

Business Cycles and Police Hires*

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Abstract

We show that the quality of police hires varies over the business cycle. Officers hired when the unemployment rate is high have fewer complaints, disciplines, and are less likely to be fired than officers hired when the unemployment rate is low. Effects are larger for younger workers who have weaker outside options in recessions. We find that the size and quality of the applicant pool increases in high unemployment years—more people take entry exams and a larger fraction pass the exam. Our findings shed light on how outside options affect police hires and speak to policy questions about police recruitment.

Keywords: Police hiring, police quality, public sector labor markets, outside options

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1 Introduction

The public-sector workforce is the backbone of a well-functioning state. Despite the critical importance of this input in the production of effective government, the best approach to recruitment and selection of public-sector employees remains unclear. While offering financial incentives may attract higher-skilled applicants, it might crowd out applicants with an intrinsic desire to work in government (Bénabou and Tirole, 2003; Delfgaauw and Dur, 2008; Barfort et al., 2019). This tension between extrinsic and intrinsic motivations raises an important question: should government agencies use financial incentives to attract workers?

In this paper, we study the power of financial incentives in the context of police recruitment and selection in the United States. Specifically, we examine how the quality of police hires responds to the availability of outside options in the labor market. We use the unemployment rate at career start as a source of variation in the relative financial attractiveness of policing, as recessions may improve the relative earnings and the likelihood of finding a job in policing relative to outside options. Our analysis thus informs how changing the financial attractiveness of policing affects new police hires.

Studying these issues in the context of policing is both substantively important and policy relevant. First, over 1 million people are employed as police officers in the US, making it one of the largest public-sector occupations in the country. State and local governments in the United States spent an estimated \$123 billion on police protection in 2019, with approximately 60% of that total funding police salaries (US Census Bureau, 2020). Second, although the role of police is to promote public safety, a long history of police violence, racial harassment, and a host of other negative police-civilian interactions have cast doubt on the ability of police to fulfill that role (Pierson et al., 2020; Harrell and Davis, 2020; Ba et al., 2021; Hoekstra and Sloan, 2022). Over the last two decades, public confidence in police has fallen from a peak of 64% in 2004 to 51% in 2021 (Brenan, 2021). Some proponents of police reform have proposed reducing the size of police forces, while others have argued for a focus on hiring higher quality officers. The latter approach, however, is stymied by recent increases in officer quit rates and lulls in recruiting (Shalal and Moore, 2020).

Furthermore, the nature of policing differs in two core ways from other public-sector occupations. First, the intrinsic motivation of police may be associated with negative rather than positive outcomes. If the intrinsic motivations of police are biased against the interest of suspects (Prendergast, 2007) or driven by a taste for harsh punishment (Dharmapala et al., 2016), then crowding out those applicants might improve police treatment of civilians. Second, police training and police culture might transform new recruits in a way that negates gains made on the recruitment margin (Adger et al., 2022). It is an open question whether it

is possible to improve policing via recruitment at all, and if so, whether it can be accomplished with financial incentives.

This paper provides the first evidence on these questions by studying how economic conditions, and hence financial incentives, shape the quality of officer hires. We study this in the context of Florida, which has the fourth largest police workforce in the United States, representing nearly 6.5% of nationwide police employment since 1980. We leverage a detailed database of all police employment spells in the state from 1985-2015. These records contain information on officer demographics, employment start date, separation date, and multiple outcomes related to officer quality. We observe all agency-sustained complaints made against an officer, corresponding disciplinary actions, and if they separate from the agency, the reason for separation. We link these records to annual data on economic conditions and assess the relationship between the unemployment rate in an officer’s starting year and that officer’s quality.

We show the quality of new hires does vary in response to economic conditions at career start. Over these three decades in Florida, officers hired when the unemployment rate is higher have subsequently fewer complaints filed against them and fewer disciplinary actions. Officers who enter policing when alternative job opportunities are more scarce are also less likely to be fired. Since a significant share of officers leave their jobs early into their careers, we focus on the officer’s first five years on the force. In our main specification, which controls for agency fixed effects and contemporaneous economic conditions, we find that officers hired when the unemployment rate is one percentage point higher, receive 8% fewer complaints and 13% fewer disciplinary actions within the first five years of their start date. They are also 5% less likely to be fired and 5% less likely to be fired for violating agency policy in those first five years. Furthermore, the effect of initial unemployment across the four outcomes emerges as early as the second year since hire, indicating our results are not driven by early-career attrition.

We show that our results are robust to including agency-by-year fixed effects, thus accounting for the possibility that the nature of policing may vary over time. Although our main specification focuses on the first five years of an officer’s career, our results are also robust to relaxing this restriction and including all years of the employment spell. We also show that our results are robust to including agency-specific linear trends in starting year, controlling for various leads and lags of initial unemployment, removing the contemporaneous unemployment rate as a control, and controlling for officers’ characteristics. We find similar results when using unemployment rates across seven sub-state regions as our measure of economic conditions at career start. Moreover, we find larger impacts for younger officers across the four quality measures — this group may exhibit stronger sorting responses

to economic conditions, since firms are less likely to hire young workers during recessions (Forsythe, 2022).

From these results alone, we do not know if the changing quality of hires is due to changes in the pool of applicants or changes in agency hiring decisions. To overcome this empirical challenge, we take advantage of additional data on entry exams and on the number of officers hired each year. This allows us to provide direct evidence on the labor supply channel and to assess the relative importance of labor supply and labor demand in driving our results.¹ It is possible that both of these forces are important in this setting. If Florida police agencies are more constrained when unemployment rates are high, they may be more selective in their hiring decisions. On the other hand, workers might find policing more appealing when alternative job opportunities are scarce.

Why might we expect the applicant pool to change in response to economic conditions? First, prior work has documented that the labor supply of teachers and nurses changes across the business cycle (Propper and Van Reenen, 2010; Britton and Propper, 2016; Nagler et al., 2020). Compared to these public sector occupations, policing has relatively low entry costs. In Florida, individuals can enter policing after completing a training program that is typically only 20-30 weeks long. Second, since public sector employment is less pro-cyclical than in the private sector (Kopelman and Rosen, 2016), more high-skilled individuals may apply to become officers in recessions due to higher expected earnings relative to other jobs. In our setting, we show that starting salaries in policing in Florida do not vary across the cycle, in contrast to the pro-cyclicality of starting wages in the private sector.

To assess the importance of the labor supply channel, we draw on a dataset of entry-level exams taken by police applicants in Florida from 1996-2015. These exams are taken after applicants have finished basic recruit training but before they are hired. Not all applicants pass the exam and not all those who pass are hired, making this a good proxy for labor supply. We find that the number of people taking the exam in a year is positively correlated with the unemployment rate. We further show that the fraction of people who pass their initial exam attempt is higher when the unemployment rate is higher. We then confirm that passing the exam on the first attempt is a good proxy for officer quality. First, agencies are significantly more likely to hire officers who pass the exam on their initial attempt. Second, conditional on being hired, officers who pass the exam on their initial attempt have fewer complaints, fewer disciplinary actions, and are less likely to be fired than officers who do not pass on their initial attempt.

¹We also assess the possibility that the nature of policing may change during recessions. We do not find significant changes in the types of agencies that officers joined across the cycle or in their job training participation. Our results are also robust to directly controlling for current and starting year crime rates.

Motivated by the finding that passing on the initial attempt is correlated with officer outcomes, we use machine learning to predict agency-sustained complaints and disciplines for each applicant, leveraging the information from the exam, other applicant characteristics and their police academy training records (Chalfin et al., 2016). We first verify that these measures capture a meaningful dimension of quality by showing that our measures of predicted misconduct events are negatively associated with likelihood of being hired. Finally, we show that applicants have lower predicted complaints and disciplines, on average, when the unemployment rate is high. Taken together, these analyses provide direct evidence that the pool of applicants is stronger when outside options are scarce.

At the same time, if Florida police agencies are more constrained when unemployment rates are high, they may be more selective in their hiring decisions. To assess the role of the labor demand channel, we take advantage of additional data on the number of officers hired each year. The number of new hires is pro-cyclical in Florida, suggesting that demand for police does rise and fall with the state of the economy at large. However, we do not find evidence that agencies in Florida alter their hiring criteria across the cycle. Although passing the exam on the initial attempt is associated with an increased likelihood of being hired, we do not find that this relationship is stronger in years with higher unemployment rates. Likewise, we do not find a stronger relationship between predicted misconduct events and the likelihood of hire when economic conditions are worse. This suggests agency labor demand does fall as the unemployment rate rises, but the associated hiring decrease is not accompanied by agencies setting a higher bar for applicants.

While these results provide direct evidence that police labor supply plays changes across the cycle, we also conduct a bounding exercise to estimate the potential importance of the labor demand channel. With this exercise, our principal goal is to demonstrate that labor supply can account for an important share of the main effect, not necessarily that it is the sole driver of our results. Under conservative assumptions about how agencies screen out lower quality workers, we find that labor demand responses can account for 15-30% of the overall effect of the initial unemployment rate on misconduct events.

These results suggest that when markets are weak, the supply of potential police officers increases and agencies are able to hire better officers as a result. As such, we first contribute to a growing literature on the determinants of police quality. In this context, previous papers have studied how various interventions within an agency affect the performance of officers already on the force (e.g., Owens et al. (2018); Goncalves (2021); Owens and Ba (2021); Rivera and Ba (2022)). Within that set of papers, a few study the importance of officer pay or other financial motivations (e.g., Mas (2006); Foltz and Opoku-Agyemang (2020); Chalfin and Goncalves (2021)). While these papers offer valuable evidence on how to

improve performance of existing police, a separate literature has developed on selection into policing and officer recruitment (McCrary, 2007; Prendergast, 2007; Dharmapala et al., 2016; Ornaghi, 2019; Linos and Riesch, 2020; Cox and Cunningham, 2021; Cox et al., 2021). These studies, however, do not consider the empirical relationship between outside options, officer selection, and labor supply. Our paper is one of the first to examine the role of improving the relative appeal of policing as an occupation, whether that be through financial incentives or other means, in attracting better officers. Importantly, our findings suggest that agencies attract higher quality applicants when policing is more appealing than outside options. As a result, better officers are hired and those improvements persist even in the face of existing police culture and training. In general, recruitment, selection, and retention of public sector workers are critical policy levers, and our paper informs how government agencies can hire and retain good workers. Therefore, we build on an existing literature about the effect of financial or career incentives on the recruitment and performance of public-sector employees (Propper and Van Reenen, 2010; Ferraz and Finan, 2011; Dal Bó et al., 2013; Nagler et al., 2020; Bertrand et al., 2020).

Furthermore, our findings contribute to an extensive literature on the cyclicity of labor market flows, and how this differs by worker skill and across the public and private sectors (Krueger, 1988; Kopelman and Rosen, 2016), as we show that applicant pool of police officers changes across the cycle, and so does their on-the-job performance. By showing that the applicant pool for policing in Florida improves during recessions, we also contribute to a literature that studies how the quality of the unemployment pool varies across the business cycle (Pries, 2008; Elsby et al., 2015; Mueller, 2017). We further connect to a related literature that explores how the quality of new teachers and nurses responds to outside options in the labor market (Propper and Van Reenen, 2010; Britton and Propper, 2016; Nagler et al., 2020; Deneault, 2023).² Most closely related to our work is Nagler et al. (2020)’s paper on the relationship between economic conditions and teacher quality. We build on their creative approach to understanding public sector labor supply in a number of ways. First, we document similar patterns for police, a separate, large public sector occupation. Policing has lower entry costs with respect to training and credentials than teaching, which may make it particularly responsive to economic conditions. Second, we employ data that includes each year of an officer’s employment spell since 1985, allowing us to observe and analyze attrition directly. With this data we can also estimate dynamic impacts on officer quality, rather than the effect on quality at a single point in time. Lastly, we use rich data

²Our paper also adds to a growing literature on the relationship between economic conditions at career start and subsequent labor market outcomes (Oyer, 2008; Kahn, 2010; Oreopoulos et al., 2012; Böhm and Watzinger, 2015; Altonji et al., 2016; Schwandt and Von Wachter, 2019)

on entry exams and on the number of officers hired to explore the labor supply and demand mechanisms, respectively.

2 Data, Institutional Context and Methodology

2.1 Data Sources

Our empirical analysis relies on administrative data from the Florida Department of Law Enforcement (FDLE) (FDLE, 2016; Goncalves, 2021; Dharmapala et al., 2022). In particular, we use data on all employment spells for police officers in Florida from 1985 through 2015, which include information on the start and end date for each spell, the reason for separation, the employing agency, and various outcomes related to police quality. We also observe the officer’s name, birth year, gender, and race. Following Grunwald and Rappaport (2019), we restrict our analysis to full-time law-enforcement officers. Moreover, we drop a small number of officers who started their employment at two different agencies at the same time.

Importantly, FDLE data includes multiple measures of officer performance. Specifically, we use FDLE administrative data covering misconduct events and disciplinary actions held against officers. Misconduct events included in the FDLE data encompass civilian or internal affairs complaints previously sustained by a local law enforcement agency.³ The FDLE then opens a ‘complaint’ file and starts a disciplinary investigation — these FDLE-level investigations comprise the agency-sustained complaints considered in our analysis. Complaints are evaluated by an FDLE commission and if the commission finds ‘clear and convincing’ evidence of officer misconduct, it can discipline the officer following existing guidelines (Dharmapala et al., 2022). We follow Grunwald and Rappaport (2019) and Dharmapala et al. (2022), among others, and use complaints and disciplines as proxies for officer quality. Close to 30% of complaints result in a disciplinary action in the FDLE data.

We also rely on information in the FDLE data on the reason for officer separations. We consider whether an officer was fired from their position as a proxy for officer quality, motivated by the idea that police agencies are more likely to fire officers with poor performance. Moreover, since FDLE data includes the specific reason for which an officer was fired, we also analyze whether an officer was fired for directly ‘violating agency policy,’ which may more closely capture events in which an officer was fired due to poor performance on the job. In our data, among the sample of fired officers, 67% of them were dismissed for violating agency policy. These outcomes capture officers at increasingly worse parts of the quality distribu-

³Local agencies sustain a misconduct event if the purported offense by the officer violates a “good moral character” requirement. If the offense is sustained, the local agency must report its findings to the FDLE.

tion, allowing us to observe effects for a range of police misconduct. At the other end, we also observe whether officers separate from their agency voluntarily, a potential indication that they are able to find better outside options once economic conditions improve.

Our main variable for economic conditions at career start comes from annual variation in the national unemployment rate.⁴ We follow the literature on the impacts of graduating in a recession and use the unemployment rate as our preferred measure of business cycle conditions as it captures the extent to which potential police officers' outside options may be limited. We also use alternative measures of economic conditions, such as the Florida-wide unemployment rate. In supplementary analyses, we use information on county-level unemployment rates (available since 1990) aggregated to seven sub-state regions designated by the FDLE. Altogether, we merge the measures capturing economic conditions to each year of employment for all officers included in our sample.

2.2 Institutional Background

Becoming a police officer in Florida involves multiple steps. First, candidates must meet a number of minimum requirements, including being at least 19 years old, having a high school degree or GED, and passing a background check. Moreover, they must pass an exam evaluating basic competencies and a physical examination. Upon meeting these requirements, candidates must complete the Basic Recruitment Training Program, which prepares them for becoming an officer through academic courses and physical preparation, amounting to a total of 770 hours. After the training course (or if they have previously served as police officers either in Florida or in other states), applicants must pass the State Officer Certification Exam (SOCE), which evaluates their preparation to become police officers. Upon passing the exam, candidates apply to a police agency, and hiring is done at the discretion of the agency. In the FDLE data, 59% of applicants were hired within one year after passing the SOCE.

We use the FDLE data to assess the characteristics of new police officers in Florida. New hires are on average 29 years old, and close to 20% of officers are older than 35 at their career start. The vast majority of new officers in Florida are male (86%) and white (76%), see Table A1. To understand officers' occupational trajectories, we take advantage of the longitudinal component of the Current Population Survey in 1980-2019. We identify all individuals who transitioned into or out of policing and examine their occupational transitions. A substantial share of officers are previously employed in other public and private protective service occu-

⁴We use the unemployment rate faced by an officer at the start of each employment stint, except when the previous stint resulted in a transfer within agency. For those officers, we use the unemployment rate at the start of their careers. Results are robust to alternative definitions.

pations (16% and 10%, respectively), but the majority come from non-employment (15%) or from more distinct occupational categories like administrative support (10%), management (8%), business (6%), or sales (5%), see the second panel of Table A1. Likewise, individuals transition to a similar set of occupations after their employment as police. This evidence indicates that officers move across a variety of occupations, suggesting they may adjust their labor supply in response to economic conditions.

We turn back to the FDLE data to describe long-term employment outcomes for a group of young officers hired in 1985-1989 (Figure 1). First, a sizable share of officers leave their jobs early into their careers, with 61% remaining employed after five years and 25% after twenty-five years. The majority of officers who leave within ten years do so voluntarily, with smaller shares being fired for violating agency policy or fired for other reasons. Figure A1 further shows that despite a small increase in retention rates for more recent entry cohorts, around 60% of officers remain employed in FDLE five years after being hired.

External Validity. An important advantage of our data is that it covers all agencies in the state of Florida rather than a single large urban department. Studying these patterns in Florida alone is substantively important because it has the nation’s fourth largest police workforce, representing 6.5% of police employment in the US since 1980. A potential drawback, however, is that the labor market for police officers in Florida may differ in important ways from the labor market for police in other states. To explore this concern, we compare the firing and separation rates in Florida to all other states in the US. In Figure A2, we draw on information from a 2008 report by the Bureau of Justice Statistics, on data from the 2003 LEMAS, and the CPS for 1985-2015, where we find that Florida has firing and separation rates that are similar to nationwide rates and fall approximately in the middle of the state-level distribution.⁵ The similarity across these features of the police labor market suggests that estimates from Florida may be more broadly informative across the country. It bears emphasis that policing in Florida is interesting in its own right given its large share of overall police employment in the United States.

2.3 Summary Statistics

Our sample includes 103,971 unique employment stints, which last an average of 9 years. We present summary statistics in Table 1. The first column presents average statistics for our sample. 7% of officers receive a complaint against them, and close to 30% of these

⁵Within Florida, agencies that have unionized at some point before 2019 have lower firing rates than agencies that have not unionized. However, we show in Section 3.3 that our main results emerge for new hires in both unionized and non-unionized agencies.

complaints eventually result in disciplinary action. On average, 9.4% of officers are fired from their jobs, and two-thirds of those fired are fired for violating agency policy. A sizable share of officers (54%) leave their jobs voluntarily. In the remaining columns, we split the sample by whether the officer was hired in a year above or below the median unemployment rate (5.8%), where the difference in unemployment rates at start across these two groups amounts to 2.1 percentage points. Officers hired in high-unemployment years are less likely to have a complaint filed against them (6.1% vs. 7.7% of officers), less likely to be fired (8.6% vs. 10%), and less likely to be fired for violating agency policy (5.7% vs. 6.7%). The raw data also shows that officers hired in high unemployment years are less likely to receive complaints or be fired than those hired in low unemployment periods (Figure A3).

2.4 Empirical Strategy

We examine the impact of economic conditions at career start on police officer quality by estimating the effect of the unemployment rate at the time of hire on our main officer outcomes in the following equation:

$$Y_{iat} = \alpha + \gamma UR_i^S + \beta UR_t + \theta_a + \varepsilon_{iat} \quad (1)$$

where Y_{iat} represents an outcome of interest for officer i employed in agency a in year t ; UR_i^S captures the national unemployment rate that prevailed in the year in which the officer was first hired. θ_a capture agency fixed effects which account for differences in outcomes across police agencies in Florida. In equation (1), we control for contemporaneous unemployment (UR_t) implying that γ allows us to recover the effect of the initial economic conditions net of all subsequent unemployment rates (Von Wachter, 2020). This accounts, for example, for the possibility that officers may adjust their policing behavior due to facing weak concurrent outside options. We additionally estimate a specification that accounts for potential changes in the policing environment by including agency-by-year fixed effects (θ_{at}) in equation (1).

To implement equation (1), we need to account for the fact that a sizable share of officers leave their jobs early into their careers. As such, we could estimate the equation including officers' entire employment stints — implying that the latter years would only include a few individuals — or focusing early in their careers, yet missing potential longer-term impacts. In our main specification, we take a middle ground approach and focus on officers' outcomes in their first five years since hire, yet we examine robustness to this choice by looking at alternative time windows.

We conduct an extensive set of robustness tests along other dimensions as well. First, we re-estimate equation (1) without including the contemporaneous unemployment rate, which

allows us to recover the total impact of initial conditions on police quality. In addition, we assess the robustness of the results to the inclusion of time trends and to multiple leads and lags of the unemployment rate. We estimate the results using the Florida-wide unemployment rate, restricting our sample to stints starting before 2011 (to observe outcomes fully through the end of our data) and analyzing outcomes through the first two years of employment. We also estimate a specification in which we control for officer characteristics at career start and a specification in which we control for officers’ experience. We also estimate a version of equation (1) that controls for current and starting year crime rates. These results and others are described in more detail in Section 3 and in their respective table and figure notes.

We further analyze the dynamic impacts of initial economic conditions on officer outcomes by estimating equation (1) separately for each year s since the officer was hired, where s ranges between zero and five. This specification allows us to present novel evidence by tracing out the dynamic effects of unemployment rates at career start on officer quality across their first five years of employment.

3 Results

3.1 Main Results

In Table 2, we present the estimates from equation (1). In the first column, we show that officers hired when the unemployment rate is one percentage point higher receive 0.00077 fewer complaints, or 8.5% of baseline complaints in the first five years of employment.⁶ As such, an officer who is hired during a period with an above-median unemployment rate is 20 percent less likely to receive an agency-sustained complaint against them compared to a counterpart hired during below-median periods. This is similar in magnitude to civilian complaint reductions caused by increased police oversight (Jordan and Kim, 2022; Rivera and Ba, 2022), procedural justice training (Wood et al., 2020), and the introduction of body-worn cameras (Kim, 2022). We find similar impacts on the prevalence of disciplinary actions, as a one percentage point increase in the unemployment rate at career start results in 0.00039 fewer disciplinary actions, or 13% of baseline disciplinary actions in the first five years. Altogether, the effects of initial economic conditions on police officer quality, as proxied by both the prevalence of complaints and disciplinary actions, are both economically and statistically significant.

⁶In Table A2, we show summary statistics on the baseline prevalence of complaints and disciplinary actions across both the first five years and the full employment stint for officers in our sample.

In the last two columns of Table 2, we present the estimated impacts on the likelihood of being fired. We find significant impacts of the initial unemployment rate on the likelihood that officers are subsequently fired and that this firing is due to violating agency policy. In fact, the estimated coefficients for both variables are such that a one percentage point increase in the initial unemployment rate results in a 4.9% decrease in the likelihood of being fired altogether and of being fired for violating agency policy in the first five years of employment.

In Figure 2, we present the dynamic effects of initial unemployment on officer outcomes across the first five years of employment. In the first panel, we present the estimated impacts on the prevalence of complaints. We find no significant impacts of initial unemployment rates on complaints in the year of hire, yet the effects become negative and statistically significant by the second year since hire. The coefficients remain negative through the next three years, with varying statistical significance. For disciplines, we similarly fail to find an effect of economic conditions at start in the initial year, yet the effects become negative and significant for the first three subsequent years. As such, we find that the estimated impacts of initial unemployment rates on officer quality emerge as early as the second year since starting the job, indicating the effects are not driven by attrition. In their analysis of business cycles and teacher quality, Nagler et al. (2020) use a time-invariant measure of teacher quality (value-added). Our estimated dynamic effects thus constitute novel evidence in the literature by allowing us to understand how early police quality is revealed in light of starting conditions. In the last two panels, we show that officers hired in high-unemployment periods are less likely to be fired within two years of their career start, and to be fired for violating agency policy in their second year of employment.⁷

Measuring Officer Quality. In this context, we note that the policing literature has previously highlighted the difficulty of assessing officer quality (Rim et al., 2020; Hoekstra and Sloan, 2022). We do not aim to determine a holistic measure of officer quality, instead we posit that the outcomes we study are negative aspects of officer quality. For instance, a common concern with using complaints as a measure of quality is that officers who are more proactive on the job may receive more complaints. If that were the case, then complaints could reflect good policing performance. However, Rozema and Schanzenbach (2019) have shown that officers who receive complaints against them have a far higher likelihood of facing future civil rights litigation. Moreover, the complaints we study are agency-sustained

⁷Our results show that officers hired in a recession are less likely to have been fired early in their careers. These officers are at the bottom of the quality distribution, suggesting that had they remained employed, they would have likely incurred additional complaints and disciplines.

complaints, not civilian complaints and complaints with disciplinary actions are further sustained by FDLE.

In Section 4, we further show that a distinct measure of officer quality from the civil service exam is correlated with our outcome measures – officers who pass the entry exam on the first try are less likely to have received complaints and disciplinary actions. Lastly, 55% (86%) of officers with a complaint (discipline) in their first two years on the force are eventually fired, compared to 11% (12%) of officers who do not receive complaints (disciplines) in their early career. Moreover, 86% of officers who receive a complaint in their first two years are eventually decertified from policing in Florida, compared to just 11% of those who did not receive a complaint.⁸ Altogether, these results support the claim that complaints and disciplines capture negative aspects of officer quality in our setting.

Addressing Differences in the Nature of Policing. There are two distinct potential concerns related to time-varying differences in the nature of policing in our setting. The first is that the contemporaneous policing environment may be correlated with the starting year unemployment rate in a way that confounds our main estimates. The second is that the starting year policing environment could have path-dependent effects on officer quality, and may thus be one mechanism through which our effects arise. In this section, we examine the robustness of our results to account for the fact that the contemporaneous nature of policing may vary across agencies and over time. In Section 4.3, we explore the possibility that the policing environment in one’s starting year may partially explain our results.

We first estimate a modified version of equation (1) that includes agency-by-year fixed effects, implying we compare outcomes for officers working in the same agency in the same year but who were hired at different points in the cycle. We present the estimated results in the first panel of Table 3, finding that officers in high unemployment years are less likely to have received disciplines against them, to have been fired, and to have been fired for violating agency policy. Since this specification focuses on the first five years since career start, the year fixed effects are effectively identified *only* from officers who were hired in the past five years.

To improve the identification of the year fixed effects, we re-estimate this specification using all employment years, and we find significant effects of initial unemployment on the prevalence of complaints, disciplines and the likelihood of being fired for violating agency policy. However, these estimates suffer from attrition bias, as a sizable share of officers leave

⁸We leverage additional information capturing whether officers were decertified in Florida, where we observe the decertification date for 55% of decertified officers. We estimate a specification that relates the unemployment rate at hire to whether an officer is ever decertified (Table A3). Officers hired in high-unemployment years are less likely to have been decertified (p-value = 0.166).

policing early into their career. To identify year fixed effects from all employed officers and limit attrition bias, we estimate an additional specification in which we first residualize each outcome of interest from agency-year fixed effects, controlling for differences in the prevalence of each outcome for *all* officers employed in a particular agency in that year. We then use the residualized variable as our outcome of interest in equation (1). We present the results in the third panel of Table 3, where we find that officers hired in high unemployment years are less likely to receive complaints and disciplines, and are less likely to be fired and fired for violating agency policy. Altogether, these results indicate that our main results are robust to controlling for differences in the contemporaneous policing environment in a number of ways.

We also assess the robustness of our results by estimating a modified version of equation (1) that uses variation in initial local unemployment rates across seven sub-state regions and controls for agency-by-year fixed effects (Table A4). We find that officers who started with higher local unemployment rates have fewer complaints, disciplines, and a lower likelihood of being fired/fired for violating agency policy, with significant effects on the last three outcomes.⁹ We do not use local unemployment rates in our main specifications because it requires us to focus on officers hired since 1990, it does not account for potential within-Florida migration flows, and it requires using local unemployment estimates that are inherently noisier than national-level estimates. Nevertheless, these results are consistent with the evidence presented in Table 2.

Agency-Level Public Safety Outcomes. Our main results focus on four outcomes that are negative aspects of officer quality. To our knowledge, there is no empirical evidence supporting the concern that officers who are worse along these dimensions are, on the other hand, better at deterring crime or clearing cases. Nonetheless, we explore that possibility to the best of our ability using variation in agencies’ employment composition, as we do not observe officer-level arrest data. In particular, we analyze whether agencies with a larger share of officers hired in high-unemployment years attain better public safety outcomes. We capture agencies’ annual outcomes using Uniform Crime Report (UCR) data on agency-level crime rates and clearance rates between 1985-2015. We study how these outcomes vary depending on the composition of officers in an agency, including average race, age, gender and unemployment rate at career start of all officers employed in the agency in that year. We control for agency size, contemporaneous unemployment, as well as agency and year fixed effects. Agencies with a larger share of officers hired in high unemployment years have lower

⁹In the second panel of Table A4, we re-estimate this specification using all employment years and find significant effects across all four outcomes.

crime rates and higher clearance rates (Table A5).

3.2 Robustness Checks

We test the robustness of our main results to alternative specifications in Figure 3. First, the estimated impacts on complaints, disciplines and the likelihood of being fired are robust to removing contemporaneous unemployment as a control, implying that initial unemployment affects officer quality regardless of subsequent economic conditions. Moreover, all estimates remain significant upon including linear time trends. We also show that our results are robust to including agency-specific linear trends in the starting year, which controls for secular changes in policing environment. The effects on complaints and both firing outcomes remain similar even when conditioning on various leads and lags of the initial unemployment rate. Our results are also robust to using the Florida unemployment rate for capturing initial economic conditions, to focusing on officers who start their jobs in 1985-2011, to controlling for worker observables at baseline, and to controlling for current and starting year crime rates. We also include a specification in which we control for officer’s experience—despite introducing this endogenous control, we find similar results. Furthermore, we find larger estimated coefficients when focusing on officers’ first two years since hire, which is consistent with the dynamic impacts presented in Figure 2.¹⁰

In all of the main specifications, we cluster standard errors at the level of the officer’s starting year. However, we consider alternative methods of statistical inference with small clusters. First, we show that the results are fundamentally unchanged by estimating standard errors via a wild cluster bootstrap procedure (Table A7). Second, we construct placebo estimates in two ways: (1) by assigning random starting years to entire starting cohorts and (2) by assigning random starting years to individual officers (Figures A6 and A7). In both cases, our estimates are outliers relative to the distribution of placebo estimates.

To assess the robustness of our results to alternative time windows, we estimate the survival function of police officers receiving a complaint or discipline by the number of years elapsed since hire. In particular, we compare the survival function of the time until an officer receives a complaint/discipline by whether the officer was hired in an above- or below-median unemployment year (Figure A5). Officers hired in low-unemployment years are more likely to receive complaints early in their careers than those starting in high-unemployment periods, with similar results emerging for disciplines. This, in addition to our results that include all years of an officer’s employment spell, further highlights that our findings are robust to

¹⁰Our results are also robust to measuring economic conditions at start using monthly unemployment rates, and to using lagged measures of initial monthly unemployment (Table A6). Moreover, our main estimates are robust to dropping any given entry cohort from the sample (Figure A4).

alternative time windows.¹¹

3.3 Labor Market Dynamics

In Table 4, we present estimates of equation (1) separately by officers' age to analyze whether heterogeneous impacts emerge for younger versus older officers. We find significantly larger effects for young officers (hired when younger than 35) than for their older counterparts. The estimated impacts are statistically different across the two age groups across complaints, disciplines, and the two fired outcomes, suggesting that the quality of younger officers exhibits far stronger responses to business cycle conditions.¹² This is consistent with Forsythe (2022), who shows that during recessions, the hiring rate for young workers falls faster than for older workers. As such, young workers face weaker outside options during slack labor markets, leading to stronger sorting responses into policing than for their older counterparts.^{13,14} We also explore whether the demographic characteristics of hires vary based on the unemployment rate, but do not find any evidence to this end (Table A11).

To provide further evidence on initial unemployment rates and subsequent labor market dynamics, we estimate equation (1) and find that officers hired in high-unemployment years are more likely to voluntarily separate soon after joining the force, with significant effects in their second year since hire (Figure A8). Interestingly, when we do not control for contemporaneous unemployment in this analysis, the estimated coefficient becomes insignificant and is close to zero. The difference in the estimates is consistent with officers remaining on the force in response to their outside options. Since unemployment rates are correlated over time, officers hired in recessions are unlikely to voluntarily separate when outside options remain limited. However, once we account for their outside options — by controlling for contemporaneous unemployment — we find that officers hired in a recession are more likely to subsequently leave voluntarily. Altogether, since higher-quality officers are more likely to have strong outside options in subsequent years, the estimated effect on voluntary separations is consistent with our earlier results.

¹¹We find the same results when controlling for agency fixed effects and officers' observable characteristics.

¹²This result is robust to defining age at start linearly and for officers under 30 at start (Table A8).

¹³Table A9 presents effects by race and gender, respectively. We do not find statistically significant differences across outcomes by these characteristics.

¹⁴Table A10 shows that the estimated effects of initial unemployment on officers' complaints, disciplines and the likelihood of being fired are similar for officers employed in unionized and non-unionized agencies.

4 Mechanisms

We have so far shown consistent evidence that the quality of police officers is significantly higher for officers hired in high-unemployment periods. This result could emerge through different channels. First, recessions may lead to large employment losses in related industries in which potential officers may find work. Thus, during economic downturns, high-quality workers may choose to seek employment as police officers in lieu of lower-paying alternative jobs. At the same time, a recession may lead to a reduction in police department budgets, which would reduce the number of officers agencies could hire. In this case, our findings could arise through police departments selecting the highest-quality officers among the applicant pool. A third possibility is that the nature of policing could vary in a downturn vis-à-vis an expansion, and that starting under different conditions may have a path-dependent effect on quality. In fact, these mechanisms could be taking place at the same time in our setting. Importantly, we do not need to rule out the last two channels to show that labor supply changes contribute to our findings. We provide direct evidence on the size and quality of the applicant pool. We then assess the potential importance of the other channels to paint a comprehensive picture of the mechanisms driving our results.

4.1 Labor Supply

As noted above, labor supply responses may contribute to our results if the relative attractiveness of policing increases during economic downturns. To this end, we use Florida Criminal Justice Agency Profile (CJAP) data in 2011-2019 to examine how posted entry salaries in police agencies and sheriff’s offices vary across the business cycle. In Table A12, we show that starting salaries across police agencies do not vary across the business cycle, which stands in contrast with extensive evidence showing that wages of private sector hires are strongly procyclical (Bils, 1985; Martins et al., 2012; Carneiro et al., 2012; Gertler et al., 2020). As such, these results suggest that becoming a police officer becomes relatively more attractive during high unemployment periods.¹⁵

4.1.1 Entry Exams

Changes Over the Cycle. To assess the potential importance of the labor supply channel, we rely on information from a dataset comprised of all entry-level State Officer Certification Exams taken by police applicants in Florida from 1996-2015. As discussed in Section 2,

¹⁵Employment in industries that new officers tend to come from — professional and business services, retail trade, manufacturing, construction and transportation — is more responsive to business cycle conditions than local government employment (Table A13).

these exams are taken after applicants have finished basic recruit training, and passing the SOCE exam is a requirement to be eligible to be hired by a police agency. In this context, we assess the relationship between the unemployment rate in different years and the number of exam-takers, which we consider a proxy for labor supply. We present the results in the first panel of Figure 4. We find a positive correlation between the unemployment rate and the total number of exam-takers in each year, finding that a one percentage point increase in the unemployment rate leads an additional 62 candidates to take the entry-level exam.

We further examine the relationship between the unemployment rate and the fraction who pass the exam on their initial attempt, which we consider a proxy for quality of the applicant pool. Panel (b) in Figure 4 shows a positive relationship between the unemployment rate and the fraction of people who pass the exam on their initial attempt, as a one percentage point increase in the unemployment rate is associated with a 0.6 percentage point increase in the SOCE pass rate.

Passing as a Proxy for Officer Quality. In this context, we note that applicants who fail the SOCE exam are allowed to re-take the exam up to three times, and 66% of applicants who failed the test on their first try eventually pass the SOCE exam in our data. In the first column of Table 5, we show that among a sample of candidates who eventually pass the test, having passed the SOCE on the first try substantially increases the likelihood of being hired within one year of passing, from 50% to 62%. Moreover, passing the exam on the first try also increases the likelihood that candidates are hired quickly (Figure A9). These results additionally suggest that officers who pass the exam on the first try have qualities that agencies value.

To further assess the link between passing on the first try and officer quality, we analyze the relationship between passing on the first try and our four outcomes of interest. We present the results in the remaining columns of Table 5, which shows that officers who passed on the first try receive 0.005 fewer complaints than those who failed the exam on the first try, such that officers who passed receive 33% fewer complaints in their first five years of employment than their peers who failed. Furthermore, officers who passed the exam receive 0.0016 fewer disciplines, which also amounts to a one-third reduction relative to their peers who did not pass on the first try. We also find significant differences in the likelihood of being fired and of being fired for violating agency policy. Altogether, since passing on the first try is associated with improved measures of officer performance, these results suggest that the pool of applicants improves when outside options are scarce.

4.1.2 Predicted Complaints and Disciplines

To further understand how the quality of the applicant pool varies across the cycle, we predict the complaints and disciplines that candidates would receive if they were hired as officers (Chalfin et al., 2016, 2020). To do so, we leverage information on applicant training outcomes, applicant characteristics, and applicant exam outcomes to predict their misconduct events. In particular, we first restrict the sample to applicants who were eventually hired as officers and use machine learning to predict whether these officers would receive complaints and/or disciplines against them based on whether they passed the SOCE on the first try, their age when they took the exam (divided into nine age brackets), and the specific academy in which they completed their training (there are 43 police training academies in Florida). Specifically, we implement a LASSO logit approach, where we select the penalization parameter through ten-fold cross-validation.

The first two columns in Table A15 present the set of selected predictors for complaints and disciplines, respectively. Training in specific agencies predicts the likelihood of receiving complaints and disciplines, as does the age at taking the exam and having passed it on the first try. Altogether, the LASSO logit model has strong predictive power, as 5.8% of officers in the highest decile of predicted complaints actually receive a complaint against them, compared to 3.4% in the overall sample.¹⁶ In turn, since we also observe the candidate predictors for applicants who were not hired, we have a measure of predicted complaints and disciplines for *all* applicants.

We subsequently correlate applicants' predicted misconduct events to the annual unemployment rate to assess how the applicant pool varies across the cycle. In Figure A10, we show that applicants in high-unemployment years have fewer predicted complaints and fewer predicted disciplines, further showing that the pool of applicants is substantively better during economic downturns.

4.2 Labor Demand

To assess the role of labor demand, in the third panel of Figure 4, we show that the number of hires in police departments in Florida is strongly correlated with the business cycle: the number of officers hired falls significantly during high unemployment years. While we do not observe information on agencies' job postings or vacancies to directly measure police agencies' labor demand, this result suggests that changes in labor demand by police

¹⁶Similarly, 1.8% of officers in the highest decile of predicted disciplines actually receive a discipline against them, compared to 0.9% in the overall sample. Thus, the model's lift, a measure of its predictive accuracy given by the ratio of misconduct events for officers in the top predicted decile to the average prevalence of such events, equals 1.7 and 2.0 for complaints and disciplines, respectively (Chalfin et al., 2020).

departments may play a role in driving our results.

4.2.1 Characteristics of Hired Officers Across the Cycle

While we have so far shown that both the quantity and quality of applicants increases in recessions, agencies also hire fewer officers in such years, implying that labor demand may play an important role in driving our results if agencies screen out lower quality officers in recessions. For instance, agencies may adopt more stringent screening practices in high-unemployment years, thus only hiring the best applicants, such that labor demand would contribute to the overall results presented in Table 2. To assess the importance of labor demand in shaping our findings, we focus on the sample of applicants used in the machine learning exercise and we analyze how the characteristics of these applicants relate to the probability of being hired and importantly, whether this relationship varies across the cycle. Specifically, we estimate the following specification:

$$Hired_i = \alpha + \beta X_i + \gamma UR_i^S + \eta X_i \cdot UR_i^S + v_i \quad (2)$$

where $Hired_i$ is a binary variable capturing whether applicant i was hired within one year of passing the SOCE exam, X_i denotes a characteristic of the applicant (e.g., age, passing the exam on the initial attempt, or predicted complaints), UR_i^S captures the unemployment rate in the year they passed the SOCE exam, and η captures whether the applicants' characteristic differentially affects probability of hire across the cycle. In particular, we consider how passing the SOCE in the first try, the age at the exam, and their predicted complaints and disciplines affect the likelihood of being hired across the cycle.

We present the results in Table 6. We find that candidates who passed the exam on the first try, younger applicants and those with lower predicted complaints and disciplines are more likely to be hired in Florida within a year of passing the exam. However, the importance of these characteristics in driving the likelihood of being hired does not change across the business cycle. For instance, we do not find evidence that agencies are differentially more likely to hire first-time SOCE passers in recessions, nor are they more likely to hire applicants with low predicted complaints in such years.^{17,18} Altogether, these results indicate that while police agencies' labor demand may fall in high-unemployment years, these agencies are not

¹⁷Young applicants are more likely to be hired, despite being more likely to receive complaints. However, this relationship weakens in recessions — this change in labor demand would work against our main findings.

¹⁸Table A17 presents corresponding evidence where the outcome variable captures whether candidates were ever hired to become officers in Florida. We show the results are robust to this alternative definition of being hired. Our results are also robust to using a predicted measure of the *number* of complaints and disciplines that candidates would receive if they were hired.

changing their hiring practices across the cycle.¹⁹ As such, this finding further points to the importance of labor supply – through an improved applicant pool – as an important driver of our main results.²⁰

4.2.2 Bounding Exercise

To further examine the potential importance of labor demand, we assess how our results would change if agencies had not engaged in procyclical hiring. Specifically, we first estimate the counterfactual hiring reduction that would have occurred in each year if the agencies in that year had faced an unemployment rate of 9.6%, the highest rate in our sample, using the relationship between the unemployment rate and number of hires estimated in panel (c) in Figure 4.²¹ We then impose that hiring reduction in the data by removing officers from the sample assuming that agencies screen out those applicants with a high predicted likelihood of police misconduct. In particular, we assume that agencies would stop hiring the officers with the highest predicted probability of having a complaint or a discipline against them in low-unemployment years.^{22,23} This is a *conservative* assumption since we show in Table 6 that agencies screening on predicted misconduct does not vary across the cycle. After ‘removing’ the high predicted misconduct officers from the sample, we re-estimate our main specification relating the initial unemployment rate to *actual* misconduct events, having shut down the labor demand channel. We then compare the estimated γ coefficient from this analysis and compare it to the estimated coefficients presented in Table 2 to recover the share of the main effect potentially explained by labor demand, under the conservative

¹⁹The relationship between passing the SOCE on the first try and the initial unemployment rate is similar for applicants than for hired officers (Figure A11). The last two panels of the exhibit present similar evidence for predicted complaints, remarking that agencies’ selectivity does not change across the cycle.

²⁰To assess whether screening practices change across the cycle, we use CJAP data in 2011-2019 which includes information on agencies’ use of different screening mechanisms. In Table A16, we show that agencies in Florida do not change the likelihood of using various screening mechanisms across the cycle, including requiring interviews, polygraph exams, and previous criminal justice experience, among others.

²¹For instance, in 2000 the unemployment rate is approximately 4%, which is 5.6 percentage points lower than the highest unemployment rate in our sample. We multiply 5.6 by our estimate of -314 from Figure 4, giving us a counterfactual hiring reduction of approximately 1,760 officers if the unemployment rate had been 9.6% in 2000.

²²Since this analysis comprises officers who are already employed, we use a different sample than in Section 4.1.2. However, we still restrict the sample to officers for whom we observe SOCE performance and police academy records. In this sample, we also estimate a LASSO logit model to predict complaints and disciplines using SOCE performance, age at test and training agency as the candidate predictors. The selected predictors are presented in the last two columns of Table A15.

²³Agencies likely have access to additional information about applicants compared to what we observe in this context. However, for the purposes of this exercise, we note the predictive power of our machine learning model for misconduct events (as measured by the ‘area under the curve’ (AUC) in Table A15) is largely similar to that estimated by Chalfin et al. (2016) when predicting misconduct events among police officers in Philadelphia, despite the fact that they have access to extensive information on applicants’ characteristics.

assumption that agencies screen more heavily on predicted misconduct in recessions.

We present the estimated results in Figure 5. About 20-30% of the estimated effect on complaints could be explained by labor demand if agencies stopped hiring officers with high predicted complaints/disciplines in low-unemployment years. Similarly, 12-28% of the effect on disciplines could be driven by labor demand under this bounding exercise. Altogether, this analysis indicates that even when assuming that agencies screen out high-misconduct officers in low demand years, labor supply can still account for a sizable share of the overall effect.

4.2.3 Additional Tests of the Demand Channel

We further assess the importance of labor demand in our context by re-estimating our analyses in settings where labor demand forces are muted. First, we use information on agencies' receipt of hiring grants from the Community Oriented Policing Services (COPS) program to analyze if our main results change in agencies that receive these grants (Chalfin et al., 2022).²⁴ We re-estimate an interacted version of equation (1) and present our results in Table A18. We find that the estimated relationship between initial unemployment and subsequent misconduct (and firing) events does not vary by whether agencies receive hiring (and non-hiring) grants, further suggesting that labor demand does not drive our results.

Second, we present further evidence by focusing on the relationship between initial economic conditions and subsequent officer outcomes for individuals hired in the Florida Department of Highway Safety and Motor Vehicles, which is the second largest agency in Florida. Importantly, the number of hires in this state-level agency does not fall during high unemployment periods (Figure A12). We re-estimate equation (1) for the officers hired in this department and present the results in Table A19. In this small sample of workers, we still find that officers hired in high unemployment years are significantly less likely to face disciplinary actions. Our estimate on the likelihood of being fired is similar in magnitude to the main results, albeit noisier. This result offers further suggestive evidence that our findings are primarily shaped through the labor supply mechanism posited above.

²⁴If our main effects were driven by changes in labor demand across the cycle, we may expect the overall effect in Table 2 to be muted in agencies receiving COPS grants, as having access to COPS grants could allow these agencies to hire relatively more in recessions, but getting worse-quality officers. As such, if labor demand were driving the results, we would expect a weaker relationship between initial unemployment and subsequent misconduct for agencies receiving COPS grants.

4.3 Changing Job Characteristics

As noted above, the characteristics of the job may also change during recessions and starting under certain conditions may have path-dependent effects on performance. For example, the nature of policing could vary during high unemployment periods (Horrace et al., 2022). Officers hired at different points in the business cycle may need to engage in more or less ‘proactive’ policing practices. First, since crime increases during high-unemployment periods (Raphael and Winter-Ebmer, 2001; Machin and Meghir, 2004), officers starting in those periods may need to engage in aggressive practices more often, resulting in more complaints. Yet our main estimates show the opposite pattern. Moreover, our results are robust to directly controlling for current and starting year crime rates and to controlling for agency-specific trends in starting year (Figure 3).²⁵ Finally, officers entering in high-unemployment periods also receive fewer complaints across categories that are not directly related to the policing environment, such as drug-related misconduct events (Table A21).

At the same time, job training opportunities experienced by new officers may differ across the business cycle (Arellano-Bover, 2020). We test this using data on participation in job training courses, and we do not find a relationship between the initial unemployment rate and the likelihood of job training participation (Table A22). This suggests the quality differences we observe are not necessarily driven by differential learning opportunities across the cycle.

Overall, these results indicate that the labor supply channel likely plays a principal role in driving our main findings. Entry exam records allow us to describe the size and the quality of the applicant pool. We show, using exam outcomes and measures of predicted misconduct, that the applicant pool improves when unemployment rates are high. While agencies hire less during recessions, they do not change their hiring practices across the cycle, suggesting that the improved performance for officers hired in recessions is likely driven by changes in the applicant pool.

5 Conclusion

In this paper, we examine how economic conditions at career start affect the quality of police hires. Taking advantage of detailed data on employment spells over three decades in Florida, we show that officers hired during high-unemployment periods are of higher

²⁵New hires do not enter different agencies across the cycle, where we characterize agencies by the complaints, disciplines and dismissal rates for incumbent officers (Table A20).

quality than their counterparts hired in strong labor markets, as measured by the number of complaints, disciplines, and the likelihood of being fired. We further show these effects are stronger for younger officers, who face weaker outside options during recessions. Future work should explore whether these features of the police labor market hold true in other states or in other time periods, particularly time periods or places where public confidence in police may be lower than it was in Florida from 1985-2015.

We also present novel evidence of the mechanisms underlying our findings. First, the number of people taking entry exams increases in high-unemployment years and the fraction of those who pass the initial attempt increases. Since passing the exam on the first try is associated with improved officer quality, the pool of applicants improves during high unemployment periods, indicating an important role for labor supply responses. While the number of hires is procyclical in Florida, indicating a potential role for a labor demand channel, we show that even under conservative assumptions about police agencies' screening, 15-30% of the effect of initial unemployment on officer misconduct can be explained by labor demand. As such, the evidence on labor supply and on the ability of agencies to effectively screen applicants suggests agencies could improve quality by engaging in countercyclical hiring. Further work should examine how agency recruitment practices can result in improved police quality, analyzing the specific aspects that agencies can change to make the occupation more appealing relative to outside options.

In general, the size of policing in the United States and the active policy debate about police reform and police funding underscores the need to understand whether financial incentives can improve policing. We make headway on this general question by leveraging variation in economic conditions and the relative appeal of outside options. While prior work offers important evidence on other public-sector occupations, policing is distinct in several ways. First, unlike teaching or nursing, policing requires relatively little formal training. Second, typical concerns about financial incentives crowding out intrinsically motivated applicants depend on positive selection into the profession. Finally, if there are fundamental issues with police culture or police training, it is possible that improving recruitment fails to change on-the-job behavior. Our findings suggest that agencies attract higher quality applicants when policing is more appealing than outside options. As a result, better officers are hired and those improvements persist even in the face of existing police culture and training.

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Tables and Figures

Table 1: Summary Statistics

| | Full Sample | Above Median UN at Start | Below Median UN at Start |
|------------------------------------|------------------|--------------------------|--------------------------|
| Complaints | 0.070 (0.256) | 0.061 (0.240) | 0.077 (0.267) |
| Disciplinary Actions | 0.020 (0.141) | 0.018 (0.133) | 0.022 (0.147) |
| Fired | 0.094 (0.292) | 0.086 (0.281) | 0.100 (0.301) |
| Fired for Violating Agency Policy | 0.063 (0.243) | 0.057 (0.232) | 0.067 (0.250) |
| Voluntary Separation | 0.540 (0.498) | 0.539 (0.499) | 0.542 (0.498) |
| Unemployment Rate in Starting Year | 5.936 (1.332) | 7.116 (1.145) | 5.041 (0.521) |
| Observations | 103,971 | 44,833 | 59,138 |

Notes: This table produces summary statistics for the sample of full time police officers beginning an employment spell from 1985-2015. Column 1 shows statistics for all officers, column 2 limits to officers who start in a year with an above median unemployment rate (UN), and column 3 limits to officers who start in a year with a below median unemployment rate. Officers may appear more than once in the data if they have multiple employment spells. The rows are defined as follows: fraction to receive any complaint during their spell, fraction to receive any disciplinary action during the spell, fraction fired during their spell, fraction fired for violating agency policy during their spell, fraction who separate voluntarily, and the mean unemployment rate in the starting year.

Table 2: Effect of Starting Year Unemployment Rate on Officer Quality

| | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|------------------------------------|--------------------------|--------------------------|-------------------------|-------------------------|
| | (1) | (2) | (3) | (4) |
| Unemployment Rate in Starting Year | -0.00077*** (0.00027) | -0.00039*** (0.00013) | -0.00073** (0.00027) | -0.00044** (0.00017) |
| Constant | 0.0109*** (0.0015) | 0.0031*** (0.0006) | 0.0181*** (0.0025) | 0.0097*** (0.0016) |
| Observations | 468,924 | 468,924 | 468,924 | 468,924 |
| R^2 | 0.0028 | 0.0007 | 0.0043 | 0.0034 |

Notes: This table displays results estimated from equation (1). Standard errors clustered at the level of officer's starting year in parentheses. Each observation corresponds to an officer-by-year employment record. Column 1 estimates the relationship between the unemployment rate in the officer's starting year and the number of complaints on their file from years 0-5 since the starting year. Column 2 estimates the relationship between starting year unemployment rate and disciplinary actions. Column 3 estimates the relationship between starting year unemployment rate and likelihood of being fired. Column 4 estimates the relationship between starting year unemployment rate and likelihood of being fired for violating agency policy. All columns limit the sample to years 0-5 from each officer's starting year. All specifications include a control for current year unemployment rate and agency fixed effects. Table A2 reports the overall mean of each of these outcomes within the first five years of an officer's career. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Effect of Starting Year Unemployment on Officer Quality,
Alternative Specifications

Panel A: Agency-by-Year Fixed Effects, Five Year Window

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|-------------------------|--------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00034 (0.00038) | -0.00020** (0.00009) | -0.00097*** (0.00030) | -0.00069** (0.00026) |
| Constant | 0.01140*** (0.00230) | 0.00396*** (0.00051) | 0.02075*** (0.00171) | 0.01347*** (0.00156) |
| Agency-Year FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0149 | 0.0039 | 0.0139 | 0.0140 |
| Observations | 468361 | 468361 | 468361 | 468361 |

Panel B: Agency-by-Year Fixed Effects, All Years

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|--------------------------|-------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00037** (0.00017) | -0.00017*** (0.00005) | -0.00037 (0.00036) | -0.00036* (0.00020) |
| Constant | 0.01077*** (0.00098) | 0.00329*** (0.00026) | 0.01289*** (0.00200) | 0.00923*** (0.00113) |
| Agency-Year FE | Yes | Yes | Yes | Yes |
| Years Since Hire | All Years | All Years | All Years | All Years |
| R^2 | 0.0119 | 0.0057 | 0.0158 | 0.0133 |
| Observations | 941108 | 941108 | 941108 | 941108 |

Panel C: Residualized Outcomes, Five Year Window

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|------------------------|--------------------------|--------------------------|--------------------------|
| Unemployment Rate in Starting Year | -0.00034* (0.00019) | -0.00023*** (0.00006) | -0.00064*** (0.00022) | -0.00046*** (0.00015) |
| Constant | 0.00148 (0.00128) | 0.00131*** (0.00037) | 0.00765*** (0.00197) | 0.00354*** (0.00122) |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0006 | 0.0005 | 0.0011 | 0.0007 |
| Observations | 468703 | 468703 | 468703 | 468703 |

Notes: In the first two panels of this table, we present the results from a modified version of equation (1) that includes agency-by-year fixed effects in:

$$Y_{iat} = \alpha + \gamma UR_i^S + \theta_{at} + \varepsilon_{iat} \quad (3)$$

where θ_{at} captures agency-by-year fixed effects. Panel A estimates equation (3) restricting the sample to the first five years of an officer's employment spell, whereas Panel B includes all years of employment. Panel C presents the results from a modified version of equation (1), which uses residualized outcomes as the outcome variable of interest. We residualize the four outcome variables from agency-by-year characteristics in a preliminary step by estimating the following regression: $Y_{iat} = \alpha + \phi_{at} + v_{iat}$, and we then use the residualized outcome from this equation (\widehat{Y}_{iat}) as the outcome variable in equation (1). See Table 2 notes for a description of the outcome variables. Standard errors are clustered at the officer's starting year level in parentheses in all specifications. * p<0.1, ** p<0.05, *** p<0.01.

Table 4: Effect of Starting Year Unemployment Rate on Officer Quality by Age

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|--------------------------|--------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00041 (0.00028) | -0.00024*** (0.00009) | -0.00002 (0.00034) | -0.00006 (0.00017) |
| Young | 0.00939*** (0.00213) | 0.00450*** (0.00118) | 0.02040*** (0.00350) | 0.01280*** (0.00246) |
| Young \times UR Start | -0.00080** (0.00036) | -0.00035* (0.00019) | -0.00163*** (0.00052) | -0.00094** (0.00035) |
| Constant | 0.00651*** (0.00159) | 0.00108* (0.00055) | 0.00868** (0.00340) | 0.00392*** (0.00135) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0034 | 0.0012 | 0.0063 | 0.0048 |
| Observations | 468921 | 468921 | 468921 | 468921 |

Notes: This table displays results estimated from equation (1) but including a binary variable equal to one if the officer is below 35 upon starting (i.e., young) and equal to zero if not and an interaction between that variable and the starting year unemployment rate. Standard errors clustered at the level of officer's starting year in parentheses. Each observation corresponds to an officer-by-year employment record. Columns 1-4 are estimated on the same outcomes described in the notes for Table 2. All columns limit the sample to years 0-5 from each officer's starting year. All specifications include a control for current year unemployment rate and agency fixed effects. Table A2 reports the overall mean of each of these outcomes within the first five years of an officer's career. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Passing the SOCE Exam on the First Try and Subsequent Outcomes

| | Hired (1) | Complaints (2) | Disciplines (3) | Fired (4) | Fired for VAP (5) |
|-------------------------------|-------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Pass Exam on First Try | 0.12003*** (0.00743) | -0.00520*** (0.00097) | -0.00159*** (0.00036) | -0.00932*** (0.00152) | -0.00410*** (0.00075) |
| Constant | 0.49909*** (0.02086) | 0.01116*** (0.00206) | 0.00282*** (0.00066) | 0.02246*** (0.00283) | 0.01057*** (0.00153) |
| Baseline If Failed First Exam | 0.4991 | 0.0145 | 0.0039 | 0.0240 | 0.0130 |
| Current UN | — | Yes | Yes | Yes | Yes |
| Agency FE | — | Yes | Yes | Yes | Yes |
| Years Since Hire | — | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0056 | 0.0026 | 0.0008 | 0.0055 | 0.0037 |
| Observations | 62818 | 253742 | 253742 | 253742 | 253742 |

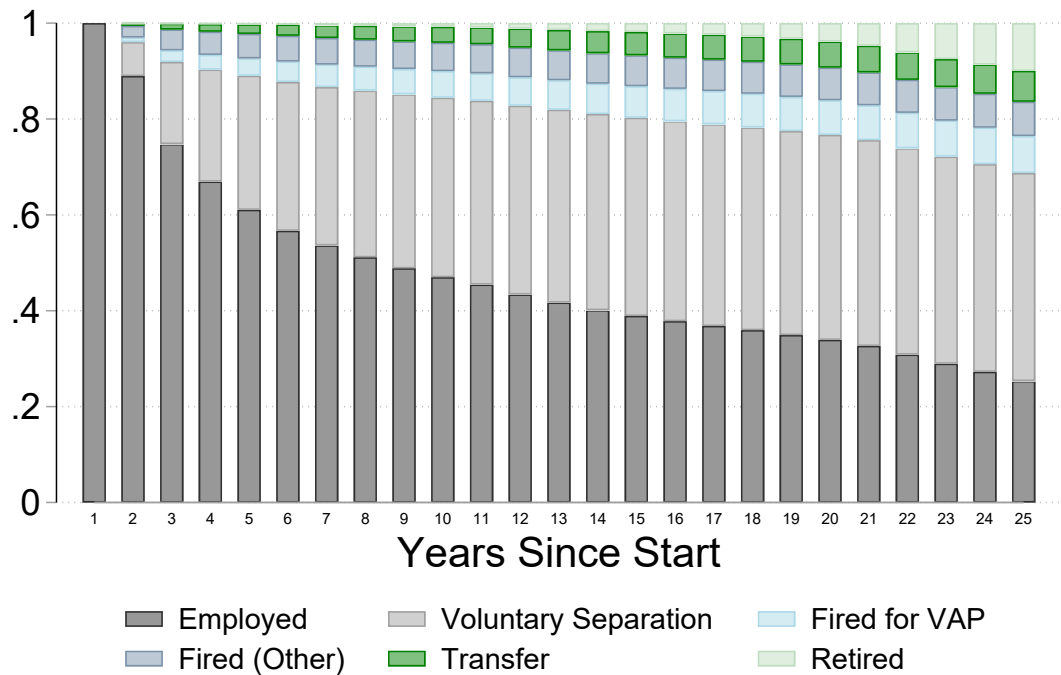
Notes: This table uses data from Florida police entry exams, which are taken after the applicant completes basic recruit training and determine eligibility for hire. Not all applicants who take the exam pass and not all who pass are hired. This table examines the relationship between passing the exam on the first try and the likelihood that officers receive complaints, disciplines, are fired or are fired for violating agency policy. We restrict the sample to officers hired in 1996-2015, the years in which we observe exam records. In the first column, we estimate a specification that relates passing on the first try to the likelihood of being hired within one year of passing. In columns 2-5, we estimate a version of equation (1) that includes whether the officer passes on the first try as the main explanatory variable. Results are robust to including the initial unemployment rate as a control. For context, we include the baseline prevalence of the five outcomes for officers who did not pass the exam on their first try. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Applicants' Characteristics and the Likelihood of Being Hired Across the Cycle

| | Hired Within One Year of Passing SOCE | | | |
|------------------------|---------------------------------------|--------------------------|--------------------------|-------------------------|
| | (1) | (2) | (3) | (4) |
| Pass Exam on First Try | 0.15311*** (0.03559) | | | |
| Age at SOCE Exam | | -0.01504*** (0.00263) | | |
| Pred. Complaints | | | -0.07126*** (0.02053) | |
| Pred. Disciplines | | | | -0.16351** (0.06446) |
| UR at Test | -0.04353*** (0.01098) | -0.07663*** (0.00740) | -0.05818*** (0.01762) | -0.03972** (0.01520) |
| Variable x UR at Test | -0.00203 (0.00573) | 0.00112*** (0.00036) | 0.00367 (0.00361) | -0.00590 (0.01134) |
| Constant | 0.70938*** (0.06707) | 1.27556*** (0.05358) | 1.09893*** (0.10102) | 0.99800*** (0.08462) |
| Observations | 52195 | 52195 | 52195 | 52195 |
| R^2 | 0.030 | 0.038 | 0.027 | 0.029 |

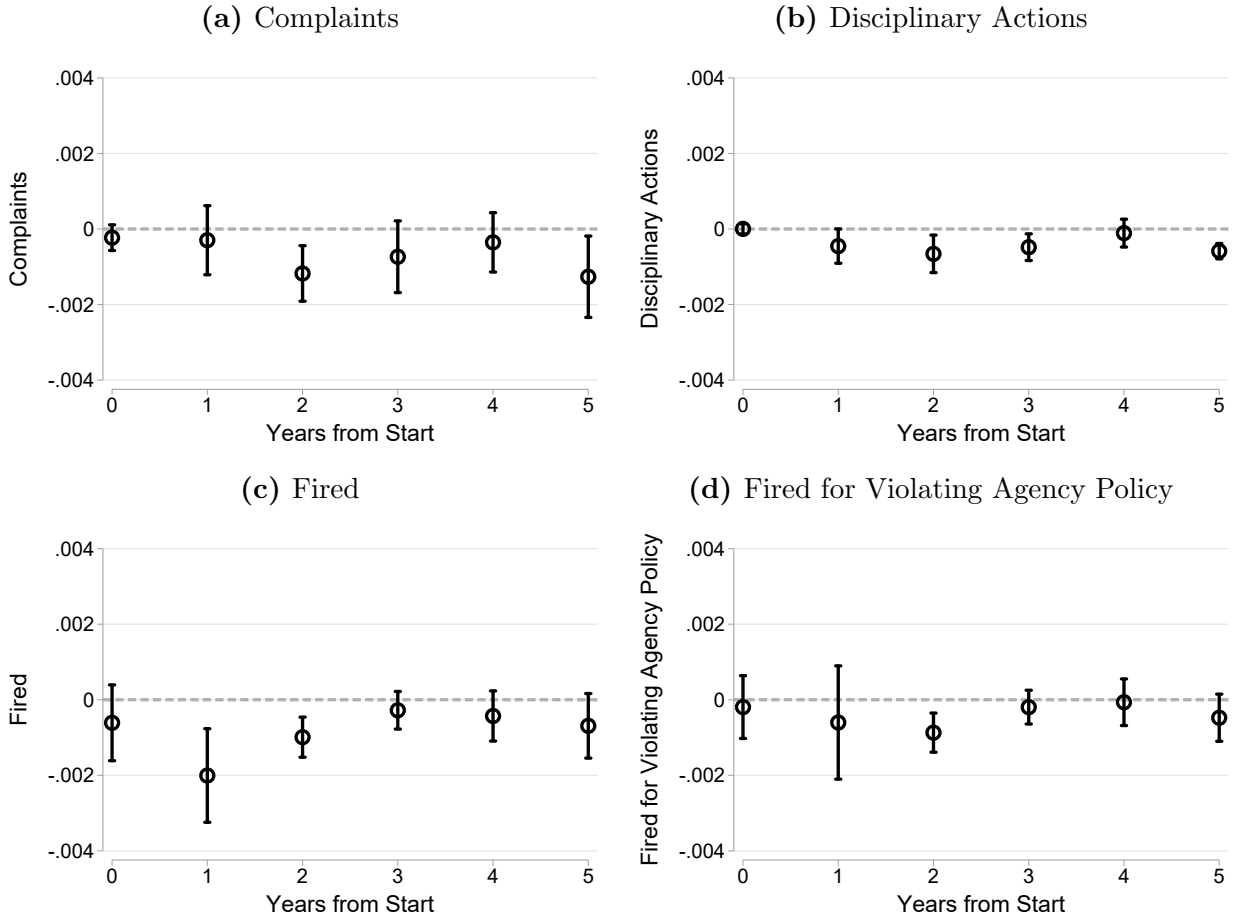
Notes: This table displays results estimated from equation (2). We assess how applicant characteristics shape the likelihood of getting hired within one year of passing the SOCE exam for the sample of candidates for whom we observe information on their police academy training. The first column considers the importance of passing the certification exam on the first try and the second column examines the role of the applicants' age at the time of the test. The last two columns consider how applicants' predicted complaints and disciplines (from the machine learning exercise described in Section 4.1.2) relate to the likelihood of being hired in policing in Florida. In all columns, we examine how these characteristics affect likelihood of hire and how this effect varies across the cycle. Standard errors are clustered at the year of the exam in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure 1: Employment Outcomes by Years since Hire



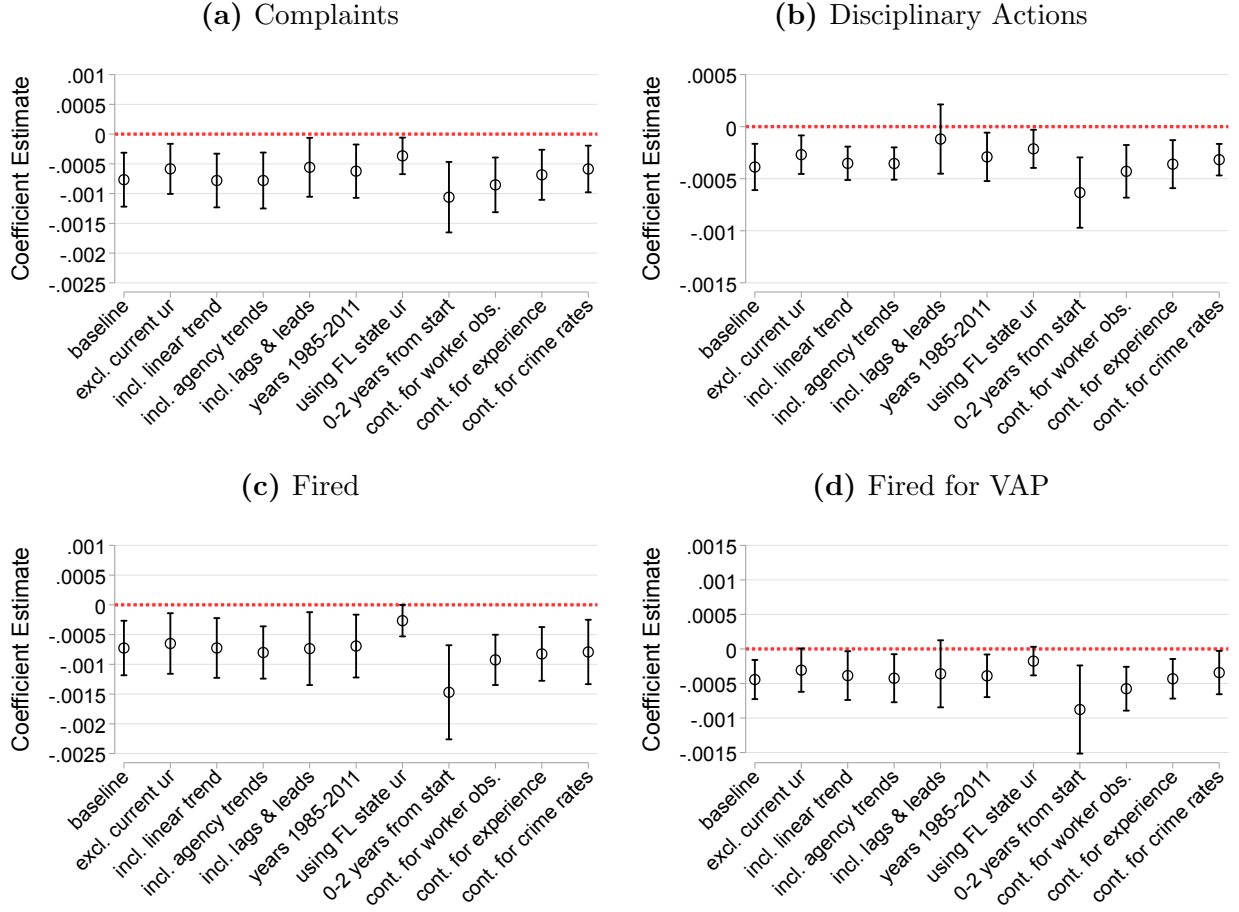
Notes: This figure presents employment outcomes for a cohort of officers younger than 35 at hire who started between 1985 and 1989. We show the share of workers who remained employed 25 years after starting, as well as the proportion who separated voluntarily, were fired for violating agency policies, fired for other reasons, transferred to other agencies or eventually retired.

Figure 2: Starting Year Unemployment Rate and Officer Quality, By Year



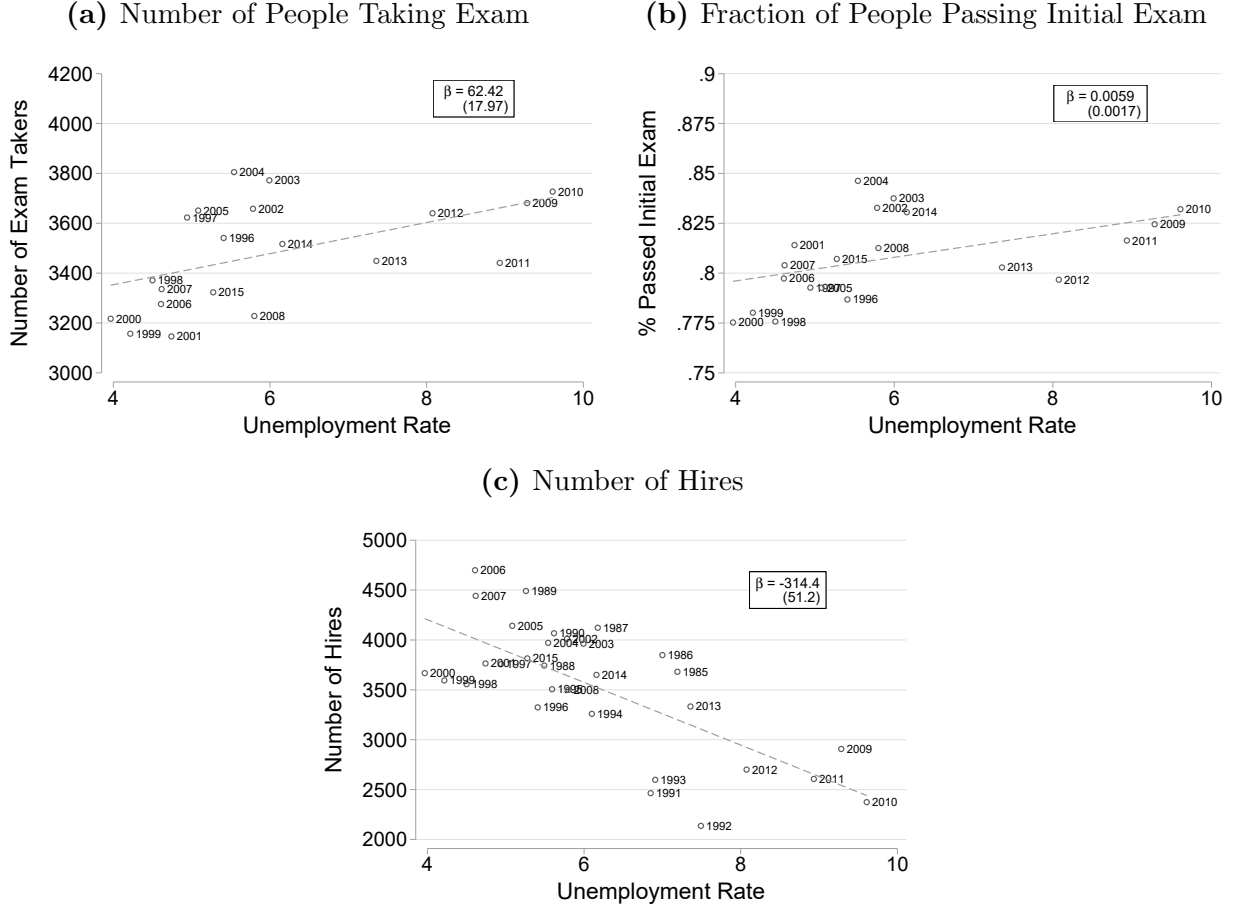
Notes: This figure displays coefficients estimated from equation (1) estimated separately for each year since start. With each coefficient, we plot the 90 percent confidence interval, based on standard errors clustered at the level of officer's starting year. Panel (a) displays estimates of the effect of starting year unemployment rate on complaints in the year the officer starts, the year after, 2 years after, 3 years after, 4 years after, and 5 years after. Panel (b) displays the same time path of estimates for disciplinary actions. Panel (c) shows estimates for the likelihood that the officer is fired and panel (d) shows estimates for the likelihood that they are fired for violating agency policy.

Figure 3: Effect of Starting Year Unemployment Rate on Officer Outcomes, Robustness Tests



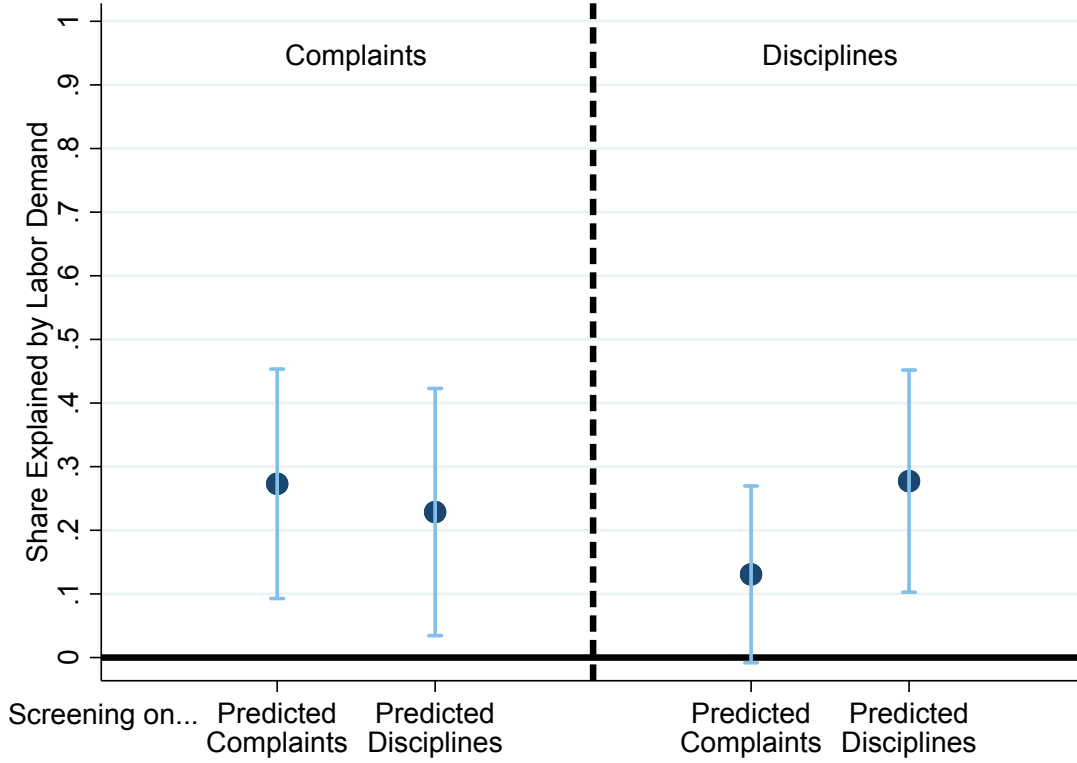
Notes: This figure shows an array of robustness tests that we conducted for our main results. In the following order, it shows: our baseline estimate from Table 2, excluding the current year unemployment rate, including a linear time trend, including agency-specific linear trends in the starting year, including leads and lags of the starting year unemployment rate, restricting to years 1985-2011, using the Florida-specific unemployment rate, restricting to the first 0-2 years of an officer's career, controlling for worker observable characteristics, controlling for their experience as officers, and controlling for national current and starting year crime rates (results are robust to using Florida crime rates). With each coefficient, we plot the 90 percent confidence interval, based on standard errors clustered at the level of officer's starting year. See Table 2 notes for a description of the outcome variables.

Figure 4: Florida Law Enforcement: Hires and Entry Exams



Notes: The first and second panels use data from Florida police entry exams, taken after the applicant completes basic recruit training. These data are available from 1996-2015. Panel (a) plots the relationship between the number of applicants who take the exam in a given year and the unemployment rate in that year. Panel (b) plots the relationship between the fraction who pass their initial attempt in a given year and the unemployment rate in that year. Panel (c) plots the number of full-time police hires per year in Florida and the unemployment rate in each year, suggesting that Florida police agencies exhibit a labor demand response to economic conditions. All panels also display the linear fit, the slope coefficient, and the standard error (clustered at the level of the officer's starting year). Table A14 presents the regression estimates for the test-score results and documents robustness to controlling for population and a linear time trend.

Figure 5: Bounding the Effect of Labor Demand on Police Misconduct Events



Notes: This figure presents results from the bounding exercise described in Section 4.2.2. First, we use a linear regression of number of hires on the unemployment rate to estimate the hiring reduction that would have occurred in each year, if the unemployment rate had been 9.6% (the highest unemployment rate in our sample period). Then, we remove that number of officers from the data in each year, making different assumptions about which officers would have been screened out. First, we assume that agencies rank applicants based on predicted complaints, screening out those with higher predicted complaints when labor demand falls. Second, we assume that agencies rank applicants based on predicted disciplines, screening out those with higher predicted disciplines when labor demand falls. After dropping the officers based on predicted misconduct, we estimate our main equation on the new sample. This yields a coefficient that captures the effect of the initial unemployment rate on our outcomes after shutting down the labor demand channel. We then use that coefficient to calculate the share of the main effect that can be explained by the labor demand channel. We present that estimate of the share explained by labor demand for complaints and disciplines, under two different assumptions about agency screening. Predicted complaints and predicted disciplines are estimated through a machine learning algorithm described in Section 4.1.2. We present 95% confidence intervals based on bootstrapped standard errors from 200 replications of this procedure.

- SUPPLEMENTARY APPENDIX -
For Online Publication

A Additional Tables and Figures

Table A1: Officer Characteristics at Hire

Panel A. Characteristics of Hired Officers in Florida

| | (1) |
|-------------|--------|
| Age | 29.126 |
| Male | 0.861 |
| White | 0.756 |
| Black | 0.100 |
| Hispanic | 0.123 |
| Sample Size | 100887 |

Panel B. Occupational Transitions of Police Officers, Current Population Survey

| | New Officers | Old Officers |
|------------------------------------|---------------|--------------|
| | Previous Occ. | Next Occ. |
| | (1) | (2) |
| Public Protective Service | 0.159 | 0.158 |
| Not Employed | 0.154 | 0.145 |
| Private Protective Service | 0.098 | 0.125 |
| Office, Administrative Support | 0.106 | 0.106 |
| Management | 0.075 | 0.078 |
| Business, Financial Operations | 0.059 | 0.069 |
| Sales and Related | 0.048 | 0.041 |
| Transportation and Material Moving | 0.037 | 0.035 |
| Community, Social Service | 0.028 | 0.030 |
| Construction and Extraction | 0.034 | 0.029 |
| Other | 0.202 | 0.184 |
| Sample Size | 8672 | 6424 |

Notes: The first panel of Table A1 presents the age, race and gender of police officers in the FDLE data when they are first hired. The second panel takes advantage of the longitudinal component of the Current Population Survey between 1980 and 2009 to identify individuals who transition into and out of policing. We define a transition into policing as an individual who started working as a ‘Police Officers and Detectives’ and had not previously worked in that occupation or as a ‘First-Line Supervisor of Police and Detectives.’ We follow the same definition for capturing transitions out of policing. In the CPS data, we identify 8,672 individuals who become police officers at some point in the survey, and 6,424 individuals who transition out of policing. The second panel exhibits the ten most common two-digit occupations from which these individuals were previously employed in (column 1) and subsequently employed in (column 2), including those who have an unemployment spell. We split protective service occupations into public and private protective service, where the latter category includes ‘Private Detectives and Investigators,’ ‘Security Guards,’ and ‘Gaming Surveillance Officers.’

Table A2: Summary Statistics

| | Within 5 Years of Hiring | All Years |
|------------------------------------|--------------------------|------------------|
| Complaints | 0.009 (0.100) | 0.009 (0.096) |
| Disciplinary Actions | 0.003 (0.053) | 0.002 (0.049) |
| Fired | 0.015 (0.122) | 0.011 (0.103) |
| Fired for Violating Agency Policy | 0.009 (0.096) | 0.007 (0.084) |
| Voluntary Separation | 0.083 (0.276) | 0.063 (0.244) |
| Unemployment Rate in Starting Year | 5.929 (1.355) | 5.828 (1.187) |
| Unemployment Rate in Current Year | 6.019 (1.471) | 6.116 (1.582) |
| Observations | 468,924 | 941,593 |

Notes: This table produces summary statistics based on each year of employment for the sample of full time police officers beginning an employment spell from 1985-2015. Column 1 limits to the first 0-5 years of an officer's employment spell while column 2 includes all years. The rows are defined as follows: number of complaints per year, number of disciplinary actions per year, likelihood of being fired in a given year, likelihood of being fired for violating agency policy in a given year, likelihood of separating voluntarily in a given year, the starting year unemployment rate for officers in the sample, and the current year unemployment rate.

Table A3: Effect of Starting Year Unemployment on Officer Decertification

| | Decertification | |
|------------------------------------|-----------------------|-----------------------|
| | (1) | (2) |
| Unemployment Rate in Starting Year | -0.0022 (0.0016) | -0.0021 (0.0015) |
| Constant | 0.0393*** (0.0094) | 0.0391*** (0.0090) |
| Agency FE | No | Yes |
| R^2 | 0.0003 | 0.0044 |
| Observations | 75209 | 75209 |

Notes: This table estimates the relationship between the unemployment rate at career start and the likelihood that an officer is decertified from policing in Florida. The second column includes agency-at-hire fixed effects. Note, we cannot estimate our typical specification for this outcome since date of decertification is missing for a large share of officers. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effect of Starting Year Unemployment on Officer Quality,
Within-Florida Unemployment Rates

Panel A: Five-Year Window

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|-------------------------|--------------------------|--------------------------|
| Unemployment Rate in Starting Year | -0.00017 (0.00018) | -0.00010* (0.00005) | -0.00048*** (0.00016) | -0.00044*** (0.00014) |
| Constant | 0.01072*** (0.00105) | 0.00315*** (0.00030) | 0.01811*** (0.00092) | 0.01158*** (0.00081) |
| Agency-Year FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0163 | 0.0050 | 0.0148 | 0.0155 |
| Observations | 382952 | 382952 | 382952 | 382952 |

Panel B: All Years

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|--------------------------|-------------------------|--------------------------|
| Unemployment Rate in Starting Year | -0.00017** (0.00007) | -0.00009*** (0.00003) | -0.00025* (0.00012) | -0.00022*** (0.00007) |
| Constant | 0.00992*** (0.00043) | 0.00270*** (0.00015) | 0.01273*** (0.00068) | 0.00851*** (0.00045) |
| Agency-Year FE | Yes | Yes | Yes | Yes |
| Years Since Hire | All Years | All Years | All Years | All Years |
| R^2 | 0.0132 | 0.0061 | 0.0173 | 0.0144 |
| Observations | 706564 | 706564 | 706564 | 706564 |

Notes: In this table, we take advantage of data on county-level unemployment rates — which we aggregate up to seven sub-state regions, including Fort Myers, Jacksonville, Miami, Orlando, Pensacola, Tallahassee and Tampa Bay — to assess the effect of local economic conditions on officer quality. We estimate the following specification leveraging within-state variation in unemployment rates at career start on officer quality:

$$Y_{iat} = \alpha + \gamma UR_{ir}^S + \theta_{at} + \varepsilon_{iat} \quad (\text{A1})$$

where UR_{ir}^S captures the unemployment rate in region r faced by officer i at career start; θ_{at} captures agency-by-year fixed effects. This analysis does not incorporate potential within-state migration responses to local economic conditions. Moreover, local unemployment rates are only available starting in 1990. Panel A estimates equation (A1) restricting the analysis to the first five years of a officer's employment spell, whereas Panel B includes all years of an officer's employment spell. See Table 2 notes for a description of the outcome variables. Standard errors are clustered at the starting year by region level in parentheses.

* p<0.1, ** p<0.05, *** p<0.01

Table A5: Agency-Level Employment Composition and Crime Related Outcomes

Panel A: Agency-Level Crime Rates

| | (Log) Crimes | | | |
|-----------------------------------|-----------------------|----------------------|----------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| Agency's Mean UR in Starting Year | -0.0662** (0.0272) | -0.0427* (0.0252) | -0.0432* (0.0254) | -0.0627** (0.0248) |
| County Unemployment | | | 0.0095 (0.0061) | 0.0145** (0.0061) |
| Year FE | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Agency Characteristics | Yes | Yes | Yes | Yes |
| Population | No | Yes | Yes | Yes |
| Local Unemployment | No | No | Yes | Yes |
| Linear Trend | No | No | No | Yes |
| R^2 | 0.9724 | 0.9740 | 0.9740 | 0.9807 |
| Observations | 7006 | 7006 | 7006 | 7006 |

Panel B: Agency-Level Crime Clearance Rates

| | Agency Crime Clearance Rates | | | |
|-----------------------------------|------------------------------|---------------------|---------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Agency's Mean UR in Starting Year | 0.0360* (0.0199) | 0.0329* (0.0181) | 0.0326* (0.0180) | 0.0368* (0.0193) |
| County Unemployment | | | 0.0064 (0.0044) | 0.0084* (0.0050) |
| Year FE | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Agency Characteristics | Yes | Yes | Yes | Yes |
| Population | No | Yes | Yes | Yes |
| Local Unemployment | No | No | Yes | Yes |
| Linear Trend | No | No | No | Yes |
| R^2 | 0.1491 | 0.1497 | 0.1498 | 0.1786 |
| Observations | 6850 | 6850 | 6850 | 6850 |

Notes: This table presents evidence on the relationship between agencies' employment composition and their crime and clearance rates. We match agency-level annual outcomes from the Unified Crime Report (UCR) in 1985-2015, covering information on the number of crimes committed in the area assigned to each agency and the number of cases cleared. We relate these outcomes to agency-level characteristics in the following specification:

$$Y_{at} = \alpha + \gamma UR_{at}^S + X_{at} + X_{ct} + \lambda_t + \theta_a + e_{at} \quad (\text{A2})$$

where Y_{at} captures one of the crime-related outcomes described above. UR_{at}^S represents the average unemployment rate at career start among all officers employed in agency a in year t . X_{at} captures agency-level characteristics, including the agencies' race, gender and age composition as well as the number of officers employed in a particular year. X_{ct} includes county-level time-varying characteristics, including county-level unemployment and its population. Panel A uses log crimes as the outcome variable of interest. Panel B uses the annual agency-level crime clearance rate as the outcome of interest, defined as the number of clearances divided by the number of crimes in that year. In each panel, the first column, controls for agency and year fixed effects, as well as for average agency-characteristics. Subsequent columns add population, local unemployment and a linear trend as controls. Standard errors are clustered at the agency level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of Starting Year Unemployment on Officer Quality,
Monthly Unemployment

Panel A. Concurrent Unemployment at Start

| | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|-------------------------------------|-------------------------|-------------------------|--------------------------|--------------------------|
| | (1) | (2) | (3) | (4) |
| Unemployment Rate in Starting Month | -0.00004** (0.00002) | -0.00002** (0.00001) | -0.00007*** (0.00002) | -0.00004*** (0.00001) |
| Constant | 0.0006*** (0.0001) | 0.0001*** (0.0000) | 0.0017*** (0.0003) | 0.0010*** (0.0001) |
| Observations | 4508353 | 4508353 | 4508353 | 4508353 |
| R^2 | 0.0003 | 0.0001 | 0.0005 | 0.0004 |

Panel B. Lagged Unemployment at Start (6- and 12-Months)

| | Complaints | Disciplinary Actions | Fired | Fired for VAP | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|----------------|-------------------------|--------------------------|-------------------------|-------------------------|--------------------------|--------------------------|-------------------------|------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Lagged UN Rate | -0.00006** (0.00002) | -0.00002*** (0.00001) | -0.00007** (0.00002) | -0.00004** (0.00001) | -0.00007*** (0.00002) | -0.00002*** (0.00001) | -0.00006** (0.00003) | -0.00003* (0.00002) |
| Constant | 0.0007*** (0.0001) | 0.0002*** (0.0000) | 0.0017*** (0.0003) | 0.0010*** (0.0001) | 0.0008*** (0.0001) | 0.0002*** (0.0000) | 0.0017*** (0.0003) | 0.0010*** (0.0001) |
| UR Start Lag | 6 Months | | | | 12 Months | | | |
| Observations | 4508353 | 4508353 | 4508353 | 4508353 | 4508353 | 4508353 | 4508353 | 4508353 |
| R^2 | 0.0003 | 0.0001 | 0.0005 | 0.0004 | 0.0003 | 0.0001 | 0.0005 | 0.0004 |

Notes: Table A6 presents results from equation (1) using monthly unemployment rates and month-level outcomes for each officer. The first panel presents estimates of the relationship between the unemployment rate in the officer's starting month and the prevalence of the four quality measures considered in the paper (see notes for Table 2). The second panel examines the relationship between lagged unemployment rates at start and month-level outcomes. The first four columns present evidence using a six-month lag in the initial unemployment rate. The last four columns present evidence using a twelve-month lag in the initial unemployment rate. Note, the coefficients in this table are much smaller than the coefficients in our main specification because the outcomes are at the monthly level. Multiplying the coefficients in this table by 12 yields estimates that are more comparable to our year-level results. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Effect of Starting Year Unemployment on Officer Quality,
Wild Cluster Bootstrap

| | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|------------------------------------|--------------------------|--------------------------|-------------------------|-------------------------|
| | (1) | (2) | (3) | (4) |
| Unemployment Rate in Starting Year | -0.00077*** (0.00027) | -0.00039*** (0.00013) | -0.00073** (0.00027) | -0.00044** (0.00017) |
| Constant | 0.0109*** (0.0015) | 0.0031*** (0.0006) | 0.0181*** (0.0025) | 0.0097*** (0.0016) |
| Wild Cluster Bootstrap 95% CI | [-0.0013,-0.0002] | [-0.0007,-0.0001] | [-0.0013,-0.0002] | [-0.0008,-0.0001] |
| Wild Cluster Bootstrap P-Value | 0.015 | 0.004 | 0.004 | 0.007 |
| Observations | 468,924 | 468,924 | 468,924 | 468,924 |
| R^2 | 0.0028 | 0.0007 | 0.0043 | 0.0034 |

Notes: See notes for Table 2. This table reproduces those main results, but adds the 95 percent confidence interval and the p-value derived from a wild cluster bootstrap procedure. * p<0.1, ** p<0.05, *** p<0.01

Table A8: Effect of Starting Year Unemployment on Officer Quality by Age

Panel A: Linear Age Interaction

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|--------------------------|--------------------------|--------------------------|--------------------------|
| Unemployment Rate in Starting Year | -0.00229*** (0.00082) | -0.00117** (0.00047) | -0.00413*** (0.00099) | -0.00233*** (0.00073) |
| Age at Start | -0.00053*** (0.00011) | -0.00027*** (0.00006) | -0.00123*** (0.00016) | -0.00075*** (0.00011) |
| Age \times UR Start | 0.00004** (0.00002) | 0.00002** (0.00001) | 0.00009*** (0.00002) | 0.00005*** (0.00002) |
| Constant | 0.03036*** (0.00491) | 0.01299*** (0.00267) | 0.06345*** (0.00609) | 0.03753*** (0.00545) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0035 | 0.0013 | 0.0071 | 0.0053 |
| Observations | 467447 | 467447 | 467447 | 467447 |

Panel B: Young (Age 30) Interaction

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|--------------------------|--------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00064** (0.00025) | -0.00033*** (0.00009) | -0.00040 (0.00025) | -0.00031** (0.00014) |
| Young | 0.00888*** (0.00263) | 0.00489*** (0.00153) | 0.02514*** (0.00381) | 0.01490*** (0.00308) |
| Young \times UR Start | -0.00072* (0.00042) | -0.00038 (0.00023) | -0.00197*** (0.00056) | -0.00105** (0.00043) |
| Constant | 0.00880*** (0.00153) | 0.00201*** (0.00048) | 0.01224*** (0.00267) | 0.00640*** (0.00126) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0033 | 0.0012 | 0.0068 | 0.0051 |
| Observations | 468921 | 468921 | 468921 | 468921 |

Notes: See notes for Table 4. In Panel A, instead of using a binary term to capture officer age, the results in this table are based on a model that uses a linear term in age. In Panel B, we include a binary variable that equals one if the officer is below 30 upon starting (i.e. young) and equal to zero if not, and an interaction between that variable and the unemployment rate. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Effect of Starting Year Unemployment on Officer Quality by Race and Gender

| Panel A: Heterogeneity by Race | | | | |
|------------------------------------|--------------------------|-------------------------|--------------------------|-------------------------|
| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
| Unemployment Rate in Starting Year | -0.00071** (0.00032) | -0.00040 (0.00025) | -0.00080** (0.00033) | -0.00017 (0.00030) |
| White | -0.00375*** (0.00128) | -0.00182 (0.00125) | -0.00923*** (0.00209) | -0.00159 (0.00192) |
| White \times UR Start | -0.00008 (0.00019) | 0.00002 (0.00019) | 0.00009 (0.00034) | -0.00038 (0.00031) |
| Constant | 0.01387*** (0.00212) | 0.00456*** (0.00130) | 0.02530*** (0.00268) | 0.01110*** (0.00252) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0031 | 0.0008 | 0.0051 | 0.0037 |
| Observations | 468921 | 468921 | 468921 | 468921 |

| Panel B: Heterogeneity by Gender | | | | |
|------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
| Unemployment Rate in Starting Year | -0.00068** (0.00027) | -0.00019 (0.00011) | -0.00079* (0.00046) | -0.00026 (0.00031) |
| Male | 0.00385* (0.00195) | 0.00309*** (0.00080) | -0.00150 (0.00306) | 0.00297 (0.00217) |
| Male \times UR Start | -0.00011 (0.00033) | -0.00023* (0.00013) | 0.00008 (0.00044) | -0.00022 (0.00034) |
| Constant | 0.00761*** (0.00180) | 0.00050 (0.00067) | 0.01935*** (0.00324) | 0.00719*** (0.00208) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0029 | 0.0008 | 0.0043 | 0.0034 |
| Observations | 468921 | 468921 | 468921 | 468921 |

Notes: See notes for Table 4. Instead of exploring heterogeneity in effects by officer age, this table explores heterogeneity by other officer demographics. Panel A tests for heterogeneity in effect size by race and Panel B tests for heterogeneity in effect size by gender. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A10: Effect of Starting Year Unemployment Rate on Officer Quality
by Agency Unionization Status

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00070** (0.00029) | -0.00036** (0.00017) | -0.00077* (0.00038) | -0.00054* (0.00032) |
| Agency Has Union \times UR Start | -0.00014 (0.00029) | -0.00003 (0.00017) | 0.00015 (0.00035) | 0.00016 (0.00028) |
| Constant | 0.01199*** (0.00167) | 0.00339*** (0.00067) | 0.01804*** (0.00240) | 0.01000*** (0.00176) |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0023 | 0.0005 | 0.0038 | 0.0030 |
| Observations | 394385 | 394385 | 394385 | 394385 |

Notes: This table displays results estimated from equation (1) but including a binary variable equal to one if the officer joined an agency that was unionized by the time of entry and equal to zero if not, along with an interaction between that variable and the starting year unemployment rate. Standard errors clustered at the level of officer's starting year in parentheses. Each observation corresponds to an officer-by-year employment record. Columns 1-4 are estimated on the same outcomes described in the notes for Table 2. All columns limit the sample to years 0-5 from each officer's starting year. All specifications include a control for current year unemployment rate and agency fixed effects. Table A2 reports the overall mean of each of these outcomes within the first five years of an officer's career. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A11: Effect of Starting Year Unemployment Rate on Hired Officer Demographics

| | White (1) | Black (2) | Hispanic (3) | Male (4) | Age (5) | White (6) | Black (7) | Hispanic (8) | Male (9) | Age (10) |
|------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|--------------------------|-------------------------|-------------------------|-------------------------|-------------------------|--------------------------|
| Unemployment Rate in Starting Year | 0.00065 (0.00642) | -0.00062 (0.00126) | -0.00067 (0.00473) | 0.00020 (0.00124) | -0.17782 (0.10704) | -0.00232 (0.00606) | -0.00006 (0.00109) | 0.00174 (0.00461) | -0.00016 (0.00126) | -0.20931* (0.12114) |
| Constant | 0.73779*** (0.03814) | 0.10717*** (0.00733) | 0.13901*** (0.02794) | 0.85535*** (0.00767) | 36.11348*** (0.67098) | 0.75538*** (0.03640) | 0.10384*** (0.00624) | 0.12473*** (0.02727) | 0.85746*** (0.00783) | 36.30045*** (0.75175) |
| Agency FE | No | No | No | No | No | Yes | Yes | Yes | Yes | Yes |
| R^2 | 0.0000 | 0.0000 | 0.0000 | 0.0000 | 0.0006 | 0.1975 | 0.0694 | 0.1880 | 0.0122 | 0.0900 |
| Observations | 103971 | 103971 | 103971 | 103971 | 103664 | 103971 | 103971 | 103971 | 103971 | 103664 |

Notes: This table estimates the relationship between unemployment rate in a given year and the demographic characteristics of officers hired in that year. Columns 1-3 and 6-8 test whether economic conditions influence the race or ethnicity of new hires. Columns 4 and 9 test whether economic conditions affect the sex of new hires. Finally, columns 5 and 10 test whether economic conditions impact the age of new hires. Columns 1-5 exclude agency fixed effects and columns 6-10 include them. Standard errors are clustered at the year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A12: Unemployment Rate and Starting Salaries in Policing

| | Log Starting Wages | | |
|---------------------------|------------------------|------------------------|------------------------|
| | (1) | (2) | (3) |
| County-Level Unemployment | 0.0036 (0.0040) | 0.0064 (0.0054) | 0.0008 (0.0048) |
| Constant | 10.7041*** (0.0241) | 10.7281*** (0.0323) | 10.6818*** (0.0296) |
| Agencies | All | Police Dept. | Sheriff's Office |
| Year FE | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes |
| R^2 | 0.8787 | 0.8202 | 0.9461 |
| Observations | 2741 | 2155 | 586 |

Notes: we use Florida Criminal Justice Agency Profile (CJAP) data in 2011-2019 to examine how posted entry salaries in police agencies and sheriff's offices vary across the business cycle. We estimate the following specification: $W_{at}^S = \alpha + \beta UR_{ct} + \lambda_t + \theta_a + v_{at}$, where W_{at}^S denotes either a police agency or sheriff's office posted entry salary in a given year over the period 2011-2019. We cluster standard errors at the county-year level, presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A13: Business Cycle and Industry-Level Employment

| | Private (1) | Construction (2) | Manufacturing (3) | Retail Trade (4) | Transportation (5) | Professional (6) | Local Govt. (7) |
|-------------------|----------------------|----------------------|----------------------|----------------------|-----------------------|----------------------|---------------------|
| Unemployment Rate | -0.021*** (0.003) | -0.053*** (0.008) | -0.039*** (0.003) | -0.017*** (0.003) | -0.030*** (0.003) | -0.028*** (0.007) | -0.001 (0.004) |
| Constant | 11.344*** (0.021) | 8.718*** (0.054) | 10.286*** (0.025) | 9.463*** (0.021) | 8.077*** (0.024) | 9.062*** (0.049) | 8.340*** (0.029) |
| R^2 | 0.959 | 0.757 | 0.968 | 0.873 | 0.975 | 0.948 | 0.890 |
| Observations | 35 | 35 | 35 | 35 | 35 | 35 | 35 |

Notes: In this table, we use Bureau of Labor Statistics data on industry-level annual employment outcomes to assess the cyclicity of employment across different sectors in the following specification: $E_{st} = \alpha + \gamma UR_t + t + \varepsilon_{st}$ where E_t represents the natural logarithm of total employment in industry s in year t , UR_t denotes the national unemployment rate in year t and t represents a linear time trend. We focus on the years 1985-2019 to match the time period considered in the paper. The outcome variable in the first column encompasses total private sector non-farm employment. The second through sixth columns consider employment in the construction, manufacturing, retail trade, transportation and professional and business services sectors, respectively. The last column focuses on local government employment excluding education, which serves as a proxy for police employment since disaggregated data on total police employment is not directly available. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Unemployment Rate and Test-Taking Outcomes

| | Number of Test-Takers (1) | Share of Passed Exams (2) | Number of Passed Exams (3) |
|-------------------|------------------------------|------------------------------|-------------------------------|
| Unemployment Rate | 92.2971*** (20.7841) | 0.0059*** (0.0020) | 95.3134*** (22.2012) |
| Constant | 635.8820 (2092.6862) | 0.3320 (0.2117) | -1169.0557 (2115.6511) |
| FL Population | Yes | Yes | Yes |
| Linear Time Trend | Yes | Yes | Yes |
| R^2 | 0.2493 | 0.3062 | 0.3041 |
| Observations | 20 | 20 | 20 |

Notes: This table uses data from Florida police entry exams, taken after the applicant completes basic recruit training. These data are available from 1996-2015. The first column examines the number of annual test-takers as the outcome of interest. The second column presents evidence on the share of passed exams. The last column analyzed the total number of passed exams in each year. All specifications include the Florida population as a control variable and a linear time trend as a control. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A15: Machine Learning Predictors for Complaints and Disciplines

| | Applicant Sample | | Officer Sample | |
|--|-------------------|--------------------|-------------------|--------------------|
| | Complaints (1) | Disciplines (2) | Complaints (3) | Disciplines (4) |
| Pass Exam on First Try | Yes | Yes | Yes | Yes |
| Age | | | | |
| 18 to 19 | Yes | | | |
| 25 to 29 | Yes | Yes | | Yes |
| 35 to 39 | Yes | | | |
| 40 to 44 | Yes | | Yes | Yes |
| 45 to 49 | Yes | Yes | Yes | Yes |
| 50 to 54 | Yes | Yes | Yes | |
| Police Academy | | | | |
| Citrus County Public Safety Training Center | Yes | Yes | Yes | Yes |
| Criminal Justice Academy Of Osceola | Yes | Yes | | |
| Daytona State College, School Of Emergency Services | Yes | | Yes | |
| Eastern Florida State College, Public Safety Institute | Yes | | Yes | |
| Florida Highway Patrol Training Academy | Yes | Yes | | |
| Florida Keys Community College | Yes | | Yes | Yes |
| Gulf Coast State College | Yes | | | Yes |
| Indian River State College | Yes | | | Yes |
| Lake Technical College Criminal Justice Academy | Yes | | Yes | |
| Manatee Technical College | Yes | Yes | Yes | |
| Miami Police Training Center | Yes | | Yes | Yes |
| North Florida Community College Public Safety Academy | Yes | Yes | | |
| Pinellas Criminal Justice Institute | Yes | Yes | | |
| Polk State College-K. C. Thompson Institute Of Public Safety | Yes | | | Yes |
| Seminole State College Center For Public Safety | Yes | | | |
| South Florida State College Criminal Justice Academy | Yes | | Yes | Yes |
| St. Petersburg College Southeastern Public Safety Institute | Yes | Yes | | |
| Tampa Police Academy | Yes | Yes | | |
| Florida Panhandle Technical College Public Safety Institute | | Yes | Yes | |
| Hillsborough Community College | | Yes | Yes | |
| Miami-Dade College | | | Yes | Yes |
| Northeast Florida Criminal Justice Center | | | Yes | Yes |
| Northwest Florida State College | | | Yes | |
| Palm Beach State College, Criminal Justice Training Center | | | Yes | |
| Southwest Florida Public Service Academy | | | Yes | |
| Valencia College, Criminal Justice Institute | | | Yes | Yes |
| Chipola College | | | | Yes |
| George Stone Area Vo-Tech Center | | | | Yes |
| Santa Fe College Institute Of Public Safety | | | | Yes |
| Observations | 36237 | 36237 | 48724 | 48724 |
| AUC | 0.5657 | 0.5816 | 0.5716 | 0.5936 |

Notes: This table presents the selected predictors from the machine learning exercise described in Section 4.1.2. The first two columns present the selected predictors for the analysis using the sample of police officer applicants for their predicted complaints and disciplines, respectively, from the set of available predictors that include passing on the first try, nine categories of age at the SOCE exam, and their police academy agency. The last two columns present corresponding evidence for the machine learning exercise for hired officers, using the same set of candidate predictors (using age at hire instead of age at the SOCE exam). The last row includes the ‘area under the curve’ (AUC) for each estimated model, where the AUC assesses the model’s accuracy in predicting misconduct events.

Table A16: Relationship between Unemployment Rate and Screening Practices

| | Education (1) | Interview (2) | Polygraph (3) | Psych. Exam (4) | Fitness (5) | Agility Test (6) | CJ Experience (7) | Swimming (8) | Driving (9) |
|-----------------|------------------------|-----------------------|-----------------------|-----------------------|-----------------------|-----------------------|----------------------|-----------------------|-----------------------|
| County-Level UR | 0.0209 (0.0200) | -0.0002 (0.0085) | -0.0093 (0.0096) | 0.0101 (0.0107) | -0.0175 (0.0166) | 0.0040 (0.0147) | -0.0018 (0.0083) | -0.0002 (0.0054) | -0.0018 (0.0129) |
| Constant | 12.1287*** (0.1254) | 0.9377*** (0.0528) | 0.5871*** (0.0590) | 0.7414*** (0.0669) | 0.6837*** (0.1044) | 0.5453*** (0.0924) | 0.0850* (0.0513) | 0.0948*** (0.0341) | 0.8648*** (0.0807) |
| Agencies | All | All | All | All | All | All | All | All | All |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| R^2 | 0.7019 | 0.4921 | 0.8577 | 0.7849 | 0.6106 | 0.7352 | 0.6097 | 0.8486 | 0.5206 |
| Observations | 2840 | 2842 | 2842 | 2841 | 2842 | 2841 | 2841 | 2842 | 2842 |

Notes: We use Florida Criminal Justice Agency Profile (CJAP) data in 2011-2019 to examine how screening practices in police agencies and sheriff's offices in Florida vary across the business cycle. We estimate the following specification: $Screening_{at} = \alpha + \beta UR_{ct} + \lambda_t + \theta_a + v_{at}$, where $Screening_{at}$ denotes a screening practice adopted by police agencies and sheriff's offices in Florida in a given year over the period 2011-2019. The table presents evidence, in order across columns, for whether agencies are more likely to require that applicants have a minimum number of years of completed schooling, to require them to do an interview in front of a board, to take a polygraph exam, to undergo a psychological examination, to complete a fitness program, to complete a physical fitness agility test, to have previous experience in criminal justice, to take a swimming test, and to share their driving history prior to being hired. Across all columns, we fail to find evidence that the prevalence of screening requirements changes across the cycle. We cluster standard errors at the county-year level, presented in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A17: Applicants' Characteristics and the Likelihood of Being Hired Across the Cycle

| | Hired As a Police Officer in Florida | | | |
|------------------------|--------------------------------------|--------------------------|--------------------------|-------------------------|
| | (1) | (2) | (3) | (4) |
| Pass Exam on First Try | 0.12370*** (0.03478) | | | |
| Age at SOCE Exam | | -0.01467*** (0.00332) | | |
| Pred. Complaints | | | -0.02743** (0.01297) | |
| Pred. Disciplines | | | | -0.03558 (0.05542) |
| UR at Test | -0.02804*** (0.00778) | -0.05334*** (0.01069) | -0.03255*** (0.01041) | -0.01603 (0.01227) |
| Variable x UR at Test | -0.00526 (0.00585) | 0.00073 (0.00046) | -0.00006 (0.00211) | -0.01827* (0.00968) |
| Constant | 0.78121*** (0.05095) | 1.31283*** (0.08581) | 0.98970*** (0.06817) | 0.92646*** (0.07266) |
| Observations | 52195 | 52195 | 52195 | 52195 |
| R^2 | 0.018 | 0.039 | 0.016 | 0.017 |

Notes: This table displays results estimated from equation (2). We assess how applicant characteristics shape the differential likelihood of ever getting hired in policing in Florida across the cycle for a sample of candidates for whom we observe information on their police academy training outcomes. The first column considers the importance of passing the SOCE exam on the first try; the second column examines the role of the applicants' age at the time of the test. The last two columns consider how applicants' predicted complaints and disciplines (from the machine learning exercise described in Section 4.1.2) relate to the likelihood of being hired in policing in Florida. Clustered standard errors at the year of exam presented in parentheses. * p<0.1, ** p<0.05, *** p<0.01

Table A18: Effect of Starting Year Unemployment Rate on Officer Quality,
Heterogeneity by COPS Grant Receipt

| | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|---|--------------------------|--------------------------|-------------------------|-------------------------|
| | (1) | (2) | (3) | (4) |
| Unemployment Rate in Starting Year | -0.00080*** (0.00028) | -0.00040*** (0.00014) | -0.00072** (0.00028) | -0.00043** (0.00017) |
| UR x Hiring Grants in Starting Year | 0.00000 (0.00001) | 0.00000 (0.00000) | 0.00000 (0.00001) | -0.00000 (0.00000) |
| UR x Non-hiring Grants in Starting Year | 0.00005 (0.00004) | 0.00002 (0.00003) | 0.00001 (0.00004) | -0.00002 (0.00005) |
| Constant | 0.0111*** (0.0016) | 0.0032*** (0.0006) | 0.0181*** (0.0026) | 0.0096*** (0.0016) |
| Observations | 468,924 | 468,924 | 468,924 | 468,924 |
| R^2 | 0.0028 | 0.0006 | 0.0043 | 0.0034 |

Notes: This table displays results estimated from equation 1, with an interaction term for the amount of funding the agency receives in hiring and in non-hiring grants in each year. In particular, we use information on agencies' receipt of hiring grants from the Community Oriented Policing Services (COPS) program to analyze if our main results change in agencies that receive these grants. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A19: Effect of Starting Year Unemployment Rate on Officer Quality,
Department of Highway Patrol

| | Complaints | Disciplinary Actions | Fired | Fired for VAP |
|------------------------------------|-----------------------|--------------------------|-----------------------|-----------------------|
| | (1) | (2) | (3) | (4) |
| Unemployment Rate in Starting Year | -0.00027 (0.00137) | -0.00072*** (0.00023) | -0.00099 (0.00083) | -0.00042 (0.00033) |
| Constant | -0.0069 (0.0061) | 0.0014 (0.0015) | 0.0159 (0.0095) | 0.0003 (0.0030) |
| Observations | 16,044 | 16,044 | 16,044 | 16,044 |
| R^2 | 0.0016 | 0.0007 | 0.0000 | 0.0005 |

Notes: See Table 2 notes for a description of the outcome variables. This table estimates equation (1) restricting the sample to officers employed in the Florida Department of Highway Safety and Motor Vehicles, which is the second largest agency in Florida. We limit to this agency because as a state-wide agency its hiring is not as responsive to business cycle conditions (Figure A12). We still find that officers hired in high unemployment years are significantly less likely to face disciplinary actions. Our estimate on the likelihood of being fired is similar in magnitude to the main results, but it is noisier. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A20: Effect of Starting Year Unemployment on Agency Characteristics

| | Complaints (1) | Disciplines (2) | Fired (3) | Fired for VAP (4) |
|------------------------------------|-------------------------|-------------------------|-------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00007 (0.00005) | -0.00001 (0.00001) | -0.00003 (0.00009) | -0.00003 (0.00005) |
| Constant | 0.00152*** (0.00030) | 0.00049*** (0.00008) | 0.00253*** (0.00052) | 0.00192*** (0.00029) |
| R^2 | 0.0002 | 0.0000 | 0.0000 | 0.0000 |
| Observations | 106134 | 106134 | 106134 | 106134 |

Notes: This table displays the relationship between new officers' unemployment rate at hire and the average characteristics of the agency they entered, where agencies are represented by their (residualized) incumbent officers' complaints, disciplines, proportion fired and proportion fired for violating agency policy across the four columns. The residualized characteristics are computed as follows. First, for each agency-year in our data, we first compute the average values of each of these characteristics only for incumbent officers. We then residualize these averages by regressing each characteristic against a sub-state region fixed effect and incumbent-officer-level characteristics, including their age, race and gender. We then relate the residualized characteristics to the national-level unemployment rate for new officers. Standard errors are clustered at the level of the officer's starting year in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A21: Effect of Starting Year Unemployment on Officer Complaint Types

| | All Complaints (1) | Violent (2) | Drug-Related (3) | Other (4) |
|------------------------------------|--------------------------|--------------------------|-------------------------|-------------------------|
| Unemployment Rate in Starting Year | -0.00077*** (0.00027) | -0.00026*** (0.00008) | -0.00004** (0.00002) | -0.00045** (0.00021) |
| Constant | 0.01089*** (0.00154) | 0.00344*** (0.00057) | 0.00056*** (0.00014) | 0.00689*** (0.00114) |
| Baseline Prevalence | 0.0094 | 0.0025 | 0.0004 | 0.0065 |
| Current UN | Yes | Yes | Yes | Yes |
| Agency FE | Yes | Yes | Yes | Yes |
| Years Since Hire | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years | ≤ 5 Years |
| R^2 | 0.0028 | 0.0009 | 0.0001 | 0.0023 |
| Observations | 468921 | 468921 | 468921 | 468921 |

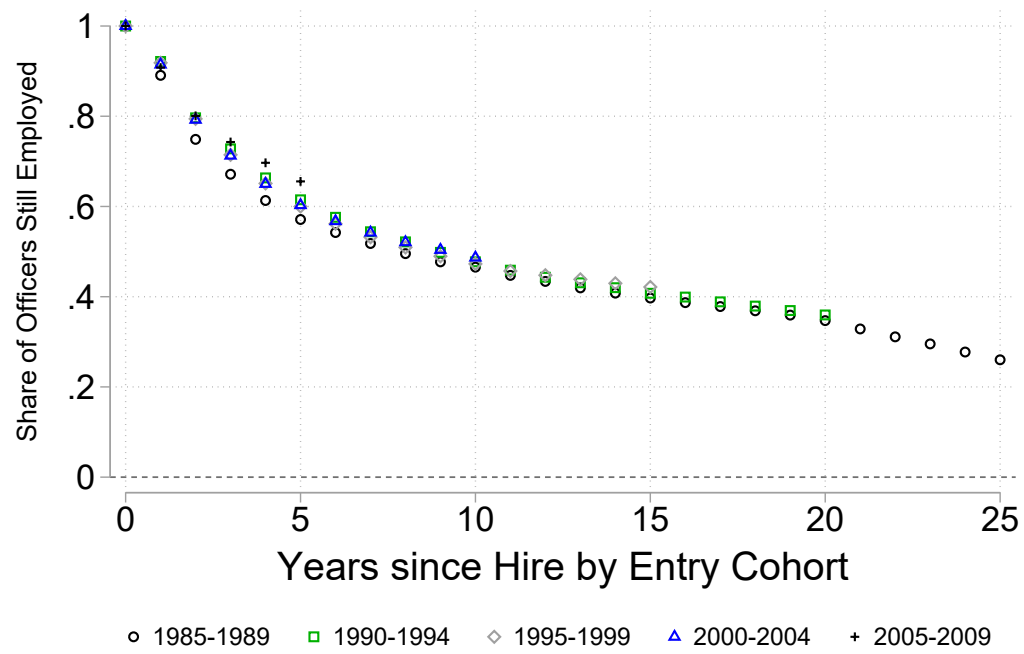
Notes: This table estimates the equation (1) using different complaint types as the outcome variable. The first column replicates the result presented in the first column of Table 2. The second column analyzes the prevalence of violent complaints, which are due to arson, assault, homicide, kidnapping, robbery and/or sex offenses. The third column examines drug-related complaints, which include those due to selling, possessing, smuggling, trafficking, or distributing drugs such as hallucinogens, heroin, cocaine, and marijuana, among others. Lastly, the 'Other' complaints comprise misconduct events due to bribery, burglary, commercialized sexual offenses, damage property, family offenses, forgery, fraud, invasion of privacy, larceny, obscenity, obstructing the judiciary, Congress, legislature or the police, violating public peace, sexual assault, smuggling, stealing property, traffic offenses, and weapon offenses. Standard errors are clustered at the officer's starting year level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A22: Effect of Starting Year Unemployment on Job Training Participation

| | Training Courses | | Hours Trained | |
|-------------------|-----------------------|---------------------|------------------------|---------------------|
| | Advanced | Specialized | Advanced | Specialized |
| | (1) | (2) | (3) | (4) |
| Unemployment Rate | -0.0012 (0.0031) | -0.0023 (0.0026) | -0.0845 (0.1436) | -0.0062 (0.0497) |
| Constant | 0.3682*** (0.0366) | 0.0295 (0.0232) | 16.5538*** (1.6536) | 0.2983 (0.3755) |
| Observations | 468921 | 468921 | 468921 | 468921 |

Notes: This table displays results estimated from equation (1) on officers' job training participation. The first two columns present the number of courses that officers participated in and passed in each year. The last two columns show the number of hours of job training participation in each year, where the number of hours are given by the minimum number of hours required for each course. Advanced courses include 117 different courses, including courses in radar speed measurement, interviews and interrogations, narcotics and dangerous drugs investigation, line supervision, stress management techniques, and advanced report writing and review, among others. Specialized courses include breath test operator courses, general instructor refresher course, laser speed measuring device courses and defensive tactics and firearms instruction, among others. Standard errors clustered at the level of officer's starting year in parentheses. Each observation corresponds to an officer-by-year employment record. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

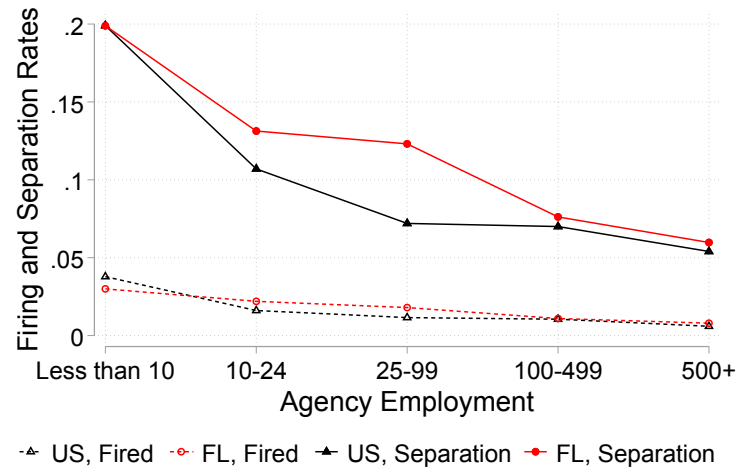
Figure A1: Proportion Employed by Years since Hire



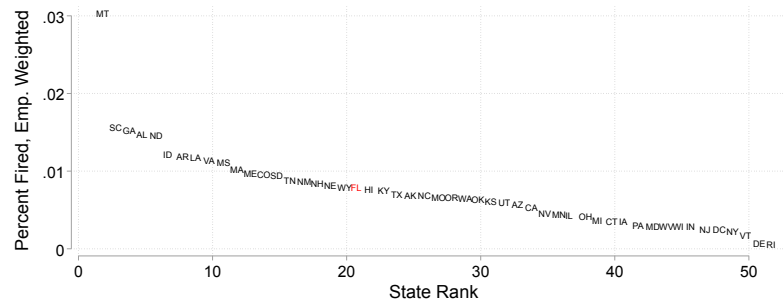
Notes: This figure shows the share of officers who remained employed in each year since their initial hire. We present evidence for five different cohorts of entrants, encompassing those who entered in 1985-1989, 1990-1994, 1995-1999, 2000-2004 and 2005-2009.

Figure A2: Labor Market Dynamics for Police Officers, Florida and United States

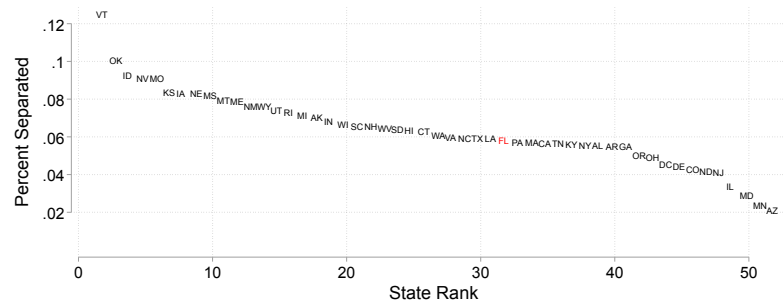
(a) Firing and Separation Rates by Agency Size, BJS 2008



(b) Firing Rates by State, LEMAS 2003



(c) Separation Rates by State, CPS 1985-2015



Notes: The first panel compares firing and separation rates by agency size in Florida to a nationwide sample from a 2008 report by the Bureau of Justice Statistics. The second panel compares firing and separation rates in Florida to all other states using a nationwide sample from the 2003 LEMAS. The last panel compares separation rates in Florida to all other states using information from the Current Population Survey across 1985-2015.

Figure A3: Starting Year Unemployment Rate and Officer Quality, Residualized

(a) Line Plot of Comovement

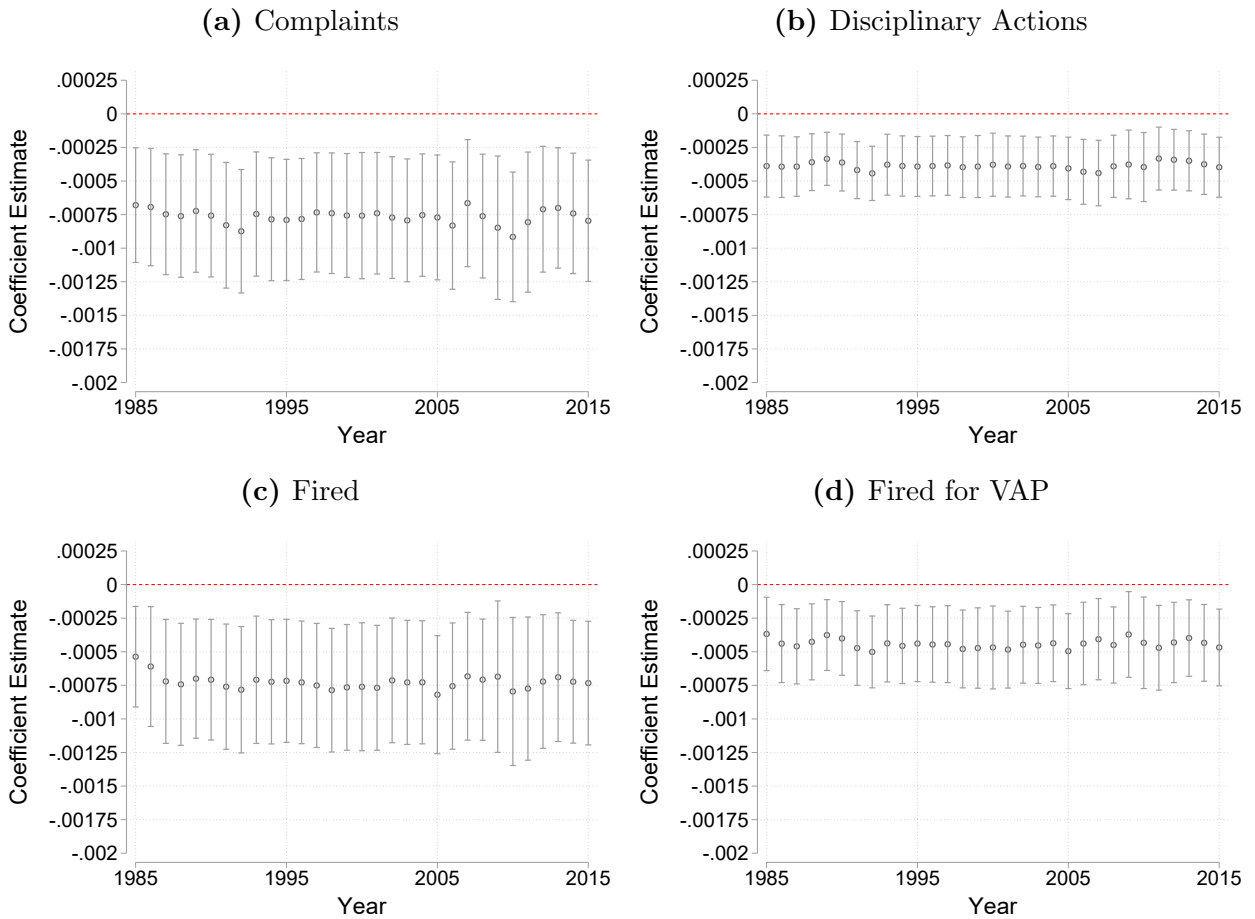


(b) Linear Fit of Starting Year UN and Officer Quality



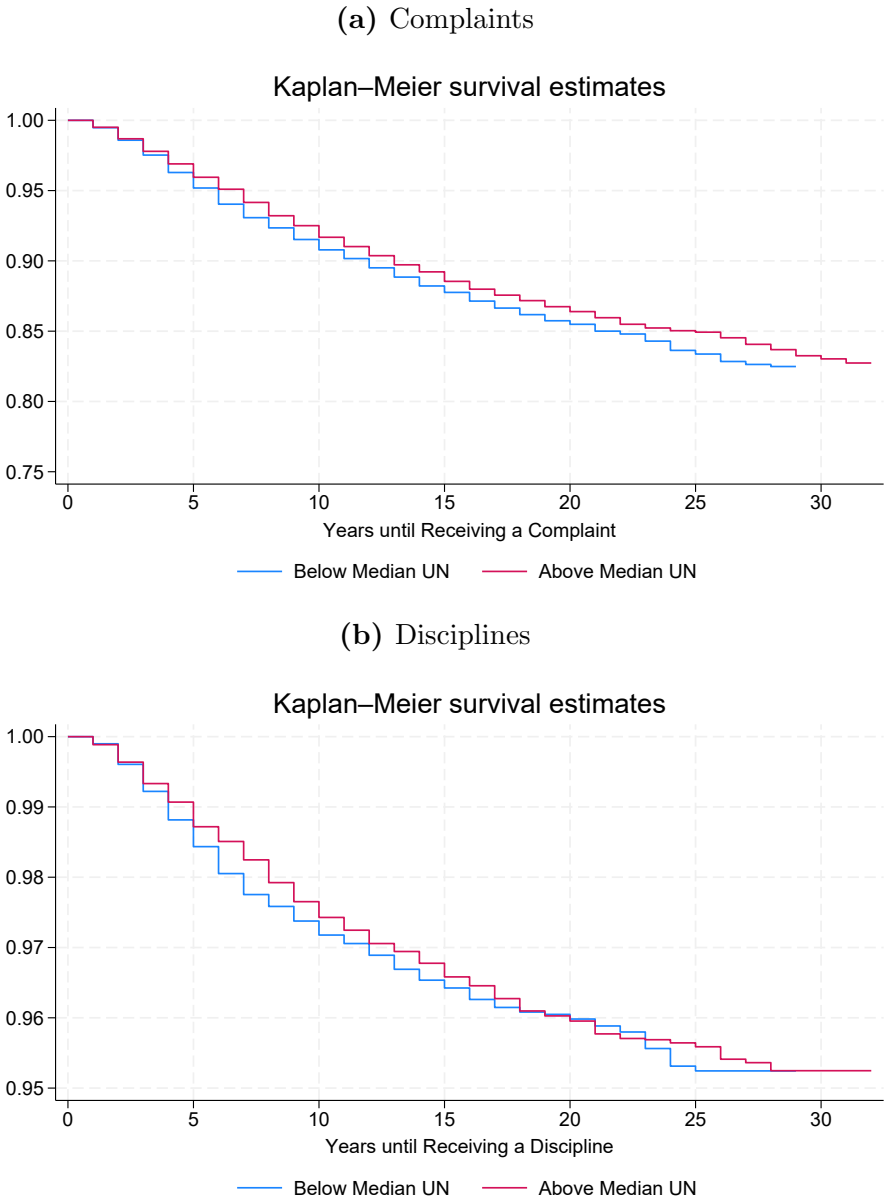
Notes: Panel (a) and panel (b) plot the relationship between a summary measure of officer quality and unemployment rate, after residualizing both using current year unemployment and agency fixed effects. Panel (a) shows how these two variable move with each other over time. Panel (b) plots the two against each other and overlays a linear fit. At the starting year level, unemployment rate in starting year explains 11.6% of the residual variation in likelihood an officer receives a complaint or is fired in their first five years.

Figure A4: Effect of Starting Year Unemployment Rate on Officer Outcomes, Dropping One Entry Cohort



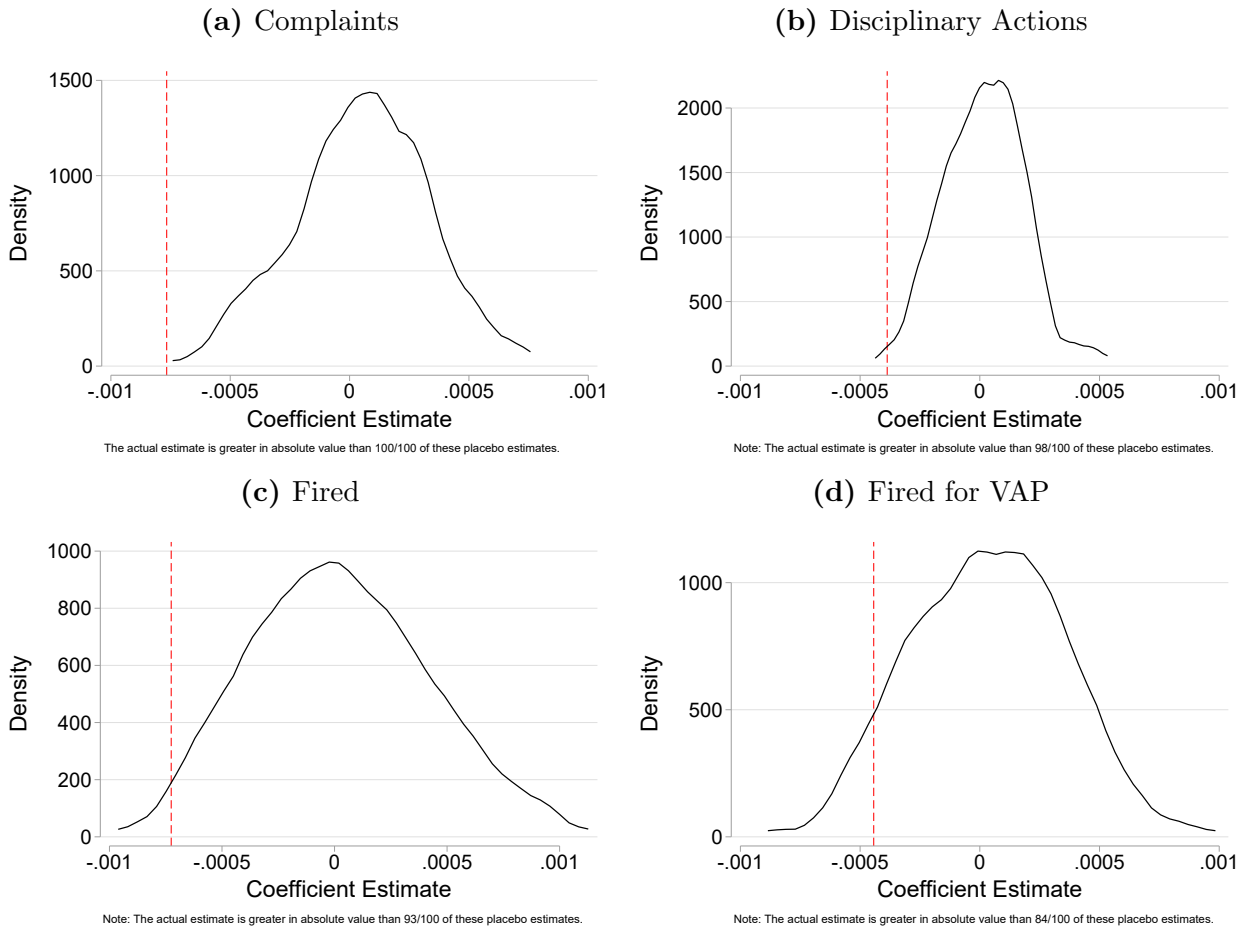
Notes: This figure shows the robustness of our main estimates to dropping a specific entry cohort. The years presented in the X-axis denote the specific cohort dropped in each robustness test. See Table 2 notes for a description of the outcome variables.

Figure A5: Survival Function Until First Misconduct Event by Initial Unemployment



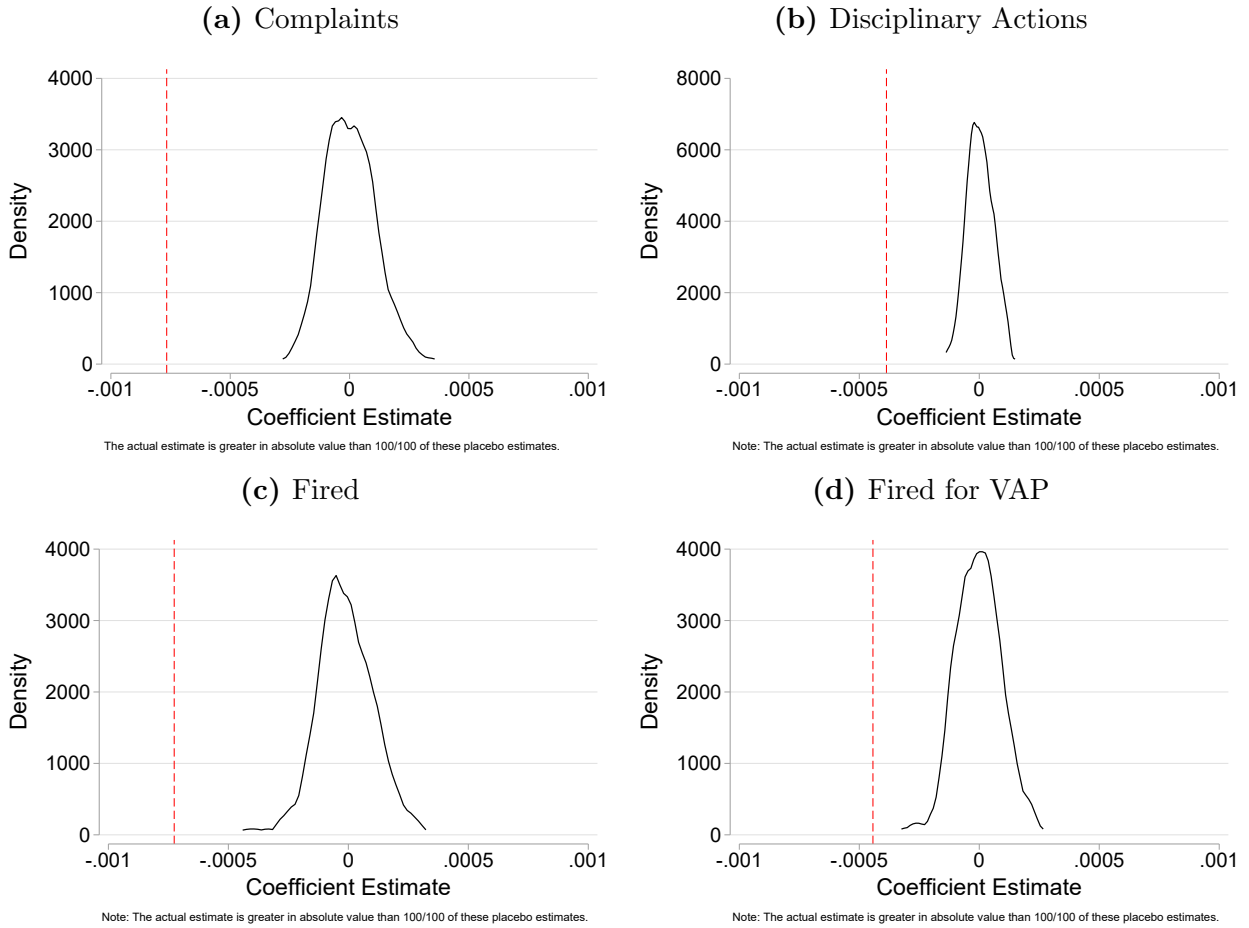
Notes: This figure shows the Kaplan–Meier survival function for police officers until the time they receive a complaint against them (panel (a)) or a discipline against them (panel (b)), measured by the years elapsed since they were hired. The figure presents the survival function of the time until an officer receives a complaint/discipline by whether the officer was hired in an above- or below-median unemployment year.

Figure A6: Placebo Estimates from Randomizing Starting Year for each Cohort



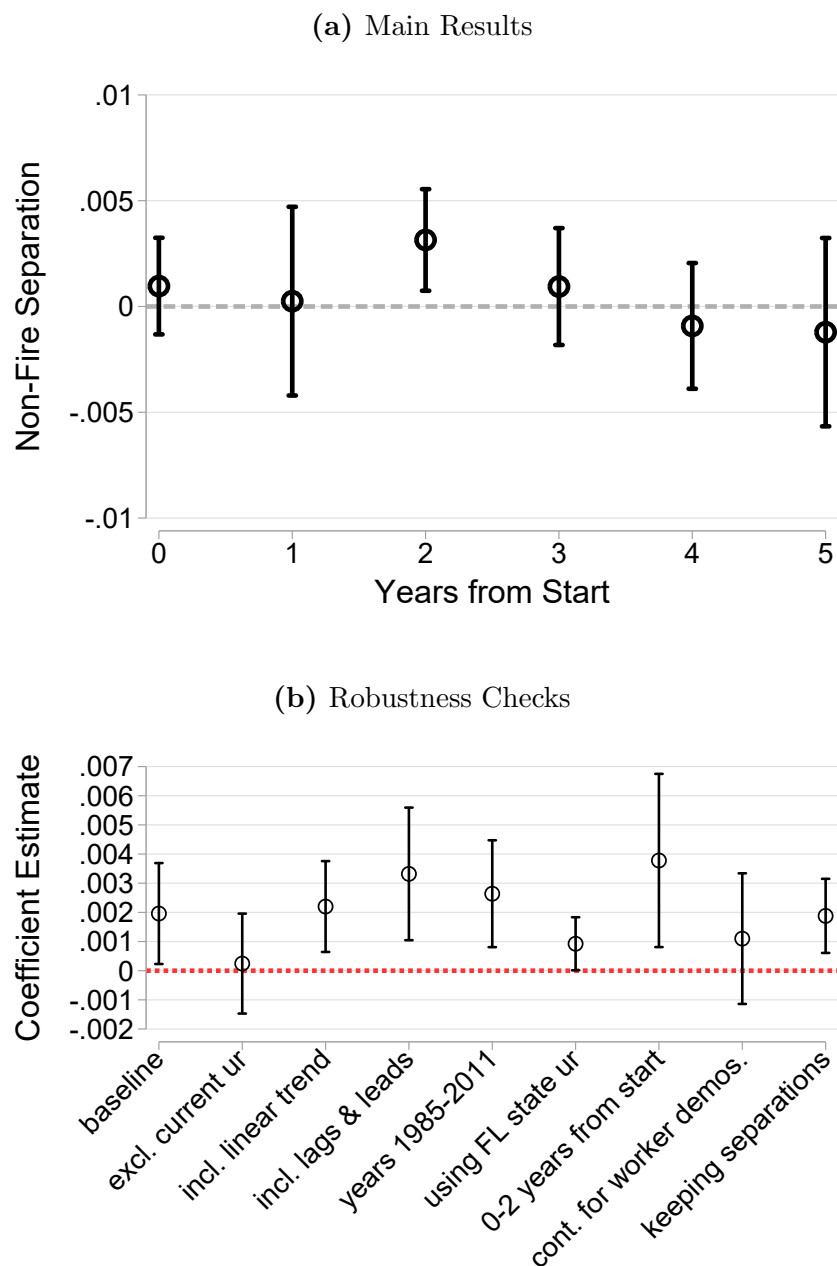
Notes: We randomly assign starting years to entire starting cohorts of police and re-estimate our main results over 100 replications. In this figure, we plot those placebo estimates for our four main outcomes. The actual estimate in each case is marked by the dashed red line. See Table 2 notes for a description of the outcome variables.

Figure A7: Placebo Estimates from Randomizing Starting Year for each Individual



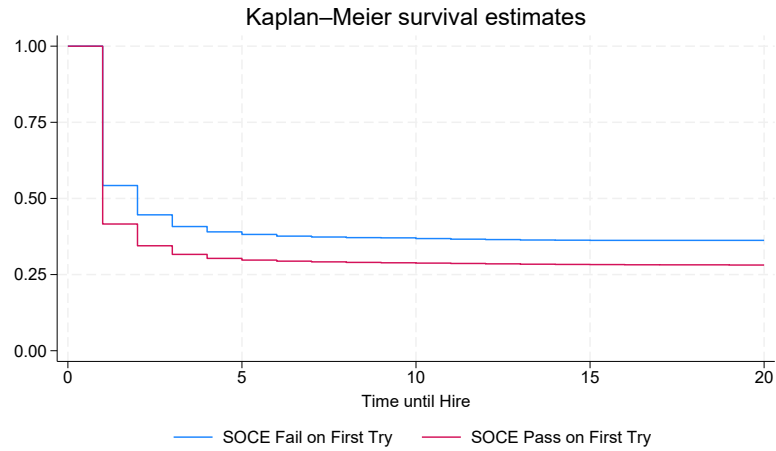
Notes: We randomly assign starting years to individual officers and re-estimate our main results over 100 replications. In this figure, we plot those placebo estimates for our four main outcomes. The actual estimate in each case is marked by the dashed red line. See Table 2 notes for a description of the outcome variables.

Figure A8: Effect of Starting Year Unemployment Rate on Officer Separations



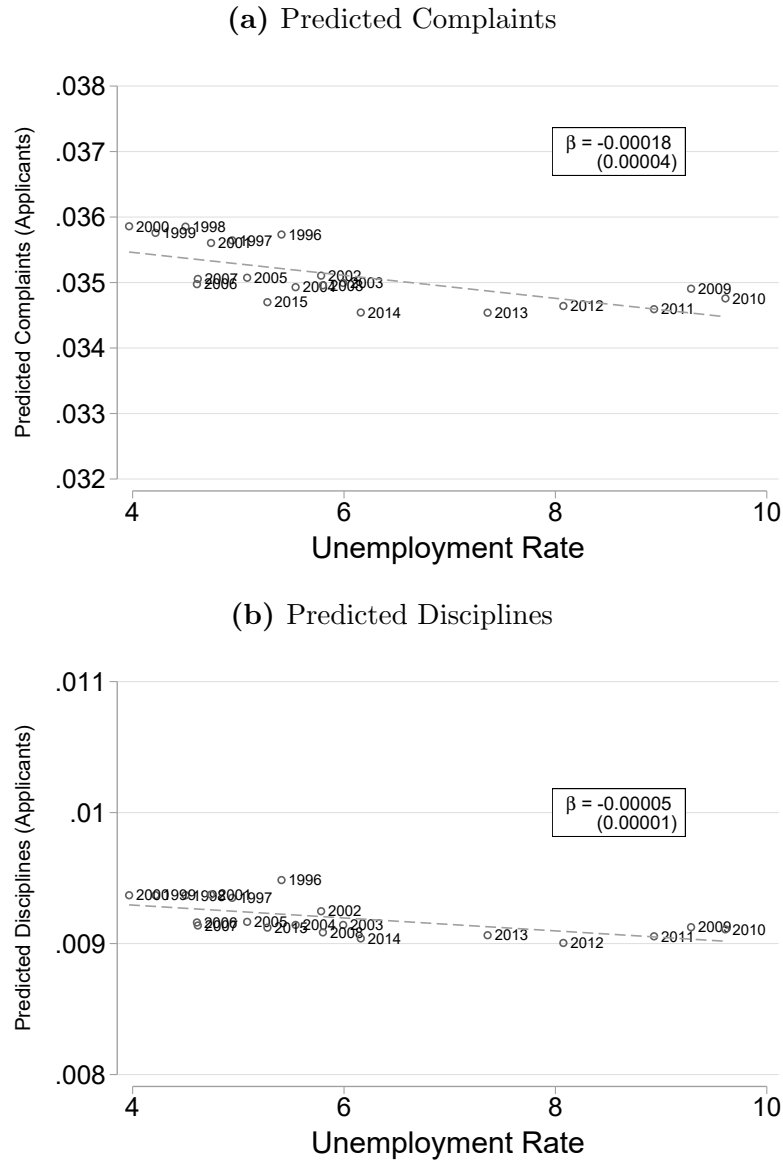
Notes: Panel (a) estimates equation (1) with voluntary separation as the dependent variable separately for each year since start. Panel (b) shows our standard set of robustness checks for this outcome. In both panels, with each coefficient, we plot the 90 percent confidence interval, based on standard errors clustered at the level of officer's starting year.

Figure A9: Passing the SOCE on the First Try and the Probability of Being Hired



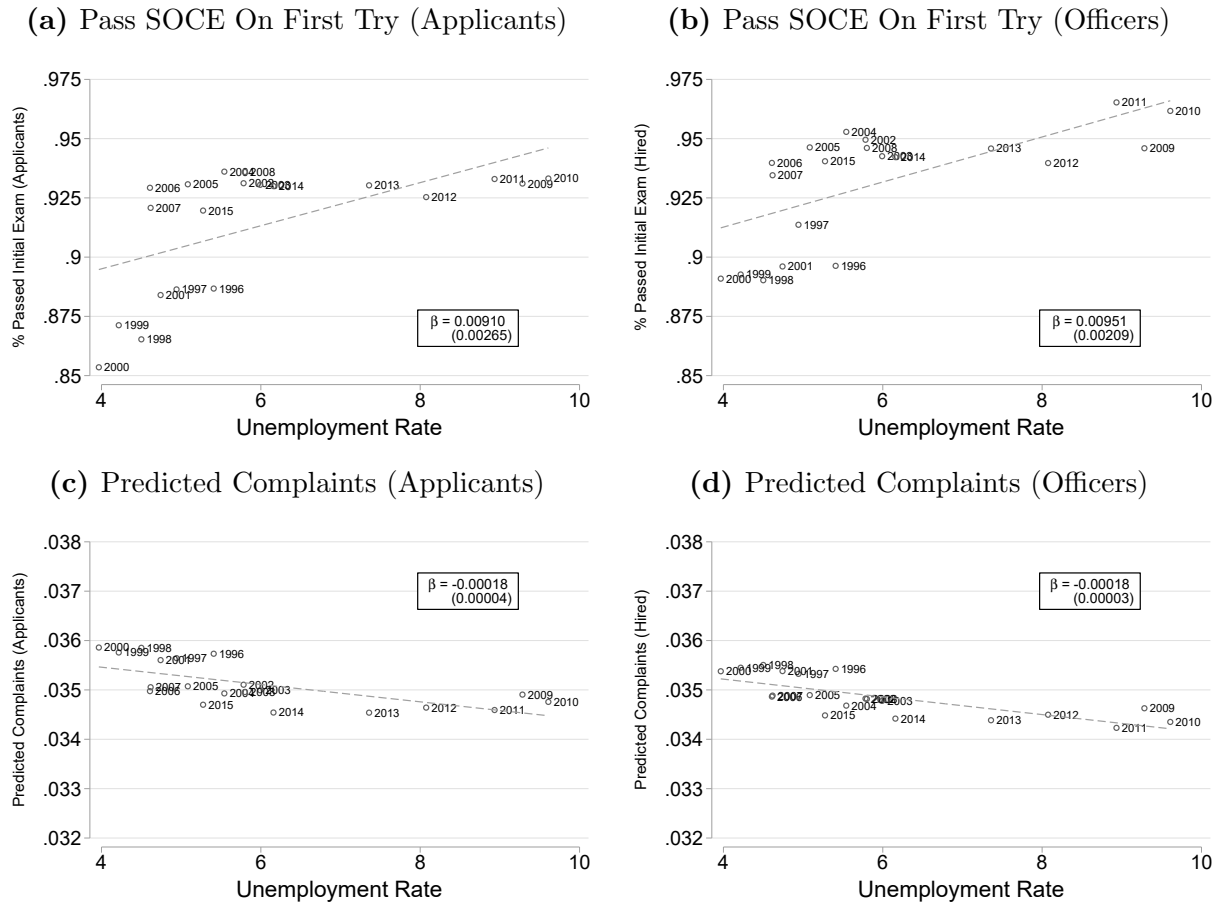
Notes: This figure plots the likelihood of being hired by a police agency in Florida as a function of time since passing the SOCE exam based on non-parametric Kaplan-Meier estimates of the survival function, divided by whether the candidate passed the SOCE exam on the first try or not. The sample only includes officers who eventually passed the SOCE exam (and were thus eligible to be hired), and for whom we observe detailed information on their police training academy information, yielding a sample of 52,195 candidates.

Figure A10: Predicted Misconduct Events Among Applicants and Unemployment Rate



Notes: This figure examines how applicants' predicted complaints and disciplines (from the machine learning exercise described in Section 4.1.2) vary across the business cycle, as captured by the unemployment rate in 1996-2015. The first panel presents evidence for applicants' predicted complaints, whereas the second panel presents corresponding evidence for predicted disciplines.

Figure A11: Characteristics of Applicants and Hired Officers and the Unemployment Rate



Notes: This figure presents evidence on how the characteristics of applicants and hired police officers vary across the business cycle, as captured by the unemployment rate in 1996-2015. The first panel presents the average SOCE-pass-on-first-try for applicants in each year, whereas the second panel presents corresponding evidence for hired officers in that year. The third panel presents the average predicted complaints (from the machine learning exercise described in Section 4.1.2) for applicants in 1996-2015, and the last panel presents predicted complaints for hired officers in those years.

Figure A12: Relationship between # of New Hires and Starting Year Unemployment Rate, Statewide Agencies



Notes: This figure plots the number of full-time police hires per year in the Florida Department of Highway Safety and Motor Vehicles and the unemployment rate in each year. This figure suggests that the Florida Department of Highway Safety and Motor Vehicles either do not exhibit a labor demand response to economic conditions or exhibit a labor demand response that is far more muted than other agencies in Florida.