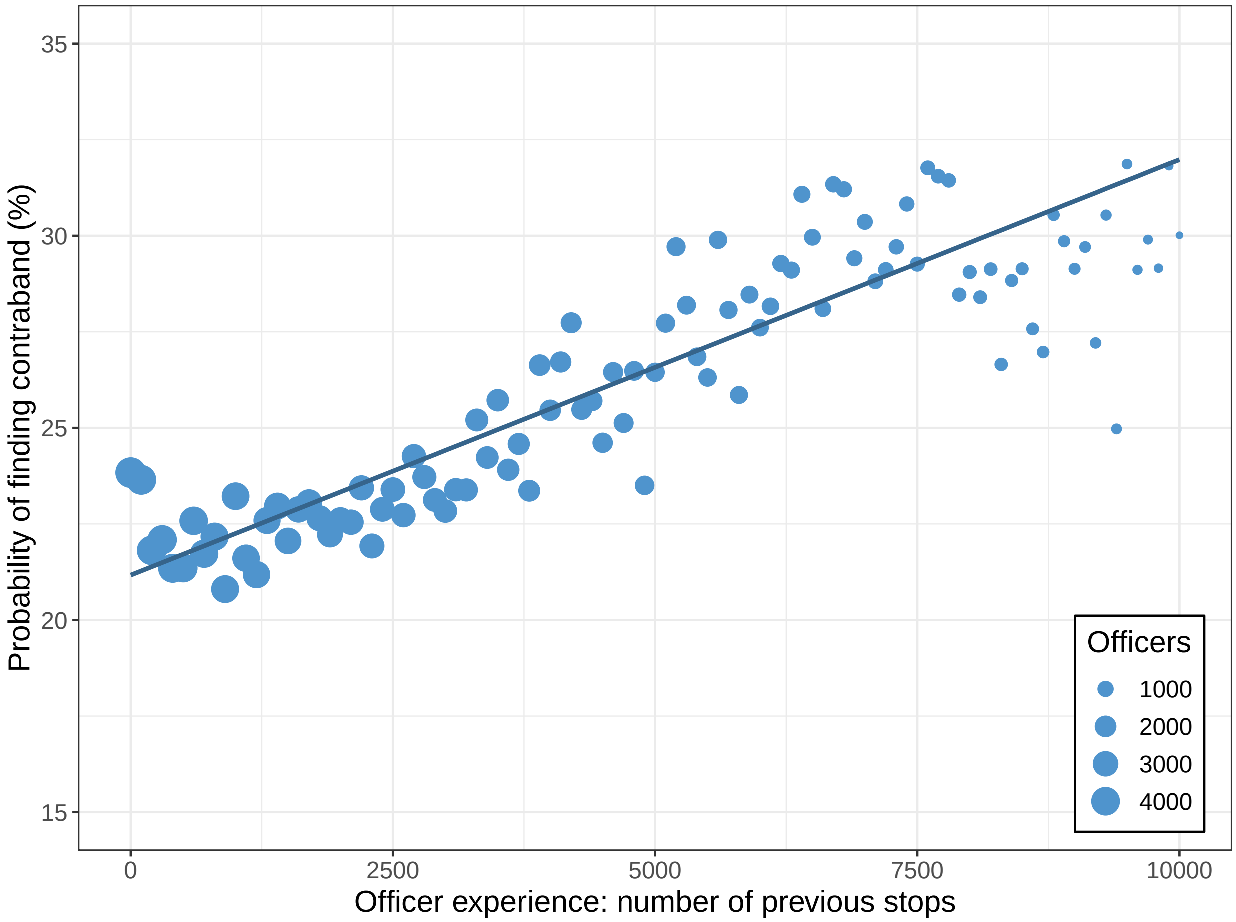
Figure 1: State data availability and inclusion



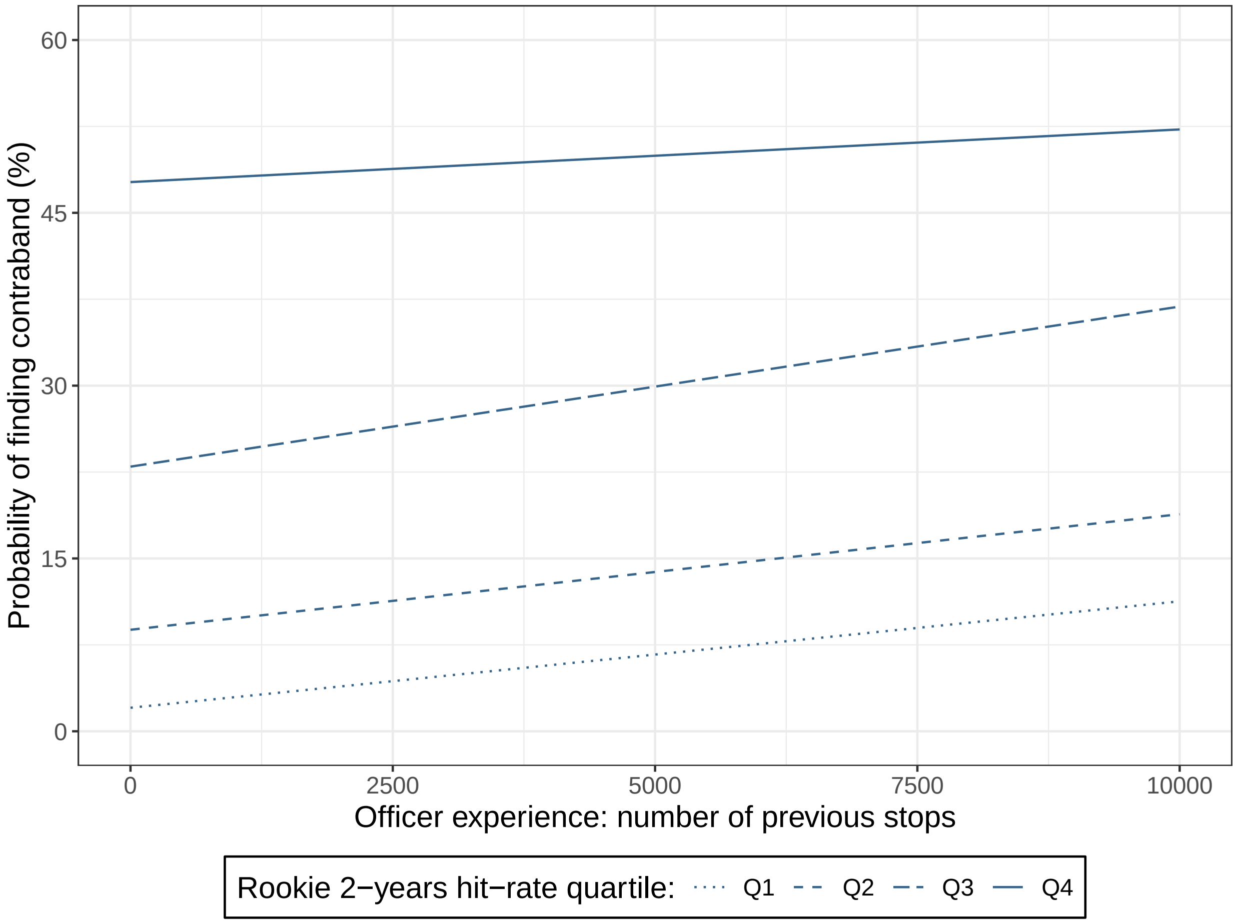

Notes: Figure 1 illustrates state inclusion in the Stanford Open Policing Project (SOPP) data and in this study. There are thirty-one states included in the original SOPP data. Of these, fifteen states are excluded from this study because they do not include officer identifiers: California, Colorado, Illinois, Massachusetts, Maryland, Missouri, Montana, North Dakota, Nebraska, New Hampshire, Nevada, Oregon, Rhode Island, South Dakota, and Tennessee. An additional eight states are excluded because they do not report search behavior or contraband information: Florida, Iowa, Michigan, Mississippi, New Jersey, Ohio, Virginia, and Wyoming. This yields eight states that are included in this study: Arizona, Connecticut, North Carolina, South Carolina, Texas, Vermont, Washington, and Wisconsin. The remaining 19 states (including Alaska and Hawaii) did not provide SOPP with any data.

Figure 2: Likelihood of finding contraband during a search by officer experience



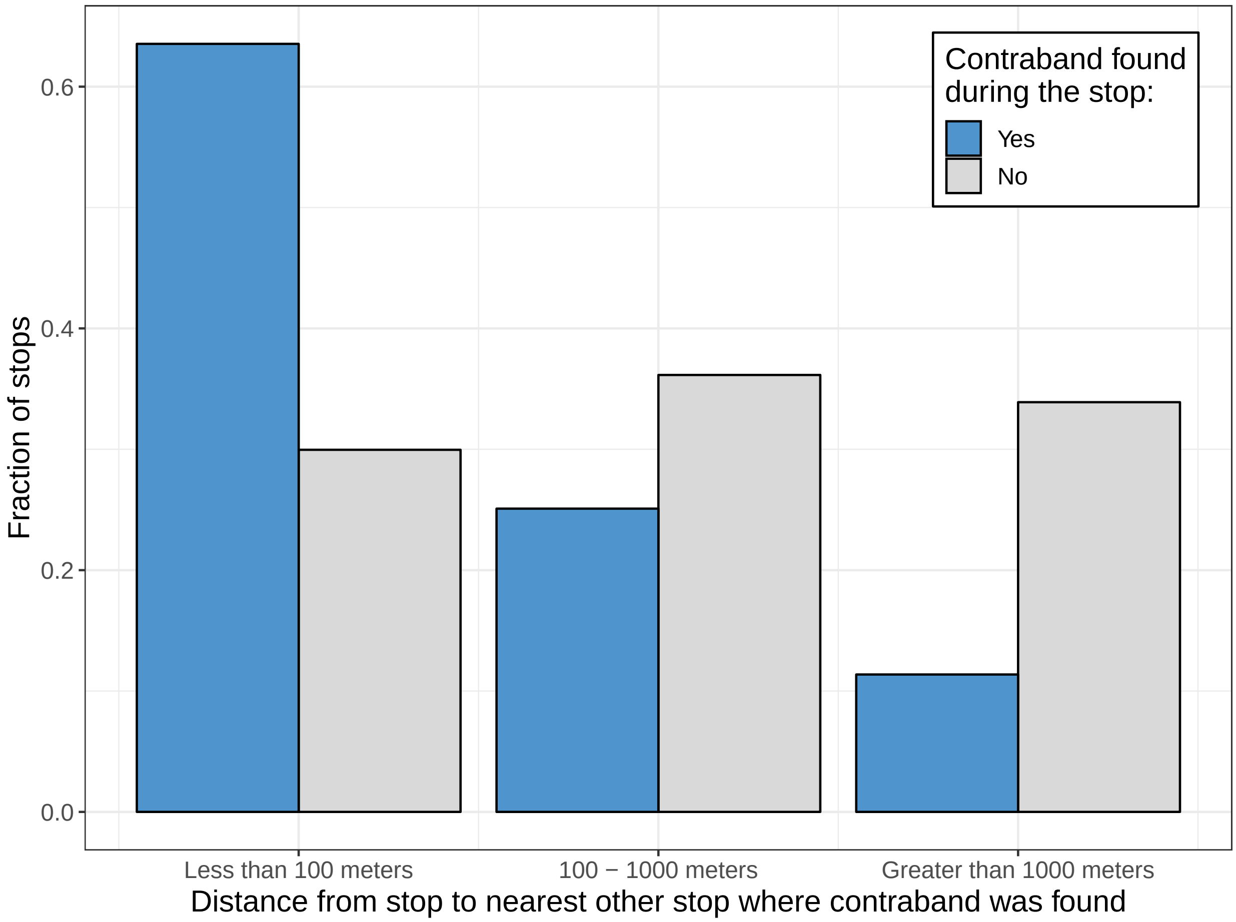

Notes: Figure 2 includes stops by state police in Arizona, Connecticut, North Carolina, South Carolina, Texas, Vermont, Washington, and Wisconsin. All included officers are initially rookies and the same officers are included across multiple levels of experience along the x-axis. The marker sizes indicate the number of officers observed at each experience level. The probability of finding contraband (search hit-rate) is defined as the fraction of searches for which any contraband was found, based on a binary outcome for each search conducted in the underlying microdata. The officer experience term counts all stops made by the officer prior to that stop, regardless of whether a search was conducted. For visual clarity, the markers are binned into hundreds of stops experience and truncated at 10,000 stops, the 95th percentile within sample. The linear regression fit line is estimated using the underlying microdata and has a y-intercept of 21.12 percent and a slope of 1.081 percentage points per thousand stops.

Figure 3: Likelihood of finding contraband by officer experience and baseline ability




Notes: Figure 3 includes the same stops as in Figure 2 and shows the linear regression fit lines separately for each quartile of rookie 2-years baseline contraband search productivity. As in Figure 2, all included officers are initially rookies and the same officers are included across multiple levels of experience along the x-axis. Similarly, the the officer experience term counts all stops made by the officer prior to that stop, regardless of whether a search was conducted. The y-axis is the regression-predicted probability of finding contraband when conducting a search.

Figure 4: Proximity distributions of stop locations to nearest found contraband




Notes: Figure 4 shows proximity distributions in meters from each stop location to the nearest stop where contraband was found, for stops with available geocoordinates. The blue bars include stops for which contraband was found and the grey bars include stops for which no contraband was found, regardless of whether a search was conducted. Geocoordinates for stops are available only for a subset of the stops made in South Carolina, Texas, Washington, and Wisconsin – 47 percent of total in-sample stops.

Table 1: Summary statistics on state police stops

| | Fraction of | | Average | |
|---|---|---|---|---|
| | Stops (1) | Searches (2) | Search rate (3) | Hit rate (4) |
| Search conducted | 0.0214 | 1.0000 | | |
| Contraband found | 0.0053 | 0.2481 | | |
| Female driver | 0.3256 | 0.2048 | 0.0138 | 0.2382 |
| Male driver | 0.6744 | 0.7952 | 0.0259 | 0.2505 |
| Asian driver | 0.0170 | 0.0115 | 0.0148 | 0.1645 |
| Black driver | 0.1569 | 0.1852 | 0.0259 | 0.2877 |
| Hispanic driver | 0.2178 | 0.3075 | 0.0310 | 0.1961 |
| White driver | 0.5905 | 0.4653 | 0.0173 | 0.2699 |
| Total count | 19,411,192 | 415,103 | 415,103 | 102,982 |

Notes: Table 1 includes stops by state police in Arizona, Connecticut, North Carolina, South Carolina, Texas, Vermont, Washington, and Wisconsin. All included officers are initially rookies. The first two columns show the fraction of stops and searches for which each covariate in rows takes a logical value of one. The third and fourth columns show the average search rate and contraband finding hit rate by driver demographic characteristics.

Table 2: Officer stop behavior and search efficacy by experience

| | Specification | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| **Panel [A] Number of stops made by officer per month (mean = 84.46)** | | | | | |
| Experience: stops/1000 | $-4.179^{***}$ | $-4.193^{***}$ | $-4.173^{***}$ | $-4.105^{***}$ | $-2.298^{***}$ |
| | (0.195) | (0.195) | (0.195) | (0.196) | (0.426) |
| **Panel [B] Likelihood officer searches during a stop (mean = 2.14%)** | | | | | |
| Experience: stops/1000 | $-0.005$ | 0.001 | 0.001 | $-0.002$ | 0.007 |
| | (0.008) | (0.008) | (0.008) | (0.008) | (0.017) |
| **Panel [C] Likelihood officer finds contraband during a stop (mean = 0.53%)** | | | | | |
| Experience: stops/1000 | $0.022^{***}$ | $0.024^{***}$ | $0.024^{***}$ | $0.024^{***}$ | $0.024^{***}$ |
| | (0.003) | (0.003) | (0.003) | (0.003) | (0.007) |
| **Panel [D] Likelihood officer finds contraband during a search (mean = 24.81%)** | | | | | |
| Experience: stops/1000 | $1.221^{***}$ | $1.235^{***}$ | $1.230^{***}$ | $1.003^{***}$ | $1.481^{***}$ |
| | (0.091) | (0.091) | (0.091) | (0.131) | (0.311) |
| Officer fixed effects | Yes | Yes | Yes | - | - |
| Hour-of-week fixed effects | No | Yes | Yes | - | - |
| Date-of-year fixed effects | No | No | Yes | Yes | Yes |
| Off. by hour-of-week FE | No | No | No | Yes | Yes |
| Year fixed effects | No | No | No | No | Yes |
| Number of officers | 4,728 | 4,728 | 4,728 | 4,728 | 4,728 |
| Number of searches | 415,103 | 415,103 | 415,103 | 415,103 | 415,103 |
| Number of stops | 19,411,192 | 19,411,192 | 19,411,192 | 19,411,192 | 19,411,192 |

Notes: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01; Standard errors are clustered by officer. Each cell presents results from a separate linear model. Panel [A] evaluates how many stops the officer conducted during the month. Panel [B] assesses whether a search was conducted during the stop. Panel [C] examines whether contraband was found during the stop in reduced-form, including all stops regardless of whether a search was conducted. Panel [D] examines whether contraband was found for the subset of stops in which a search was conducted. Reported coefficients are for the linear officer experience term, which counts all prior stops made by the officer. The officer fixed effects are unique to each officer. The hour-of-week fixed effects include 168 values for the hours of the week. The date-of-year fixed effects include 366 values for the dates of the calendar year. The officer by hour-of-week fixed effects are interactions unique to each officer by hour of the week. The year fixed effects include 16 values for the years included in the sample (2001-2016).

Table 3: Identification tests for survivor bias

| | Total observed stops made by officer (mean = 4106) | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Rookie year total stops | $2.922^{***}$ | $2.917^{***}$ | $3.238^{***}$ | $3.234^{***}$ | |
| [mean = 962] | (0.063) | (0.065) | (0.079) | (0.086) | |
| Rookie year total contraband | | 1.849 | 3.748 | 3.537 | |
| [mean = 4.87] | | (4.847) | (4.438) | (4.469) | |
| Rookie 2-years total stops | | | | | $2.075^{***}$ |
| [mean = 1788] | | | | | (0.037) |
| Rookie 2-years contraband | | | | | −0.418 |
| [mean = 9.43] | | | | | (1.808) |
| Constant | $1{,}294^{***}$ | $1{,}290^{***}$ | | | |
| | (74.843) | (75.518) | | | |
| Officers' initial date fixed effects | No | No | Yes | Yes | Yes |
| State fixed effects | No | No | No | Yes | Yes |
| Observations (officers) | 4,728 | 4,728 | 4,728 | 4,728 | 4,728 |

Notes: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01; Each column presents results from a separate linear regression model for the total within-sample stops made by each officer. Columns (1)-(4) use measures of experience and contraband finding during each officer's first year. Column (5) uses measures of experience and contraband finding pooled over each officer's first two years. The officers' initial date fixed effects are values for the date of the first observed stop made by the officer. The state fixed effects are values for the eight states included in the sample.

Table 4: Proximity of stop location to nearest found contraband by experience

| | Proximity to nearest contraband (mean = 1,703m) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Experience: stops/1000 | 3.904 | 1.237 | 1.323 | 1.184 |
| | (11.389) | (11.439) | (11.430) | (11.540) |
| Officer fixed effects | Yes | Yes | Yes | - |
| Hour-of-week fixed effects | No | Yes | Yes | - |
| Date-of-year fixed effects | No | No | Yes | Yes |
| Off. by hour-of-week FE | No | No | No | Yes |
| Number of officers | 2,388 | 2,388 | 2,388 | 2,388 |
| Number of stops | 9,104,060 | 9,104,060 | 9,104,060 | 9,104,060 |

Notes: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01; Standard errors are clustered by officer. Each cell presents results from a separate linear model for the proximity distance in meters from the stop location to the nearest (other) stop where contraband was found. Reported coefficients are for the linear officer experience term, which counts all prior stops made by the officer. Only stops with available geocoordinates are included, which are a subset of the stops made in South Carolina, Texas, Washington, and Wisconsin only – 47 percent of all in-sample stops included in this study. The officer fixed effects are unique to each officer. The hour-of-week fixed effects include 168 values for the hours of the week. The date-of-year fixed effects include 366 values for the dates of the calendar year. The officer by hour-of-week fixed effects are interactions unique to each officer by hour of the week.

Table 5: Officer stop behavior and search efficacy by experience for demographic groups

| | Driver demographic characteristic | | | | | |
|---|---|---|---|---|---|---|
| | All<br>(1) | Female<br>(2) | Male<br>(3) | Black<br>(4) | Hispanic<br>(5) | White<br>(6) |
| **Panel [A] Number of stops made by officer per month** | | | | | | |
| Experience: stops/1000 | $-4.105^{***}$ | $-0.939^{***}$ | $-3.148^{***}$ | $-0.348^{***}$ | $-0.367^{***}$ | $-3.264^{***}$ |
| | (0.196) | (0.073) | (0.132) | (0.052) | (0.089) | (0.163) |
| *Coefficient/mean* | -0.0486 | -0.0350 | -0.0567 | -0.0273 | -0.0208 | -0.0682 |
| **Panel [B] Likelihood officer searches during a stop** | | | | | | |
| Experience: stops/1000 | $-0.002$ | $0.016^{***}$ | $-0.003$ | $-0.006$ | $-0.047^{***}$ | 0.009 |
| | (0.008) | (0.006) | (0.009) | (0.015) | (0.017) | (0.007) |
| *Coefficient/mean* | -0.0009 | 0.0115 | -0.0011 | -0.0023 | -0.0153 | 0.0050 |
| **Panel [C] Likelihood officer finds contraband during a stop** | | | | | | |
| Experience: stops/1000 | $0.024^{***}$ | $0.020^{***}$ | $0.028^{***}$ | $0.022^{***}$ | $0.028^{***}$ | $0.022^{***}$ |
| | (0.003) | (0.002) | (0.004) | (0.006) | (0.005) | (0.003) |
| *Coefficient/mean* | 0.0447 | 0.0595 | 0.0432 | 0.0289 | 0.0453 | 0.0468 |
| Mean monthly stops | 84.46 | 26.79 | 55.49 | 12.73 | 17.66 | 47.89 |
| Mean search rate | 2.14% | 1.38% | 2.59% | 2.59% | 3.10% | 1.73% |
| Mean contraband rate | 0.53% | 0.33% | 0.65% | 0.75% | 0.61% | 0.47% |
| Date-of-year fixed effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Off. by hour-of-week FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Number of officers | 4,728 | 4,636 | 4,717 | 4,604 | 4,601 | 4,680 |
| Number of searches | 415,103 | 84,982 | 330,022 | 75,861 | 125,969 | 190,613 |
| Number of stops | 19,411,192 | 6,157,957 | 12,753,618 | 2,925,028 | 4,058,837 | 11,006,341 |

Notes: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01; Standard errors are clustered by officer. Each cell presents results from a separate linear model. Panel [A] evaluates how many stops the officer conducted during the month of drivers of selected demographic characteristics. Panel [B] assesses whether a search was conducted during the stop. Panel [C] examines whether contraband was found during the stop in reduced-form, including all stops regardless of whether a search was conducted. In Panels [B] and [C], the columns use the respective subsets of data corresponding to drivers of demographic characteristics indicated by the column titles. As in Table 2, reported coefficients are for the linear officer experience term, which counts all prior stops made by the officer. The date-of-year fixed effects include 366 values for the dates of the calendar year. The officer by hour-of-week fixed effects are interactions unique to each officer by hour of the week.

# A Data Appendix

All primary data included in this study were originally provided by state agencies in response to public records requests made by the Stanford Open Policing Project (SOPP, openpolicing.stanford.edu). Researchers who are interested in replication or extension should reference Pierson et al. (2019) and the SOPP data ReadMe in addition to the data documentation that I provide for this study. Data availability varies substantially across states. Of the thirty-one states in the SOPP data, sixteen include officer identifiers. Of these sixteen states, eight do not consistently report officer search behavior or contraband finding. The remaining eight states are included in this study. State categorizations are mapped in Figure 1.

The data as I received them were already partially cleaned by SOPP. I conducted moderate additional data cleaning and standardization across states, particularly for officer identifiers. This data preparation was done using my provided *R* code, and associated explanation and tabulations are provided in my ReadMe document. In particular, I correct for data entry heterogeneity such as leading-zero padding in the officer badge numbers and variation in the entry of sub-agency police divisional identifiers. In addition, although this study only focuses on officer contraband search behavior, the data and my associated cleaning/standardization include several additional police stop covariates and outcome variables. Again, data availability and specificity vary substantially across and within states.

After cleaning and standardizing officer identifiers, all stops are made by a single officer, a very minor restriction. I define a "rookie" officer to be a badge ID whose first observed stop occurs at least 365 days after the earliest observed stop made in the state. I drop observations on 6162 rookie officers who never conduct a single search in the data (many of these are likely typos in the entry of officers' badge numbers). The resulting data examined in this study include 19,411,192 traffic stop observations for 4728 rookie officers who conducted at least one traffic stop search during 2001 - 2016.

# B Relationship between officer experience and pay

This appendix provides the support for my claim in Section 1 of, "Using the *Texas Tribune*'s Government Salaries Explorer, I find that the wages of state police officers increase on average by about 1.82 percent in current dollars per year of experience." To obtain this value, I first downloaded the "State Comptroller Payroll" database of Texas state government salaries from the *Texas Tribune*.[16] I filtered the database to include only the "DEPARTMENT OF PUBLIC SAFETY" Agency Name and the "TROOPER" Class Title. As of the most recent version of the database (dated March 8, 2019), there are 2,342 Texas state troopers in the cross-sectional salary data, which includes one observation per trooper.

I then regress each trooper's annual salary (either in dollars or log-dollars) on the trooper's total years of employment experience as a Texas state trooper. I define experience as a continuous measure using the hiring date for each individual. I estimate these regressions both without controls and with fixed effects for each trooper's ethnicity, gender, and calendar month of initial employment. Table B.1 presents the regression results.

Table B.1: Estimated relationship between officer experience and pay

| | Annual salary for officer | | | |
|---|---|---|---|---|
| | Dollars | | Log-dollars | |
| | (1) | (2) | (3) | (4) |
| Experience: years employed | 1,184$^{***}$ | 1,177$^{***}$ | 0.018$^{***}$ | 0.018$^{***}$ |
| | (14.88) | (16.72) | (0.0002) | (0.0003) |
| Mean | \$64,061 | \$64,061 | 11.06 | 11.06 |
| Controls | No | Yes | No | Yes |
| Observations | 2,342 | 2,342 | 2,342 | 2,342 |

Notes: $^{*}$p<0.1; $^{**}$p<0.05; $^{***}$p<0.01; Using data in the Texas Tribune's Government Salaries Explorer database on state troopers employed by the Texas Department of Public Safety, this table presents results from linear regression models of each officer's annual salary regressed on a continuous measure of years of employment experience as a trooper. The data are cross-sectional and contain one observation per officer employed as of March 2019. The Controls include fixed effects for officers' ethnicity, gender, and calendar month of initial employment.

[16] https://salaries.texastribune.org