Police Work and Political Identity*

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Abstract

The preferences of bureaucrats are a central determinant of how governments operate, yet little is known about how these preferences are formed and the relative importance of selection versus the treatment effect of government work. This paper studies these questions in the context of policing and asks how working as a police officer impacts political preferences. We link civil service exam records to data on voting and campaign contributions to test whether becoming a police officer affects political identity. Using difference-in-differences and regression discontinuity designs, we find that joining a police force increases Republican party affiliation, contributions to Republican campaigns, and voter turnout. The treatment effect of work can explain around 40% of the difference in party affiliation between police and the general population. We then show that political affiliation relates to on-the-job behavior: Republican officers make more arrests and use more force than comparable non-Republican officers. Finally, we revisit a canonical model of bureaucratic motivation and show that a treatment effect of work on preferences can alter the government's optimal choice of *selection* into the profession. Our findings show how the experience of government work is central to bureaucratic preferences, and they highlight the constraints on worker selection and recruitment as tools to dictate the composition of the government workforce.

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1 Introduction

The motivations and preferences of bureaucrats are an important determinant of public sector effectiveness (Besley and Ghatak, 2005; Prendergast, 2007; Ash and MacLeod, 2015; Spenkuch et al., 2022). Government workers are often given broad discretion in how they conduct their work, and their preferences and ideology can thus have an important impact on how governments function. A commonly-prescribed policy tool for improving the quality of government is to change hiring practices or employment contracts to improve the selection of individuals entering government work. In that spirit, economists studying state effectiveness have focused on understanding the selection of individuals into the public sector (Dal Bó et al., 2013; Fisman et al., 2015; Ashraf et al., 2020). However, the experience of working in government may itself have an important effect on the preferences of bureaucrats. If worker preferences are in part determined by work experience, there may be limits to the ability of hiring and selection to change the motivations and preferences of bureaucrats.

Bureaucratic preferences are particularly important in the context of policing. Police officers engage in high-stakes contact with the public, and they are given broad discretion in how they carry out their workplace activity (Abrams et al., 2021; Ba et al., 2021; Gonçalves and Mello, 2021; Weisburst, 2024). The characteristics of officers also differ systematically from the broader public, including on several psychological traits (Friebel et al., 2019) and in terms of political identity (Ba et al., 2024). How do these differences arise, and what is the relative importance of selection into versus the treatment effect of police work? Popular writing on policing emphasizes the power of the work experience to change one's world view. For example, former officer Adam Platinga argues that, "police work is where academic debates about race relations, gun control, welfare, justice, and immigration reform take shape and are made real as you enter a world that few people visit and even fewer understand" (Plantinga, 2014). Despite the size and reach of policing in the US, little is known about how the identity and preferences of police are shaped by the experience of the job itself.

In this paper, we estimate the causal effect of working as a police officer on various measures of individual political identity. We do so by linking data from entry-level police civil service exams to local government payroll records, state voter registration files, and campaign contribution data. We ask how working as a police officer affects individual partisanship, voter turnout, and campaign contributions, whether these effects vary based on the characteristics of the employing department, and the relative importance of selection and treatment effects for explaining differences between police and the public.

We begin by studying these questions in Florida, where we observe a record of all individuals who apply to become a police officer. Our empirical strategy is a difference-in-differences design that compares individuals who passed the police civil service exam with individuals who failed the exam in the same year. We estimate that exam passage causes a 1.6 percentage point increase in Republican Party registration. Because applicants can retake the exam in future years and some passing individuals do not become an officer, exam passage leads to a 30 percentage point increase in the probability of ever working as a police officer in Florida. Scaling our reduced-form effect by this first-stage effect, we estimate that working as a police officer increases Republican Party registration by 5.4 percentage points.

Florida police officers in our sample are 13.7 percentage points more likely to be affiliated with the Republican party in 2020 than the general population of registered voters, a pattern that exists more broadly across the US (Ba et al., 2024). Our estimates indicate that 40% of this gap can be explained by the treatment effect of police work, while the remaining 60% is due to selection into policing.

Our effects on partisanship are concentrated among individuals who, prior to the exam, are not registered as Republicans (i.e., individuals registered as Democrats or Independents). Among those who are already registered as Republicans, police employment does not increase the likelihood of keeping this affiliation. Effects are similar across applicant gender as well as age at date of exam but differ by race, with impacts concentrated among White and Hispanic applicants. In addition to increased Republican registration, we find positive impacts on donations to Republican candidates and overall voter turnout, suggesting a general increase in political engagement.

We view these effects as substantive changes in political identity for three primary reasons. First, it is well-documented that party of registration is strongly predictive of party identification and voting (Bartels, 2000; Stapleton and Langehennig, 2024).[1] Second, party registration is a choice with real stakes in Florida, since voters can only vote in the primary of the party for which they are registered.[2] Third, we also document effects for contributions and voting, two tangible outcomes related to political preferences and real-world engagement.

We next turn to Columbus, Ohio, where we have data on exact exam scores from 2001-2016. These data allow for a regression discontinuity design that compares applicants who scored just above the passing cutoff with those who scored just below. We find statistically significant impacts on political identity that are broadly similar to our Florida estimates. In years when departments are not hiring many new officers, applicants who barely pass the civil service exam may still be unlikely to receive employment. We document that the positive political affiliation impacts are only present in years when barely passing the exam has a significant impact on

[1] In the American National Election Studies (ANES), almost 90% of people registered as Republican report identifying as Republican. In the Cooperative Election Study (CES), around 85% of people registered as Republican report voting for the Republican candidate in the last presidential election.

[2] Additionally, Gerber et al. (2010) show evidence that the mere act of registering with a party causes one to align more with that party's views.

being hired. While our data are more limited in Columbus and are restricted to a single city, the pattern of results corroborates our estimates from Florida.

We then provide several pieces of evidence to characterize policing's partisanship effect. Significant research has explored the drivers of voting behavior, and one salient factor is individual income. Can changes in income induced by police employment explain our estimates? While we do not directly measure income in our main data sources, we provide two approaches to this question. First, we link the addresses of voters to tract-level information on neighborhood income. Perhaps surprisingly, passing the entrance exam leads applicants to reside in neighborhoods with *lower* median incomes. Second, we use income and occupation data from the Current Population Survey (CPS) to measure how police work relates to earnings. Exploiting the panel nature of the survey, we find that individuals who move into policing from another occupation experience small earnings declines, and vice versa for those exiting policing. Because previous research has generally found that higher income is associated with increased Republican registration (Brunner et al., 2011), we conclude that income changes are not an important driver of our estimated partisanship impacts.

Another possibility is that our partisanship impacts reflect police becoming single-issue voters. The Republican Party has historically identified as the party of "law and order," and police may be inclined to identify as Republican solely because of the issue of public safety (Erskine, 1974). Beliefs on individual issues are hard to identify from voting behavior. Instead, we appeal to the General Social Survey, which elicits respondents' opinions on various political issues as well as occupation information. We find that, among respondents with the same political affiliation, working as a police officer is not predictive of responses on a variety of political questions. This result suggests that police officers are ideologically indistinguishable from other members of their party, and thus are not identifying as Republican based on one issue alone. Combined with our income effects, this suggests that the experience of work as a police officer leads to a broad change in attitudes.

To understand what features of police work may be most important for political identity, we examine heterogeneity in impacts across agencies. We re-estimate our baseline specification, with our main treatment variable interacted with the characteristics of applicants' first department. We do not find heterogeneity by local crime, agency size, agency unionization status, or the agency's exposure to Black Lives Matter protests in 2014-2015. We do, however, find larger effects for people entering agencies where more of their fellow officers identify as Republican, consistent with peer effects and department culture playing an important role.[3]

Is the size and the direction of the effect we find specific to police work? Or, alternatively,

[3]We also conduct a version of this specification where 'share Republican' is instrumented with 'share Republican prior to entering among recent hires', which avoids the concern that the right-hand side variable is contaminated by the agency's specific treatment effect.

is the impact more general for all public sector jobs? To answer this question, we apply the same research design to a separate record of civil service exams for Florida firefighters. In contrast to our policing estimates, we find a small and statistically insignificant impact of working as a firefighter on Republican party affiliation. Descriptively, firefighters also skew Republican, highlighting that policing's impact on political ideology is due to more than just peer composition and may be related to the specifics of police work.

While these results show that police work can impact political identity, we next document that police officers' political ideology also matters directly for their workplace behavior. We collect data from the Dallas Police Department on a wide range of officer activities on the job, including arrests linked to criminal court outcomes, use of force, and civilian complaints, which we link to officers' voter registration data. Compared to similarly-situated non-Republican officers, Republican officers make more misdemeanor arrests and use more force. However, their arrests result in a *higher* average conviction rate in court, and they are less likely to receive civilian complaints. These differences indicate that officers of different political ideologies differ in their workplace activity and in ways that are ex ante ambiguous for welfare.

In the final part of the paper, we consider the broader implications of our empirical results for the government's problem of selection into the bureaucracy, which has received more attention in the literature. Building directly on Prendergast (2007), we model a principal-agent problem of a police department delegating enforcement to an officer. Officers differ in one dimension of preferences, which is how much they weigh the utility of suspects they encounter.[4] Departments must decide how to set wages and oversight of enforcement, which affect the type of officer induced into police work. We incorporate a treatment effect of work on preferences by supposing that officer preferences are less aligned with suspects on-the-job than they are at the point of hiring. We show that this treatment effect of police work leads to a change in the optimal offered wage and on-the-job oversight. We also show that a sufficiently large treatment effect can even change the *type* of officer the department tries to recruit, from officers un-aligned with suspects ("hostile" officers) to those aligned with suspects ("sympathetic" officers). These theoretical results complement our empirical analysis by showing that the treatment effect of government work on preferences is not only important for explaining observed bureaucratic ideology; it also matters for the optimal design of selection into the public sector.

Our central contribution is to show the causal effect of a government occupation on political identity and to show that this effect is as important as selection into government work for explaining bureaucratic ideology. Several studies have examined theoretically and empirically the determinants of selection into public sector work (Besley and Ghatak, 2005; Prendergast, 2007;

[4]Our model analogue to partisanship is officer alignment with suspects. In addition to correlating with enforcement behavior, we show empirical evidence that partisanship correlates with surveyed attitudes toward criminal suspects.

Dal Bó et al., 2013; Fisman et al., 2015; Ashraf et al., 2020). A key takeaway from this literature is that features of the employment contract can be designed to induce positive selection into the public sector. Our paper complements this work on selection factors by highlighting the comparable importance of the treatment effect of government work itself, a finding that has broad implications for recruitment and performance in a wide range of mission-oriented organizations. While political ideology is only one dimension of bureaucratic preferences that can matter for how bureaucrats behave, we show that officer partisanship correlates strongly with several dimensions of enforcement behavior. In that regard, we add to a growing literature showing how personal ideology can impact the behavior of government workers in several high-stakes settings (Spenkuch et al., 2022; Ash et al., 2023; Grosjean et al., 2023).

More broadly, our paper contributes to the understanding of how political ideology is formed. A central concern in political economy is understanding the relative importance of contextual and personal factors in shaping partisan preferences. Recent work has shown that these preferences are impacted by factors like neighbors (Bursztyn et al., 2022), county of residence (Cantoni and Pons, 2022), and peers in school (Billings et al., 2021). We argue that one's work and work environment are also critical components. Partisan alignment between employers and employees and partisan segregation across firms is important in the US and abroad (Colonnelli et al., 2022; Fos et al., 2022). Moreover, prior work shows that attitudinal or behavioral change can occur *within* work settings (Mo and Conn, 2018; Hertel-Fernandez, 2018; Babenko et al., 2020; Dahl et al., 2021; Grosjean et al., 2024; Navajas et al., 2022; Adger et al., 2024). We take a distinct approach by exploring the composite effect of one's occupation and work environment on political identity. In doing so, we provide novel evidence on a largely unexplored question that is salient for virtually all working-age adults: does where you work influence your political preferences? We find that the answer is "yes" in the context of policing. While policing provides an important backdrop for this study, these results have broad implications for many occupations (e.g., social work, education, healthcare) where peer composition or the social issues one is exposed to on a day-to-day basis may shift individual ideology.

2 Institutional Background

Civil service exams are a common screening device used for employment in public-sector occupations. The use of these exams arose out of reforms aimed at replacing the "spoils system" of bureaucracy hiring with a "merit system," a change which led to dramatic declines in crime rates in the 20th century (Ornaghi, 2019). We leverage records from police civil service exams from 2001-2019 to identify a group of people who are interested in police employment where, by virtue of passing or failing the exam, some become police and some do not. Below, we explain how the police application and exam process works in each of our settings.

In Florida, police applicants must be at least 19 years old, have a high school degree or GED, and pass a background check (FDLE, 2022). In addition to meeting these minimum requirements, they must first pass a basic written exam and a physical examination. Upon passing those exams, candidates can then enroll in a Basic Recruitment Training Program. This program consists of academic courses and physical training aimed at preparing the candidate to become a police officer. In total, the program consists of 770 hours in training. After the program, applicants can take the State Officer Certification Exam (SOCE), which evaluates their comprehension of concepts covered in training. In general, if they pass the exam, candidates then apply to a police agency, agencies decide whether to make a job offer, and candidates then decide whether to accept the offer and join the force.[5,6] We observe exam results ("pass" or "fail") from the initial basic written exam and the final certification exam.

In Columbus, OH, applicants must be at least 20 years old and must also have a high school degree or GED and pass a background check (City of Columbus, 2022). After submitting an application, applicants must then pass a four phase civil service exam. The first phase consists of a multiple choice test that covers spelling, vocabulary, reading, and map reading. Applicants with a passing score in one phase proceed to the next phase.[7] The second phase is a writing sample, the third phase is an oral exam aimed at testing problem solving skills, and the final phase is a physical fitness exam. After completing the civil service exam, applicants undergo a series of other screens, including a polygraph exam, a more detailed background investigation, an interview, and a medical exam. Finally, if the applicant is offered the job and accepts, they then enter a classroom-based training academy before beginning their first assignments in field training. We use data on exam scores from the multiple choice exam (phase one).

3 Data

3.1 Data Description

Civil Service Exam Records — We use data from civil service exam records in Florida and Ohio. These data permit us to observe a group of people who are interested in police employment where, by virtue of passing or failing an exam, some are able to become police and some are not.

[5]If they fail the exam, candidates may retest after a 30-day waiting period. Candidates are allowed three retests in a four-year period.

[6]In some cases, officers may apply to an agency first, attend training academy, and then take the exam to determine if they can enter full-time employment with the agency. In general, agencies have discretion to add further requirements on top of the state's.

[7]Applicants who fail must wait until the exam is administered again to retest. In Columbus, the police exam is typically administered once every 6 months to a year.

In Florida, we use statewide records from an initial "basic abilities" written exam (BAT) and a final certification exam (SOCE) from 2013-2019. For these records, we observe a discrete indicator of whether the applicant passed or failed the exam. In addition, these records contain birth year for every applicant, which we use to match applicants to other data sets. The records from Columbus, OH contain the exact exam score for each applicant, allowing us to compare people who score just above the passing cutoff to those who score just below the passing cutoff in a regression discontinuity design.

Police Employment Records — We use administrative data from the Florida Department of Law Enforcement (FDLE) and local government payroll records in Columbus, OH to document a first-stage effect of passing the civil service exam on police employment. The FDLE records cover all police employment spells from 1985 to 2021. The local government payroll records from Columbus, OH cover all employment in the Columbus city government from 2011 to 2019.

Voter Files — State voter files are public record in Florida and Ohio. We use data from these files to measure partisanship and turnout in both states. Florida is a closed primary state (National Conference of State Legislatures, 2021). In order to vote in a party primary, the voter must be registered with that party. We append vintages of the Florida voter file from 2012-2020 to measure party affiliation in each year. The 2020 vintage also contains a history of voter turnout from 2012-2020.

Ohio is an open primary state. This means that voters can vote in any primary, although they can only vote in one party's primary per election cycle. We append vintages of the Ohio voter file from 2014 and 2021. These files contain a history of voter turnout in primary and general elections. We use the party primaries that the voter participated in going back to 2000 to infer party affiliation in each year.

Campaign Contributions — Finally, we use data on campaign contributions from the Federal Election Commission (FEC). These records allow us to observe an alternative outcome related to partisanship. We leverage contribution data from the 2010 to 2020 election cycles.

3.2 Merging Description

Merging these distinct data sources is a critical first step. In Florida, we begin the merge process by stacking vintages of the public voter file from 2012-2020 and linking that stacked data to all FDLE person records on the basis of first name, last name, and birth year. Then, we refine that match by giving preference to links that have the same recorded middle initial, sex, and/or race across files. After refining the match, we drop any individuals from the FDLE file who match to more than one person in a given vintage of the voter file and any individuals from the voter file who match to more than one person in the FDLE file. Since the FDLE records and voter file both contain unique IDs, we then collapse this data to include only unique FDLE ID to voter ID matches. We match this back to the stacked voter file based on voter ID. This allows us to find all matches for that ID from 2012-2020, even if the person in question has changed their first or last name in a way that would preclude us from matching on name. Ultimately, we match about 69.8% of FDLE person records to a voter record, which is similar to the overall voter registration rate in Florida (Florida Division of Elections, 2022).

The resulting file contains every FDLE to voter link from 2012-2020. Since this file has both an FDLE person ID and a voter ID attached, we can easily merge on other information from the FDLE or voter records. We link the exam records, employment records, and demographic information from the FDLE, and we link party affiliation, turnout, and address information from the voter files. For matches who end up employed at a police agency, we can evaluate the quality of the match by comparing the county of their agency in the FDLE records to their county of residence in the voter file. We find that approximately 70% of that sample is living and working in the same county and 90% is living and working in the same county or a border county.

Next, we add political contributions from the FEC. The contributions data do not contain birth date information so we must match on the basis of first and last name alone. In addition, the same person can make multiple contributions in a given election cycle, meaning we don't want to remove duplicate records. Instead, within each election cycle, we collapse the data by first and last name, recording the minimum value of binary variables that indicate a contribution was to a Republican candidate or group, to a Democratic candidate or group, or to a candidate or group that is classified as neither Democratic nor Republican. We then link this collapsed data to the matched FDLE-voter file.

In Columbus, OH, we do not observe birth year for the applicants. We begin the process by collapsing the civil service exam data by first and last name of the applicant, keeping their scores from the first year that we observe them in the data. Then, we merge the exam records to local government payroll records from Columbus, OH that cover the police department and other government agencies. We use this data to determine if the applicant is hired as police or for another job in local government. Next, we merge this linked exam-payroll file to a dataset that contains information from 2014 and 2021 vintages of the Ohio voter file. This combined voter file contains the latest record available for an individual and records whether the individual resides in Franklin County in the 2014 or 2021 vintage. We merge our records to this file on the basis of first name and last name. Then, we filter the match based on implied age when the person took the civil service exam. Based on information from the FDLE linkage and the minimum age requirement in Columbus, OH, we remove any matches that would imply the individual was older than 50 or younger than 20 when they took the exam. We further refine the match by giving preference to links that have the same middle initial, are between 20-40 years old when they take the exam, and/or resided in Franklin County in 2014 or 2021.[8]

The resulting file still contains duplicate matches. Since we do not observe birth year for these applicants, we retain as much information as possible by collapsing the data by first name and last name, recording the minimum values for binary variables indicating Republican affiliation, Democratic affiliation, other affiliation, and turnout. We also record the number of duplicates associated with each applicant, and thus, we can include all matches or restrict our analysis to unique matches. Approximately 86% of applicants have a match to the voter file and 61% have a unique match. Our primary analyses restrict attention to applicants with a unique match, but we expand the sample to consider any matches in robustness analyses. Finally, we add political contributions data using the same process that we use for the Florida records.

3.3 Summary Statistics

We report summary statistics for the main Florida sample in Table 1. To improve our ability to track people over time, we restrict the sample to individuals who match to at least one voter file vintage prior to the exam and to individuals who are 17 or older as of 2012. In the sample of all exam-takers, approximately 39% are affiliated with the Republican party in the year before the exam, 28% with the Democratic party, and 31% are not affiliated with either. The sample is also majority male and majority white. Columns 2 and 3 document level differences between people who fail and people who pass the exam. We test whether these groups are trending differently on outcomes of interest prior to the exam in Section 5, and we explore potential issues even further in Section 5.6.

We report summary statistics for the main Columbus sample in Table 2. Column 1 shows statistics for all individuals, column 2 shows statistics for individuals with any match to the voter file, and column 3 shows statistics for individuals with a unique match to the voter file. Among the sample of individuals who have a unique match in the voter file, 26% vote in a Republican party primary in at least one year after the exam whereas only 21% vote in a Democratic party primary. This sample is also majority male and white.

Table 3 presents overall differences between police and the general public in Florida. Among people from our sample who are employed as police in 2020, 46.9% are affiliated with the Republican party, while 33.2% of the general population of registered voters is affiliated with the Republican party. This is an overall difference of 13.7 percentage points that we will

[8]Before restricting the sample based on living in Franklin County, we test whether this is affected by treatment. We do not find a relationship between passing the exam and likelihood of living in Franklin County in 2014 or 2021.

disaggregate into selection versus the treatment effect of police work.

Considering the Counterfactual — An important question for interpreting our analysis will be what "counterfactual" occupations applicants pursue if they fail the exam. Since we do not observe earnings records for our exam takers, we instead employ alternative data to probe this question. We will use the Current Population Survey (CPS), which follows individuals over a sixteen-month period and allows us to identify occupational transitions into and out of policing. In Table A1, we list the top 50 previous occupations for new police. To understand how these occupations differ in their average political ideology, we additionally employ the General Social Survey (GSS), which elicits information on individual political affiliation as well as occupation. We use the GSS to construct a measure of political affiliation for each occupation, and we ask how police compare with the counterfactual occupations, weighted by the volume of CPS transitions. Table A3 shows that policing is markedly different from these common alternative occupations.[9] In particular, police are 15 percentage points more likely to identify as Republican and less likely to identify as Democrat or "Other." Finally, we note that the weighted average of Republican ID for the pre-police occupations is 1.3 percentage points higher than the simple, unweighted average of party ID. This gap suggests that the counterfactual occupations for individuals who fail the exam are also slightly more Republican than the general public but are far less skewed than policing.

4 Empirical Strategy

We employ two distinct research strategies to estimate the causal effect of becoming a police officer on political identity. First, we use a difference-in-differences design that leverages statewide civil service exam results from Florida. We compare outcomes for applicants who pass versus fail the exam in the years before versus after they take the exam. Second, we use exact exam scores from Columbus, OH in a regression discontinuity design. We compare outcomes for applicants who narrowly fail the first section of the exam to applicants who narrowly pass the first section of the exam.

4.1 Difference-in-Differences

In Florida, we observe statewide civil service exam results that indicate whether the applicant passed or failed an exam. Since applicants can retake exams, we focus on the first exam that an individual takes. For our main analysis, we focus on applicants taking the State Officer Certification Exam (SOCE). The SOCE is taken after recruitment training and determines

[9] Table A2 reports the top 50 new occupations for former police, and Table A4 shows that policing is also different from these occupations.

whether an officer is eligible for hire. Later, we implement the same research design but focusing on applicants taking the Basic Abilities Test (BAT). The BAT determines whether an officer is eligible to enroll in training.

In general, we estimate the following pooled difference-in-differences regression:

$$Y_{it} = \beta(\text{Passed}_i \times \text{PostExam}_{it}) + \gamma(\text{Passed}_i \times \text{YearOfExam}_{it}) + \phi_i + \pi_{e(i)t} + \varepsilon_{it} \tag{1}$$

where Y_{it} is an outcome related to police employment or political identity for individual i in year t. Passed_i is a binary variable equal to one if the applicant passes the exam and equal to zero otherwise, and PostExam_{it} is an indicator variable for year t being equal or greater than the year individual i took the exam. We also include an interaction between exam passage and an indicator for year of exam for that individual. By including this interaction, the coefficient β reflects the impact of exam passage on all years after the exam year. Individual fixed effects are denoted by ϕ_i. Finally, the inclusion of $\pi_{e(i)t}$, a set of fixed effects for each year t and exam cohort $e(i)$, means that our regression is a stacked difference-in-differences approach that compares outcomes for applicants who pass versus fail within the same exam year. Under the parallel trends assumption, the coefficient of interest β is the average treatment effect (on the treated) of passing the exam on the outcome Y_{it}.[10]

We also estimate an event study difference-in-differences model. This allows us to test whether the applicants who pass versus fail follow similar trends in the outcomes of interest before they take the exam. In addition, we can estimate the dynamic treatment effects of passing the exam. Specifically, we estimate the following:

$$Y_{it} = \alpha + \sum_{\substack{k \neq -1, \\ -T \leq k \leq T}} \beta_k (\text{Passed}_{it} \times \mathbb{1}\left[\text{YearsFromExam}_{it} = k\right]) + \phi_i + \pi_{e(i)t} + \varepsilon_{it} \tag{2}$$

Many of the variables included are defined above. $\mathbb{1}\left[YearsFromExam = k\right]_{it}$ is a binary variable equal to one when year t is k years away from individual i's exam year. For example, if individual i takes the exam in 2015, then 2015 is k=0 years away, 2014 is k=-1 years away, and 2016 is k=1 years away. We estimate this including all relative time indicators (Roth et al., 2022). We only plot estimates for relative time from k=-5 to k=5, which means that each plotted estimate is identified using at least three exam cohorts.[11] We also implement several heterogeneity tests within these two broader models, and we describe those in their respective sections.

[10] We also implement the Callaway and Sant'Anna (2021) approach in Figure A1.

[11] See Table A5 for the coefficients for more distant years.

4.2 Regression Discontinuity

In Columbus, OH, we observe exact exam scores for applicants taking the civil service exam from 2001-2016. We focus on the initial multiple choice exam. Again, since applicants can retake the exam, we use the first exam that an individual takes. We estimate the following equation:

$$Y_i = \alpha + \delta \, \text{AboveCutoff}_i + \psi \, \text{Score}_i + \rho \, (\text{AboveCutoff} \times \text{Score})_i + \varepsilon_i \tag{3}$$

where Y_i is an outcome related to police employment or political identity for individual i. $AboveCutoff_i$ is equal to one if the applicant's score is equal to or above the multiple choice passing score. $Score_i$ is equal to the applicant's score centered at zero relative to the passing score. Under the assumption that potential outcomes are not changing discontinuously at the passing score cutoff, the coefficient of interest δ is the average treatment effect of passing the exam for individuals near the cutoff.

In a similar spirit to our difference-in-differences analysis, we consider several outcomes for Y_i that are defined based on years before the applicant takes the exam. These outcomes should be unaffected by whether the applicant scores just above or just below the passing cutoff. We also implement standard density and balance tests that speak to the validity of the assumption that potential outcomes are continuous through the cutoff. We discuss these tests in more detail in the results section.

5 Difference-in-Differences Results

In this section, we discuss the results from the difference-in-differences analysis in Florida. We first present the first-stage on police employment, followed by our main results on partisanship. We then consider multiple robustness checks of our primary approach.

5.1 First Stage

Figure 1 plots the β_k estimates and confidence intervals from equation (2). It also reports the pooled β estimate and standard error from equation (1). Passing the certification exam has a large first stage effect on employment in a Florida police agency. One year after the exam, applicants who pass are 30 percentage points more likely to be employed than applicants who fail the exam. This effect decreases slightly over time as the failing applicants can retake the exam and passing applicants might separate from police employment. However, even five years after the exam, applicants who pass are substantially more likely to be employed than applicants who fail.

5.2 Registration

Before examining effects on outcomes in the voter file, we first test whether passing the exam affects the likelihood that we match someone to the voter file at all. We consider this a measure of voter registration, although it may also reflect measurement error in the matching process. Figure A2 displays these results. We do not find an effect of passing on voter registration. The coefficient estimate is less than 1% of the dependent variable mean. Since we do not find an effect on registration, we limit the sample to individuals who match to the voter file in each year for the remaining analyses of partisanship and turnout.[12] However, these results are robust to restricting the sample solely based on matching to the voter file in 2012, a year prior to the exam for all exam-takers (see Figure A3).

5.3 Partisanship

Figure 2 shows the effect of passing the exam on affiliation with the Republican party. We find that individuals who pass are trending similarly to those who fail the exam in terms of Republican party affiliation in the years prior to the exam. However, after the exam, individuals who pass are more likely to affiliate with the Republican party. Specifically, passing the exam increases Republican party affiliation by 1.6 percentage points. Assuming that passing only affects party affiliation through its effect on police employment, this would imply that becoming a police officer increases party affiliation by approximately 5.4 percentage points.

We contextualize the effect size in two ways. First, we use it to decompose the overall difference between police and the public into selection and treatment components. Overall, people employed as police in Florida in 2020 are 13.7 percentage points more likely to be registered as Republican. Assuming that passing only affects outcomes through its effect on becoming a police officer and that the effect for "compliers" (i.e., those individuals who become police because they passed on the first try) is the same as the average treatment effect, then our estimates imply that approximately 40% (5.4pp/13.7pp) of that overall difference is due to treatment. Second, we compare our results to prior work that studies political affiliation. Billings et al. (2021) find that a 10 percentage point increase in the share minority in a White student's school decreases their Republican affiliation by 8 percentage points.[13] Our effect size is approximately the same as the effect of a 7 percentage point decrease in share minority in

[12] Prior work also conducts analyses on restricted samples of registered voters, e.g., Mullainathan and Washington (2009) and Spenkuch et al. (2022).

[13] Specifically, Billings et al. (2021) estimate that a 10 percentage point increase in share minority in a White student's assigned school descreases Republican affiliation by 2 percentage points. However, a 10 percentage point increase in share minority in assigned school corresponds to a 2.5 percentage point increase in share minority at school of enrollment. We thus scale their reduced form effect by 4 to arrive at the effect of a 10 percentage point increase in share minority at the student's school.

primary school.[14]

Next, we test whether this increase in Republican party affiliation is driven by non-Republican applicants who pass being more likely to switch to the Republican party or by Republican applicants who pass being less likely to switch away from the Republican party. We re-estimate equation (2) separately for individuals who are affiliated with the Republican party in the year prior to the exam and for individuals not affiliated with the Republican party in the year prior to the exam. These results are reported in Figure 3. We find that the increase in Republican party affiliation is entirely driven by non-Republican applicants switching to the Republican party.[15]

Using data on demographic characteristics of applicants, we can also explore treatment effect heterogeneity by race/ethnicity, sex, and age. We find similar effects for male and female applicants, white and Hispanic applicants, and for applicants above 30 and below 30. The one difference we observe is with Black applicants–we do not find an effect of passing the exam on Republican affiliation within this subgroup. In a pooled analysis, the difference between Black applicants and non-Black applicants is statistically significant at the 5 percent level. Figure 4 plots these results.

5.4 Turnout

To estimate effects on turnout and political contributions, we re-code $\text{YearsFromExam}_{it}$ into two-year bins to capture entire election cycles. For the turnout results, we report estimates for up to three election cycles before the exam and three election cycles after the exam. Our last year of turnout data is 2018. Figure 5 shows these results. We find that passing the exam increases general election turnout by 3.8 percentage points. Scaling this estimate by the first stage effect on police employment implies that becoming a police officer increases turnout by 12.6 percentage points.[16] This suggests becoming a police officer alters both political party and

[14] Billings et al. (2021) also benchmark their result against the intergenerational correlation in partisanship. Using their estimate of this correlation, our estimate is one-third the size of the effect of parent Republican affiliation on child Republican affiliation (0.165 percentage points).

[15] We observe increases in Republican affiliation among Democrat and Independent applicants (see Figure A8). In Table A6, we allow non-registrants to enter the sample, coding the party affiliation outcome as zero when the individual does not match to the voter file. When we do this, we continue to find that the main effects are driven by people registered as Democrat or Independent before the exam. We see no effect for people registered as Republican before the exam or those who are not registered in the year before the exam.

[16] By comparison, Gentzkow (2006) estimates that the spread of television in the mid-1900s decreased general election turnout by approximately 1.4 percentage points a decade after introduction while Gentzkow et al. (2011) finds reading the newspaper increases turnout by 4.0 percentage points. Chyn and Haggag (2023) find that children displaced by public housing demolitions are 3.3 percentage points more likely to vote later in life. Finally, we can benchmark this estimate by comparing to get-out-

civic engagement.

5.5 Political Contributions

We further investigate how becoming a police officer shapes political identity by studying its effect on federal campaign contributions. One potential explanation for the results from Section 5.3 is that people who become police want to vote in local primary elections and at the local level, Republican candidates are more likely to cater to police interests. An alternative explanation is that becoming a police officer transforms political attitudes even outside of a person's interactions with local politics. Examining federal campaign contributions allows us to test this second explanation.

Figure 6 shows estimates of the effect of passing the exam on Republican, Democratic, and Independent federal campaign contributions. In this analysis, we don't require a match to the voter file. In addition, if an applicant does not match to the contributions data, we record them as having zero campaign contributions. We find that passing leads to a 0.5 percentage point increase in contributions to Republican politicians or political action committees but no change in contributions to Democratic or Independent campaigns.

5.6 Alternative Explanations

Leveraging the Basic Abilities Test — Our baseline empirical strategy compares outcomes of individuals passing versus failing the SOCE, which is the last exam prior to police employment eligibility. Here, we employ our difference-in-differences design on a sample of applicants taking the earlier Basic Abilities Test (BAT). Passing the BAT makes one eligible to enroll in recruitment training, but candidates must then pass the SOCE to become eligible for employment. We show the first stage effect on employment and the reduced form effect on Republican affiliation for three main groups. First, we examine effects for all applicants, comparing people who fail the BAT to people who pass. Second, we compare people who fail the BAT (and thus do not take the SOCE) to people who pass the BAT and pass the SOCE. These comparisons show a large first stage effect on employment, allowing us to estimate the effect of police employment on identity among a different, albeit overlapping, sample of applicants. Finally, we compare people who fail the BAT to people who pass the BAT and fail the SOCE. Among this group, passing the BAT is not associated with an increase in police employment, but it does correspond to eligibility for recruitment training.

Figure A6 shows the main results from this exercise. For the first two groups, passing the BAT leads to an increase in employment and in Republican party affiliation, consistent with

the-vote field experiments. Our increase in turnout is approximately the same size as the effect of in-person get-out-the-vote contact (Gerber and Green, 2000).

the main results presented in Section 5.3. For the third group, we do not find an increase in employment or in Republican party affiliation, suggesting that recruitment training alone does not shift attitudes in the same way as police employment. This interpretation is consistent with the dynamic effects in Figure 2, which suggests that the change in attitudes grows over time.[17]

These results also speak to an identification concern with the difference-in-differences design. One alternative hypothesis is that applicants who are more likely to perform well on the SOCE are also planning on switching to the Republican party, regardless of their police employment. This violation of parallel trends might appear as a spurious effect of exam passage in our difference-in-differences design. The fact that we do not find difference-in-differences impacts of passing the BAT and failing the SOCE versus passing the BAT is reassuring, since these two groups also differ in preparedness but do not differ in their police employment rate.[18]

Differential Trends — Recall that Table 1 shows 41.1% of individuals who pass the SOCE exam are registered as Republican in the year before the exam, compared to 27.6% for individuals who fail the exam. Individuals who pass also differ on demographics such as sex and race. This difference in levels may translate into different trends in party affiliation over time. In our main results, we assuage this concern by showing that people who pass versus fail follow similar trends prior to the exam and only diverge after the exam. We also show all of our results broken out by observable characteristics, for example, comparing passing and failing applicants who have the same party affiliation in the year prior to the exam. In this section, we explore additional specifications to further address this concern.

First, we use an array of applicant characteristics to predict each individual's likelihood of passing the exam on the first attempt. Then, we re-estimate our main difference-in-differences specification but interacting that predicted likelihood of passing with year fixed effects. This allows party affiliation to evolve differently over time for people with high versus low propensities for passing the exam. Our main results are similar with this approach. We find that passing the exam increases Republican affiliation by 1.9 percentage points.

Second, we control for officer-specific linear trends in party affiliation following Bhuller et al. (2013). This approach permits differential linear trends at the individual level. We find similar results under this specification as well; specifically, passing increases Republican affiliation by 1.7 percentage points. Along similar lines, we implement the approach of Rambachan and

[17]The dynamic patterns are similar if we restrict to a balanced panel of cohorts in relative time.

[18]This also speaks to the concern that the effects are driven by failing the civil service exam rather than passing (i.e., the people who fail have an averse reaction to policing and related politics). If this were the case, one might expect applicants who fail the SOCE after recruitment training to have a stronger reaction than applicants who fail the BAT. However, we find that these groups continue trending similarly after the exams, and the main effect is driven by applicants who pass the SOCE and become eligible for police employment. In addition to this, Figure A5 shows that sharp changes in affiliation occur among the 'passing' group after the exam, not the 'failing' group.

Roth (2023) to assess sensitivity to non-linear violations of the parallel trends assumption. We find that our main results are robust to non-linear shifts that are about twice as large as any pre-period coefficient from t-5 to t-2.

Finally, the results from the analysis of the Basic Abilities Test also lend support here. Among individuals who pass the BAT but fail the SOCE, 30% are registered as Republican in the year before the exam. For individuals who fail the BAT, 17.5% are registered as Republican in the year before the exam. Despite a similar level difference in party affiliation before the exam, we do not see an increase in Republican affiliation after the exam for the group that passes the BAT and fails the SOCE.

Moving and Updating Registration — Individuals who pass the exam are 2.4 percentage points more likely to move after the exam than individuals who fail. One possible explanation of the party affiliation results is that both passing and failing applicants personally identify as Republican prior to the exam, but only those who pass reveal that information upon moving and updating their registration. We note that individuals can update the address on their voter registration form without updating party affiliation. During our sample period, this could be accomplished with a change of address form or by emailing or calling the county Supervisor of Elections.

Next, we detail three results which suggest an increased likelihood of updating registration is not responsible for our main results on party affiliation. First, individuals who pass and fail are equally likely to have a new registration date post-exam (β=0.0007, se=0.002). Second, applicants who are older at the time of exam are less likely to move overall and there is no difference in moving between those who pass versus fail in this subset (β=-0.013, se=0.015). However, as we show in Figure 4, we find similar effects on affiliation for both younger and older applicants. Third, we restrict our sample to applicants who never move after the exam. This is endogenous to passing, but even among this subset, we see similar effects on party affiliation (β=0.021, se=0.010). Ultimately, it does not appear that our affiliation results are explained by this channel, and we emphasize that the turnout and contributions results are not subject to this concern.

6 Regression Discontinuity Results

Before diving into mechanisms in the Florida setting, we employ a regression discontinuity design of applicants to one police agency in Columbus, OH. Figure A10 and Table A7 show the results of standard density and balance tests that support the validity of this design. We do not observe a discontinuous break in the number of applicants at the cutoff score or a discontinuous break in applicant characteristics like race or sex.

Figure 7 documents a first stage relationship between passing the exam and employment as a police officer in Columbus, OH. We do not find that passing has an effect on employment in other local government positions in Columbus. Note that the first stage effect on police employment is likely understated in this sample since we only have payroll records from 2011-2019, while our exam records cover 2001-2016.

Next, we examine the effect of passing on likelihood of voting in a Republican party primary in any year after the exam. We report the results of various specifications in Table A9. Figure 7 shows the result for individuals who have a unique match to the voter file. We find that applicants who score just above the passing cutoff are more likely to vote in a Republican party primary than individuals who score just below the passing cutoff.[19] We do not find such a difference in likelihood of voting in a Republican primary in any year before the exam (see Table A10). We also find an imprecise null on general election turnout in this sample.

Finally, we turn to the political contributions data in Figure 8.[20] Again, we find that narrowly passing the exam increases contributions to Republican politicians or groups in the years after the exam but has no relationship with contributions from years prior to the exam. In addition, we find null effects on contributions to Democratic or Independent campaigns. The regression discontinuity design also permits us to examine outcomes that we can only observe in one election cycle. We take advantage of this feature and examine contributions to President Trump's campaign, which relied heavily on "law and order" rhetoric. We find that narrowly passing also increases the likelihood that an individual donates to his campaign or related political action committees.[21]

Hiring demands fluctuate from year-to-year in the Columbus Police Department, creating variation in the effect of passing the first phase of the exam on eventual hiring. In other words, when the department is hiring many applicants, they are more likely to hire those individuals who barely pass than when hiring needs are more limited. As a placebo test, we split the

[19]We also find results consistent with our heterogeneity analysis from Section 5.3. The effect for White applicants who narrowly pass is 0.057 (0.027) whereas the effect for Black applicants is 0.009 (0.025).

[20]See Table A11 for alternative specifications.

[21]Again, we stress that the regression discontinuity analysis is under-powered, and it is unlikely we are correctly estimating the magnitude of the effect in this analysis. This is reflected in the wide confidence intervals on the estimates. In Figure A11, we report the results of a simulation in which we assess the likelihood that, conditional on estimating a statistically significant effect, we would estimate opposite-signed results (Lu et al., 2019). We do this under various assumptions about the true treatment effect. Assuming an effect of 0.01 on party affiliation, we would estimate a statistically significant opposite-signed effect less than 10% of the time. Assuming an effect of 0.003 on Republican contributions, we would estimate a statistically significant opposite-signed effect around 5% of the time. Finally, assuming an effect of 0.0002 on contributions to President Trump, we would estimate a statistically significant opposite-signed effect around 7% of the time. These results suggest that for all outcomes, and especially for the contributions analysis, it is likely that our estimates have the correct sign.

sample into years when there is a strong first stage relationship between passing the exam and being hired and years when there is not a strong first stage relationship. Table A12 shows these results. We find that the positive effects documented above are concentrated in the years when there is a strong first stage relationship between passing and being hired as a police officer.

7 Mechanisms

The results above document the causal effect of becoming a police officer on political identity. However, becoming a police officer is in itself a bundle of different treatments, and it is unlikely that only a single aspect of the job shapes identity. Below, we explore several channels in the context of Florida to shed some light on potential mechanisms.

7.1 Income

In this section, we explore the possibility that becoming a police officer increases income. In general, households with higher income are more likely to affiliate with the Republican party (Pew Research Center, 2014). If becoming a police officer increases income, it may be the case that the effects we estimate on Republican party affiliation are driven by an income effect.

While we do not observe income directly, we can observe exact address in the voter file and link that address to data on median household income at the tract-level.[22] As shown in Figure A14, we find that applicants who pass the exam move into tracts with lower median incomes.[23,24] It is possible that applicants who fail go on to take higher paying jobs or that applicants who become police are more comfortable moving into lower income neighborhoods. The results in this section are not consistent with increases in income causing the increase in Republican party affiliation.

We supplement these results with an analysis of transitions into and out of policing in the longitudinal sample of the Current Population Survey. Table A14 shows these results. The first column documents the level difference in weekly earnings between police and non-police in the Current Population Survey. The second column documents this difference after controlling for a host of demographic characteristics as well as education and veteran status. The coefficients

[22]We create an address to tract crosswalk using the US Census Bureau's batch address processing tool. Then, we merge median household income at the tract-level in 2016 from Opportunity Insights.

[23]We find similar results even after conditioning on county fixed effects to account for the fact that people might move to accept employment at a police agency outside their prior county of residence.

[24]Cantoni and Pons (2022) find moving to a county with higher Democratic affiliation can influence one's own party affiliation, partly through a peer effects channel. We evaluate this possibility by estimating the effect of passing on zipcode-level party affiliation. We find individuals who pass move into neighborhoods with slightly higher Democratic affiliation (β=0.007, se=0.002), suggesting that, if anything, neighborhood peer effects may push our results in the opposite direction.

in both columns are positive, although the coefficient in column 2 is less than half the size of the coefficient in column 1. This is consistent with positive selection on earnings into policing, and that selection bias being partially mitigated by controlling for observables. Column 3 includes individual fixed effects, thus using variation in police employment that is induced by within-individual job transitions. In this column, we find that policing is associated with a $30 *decline* in weekly earnings. This implies an annual earnings difference of approximately $1,500. Given a gradient of 0.3 between personal income and neighborhood income (Bayer et al., 2021), these results are roughly consistent with the event study evidence. Overall, the results in this section are not consistent with increases in income causing the increase in Republican party affiliation.

7.2 Single-Issue Voting

Another possible explanation of our results is that people who become police align with Republicans solely because they agree on police-specific issues, where the alternative is that those who become police adopt a wide range of Republican viewpoints. If it is the former, our results speak to the power of occupational interests to narrow one's policy priorities and influence party affiliation. If it is the latter, our results show how work, and in particular, police work, can shape a person's world view. While both possibilities are consistent with police work affecting political identity, we would like to investigate which mechanism is more likely.

It is not possible to disentangle these two explanations using voter registration data alone because party affiliation is a one-dimensional measure of preferences or ideology. We gain traction on this question by appealing to the General Social Survey (GSS), one of the only public surveys which includes detailed occupational information. Relying on the GSS, we compare police who are registered Republican to non-police who are registered Republican across a wide range of political and ideological questions. The intuition of this test is simple: if police work shifts party affiliation because it turns new hires into single-issue voters, then we should see that police who are registered as Republican are ideologically distinct from the average Republican. Instead, we find that non-police Republicans and police Republicans hold similar views on non-police policy issues (see Table A15).[25] In other words, for police, the party ID of "Republican" is correlated with typical Republican policy positions, not only those policy positions related to policing.

7.3 County-Specific Effects

We now explore how our partisanship estimates vary across locations and agencies. In this section, we estimate county-specific effects on partisanship using each individual's county of

[25]The same is true when comparing police Democrats and non-police Democrats (see Table A16).

residence in the year before the exam.[26] We do this for all voters who took the exam, voters who were not registered as Republican before the exam, and voters who were registered as Republican before the exam. We generate a coefficient estimate and standard error for the effect of passing the exam for each county. The empirical distribution of estimated effects includes noise from estimation error and thus may overstate the degree of variation in county-level effects. We address this issue with a deconvolution procedure (Gonçalves and Mello, 2021; Kline et al., 2022).

The histogram of these estimates along with the deconvolved density is plotted in Figure A15. The county-level estimates based on all individuals are greater than zero for nearly two-thirds of counties. The distribution of estimates for individuals who were not registered as Republican before the exam is shifted to the right. For this sample, the effect is greater than zero for nearly 90% of counties. The distribution of estimates for voters who were registered as Republican before the exam is clustered slightly to the left of zero. These patterns match our main results, and provide additional information about the distribution of effects. It is not the case that the average effects statewide mask a wide range of negative and positive effects at lower geographic levels. Instead, it appears the positive effect of policing on party identification is common across most counties in Florida.

While the effects are positive across the vast majority of counties, there is considerable geographic variation in the magnitude of the effect. Considering all exam-takers, the standard deviation of the county-level estimates is 0.023. For individuals not registered as Republican before the exam, the standard deviation is 0.044. This variation is informative for thinking about mechanisms – it suggests there are aspects of police work that vary from place to place and influence the political identity of officers. We explore this further by examining heterogeneity by agency characteristics in the following section.

7.4 Heterogeneity by Agency Characteristics

Given the variation in effect size across place, we assess whether features of the agencies themselves contribute to the estimated effects from Section 5.3. The statewide data in Florida covers over three hundred police agencies and gives us substantial variation in workplace characteristics. This allows us to examine what aspects of the work environment affect political identity by comparing officers who pass the exam but join different agencies to officers who fail the exam.[27]

[26] We focus on counties as opposed to agencies for this exercise because there are only 67 counties in Florida as opposed to over 300 agencies. We can more reliably estimate separate effects at the county level than at the agency level. In addition, we observe county of agency prior to the exam for all individuals in our sample, whereas we only observe agency for those who join.

[27] Officers who pass the exam but do not join an agency are excluded from this analysis.

We consider five major agency characteristics: the local crime rate from 2000-2010, agency size, agency unionization status, the agency's exposure to county-level Black Lives Matter (BLM) protests from 2014-15, and the share Republican in the agency in 2012. For each continuous characteristic (i.e., local crime rate, agency size, and share Republican), we divide agencies into above- and below-median groups and re-estimate our main event studies split by prior party affiliation. For the binary characteristics (i.e., agency unionization status and agency exposure to BLM protests in 2014-15), we divide agencies based on the binary indicator and re-estimate our main event studies split by prior party affiliation.

Figure A16 shows the results split by agency share Republican in 2012. We find that people who pass the exam and enter agencies with a higher share of Republican employees are more likely to identify as Republican in the years after than people who enter agencies with a lower share of Republican employees. Figures A17 and A18 show event studies for all other characteristics, but in general, we do not find significant heterogeneity along those dimensions.

Since these agency characteristics may be correlated with each other, we include all characteristics in a single regression, interacting each one with a post-exam indicator and a "passes exam" indicator. Importantly, people choose the agencies to which they apply, making it difficult to disentangle heterogeneity stemming from the effect of agency characteristics and heterogeneity stemming from individual characteristics that lead people to choose different agencies. To mitigate this concern, we re-estimate this pooled regression including interactions with county of residence before the exam and with officer demographics.

Table 4 includes all agency characteristics simultaneously, focusing on applicants who are not registered as Republican prior to the exam.[28] For example, we include each agency's share Republican in 2012 and the average crime rate from 2000-2010 as continuous variables along with all other agency characteristics. We find positive and significant heterogeneity by agency share Republican. The estimate remains similar when we include county of residence by year and exam year fixed effects and when we allow heterogeneity by officer demographics. This suggests that peer effects and agency culture may play an important role in the relationship between work and identity.[29] On the other hand, we do not find a robust relationship between

[28]Table A19 shows the same specifications but using the binary indicators from the event studies.

[29]One type of peer effect in this setting might be targeted peer pressure or harassment to induce conformity, since voter registration is public information. If non-Republican officers are subject to this treatment, we might expect them to leave policing at higher rates than Republican officers. In panel (a) of Figure A19, we show attrition rates for Republican and non-Republican officers. We break out the rate for non-Republican officers who do not switch their party affiliation and those who do switch. We see non-Republican officers who do not switch leave policing at similar rates as Republican officers, which is at odds with a targeted peer pressure mechanism. However, we do see that non-Republican officers who switch party affiliation stay longer. This could be that switching improves standing in the agency or that the longer one stays in policing the more likely they are to switch. Panel (b) of Figure A19 supports the latter explanation. It further breaks out the results by when the party switch occurs

the partisanship effects and any other agency characteristic that we consider.

Based on the estimated relationship between peer composition and the effect of passing on Republicanism, we can perform a back-of-the-envelope calculation to gauge how much of the affiliation effect can be explained by peers. First, Florida police agencies, overall, are 22 percentage points more likely to register as Republican than the general voter.[30] Second, we take this 22 percentage point gap and multiply by the estimate from the last column of Table 4. The result suggests that exposure to policing's overrepresentation of Republican peers would increase one's own affiliation with the Republican party by 0.0145 percentage points, for people who are not registered as Republican before joining. Our main effect for this group is an increase of 0.046, implying that approximately 30% can be attributed to peer composition.

7.5 Comparison to Firefighters

Finally, we compare police to firefighters to provide further evidence on potential mechanisms. This is instructive for a few reasons. First, if the effects for police are driven by changes in employment or income, we might expect similar effects for individuals who become firefighters. Second, firefighting is both similar to and different from policing in many ways. By comparing the two occupations, we can test whether the effect we find is specific to becoming a police officer. To do this, we obtained a sample of applicants taking the civil service exam for firefighting in Florida. We match this sample to voter records using the process described in Section 3.2.[31] Then, we estimate equation 2 in this sample.

Using state pension records, we first show that passing the exam increases likelihood of employment as a firefighter (see Figure A20).[32] However, we do not find that employment as a firefighter increases likelihood of Republican party affiliation. In fact, we estimate an imprecise decrease in likelihood of Republican party affiliation among this group. Figure A21 shows these results. This suggests that occupations may shape political identities in various ways and with various intensities, and that our main results are unlikely to be driven solely by an increase in employment or public sector employment among the passing group.

Why do the effects differ for firefighters and police? One possible explanation, given the heterogeneity analyses in 7.4, is that fire departments are less Republican-leaning than police departments. We explore this possibility in Table A17. Descriptively, firefighters who ultimately

and shows that individuals who switch in years 3+ are more likely to be employed even in year 2.

[30]The police in our exam sample are only 13.7 percentage points more likely to register as Republican than the general voter because they are, on average, younger and have been employed for fewer years.

[31]However, we do not observe birth year information in these records.

[32]We are using data from the Florida Retirement System. This sample consists of agencies who choose to participate in this system, and within those agencies, employees who choose to participate in the defined benefit program and have not elected to make their records private. Because these data cover a subset of firefighters, our estimated employment effect understates the true impact.

pass their exam are 3.6 percentage points less likely to register as Republican *before* the exam than police who ultimately pass their exam. However, this difference in partisanship alone is not sufficient to explain the differences between police and firefighters.[33] Ultimately, this highlights that policing's impact on political ideology is due to more than just peer composition and may be related to the specifics of police work.[34]

In Figure A22, we document several key differences between the work involved in policing versus firefighting by summarizing data from the O*NET survey. Specifically, we compare the reported importance of various work activities for each occupation. Police report relatively more importance for activities such as, "resolving conflicts and negotiating with others", "selling or influencing others", and "performing for or working directly with the public," Firefighters, on the other hand, place more emphasis on, "repairing and maintaining mechanical equipment", "handling and moving objects", and "inspecting equipment, structures, or materials."

From the descriptions above, it is clear that the day-to-day work of policing is substantively different from that of firefighting. In their O*NET responses, police emphasize interactions with the public whereas firefighters highlight physical labor. It is possible that police are more often exposed to situations that inform their world view. Information encountered on the job, however, is only one slice of relevant political information and is viewed through the lens of the occupation. For example, the nature of policing may align one's views with victims at the expense of civil liberties. One could imagine that individuals in other jobs (e.g., social work or teaching) are exposed to different information and through a different occupational lens, potentially yielding different effects on partisanship.

We provide further suggestive evidence that the specifics of police work are important in shaping one's views by borrowing from an existing survey of patrol officers conducted by Greene and Piquero (2006). The survey elicited responses from 499 Philadelphia police officers in the year 2000. From these data, we observe that 51.9% of officers endorse the statement that, "police officers have a different view of human nature because of the misery and cruelty of life which they see everyday."

[33]It is possible that police have more strongly held partisan views than firefighters. The GSS provides suggestive evidence of this. Police respondents are more likely to report that they are "strong Republican" than firefighters. See Table A18.

[34]The data available on where firefighters work is much more limited than for police officers thus restricting the ability to conduct similar heterogeneity analyses across departments. Using the Florida pension data, we infer agency employment for a subset of firefighters and we find suggestive evidence of similar Republican-affiliation peer effects that are imprecisely estimated.

8 Political Affiliation and On-the-Job Behavior

We have so far shown evidence that becoming a police officer has a causal effect on individual political behavior. However, an important motivation for understanding the ideology of bureaucrats is because of the role it may play in public sector behavior. Prior work has documented partisan differences in observable police behavior (e.g., Donahue, 2023; Ba et al., 2024; Chen, 2024). In this section, we introduce further evidence on how officers behave *on the job* and how it varies by officers' political ideology.

First, we draw on the survey of patrol officers in the Philadelphia Police Department discussed in the previous section (Greene and Piquero, 2006). In that survey, over half of officers reported that "police officers have a different view of human nature because of the misery and cruelty of life which they see everyday." Do those officers who think that policing changes one's views behave differently than the officers who do not? We explore this question by leveraging additional survey questions about: (1) actual on-the-job behavior (e.g., complaints, use of force, etc.) and (2) hypothetical ethical questions (e.g., is it okay for an officer to use force as a punishment for a suspect who tried to flee?). In Table A21, we show that officers who believe policing changes one's views are more likely to have been subject to a complaint, to have been subject to an internal affairs investigation, and to have been involved with a use of force incident. In addition, we show that those officers also give relatively more lax responses on the ethical questions, for example, agreeing that sometimes an officer must bend the facts in court to keep a criminal off the streets. Although these questions do not relate directly to political ideology, the survey evidence paints a richer picture of how officers' behavior may be influenced by the way in which the job shifts their views.

Second, we use a fourth setting, the Dallas Police Department, where we have collected a rich array of data on officer on-the-job behavior through records requests to the department and linked them to voter data. We begin with a roster of officers from 2018, which lists their name, date of joining the department, race, sex, and educational attainment. It also includes each officer's badge number, which is unique to an individual officer and which we use to link across data sources. We construct an officer-by-month panel of various outcomes, measured from 2015 to 2019, and ask how these measures vary with officer political affiliation.

To measure total patrol activity, we use a record of all 911 calls for service. These data include information on every call made to the police, including date, time, location, and all responding officers. We aggregate the location information to one of the seven patrol divisions in the city, and we calculate for each officer the modal division in which they are responding to calls in a given month, which we treat as their assignment.[35] We also construct a count of the

[35]Patrol officers in the Dallas PD are assigned to one of these seven divisions, and moves across divisions typically occur at the start of each year. Officers may sometimes respond to calls outside of

number of calls an officer responds to in each month.

To measure enforcement activity, we collected a record of all arrests made by DPD officers. These data list the arresting officers for each incident, as well as date, time, location, and various information about the arrestee and type of offense. To measure the final court disposition of an arrest, we link these data to criminal court records from the Dallas County District Attorney's Office. We conduct the linkage through the arrestee name and date of offense. Following Chalfin and Goncalves (2021), we construct a measure of conviction if the case is not dismissed and they are not found innocent by judge or jury. For each officer, we construct a measure of the number of arrests made in each month, as well as the share of arrests that lead to a court conviction.

As another measure of enforcement activity, we have a record of all instances of use of force, again with information on date, time, and all involved officers. We construct an officer-by-month count of all force incidents. Finally, we have a record of all civilian complaints against officers, which records each officer name, the date of the alleged incident, the type of allegation, and the final case disposition. We restrict attention to the cases where the allegation is sustained, and we construct a count of total sustained allegations in each officer month (which we label in our table as "discipline rate per month").

Table 5 shows the results of this analysis. In all specifications, we include controls for the number of calls the officer receives in each month, division-by-month fixed effects, and the length of time the officer has been employed. These controls allow us to compare officers who are similarly positioned in their patrol activities. Panel A includes all officers and documents stark differences in policing style by partisan identity. Republican officers make more arrests, particularly misdemeanor arrests, and are more likely to use force. However, these arrests are slightly more likely to result in a conviction than arrests made by non-Republican officers. Finally, we find that Republican officers have lower discipline rates than non-Republican officers.

Ba et al. (2024) document differences along similar outcomes in Chicago and Houston. However, they find the results are entirely driven by officer race, which is highly correlated with party. In Panels B-D, we separate the analyses by officer race to explore this possibility. Panel B shows that the results are similar when we restrict to White officers. In particular, we find that White, Republican officers make more misdemeanor arrests and are more likely to use force. When limiting to White officers, the coefficient on conviction rate is noisily estimated but similar in magnitude. We also continue to find that these officers have a lower discipline rate. The direction of the estimates is similar for Hispanic officers, but not for Black officers. Ultimately, these differences in workplace behavior by partisan identity highlight the

their assigned division, but in practice the majority (97%) of calls taken are within an officer's modal division.

importance of understanding the effect of police work on partisanship.

9 Implications for Bureaucratic Selection

We have shown that police work impacts officer partisanship and that individual partisanship correlates with on-the-job enforcement behavior. These results are important for the literature on bureaucracy as they show how bureaucratic preferences are not determined solely by selection. We consider now the broader implications of our results for the optimal selection of bureaucrats, and we address multiple questions. How should features of the police employment contract change in the presence of a work treatment effect? And could the presence of a work treatment effect change *who* the department tries to induce into employment? To answer these questions, we build on Prendergast (2007)'s model of bureaucratic preferences and selection and incorporate a novel treatment effect of government work. We present this model formally in Appendix Section B and provide here a brief discussion of the main takeaways.

This model considers the problem of a police department that must hire an officer and delegate enforcement to them. The officer will encounter suspects of unknown guilt and issue a recommendation of whether to make an arrest of the suspect.[36] The officer exerts an unobserved amount of effort, which affects the precision of their arrest recommendation. After their arrest recommendation, the department has some oversight of the decision: there is a probability that an incorrect arrest of an innocent person is overturned and a probability that an incorrect non-arrest of a guilty person is overturned. The department has preferences over the allocation: they place value on matching arrests to guilt status (the "social benefit" of enforcement), but they also place some weight on the utility of the suspect, who is harmed by an arrest both when innocent and guilty. The officer similarly values both the social benefit and the impact to the suspect of an arrest, but, crucially, they potentially hold a different weight on the suspect's utility. A "hostile" officer places a lower weight on the suspect's outcome than the department, and a "sympathetic" officer places a greater weight on the suspect's outcome. This is the key dimension of officer preferences in this model.

This model fits closely with our empirical setting, and we highlight two features. First, the notion of officer preference corresponds well with officer partisanship. Evidence from the General Social Survey shows that Republicans and Democrats differ in their preferences for punitiveness in criminal justice and in their average tradeoff between Type-I and Type-II errors. Specifically, Republicans are approximately 30% more likely to believe that courts do not deal with criminals harshly enough and 33% more likely to say that allowing a guilty person to go free is a worse mistake than convicting an innocent person. For this reason, we treat officer

[36]In the original Prendergast (2007) model, this setup is applied generically to any government employee with civilian interaction, such as social workers or IRS agents.

alignment with the suspect as the model's analogue to officer partisanship. Second, officers' degree of alignment with the suspect — the model's notion of officer preferences — is intentionally ambiguous in value; it is not ex ante obvious whether the department's utility increases or decreases with officer hostility towards the suspect. In this regard, this notion of preferences differs from other standard conceptions of bureaucratic motivation, such as "prosocial motivation" (e.g., Bénabou and Tirole, 2006) or "mission motivation" (e.g., Besley and Ghatak, 2005), where the principal's utility unambiguously increases in bureaucratic motivation.

Consequently, the first insight of the model is about which preference type is desirable for the department. The department's utility increases in officer effort, so this question amounts to asking which officer type exerts the most effort in their arrest decision. Prendergast (2007) shows that hostile officers become more desirable as 1) the arrest harm to a guilty suspect increases relative to the harm for an innocent suspect, 2) it is increasingly easy to correct wrongful arrests of innocent suspects, and 3) it is increasingly difficult to correct wrongful non-arrests of guilty suspects.[37] While our goal is not to assert with certainty that more hostile officers are desirable, these conditions are all plausibly satisfied in the typical policing setting. Guilty suspects likely suffer more from an arrest, since they face a greater chance of court conviction and incarceration.[38] In addition, arrests of innocent suspects are clearly more likely to be overturned than non-arrests of guilty suspects, since suspects only have an incentive to complain when arrested. In other words, departments often desire officers who are biased against suspects because they will "over-enforce" in a way that is easier to correct afterwards through oversight.

The second insight we take from the model concerns how the employment contract changes in the presence of a treatment effect of work. At the point of hiring, the department offers a wage and commits to their level of oversight, both of which are costly to the department. The department is unable to see individual preference type, but individuals know their own type. We incorporate a treatment effect of work by supposing that the preferences of all applicants are shifted uniformly after employment but that this effect is not internalized at the point of hiring. In order to correspond to our empirical evidence, we consider treatment effects that shift officers to be more hostile towards the suspect.[39] Holding fixed the distribution of preferences

[37] The formal condition (see appendix) is that hostile officers are desirable when $\beta > \frac{1-\rho_0}{1-\rho_1}$, where β is the suspect arrest harm when guilty relative to when innocent, and ρ_0 and ρ_1 are the probabilities of correcting an arrest of an innocent person and a non-arrest of a guilty person, respectively.

[38] While there is no empirical evidence that directly estimates the causal impact of an arrest separately by guilt status, Grogger (1995) provides evidence that the earnings impact of an arrest is more negative for repeat arrestees than for first-time arrestees, which provides an indirect piece of evidence for this proposition.

[39] The appendix discusses how the conclusions here change with a treatment effect that instead makes officers more sympathetic towards the suspect.

on the job, we show that, as the treatment effect of work increases, departments will increase the offered wage and increase oversight.[40]

The intuition here is that, as the gap between preferences on-the-job and during hiring increases, officers expect job rents to decline, and the department must raise wages to continue to satisfy the participation constraint. In addition, the most hostile officers dislike oversight most, so an increase in the treatment effect reduces individuals' expected dislike of oversight. Broadly speaking, this result shows that the optimal employment contract will depend on how the job affects preferences.

The final takeaway from the model builds on the first two and asks whether the treatment effect of work can change the type of officer that the department desires. Through their choice of oversight levels, departments can change which officer type exerts the most effort and thus gives the highest utility net of wage and oversight costs. We show that, in any case where departments optimally induce the most hostile officer in the absence of a work treatment effect, a sufficiently large treatment effect would lead the department to instead want to induce the officer most *sympathetic* to the suspect.

The intuition is that, as the work treatment effect increases, it becomes increasingly expensive for the department to induce the most hostile officer, since at the point of selection the most hostile applicants expect to receive increasingly smaller on-the-job rents. In contrast, it becomes increasingly less expensive to induce the most sympathetic applicants, since at the point of selection they expect to receive increasingly larger on-the-job rents. There is a magnitude of treatment effect where the department's utility is higher from switching to inducing the most sympathetic officer. Note from our discussion above that one way of inducing the most sympathetic officers is to increase oversight of non-arrests of guilty suspects. This type of oversight is less common, but it can be achieved through the use of complaints by *victims* of crime rather than arrestees. For example, Chicago residents can file complaints against the police for "failure to provide service" (Ba, 2020).

These model insights show the deep connection between our empirical findings on how police work shapes officer preferences and the broader literature on bureaucratic selection. Our results are not only valuable as descriptive evidence that the observed preferences of bureaucrats are partly due to the experience of work. This section shows how the presence of a treatment effect of work can also dramatically change the optimal way to conduct the hiring and selection of bureaucrats.

[40] The reason for holding fixed on-the-job preferences is to clarify that the change in wage and oversight is due to a change in the treatment effect of work rather than a change in officer type on the job.

10 Conclusion

In this paper, we document a stark difference in party affiliation between police and the general public. This is consistent with prior qualitative and quantitative descriptive work. A key question then is how much of this difference is due to selection versus treatment. Is it simply that people who choose to become police are different from people who do not wish to become police, or does the job itself shape a person's identity? We find that both channels are important. In Florida, police are 13.7 percentage points more likely to be affiliated with the Republican party than the general registered voter. Using statewide civil service exam results and a difference-in-differences design, we find that becoming a police officer increases Republican party affiliation by 5.4 percentage points. This implies that, under some assumptions, about 40% of the overall difference can be explained by treatment and 60% can be explained by selection.

We find that the positive effect of policing on Republican registration is common across nearly all counties in Florida, yet there is considerable variation in the magnitude of the effect. The former result suggests one component of the effect may be related to features of the work itself, which are common to policing statewide. The latter result suggests another component of the effect may vary with features of the agency. We explore heterogeneity in the effect size by agency characteristics, and we find that the pre-existing Republican composition of the agency plays an important role in the effect of joining on political identity. This highlights the importance of peer effects and police culture. However, we then turn to a comparison occupation, firefighters. Firefighters are roughly similar in terms of partisan composition, but we do not find that becoming a firefighter increases Republican affiliation. This corroborates our takeaway from the county-level estimates, that the specific work activities associated with policing are also a critical determinant of identity.

Since policing can alter individual beliefs and identity, this highlights the importance of understanding how beliefs and attitudes affect police performance. Existing work finds that the identities police bring to the job do have implications for performance (e.g., Miller and Segal, 2019; Hoekstra and Sloan, 2022; Donahue, 2023; Ba et al., 2024; Chen, 2024). We provide additional evidence from the Dallas Police Department that partisan identity does influence the workplace behavior of police. In addition, our main findings speak directly to policy reforms aimed at shifting police culture or recruiting police that more closely represent the general public. Although it is possible to recruit police with a different set of beliefs or identities ex ante, the job itself may pull those people closer to the existing distribution of police attitudes.

Finally, this paper contributes to a broader literature that seeks to understand the malleability of political identity and how individuals form beliefs. A separate literature has highlighted the important interaction between work and identity, documenting how political beliefs can affect performance or hiring practices across various industries and countries. We bridge these literatures and document how work itself, and in particular public-sector work, informs identity. Nearly all adults participate in the labor force at some point in their lives, and we highlight that the experiences one has at work are an important factor in shaping individual identity.

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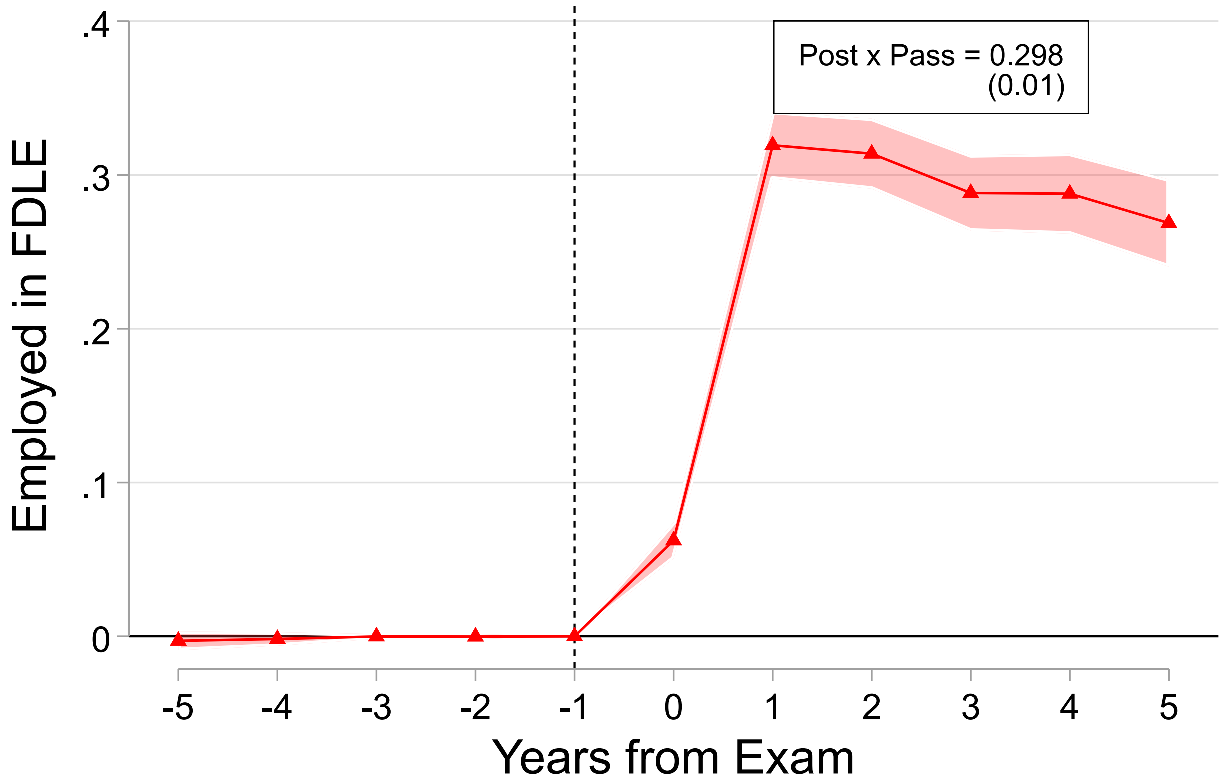
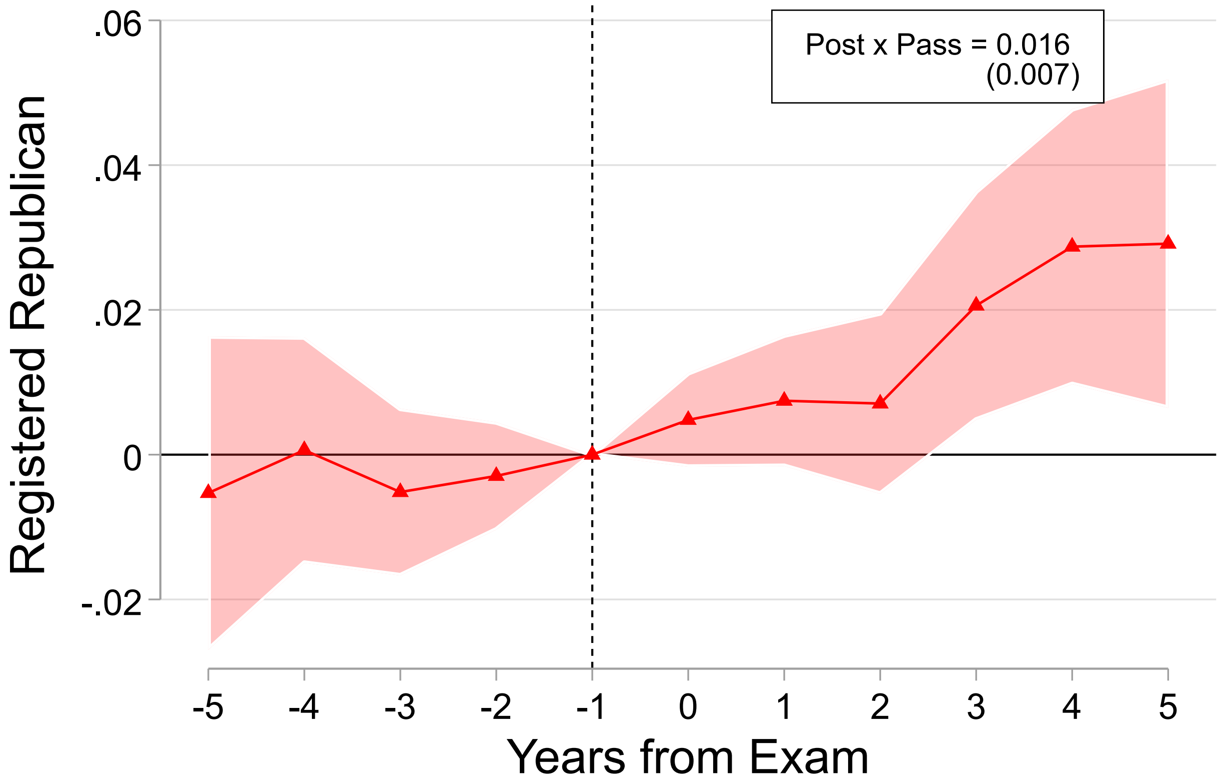
Tables and Figures

Figure 1: Effect of Passing Exam on Police Employment, Florida



Notes: This figure plots the first stage effect of passing the police civil service exam on police employment in the Florida Department of Law Enforcement (FDLE), using employment records from 2010-2021. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of employment in FDLE, but after the exam, individuals who pass are more likely to be employed in FDLE. The pooled effect estimated from equation (1) is also reported in this figure. N=173,892 and *Unique Individuals*=14,491.

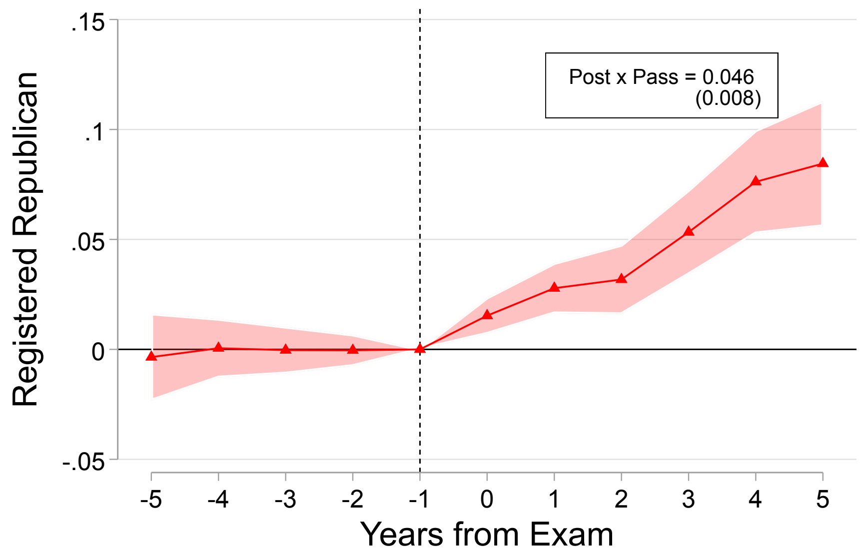
Figure 2: Effect of Passing Exam on Republican Party Affiliation, Florida



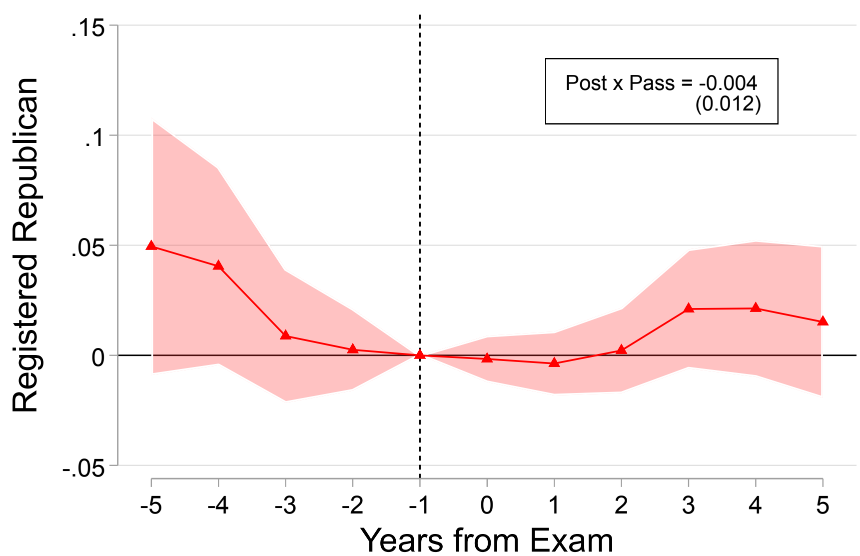
Notes: This figure plots the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of Republican party affiliation. After the exam, the individuals who pass are more likely to affiliate with the Republican party. The pooled effect estimated from equation (1) is also reported in this figure. N=119,919 and *Unique Individuals*=14,491.

Figure 3: Heterogeneity by Prior Party Affiliation, Florida

A. Not Affiliated with Republican Party in Year Before

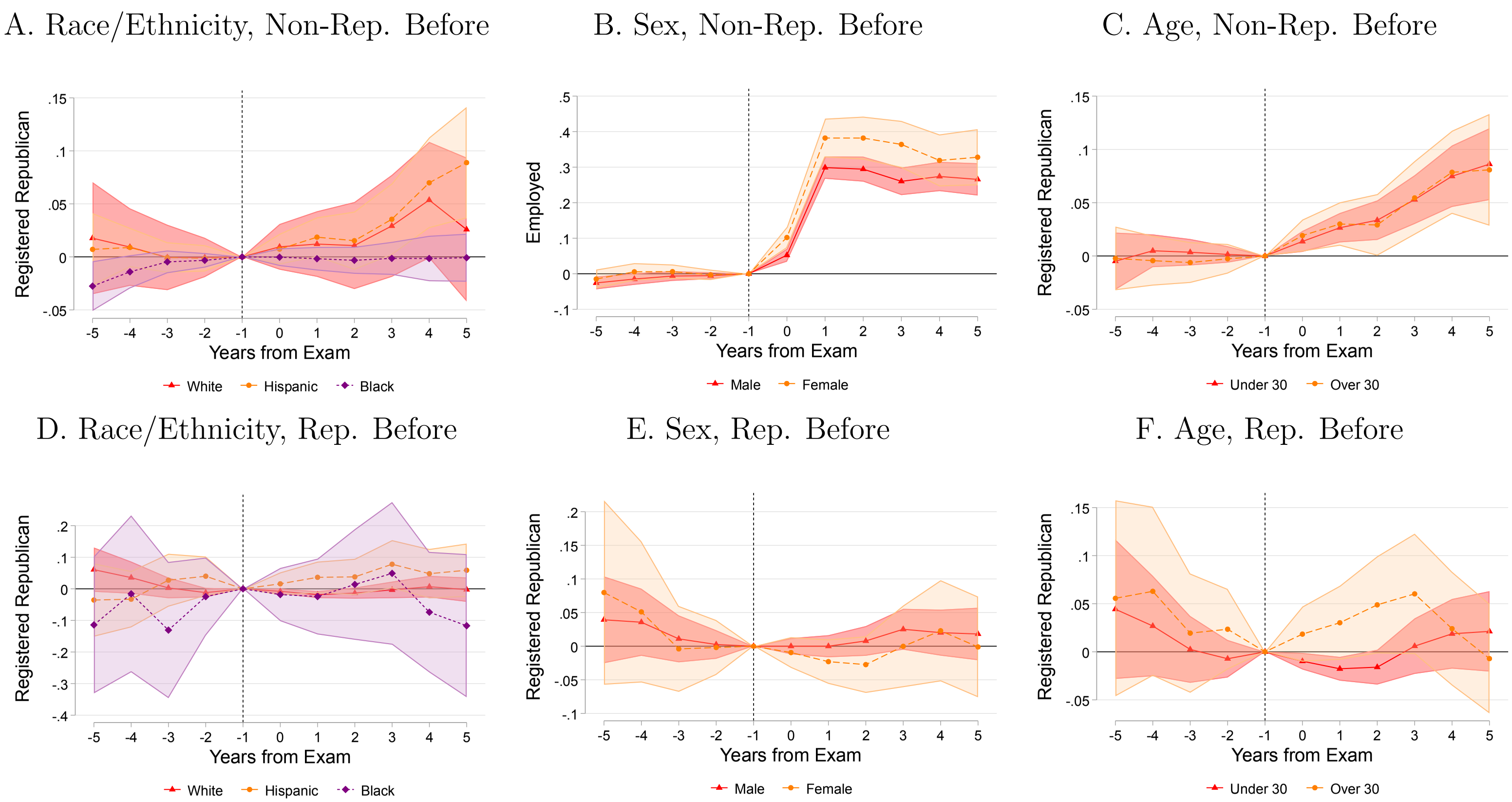


B. Affiliated with Republican Party in Year Before



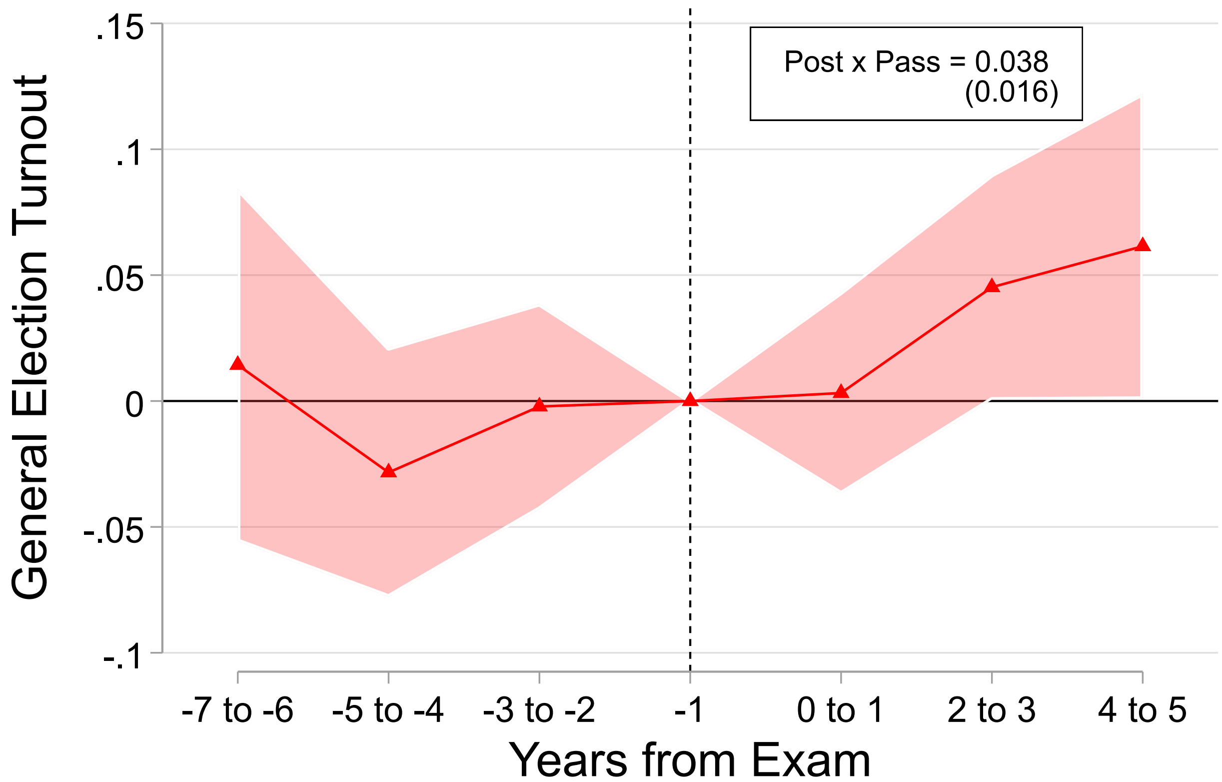
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panel (a) shows this analysis for individuals who are not affiliated with the Republican party in the year before the exam (N=72,207 and *Unique Individuals*=8,802). Panel (b) shows this for individuals who are affiliated with the Republican party in the year before the exam (N=47,712 and *Unique Individuals*=5,689). The pooled effect estimated from equation (1) is also reported in these figures.

Figure 4: Heterogeneity by Demographic Characteristics, Florida



Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panels (a)-(c) show this analysis for individuals who are not affiliated with the Republican party in the year before the exam. Panel (d)-(f) show this for individuals who are affiliated with the Republican party in the year before the exam. Panels (a) and (d) show heterogeneity by race/ethnicity, panels (b) and (e) show heterogeneity by sex, and panels (c) and (f) show heterogeneity by age. We split by prior party affiliation since it is highly correlated with these demographics. We don't find any evidence of heterogeneity by sex or age. However, we do find heterogeneity by race/ethnicity. We see an increase in party affiliation for white and Hispanic applicants but not for Black applicants. In the pooled regression for panel (a), the difference between Black and non-Black applicants is significant at the five percent level. See Table **??** for additional summary statistics on each of demographics.

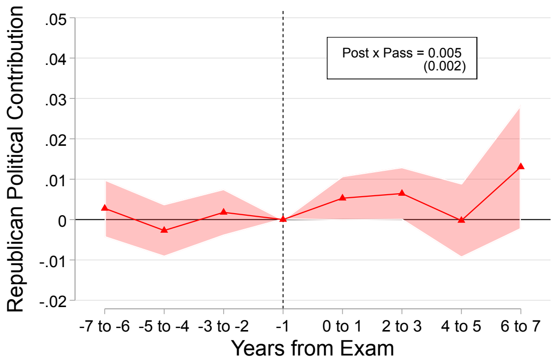
Figure 5: Effect of Passing Exam on General Election Turnout, Florida



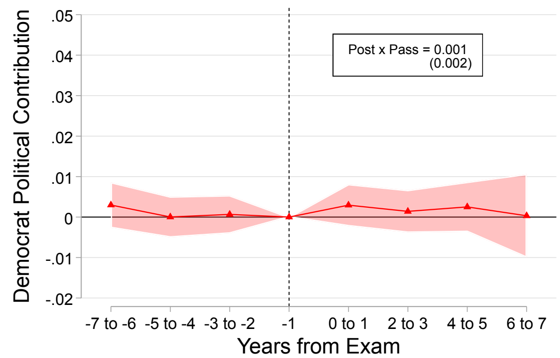
Notes: This figure plots the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. It displays coefficients estimated from equation (2) except the years from exam binary variables are two-year bins to capture election cycles. With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Since we only have turnout data through 2018, we plot one additional election cycle before the exam. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of general election turnout. After the exam, the individuals who pass are more likely to vote in general elections. The pooled effect estimated from equation (1) is also reported in this figure. N=51,976 and *Unique Individuals*=14,490.

Figure 6: Effect of Passing Exam on Campaign Contributions, Florida

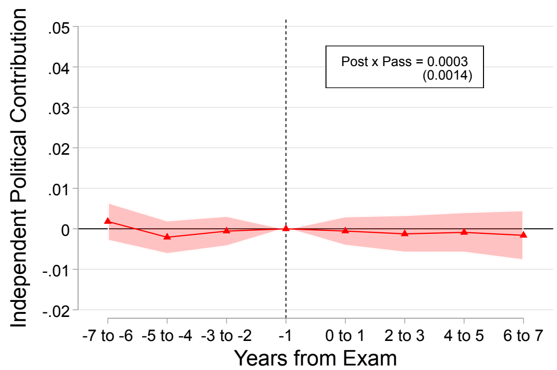
A. Republican Contribution



B. Democratic Contribution



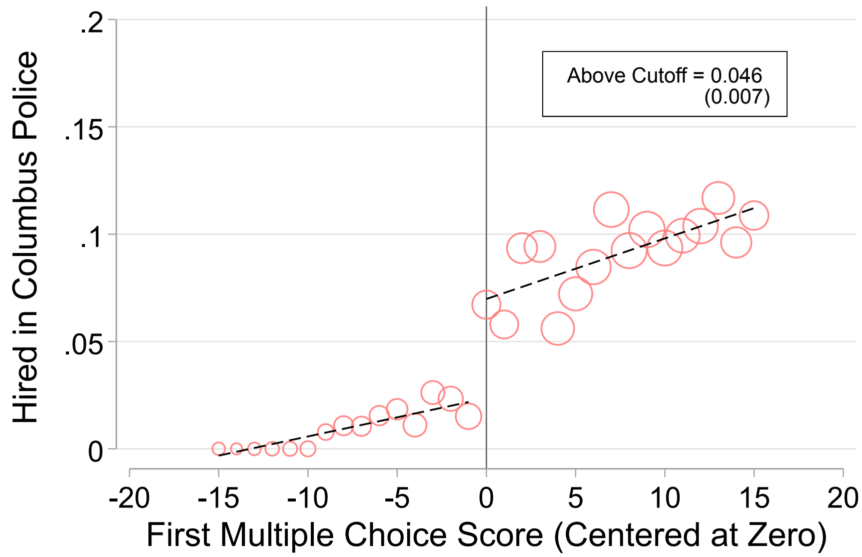
C. Independent Contribution



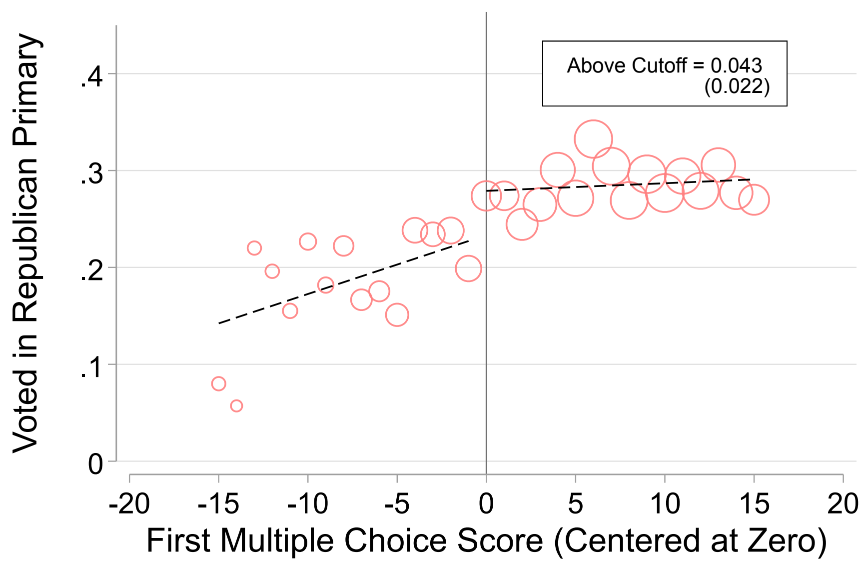
Notes: These figures plot the effect of passing the police civil service exam on likelihood of contributing to federal campaigns by party. They display coefficients estimated from equation (2) except the years from exam binary variables are two-year bins to capture election cycles. With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Since we have contribution data starting in 2010, we plot one additional election cycle before the exam. These analyses do not require matching to the voter file. Panels (a) show this analysis for contributions to Republican politicians or related political action committees, panel (b) shows this for Democratic contributions, and panel (c) shows this for Independent contributions. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms all contribution types. After the exam, the individuals who pass are more likely to contribute specifically to Republican campaigns. The pooled effect estimated from equation (1) is also reported in this figure. N=159,401 and *Unique Individuals*=14,491.

Figure 7: Effect of Passing Exam on Employment and Party Affiliation, Columbus

A. Police Employment

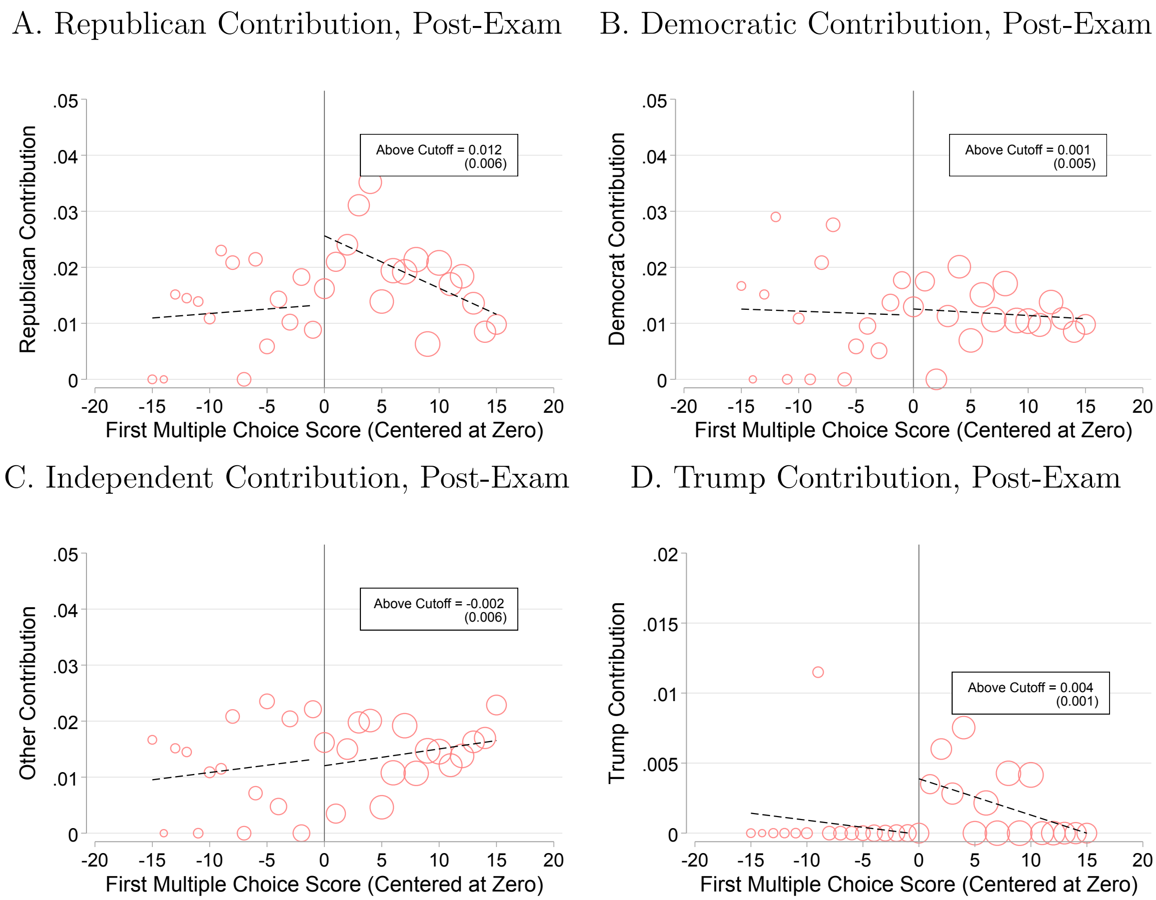


B. Voted in Republican Primary Post-Exam



Notes: These figures plot mean outcomes for individuals taking the initial civil service entry exam for police in Columbus, OH against their score on that exam. The score is centered at zero at the passing score. Panel (a) plots the relationship between exam score and likelihood of being hired as a police officer in Columbus, OH (N=11,132). Applicants who narrowly pass the exam are more likely to become police officers than applicants who narrowly fail. Panel (b) plots the relationship between exam score and likelihood of voting in at least one Republican primary after the exam (N=6,785). The resulting estimate is imprecise, but it suggests that applicants who narrowly pass are more likely to voter in a Republican primary after the exam. This is consistent with the difference-in-differences analysis of statewide exams in Florida. Both panels display coefficients estimated from equation (3) and robust standard errors. For these figures, the sample is restricted to individuals with a unique match to the voter files. See Table A9 for analyses on alternative samples and Figure A12 for analyses with alternative bandwidths.

Figure 8: Effect of Passing Exam on Political Contributions, Columbus



Notes: These figures plot mean outcomes for individuals taking the initial civil service entry exam for police in Columbus, OH against their score on that exam. The score is centered at zero at the passing score. Panel (a) plots the relationship between exam score and likelihood of contributing to a Republican federal campaign post-exam. Panel (b) studies Democratic federal campaign contributions, panel (c) studies Independent federal campaign contributions, and panel (d) examines contributions to President Trump's campaign. These results are also consistent with the difference-in-differences analysis of statewide exams in Florida. All panels display coefficients estimated from equation (3) and robust standard errors. For these figures, the sample is restricted to individuals with a unique match to the voter file or no match to the voter file (N=8,260). We exclude individual who have a duplicate match to the voter file because we have less confidence in our ability to match them correctly to the contributions data. See Figure A12 for analyses with alternative bandwidths.

Table 1: Summary Statistics, Individuals taking State Officer Certification Exam in Florida

	(1)	(2)	(3)
Republican, Year Before	0.393	0.276	0.411
	(0.488)	(0.447)	(0.492)
Democrat, Year Before	0.277	0.407	0.256
	(0.448)	(0.491)	(0.437)
Other, Year Before	0.313	0.285	0.317
	(0.464)	(0.451)	(0.465)
Turnout, Election Before	0.525	0.447	0.538
	(0.499)	(0.497)	(0.499)
Ever Employed in FDLE	0.742	0.485	0.783
	(0.438)	(0.500)	(0.412)
Age in Year Before Exam	28.34	28.96	28.24
	(7.384)	(7.951)	(7.285)
Male	0.810	0.760	0.818
	(0.392)	(0.427)	(0.386)
White	0.589	0.381	0.622
	(0.492)	(0.486)	(0.485)
Black	0.159	0.316	0.134
	(0.365)	(0.465)	(0.340)
Hispanic	0.213	0.259	0.205
	(0.409)	(0.438)	(0.404)
Republican, 2020	0.448	0.308	0.470
	(0.497)	(0.462)	(0.499)
Democrat, 2020	0.225	0.357	0.204
	(0.417)	(0.479)	(0.403)
Other, 2020	0.328	0.336	0.326
	(0.469)	(0.472)	(0.469)
Turnout, 2016	0.646	0.572	0.658
	(0.478)	(0.495)	(0.474)
Turnout, 2018	0.521	0.440	0.534
	(0.500)	(0.496)	(0.499)
Employed in FDLE, 2020	0.566	0.322	0.605
	(0.496)	(0.467)	(0.489)
Matches in Every Year	0.718	0.676	0.725
	(0.450)	(0.468)	(0.447)
Sample	SOCE Exam-Takers	Failed SOCE Exam	Passed SOCE Exam
Observations	14,491	1,990	1,2501

Notes: This table reports summary statistics for individuals taking the State Officer Certification Exam (SOCE) in Florida. We match those individuals to a panel of Florida voter files from 2012-2020. The sample is then restricted to individuals who have at least one match in a year prior to their exam year and to individuals aged 17 or above in 2012. This improves our ability to track individuals over time. Column 1 includes all individuals taking the exam, column 2 includes only individuals who fail on their first attempt, and column 3 includes only individuals who pass on their first attempt.

Table 2: Summary Statistics, Individuals taking Initial Police Entry Exam in Ohio

	(1)	(2)	(3)
Any Match to Voter Files	0.868	1	1
	(0.338)	-	-
Number of Duplicate Matches	0.908	0.971	0
	(2.716)	(2.601)	-
Votes in Rep. Primary, After Exam	0.167	0.193	0.262
	(0.373)	(0.395)	(0.440)
Votes in Rep. Primary, Before Exam	0.0544	0.0628	0.0878
	(0.227)	(0.243)	(0.283)
Votes in Dem. Primary, After Exam	0.134	0.155	0.211
	(0.341)	(0.362)	(0.408)
Votes in Dem. Primary, Before Exam	0.0529	0.0612	0.0852
	(0.224)	(0.240)	(0.279)
Black	0.182	0.177	0.189
	(0.386)	(0.382)	(0.392)
Male	0.824	0.841	0.829
	(0.381)	(0.366)	(0.376)
Min. Age of Matches	26.56	26.58	27.66
	(5.858)	(5.858)	(6.130)
Max. Age of Matches	29.49	29.44	27.66
	(6.542)	(6.527)	(6.130)
Score, Centered at Zero	4.767	4.884	4.884
	(8.912)	(8.874)	(8.915)
Ever Employed in Columbus PD	0.0762	0.0846	0.0878
	(0.265)	(0.278)	(0.283)
Ever Employed in Other Columbus Gov.	0.0339	0.0379	0.0261
	(0.181)	(0.191)	(0.159)
Sample	All Exam-Takers	All Matches	Unique Matches
Observations	12,563	10,810	7,621

Notes: This table reports summary statistics for individuals taking the initial multiple choice entry exam for police in Columbus, OH. We match those individuals to Ohio voter files from 2014 and 2021. Those files contain a history of primary voting records that we use to infer partisanship. Column 1 includes all individuals taking the exam (imputing zero for the voter file variables for individuals who do not match), column 2 includes only individuals who match to at least one voter, and column 3 includes only individuals who match uniquely to a person in the voter files.

Table 3: Party Affiliation in 2020 for General Registered Voter vs. Police Officers, Florida

	Republican (1)	Democrat (2)	Other (3)
General Population	0.332	0.369	0.300
Employed in FDLE in 2020	0.469	0.213	0.318

Notes: This table reports summary statistics for the general population of registered voters in Florida and for police employees in 2020 from our sample of exam-takers. People in our sample employed as police in 2020 are 13.7 percentage points more likely to be registered as Republican, conditional on registration, than the general registered voter in Florida. Among *all* police in 2020, not just those who took the state certification exam from 2013-2019, the share Republican, conditional on registration, is 56.7%.

Table 4: Heterogeneity by Agency Characteristics, Florida

	(1)	(2)	(3)	(4)	(5)
	Registered Republican				
Post x Pass	0.0070	0.010	0.020	0.025*	0.015
	(0.010)	(0.010)	(0.015)	(0.015)	(0.034)
... x Agency Rep. 2012	0.086***	0.063***	0.081***	0.077***	0.066***
	(0.016)	(0.019)	(0.024)	(0.024)	(0.024)
... x Crime Per Cap. 2000–10		–0.024***	–0.013**	–0.011*	–0.0067
		(0.0061)	(0.0063)	(0.0064)	(0.0064)
... x Num. Emps. 2012			–0.0042	–0.0045	–0.0017
			(0.0038)	(0.0040)	(0.0040)
... x Union			–0.018	–0.0069	–0.0069
			(0.013)	(0.014)	(0.013)
... x County BLM Protests			–0.0031	–0.014	–0.012
			(0.016)	(0.016)	(0.016)
Constant	0.045***	0.072***	0.072***	0.058***	0.073***
	(0.0032)	(0.0082)	(0.012)	(0.011)	(0.018)
Observations	72,207	72,207	72,207	72,207	72,207
County x Year x Exam Year FEs	No	No	No	Yes	Yes
Post x Pass x Off. Demographics	No	No	No	No	Yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table ??**: Standard errors clustered at the officer level in parentheses. This table estimates modified versions of equation (1) for individuals who are not affiliated with the Republican party in the year before the exam. We limit the sample to individuals who pass the exam and join an agency or individuals who fail the exam. Then, we interact the post-by-pass indicator with various agency characteristics, which are coded as zero for individuals who fail the exam. Column 1 includes Republican share in the agency in 2012. Column 2 adds agency crime per capita in 2000-2010 (standardized). Column 3 adds number of employees in the agency in 2012 (standardized), agency unionization status, and agency exposure to county-level Black Lives Matter protests in 2014-15. Column 4 adds county of residence before the exam by year by exam year fixed effects. Finally, Column 5 includes interactions between the post-by-pass indicator and officer demographics (i.e., age at time of exam, sex, and race). We find that individuals who join agencies with a higher share Republican in 2012 are more likely to register as Republican in the years after joining than individuals who join agencies with a lower share Republican in 2012.

Table 5: Relationship Between Officer Party Affiliation and Workplace Outcomes

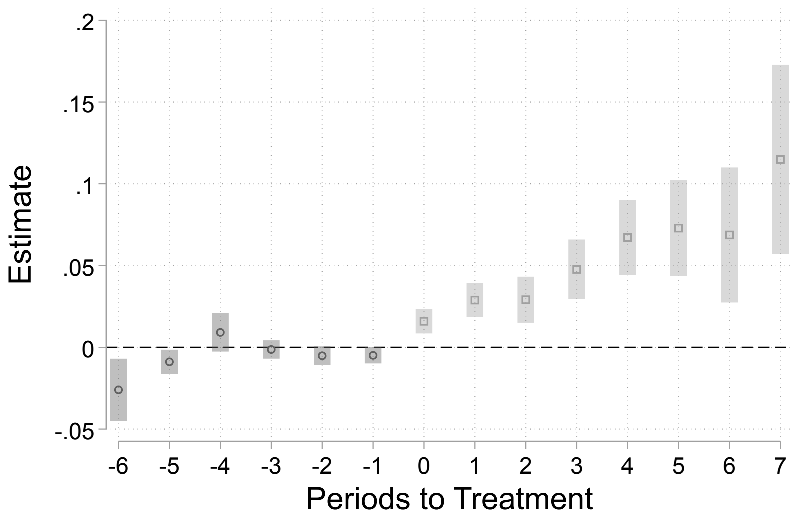
	Arrests (1)	Misd. Arrests (2)	Fel. Arrests (3)	Force Rate (4)	Conviction Rate (5)	Discipline Rate (6)
			Panel A. All Officers			
Republican	0.448***	0.367***	0.082*	0.028***	0.015**	–0.002*
	(0.156)	(0.127)	(0.042)	(0.008)	(0.007)	(0.001)
R-squared	0.150	0.146	0.081	0.077	0.051	0.028
Outcome Mean	2.509	1.983	0.527	0.094	0.338	0.007
Observations	58,393	58,393	58,393	58,393	38,572	58,393
			Panel B. White Officers			
Republican	0.436**	0.394***	0.042	0.026**	0.013	–0.003**
	(0.194)	(0.146)	(0.060)	(0.011)	(0.009)	(0.001)
R-squared	0.176	0.169	0.112	0.107	0.079	0.042
Outcome Mean	2.754	2.124	0.630	0.119	0.357	0.006
Observations	30,083	30,083	30,083	30,083	20,264	30,083
			Panel C. Hispanic Officers			
Republican	0.201	0.229	–0.027	0.003	–0.013	–0.000
	(0.483)	(0.452)	(0.080)	(0.013)	(0.017)	(0.003)
R-squared	0.187	0.182	0.154	0.151	0.152	0.108
Outcome Mean	2.699	2.182	0.517	0.092	0.321	0.006
Observations	12,477	12,477	12,477	12,477	8,752	12,477
			Panel D. Black Officers			
Republican	–0.254	–0.193	–0.061**	–0.006	–0.021	–0.003
	(0.191)	(0.167)	(0.031)	(0.006)	(0.016)	(0.004)
R-squared	0.243	0.241	0.145	0.110	0.137	0.082
Outcome Mean	1.891	1.555	0.336	0.050	0.315	0.010
Observations	15,812	15,812	15,812	15,812	9,479	15,812
Division-Month Controls	X	X	X	X	X	X
Call Volume Controls	X	X	X	X	X	X
Officer Experience Controls	X	X	X	X	X	X

Notes: * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at the division-by-month level in columns (1) through (6). Republican is defined as one if the officer has voted in any Republican primary in Texas from 1990 to 2020 and zero otherwise. Results are nearly identical if we code Republican as one if the officer has *only* voted in Republican primaries and zero otherwise.

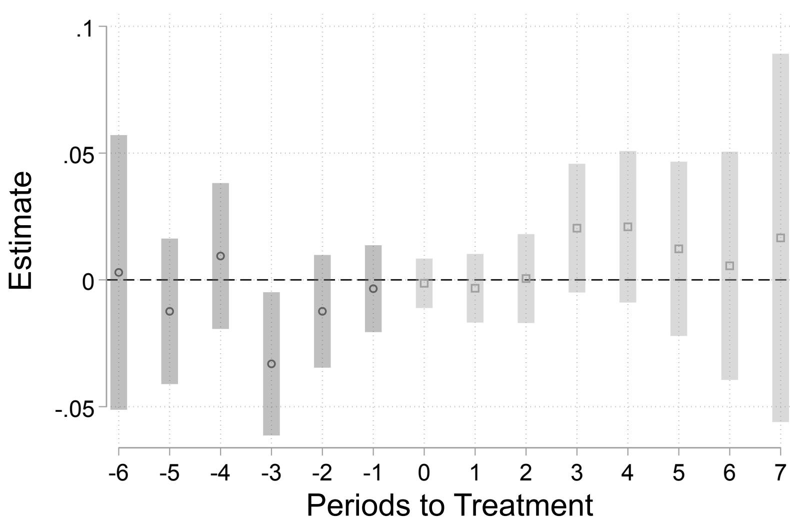
A Online Appendix

Figure A1: Heterogeneity by Prior Party Affiliation, Florida, Callaway-Sant'anna

A. Not Affiliated with Republican Party in Year Before

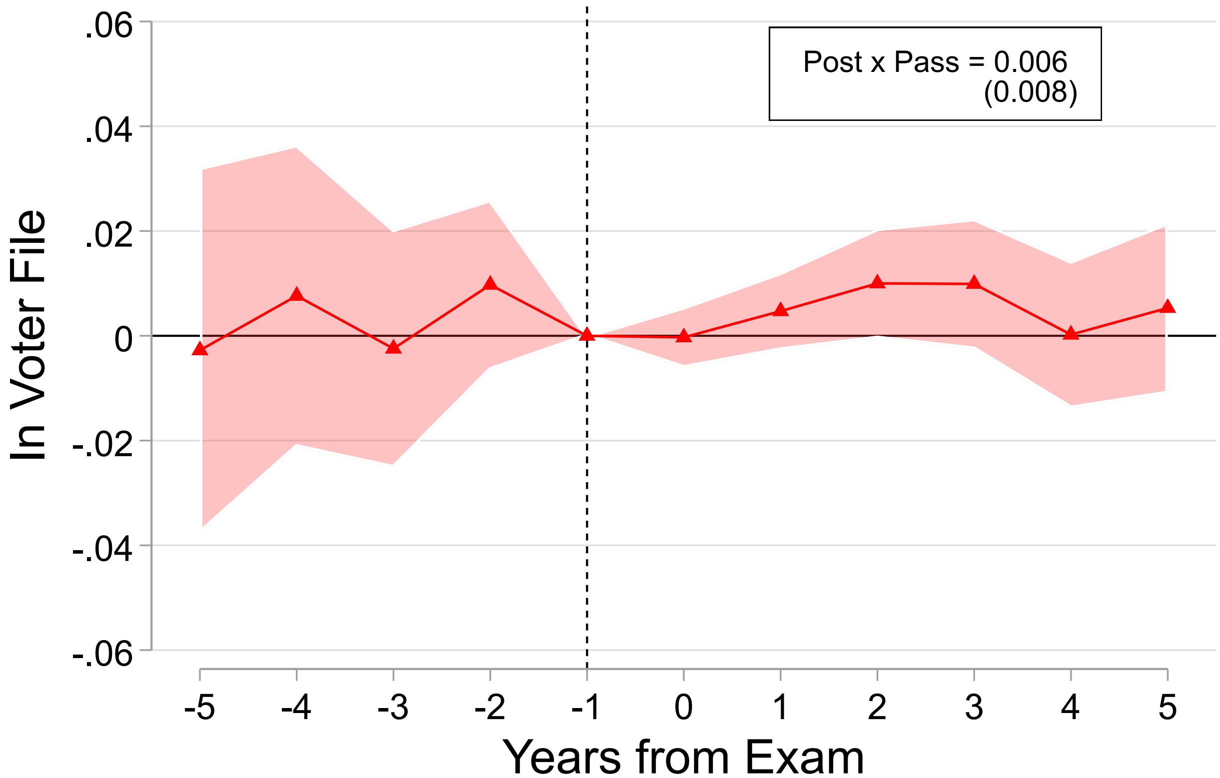


B. Affiliated with Republican Party in Year Before



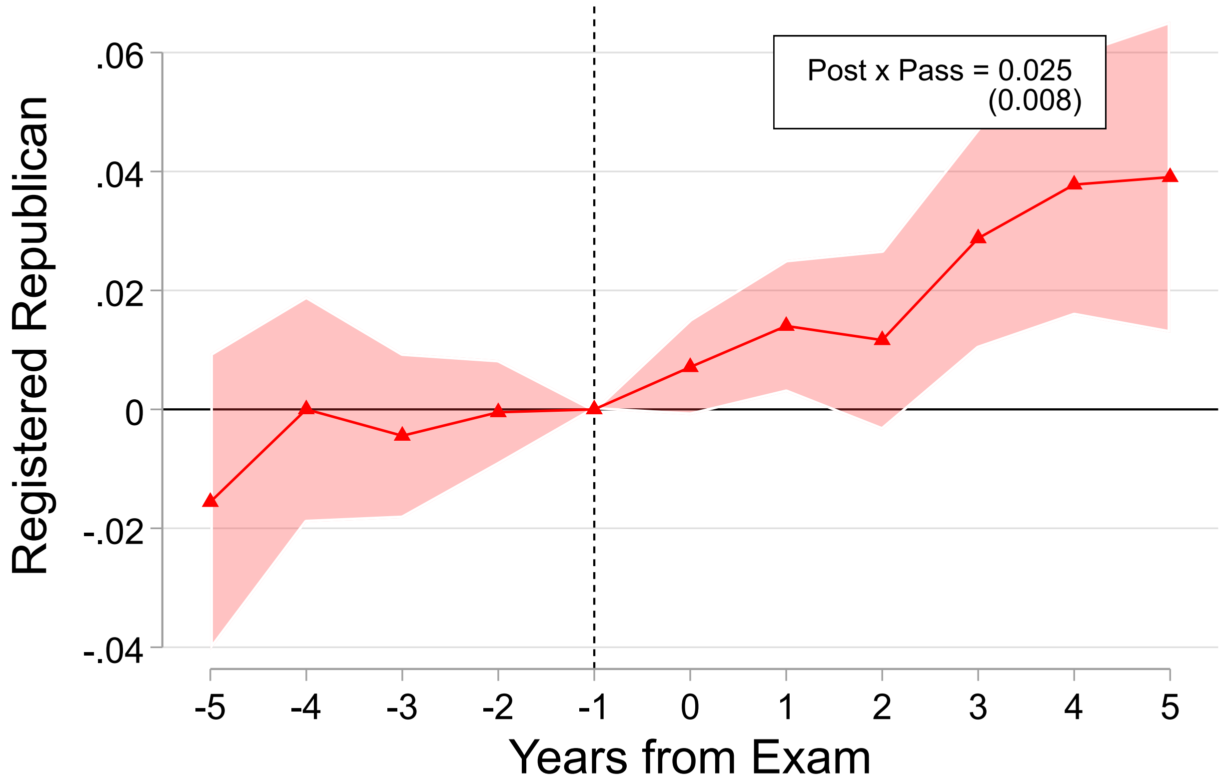
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated using the Callaway-Sant'anna approach, aggregating group-time average treatment effects into event time bins. With each coefficient, we plot the 95 percent confidence interval. Panel (a) shows this analysis for individuals who are not affiliated with the Republican party in the year before the exam. Panel (b) shows this for individuals who are affiliated with the Republican party in the year before the exam.

Figure A2: Effect of Passing Exam on Matching to Voter File, Florida



Notes: This figure plots the effect of passing the police civil service exam on likelihood that we find a match for the person in the Florida voter file. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of matching to the voter file. After the exam, these individuals continue to follow similar trends on this outcome. The pooled effect estimated from equation (1) is also reported in this figure. N=130,419 and *Unique Individuals*=14,491.

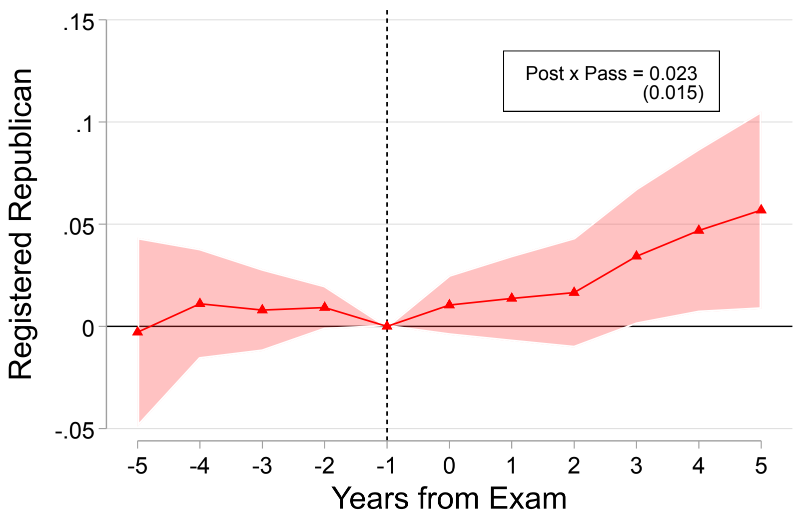
Figure A3: Effect of Passing Exam on Republican Party Affiliation, Florida, Match in 2012



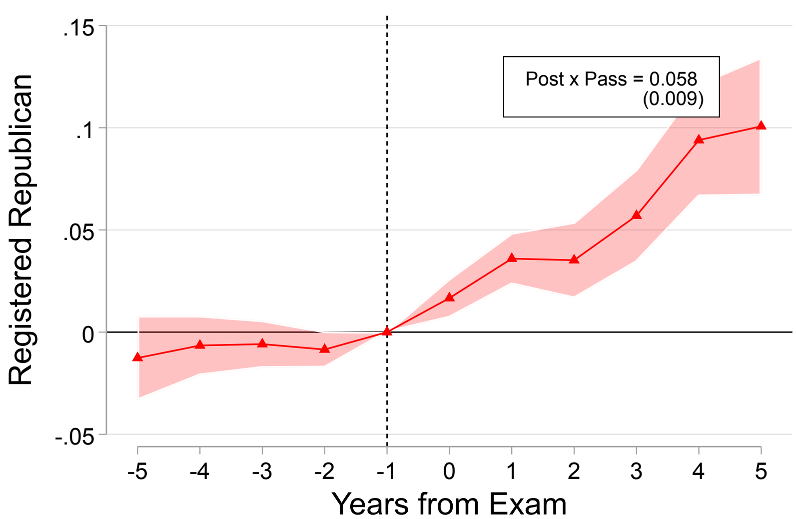
Notes: This figure plots the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. The sample is restricted solely based on having a match in the voter file in 2012, a year which is prior to the exam for all cohorts. No restrictions are made based on post-exam matching. This results in a smaller sample than restricting based on matching separately in each year. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of Republican party affiliation. After the exam, the individuals who pass are more likely to affiliate with the Republican party. The pooled effect estimated from equation (1) is also reported in this figure.

Figure A4: Heterogeneity by Prior Party Affiliation, Florida

A. Registered Independent in Year Before

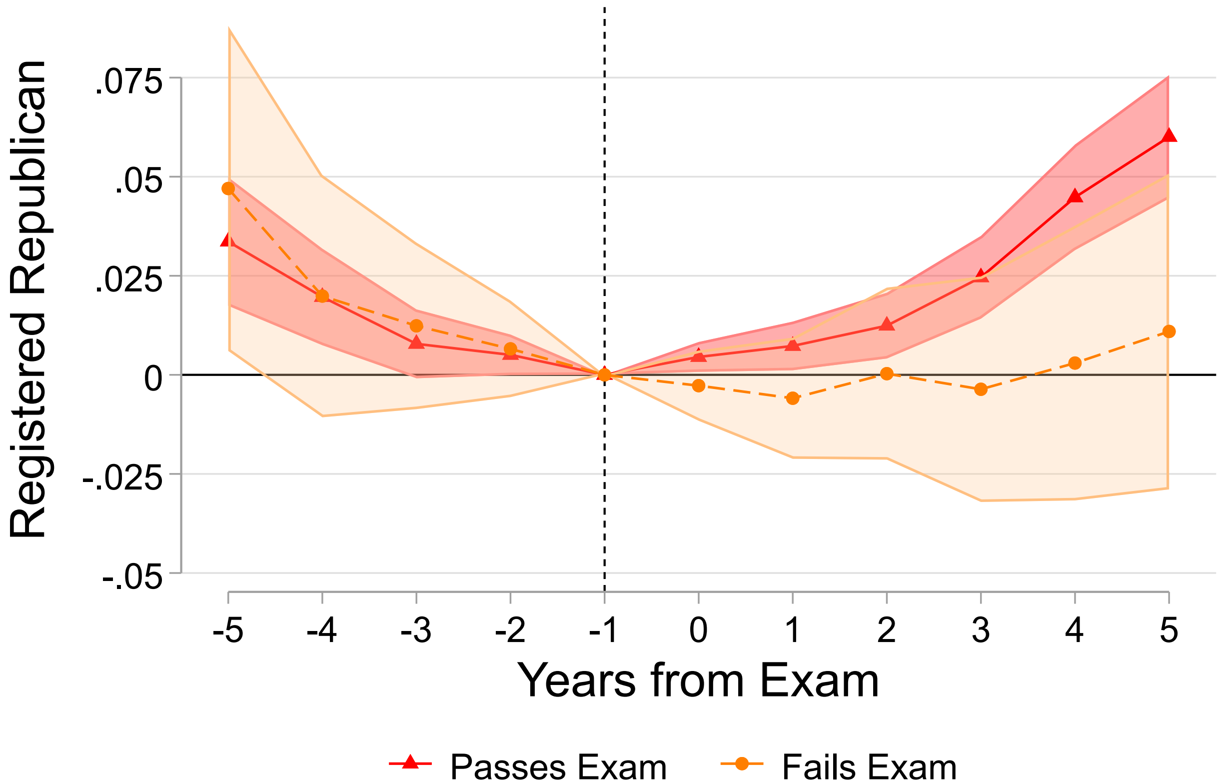


B. Registered Democrat in Year Before



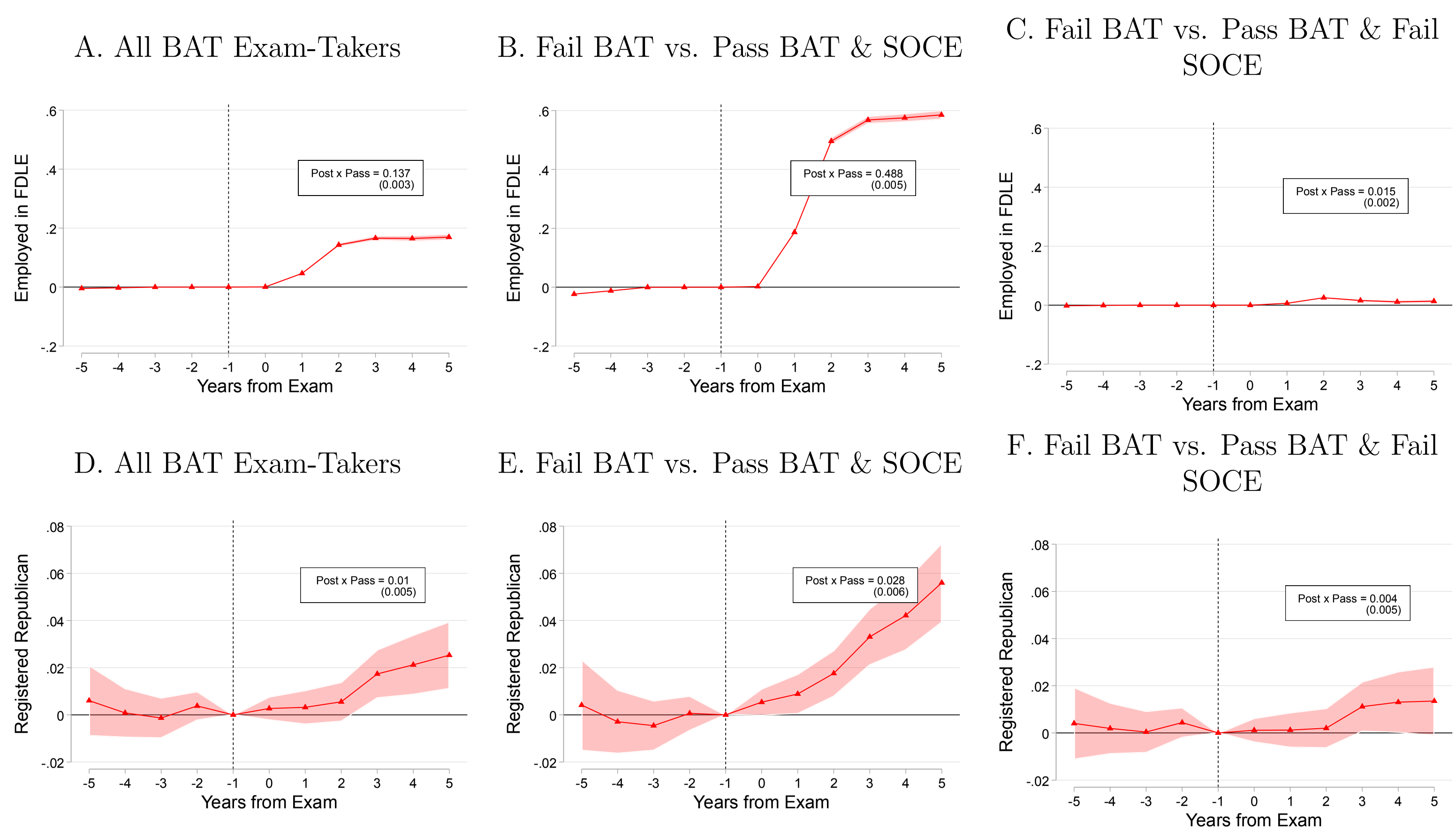
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panel (a) shows this analysis for individuals who are registered Independent in the year before the exam. Panel (b) shows this for individuals who are registered Democrat in the year before the exam.

Figure A5: Effect of Passing Exam on Republican Party Affiliation, Florida, Raw Trends



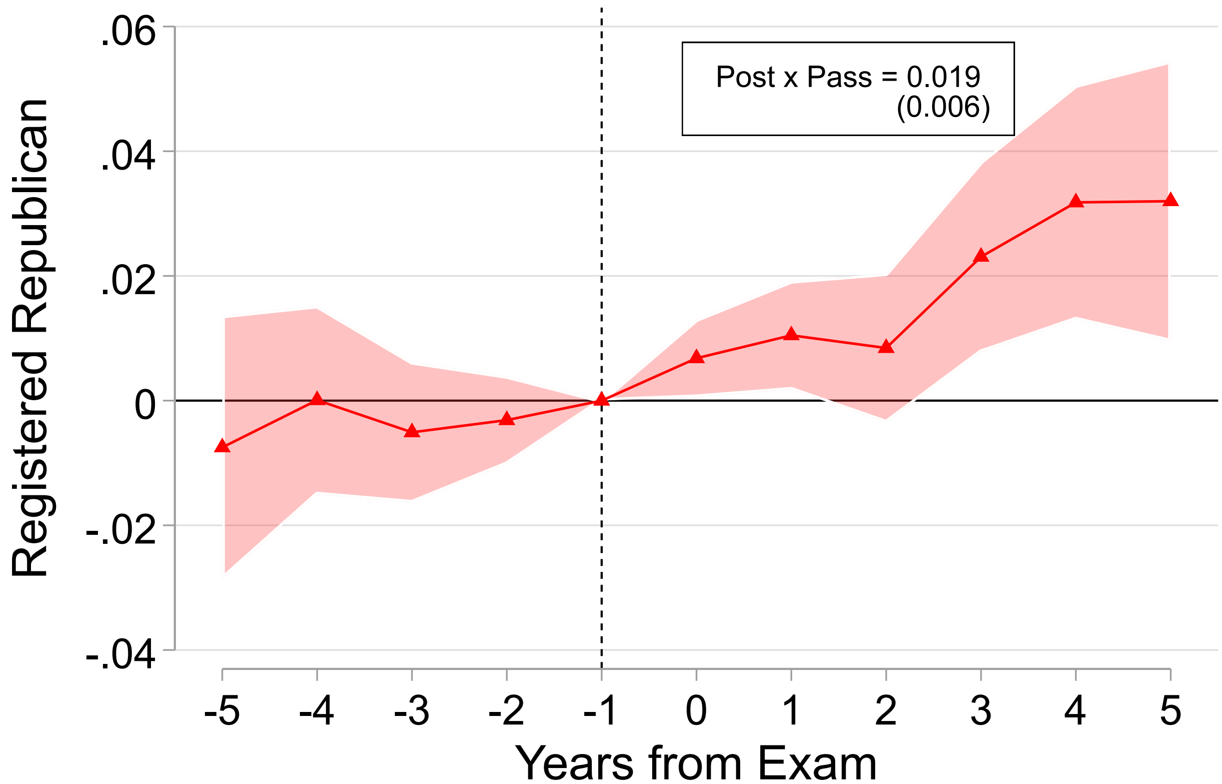
Notes: This figure plots de-trended Republican party affiliation in the years before and after the exam for individuals who pass and individuals who fail. To do this, we exstimate a regression of Republican party affiliation on relative time fixed effects and a linear trend in year that is specific to each exam cohort. With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of Republican party affiliation. After the exam, the individuals who pass are more likely to affiliate with the Republican party.

Figure A6: Effect of Passing Basic Abilities Test for Various Subgroups, Florida



Notes: These figures plot the effect of passing the Basic Abilities Test (BAT) on likelihood of employment in FDLE and likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panels (a)-(c) show the effect of passing on employment. Panel (d)-(f) show the effect of passing on Republican party affiliation. Panels (a) (N=394,692 and *Unique Individuals*=32,891) and (d) (N=269,447 and *Unique Individuals*=32,891) analyze the full sample of BAT exam-takers . Panels (b) (N=132,720 and *Unique Individuals*=11,060) and (e) (N=91,802 and *Unique Individuals*=11,060) compare individuals who fail the BAT to individuals who pass the BAT and the SOCE (the certification exam that determines hiring eligibility). Panels (c) (N=294,120 and *Unique Individuals*=24,510) and (f) (N=199,551 and *Unique Individuals*=24,510) compare individuals who fail the BAT to individuals who pass the BAT and fail the SOCE. The pooled effects estimated from equation (1) are also reported in these figures.

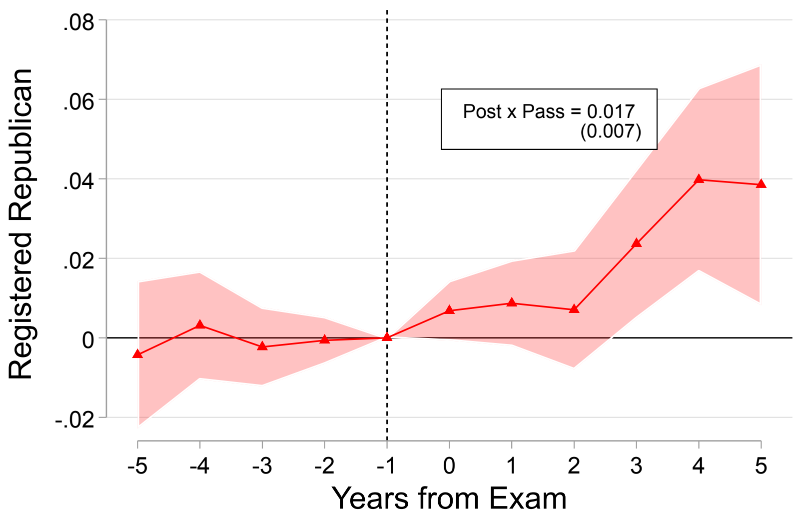
Figure A7: Effect of Passing Exam on Republican Party Affiliation, Florida, Controlling for Likelihood of Passing



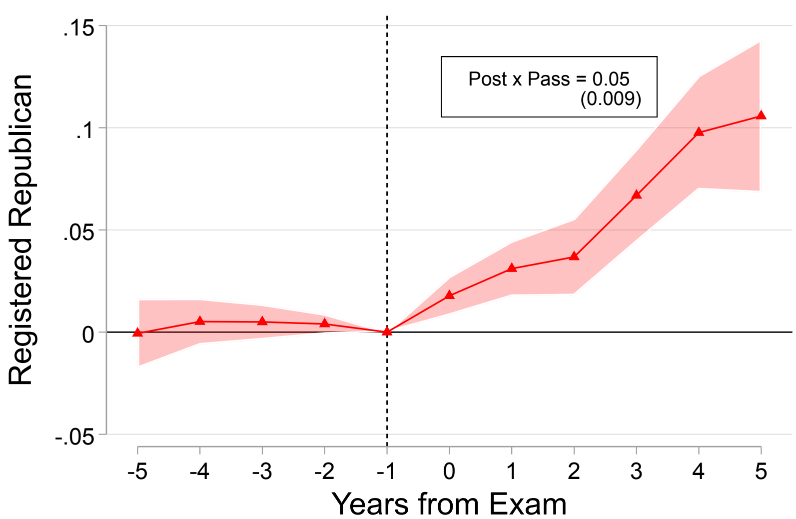
Notes: This figure plots the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. It displays coefficients estimated from a modified version of equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. For this specification, we control for each individual's estimated likelihood of passing interacted with year fixed effects. This allows registration to evolve differently over time for individuals with different propensities of passing. To predict likelihood of passing, we estimate a logistic regression of passing on race, sex, age as of 2012, age-by-sex interactions, prior party affiliation, training academy fixed effects, an indicator for passing the training academy course on the first attempt, and exam year fixed effects. We also control for year by exam year fixed effects further interacted with county of residence before the exam.

Figure A8: Effect of Passing Exam on Republican Party Affiliation, Florida, Officer Trends

A. All Applicants



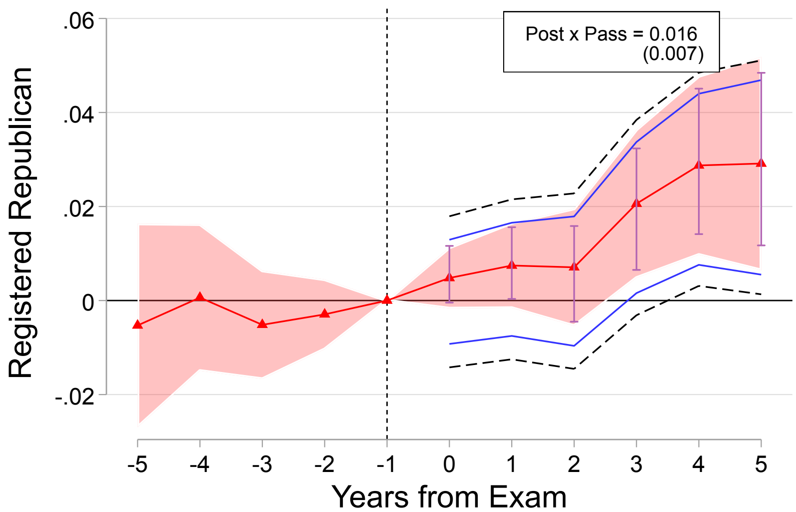
B. Not Registered Republican in Year Before



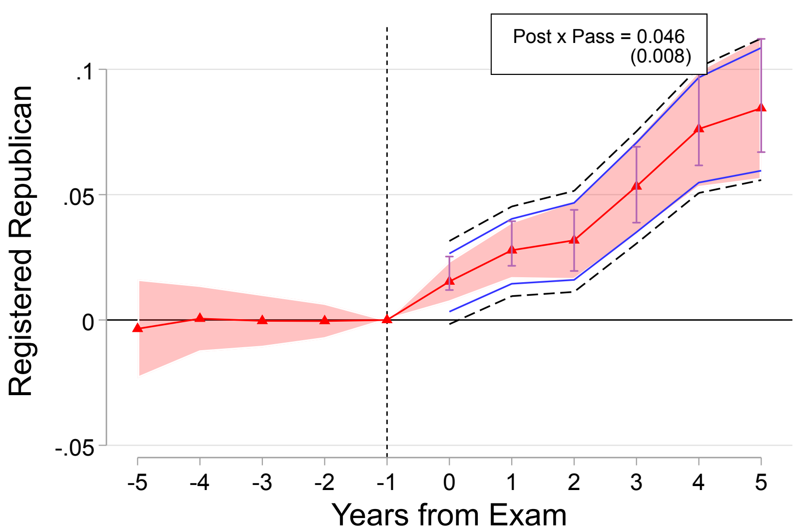
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from a modified version of equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panel (a) shows this analysis for all individuals who take the exam. Panel (b) shows this for individuals who are not registered as Republican in the year before the exam. For this specification, we control for officer-specific linear trends in party affiliation, following Bhuller et al. (2013).

Figure A9: Robustness of Main Results to Parallel Trend Violations, Florida

A. All Individuals

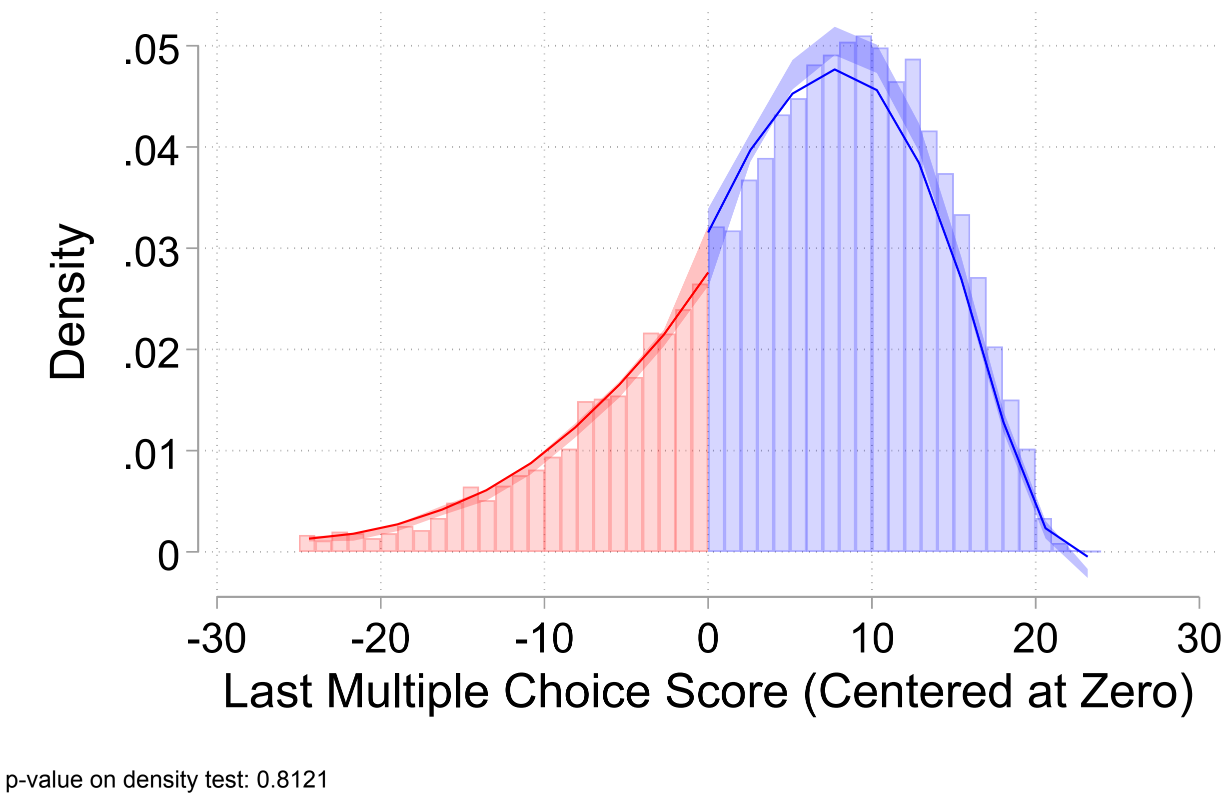


B. Not Affiliated with Republican Party in Year Before



Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panel (a) shows this analysis for all individuals. Panel (b) shows this for individuals who are not registered Republican in the year before the exam. In both panels, we add 90% confidence intervals from the approach in Rambachan and Roth (2023). The purple bars show sensitivity to linear violations of the parallel trends assumption. The blue solid lines show sensitivity to non-linear violations of the parallel trends assumption up to a value of 0.005. The black dashed lines show sensitivity to non-linear violations of the parallel trends assumption up to a value of 0.01. A value of 0.01 allows the non-linear differential trend to have a change in slope of 0.01 units, which is nearly twice as large as any coefficient from t-5 to t-2. In both figures, our main results, especially in later periods, are robust to these deviations from the parallel trends. This suggests our results are unlikely to be driven by a differential (but underpowered) pre-trend difference between the groups that pass versus fail the exam.

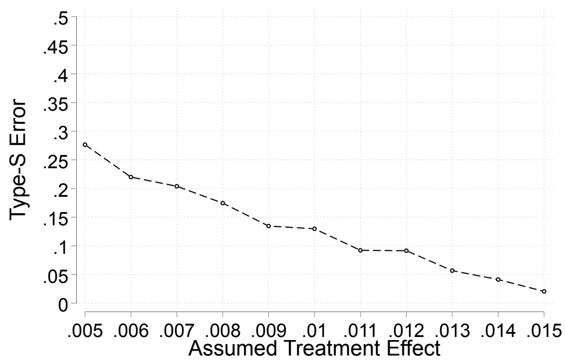
Figure A10: Density of Exam Scores, Columbus



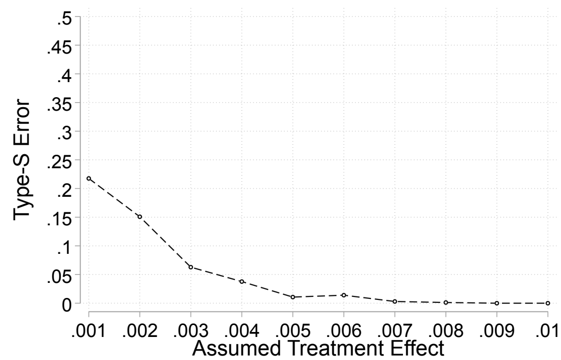
Notes: This figure plots the density of initial exam scores for the multiple choice police entry exam in Columbus, OH. It displays a histogram with width equal to one point. Scores are centered at zero at the passing score cutoff. The figure also notes the p-value on the test of the null hypothesis that there is no discontinuous break in the density of scores. This p-value is 0.8121, indicating that we cannot reject the null that there is no discontinuous break in the density.

Figure A11: Likelihood of Type-S Error by Main Outcome, Columbus

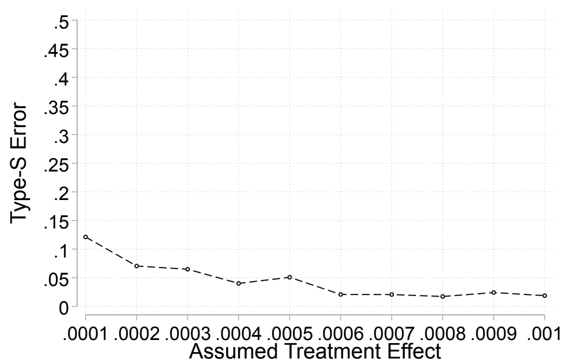
A. Republican Party Affiliation



B. Republican Contribution



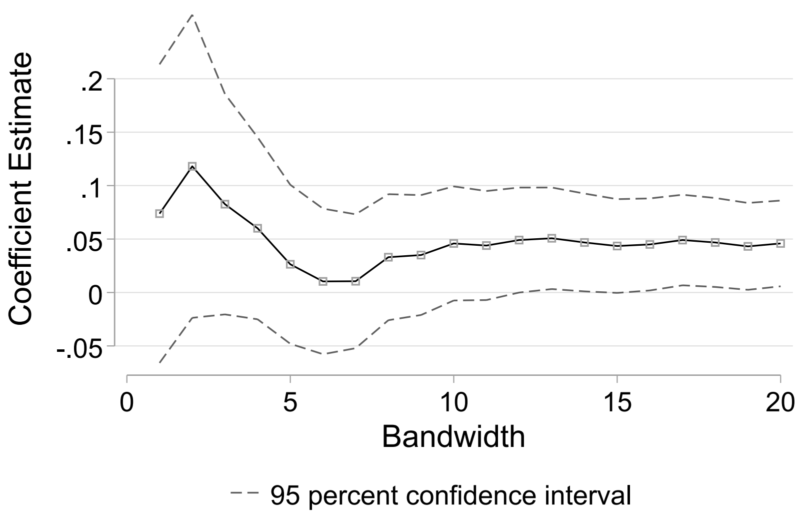
C. Trump Contribution



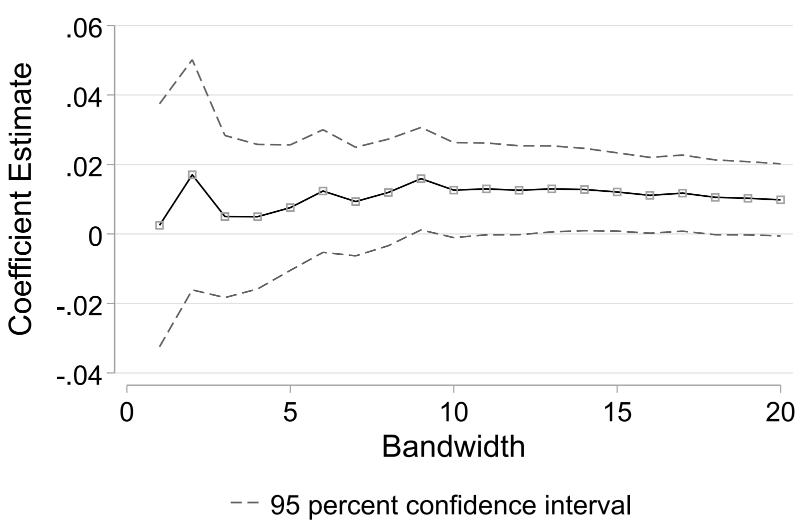
Notes: These figures plot the results of a simulation in which we assess the likelihood that, conditional on estimating a statistically significant effect, we would estimate opposite-signed results (otherwise known as a Type-S error). We do this under various assumptions about the true treatment effect, all of which are considerably smaller than the estimated effect. Assuming an effect of 0.011 on party affiliation, we would estimate a statistically significant opposite-signed effect less than 10% of the time. Assuming an effect of 0.003 on Republican contributions, we would estimate a statistically significant opposite-signed effect around 5% of the time. Finally, assuming an effect of 0.0002 on contributions to President Trump, we would estimate a statistically significant opposite-signed effect around 7% of the time.

Figure A12: Bandwidth Robustness by Main Outcome, Columbus

A. Republican Party Affiliation

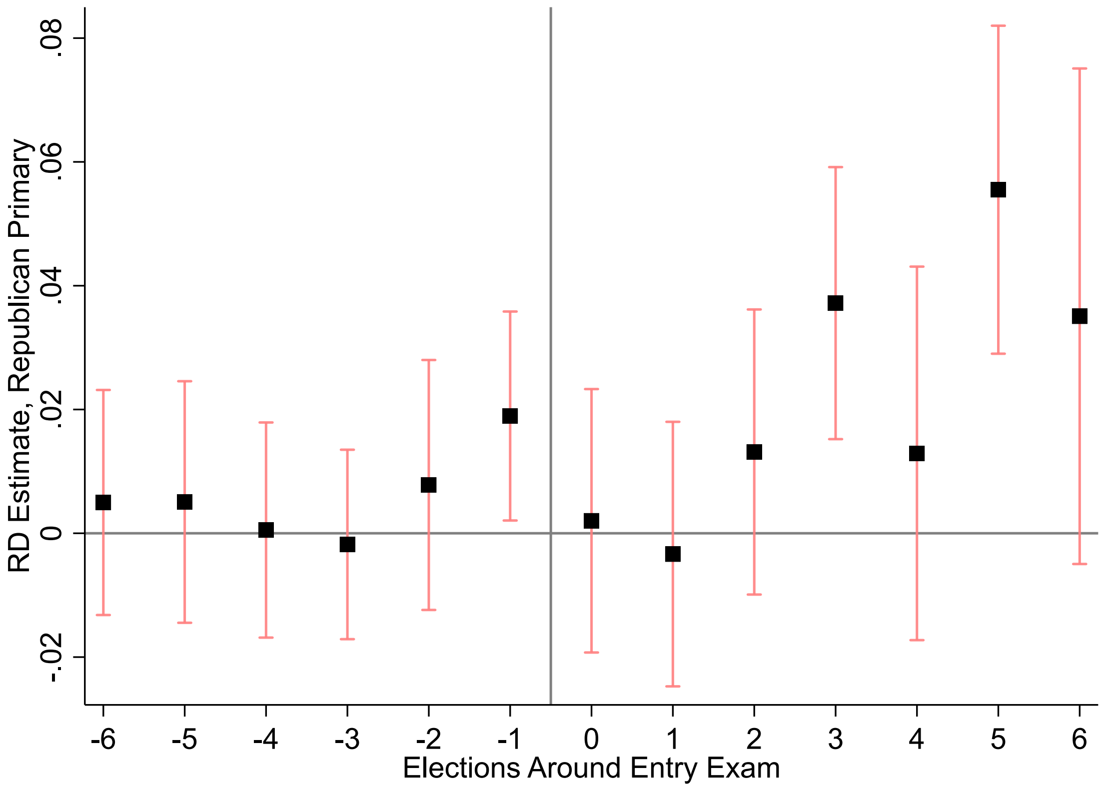


B. Republican Contributions



Notes: These figures plot point estimates and 95 percent confidence intervals from equation (3) estimated using bandwidths that range from 1-20. The main results using a bandwidth of +/- 15 are similar in size and statistical significance to almost all bandwidths from 10-20 and similar in size to many bandwidths from 1-9.

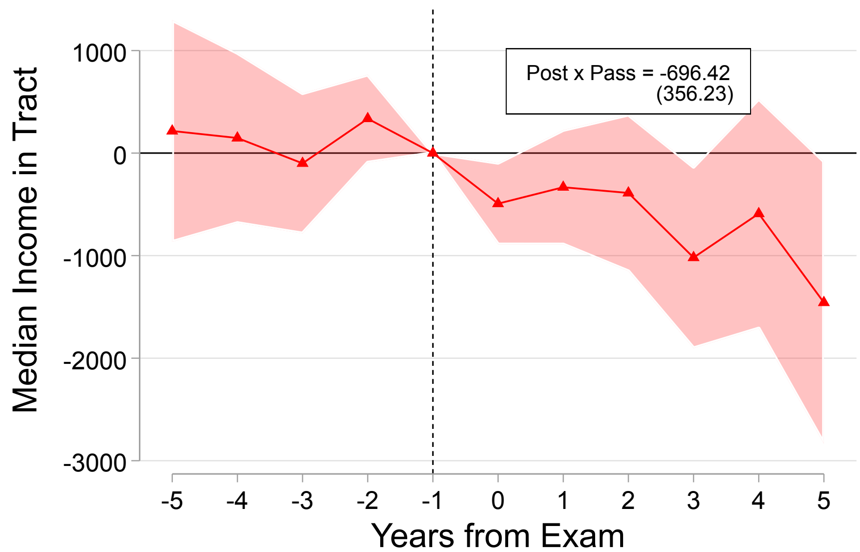
Figure A13: Effect of Passing Exam on Republican Primary Voting by Relative Election Year, Columbus, Exam Cohorts with First-Stage>0.01



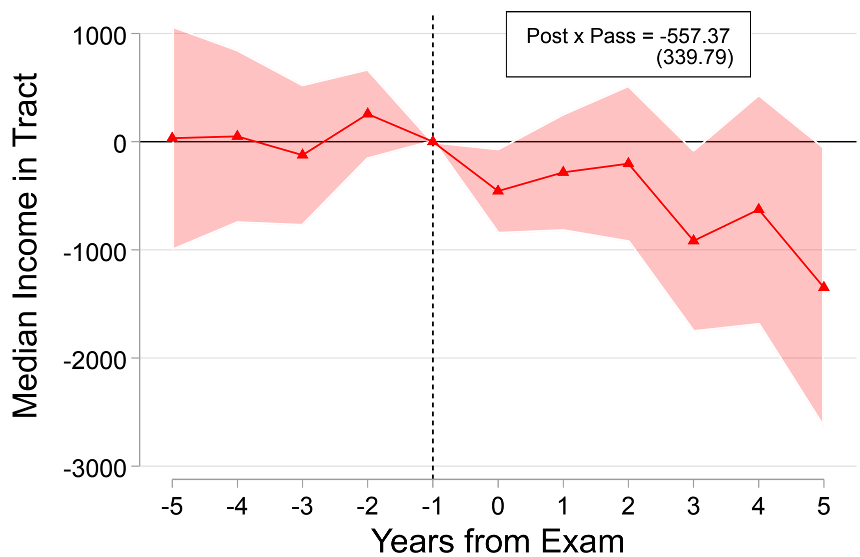
Notes: This figure plots regression discontinuity estimates of passing the exam on whether the individual votes in a Republican primary, separately by election relative to their exam year. For example, for the 2010 cohort, the 2010 primaries are Election 0, the 2012 primaries are Election 1, and the 2014 primaries are Election 2, etc. We display 95% confidence intervals for each estimate.

Figure A14: Effect of Passing Exam on Median Income in Tract, Florida

A. Without County FEs



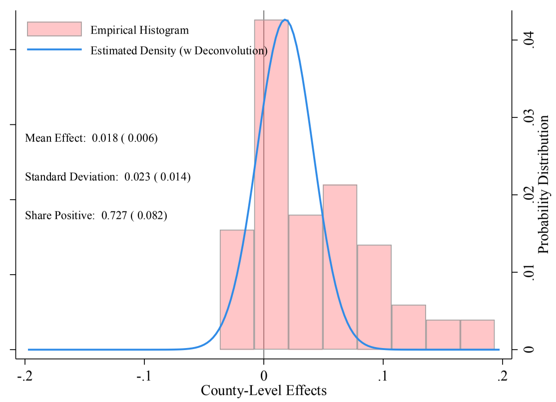
B. With County FEs



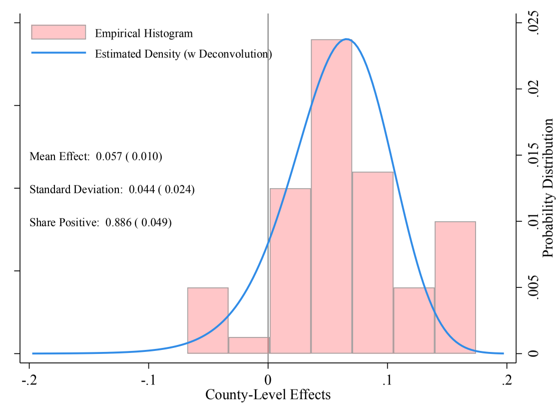
Notes: These figures plot the effect of passing the police civil service exam on median household income in the person's tract of residence. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panel (a) shows estimates from a model that does not include county fixed effects, and panel (b) shows estimates from a model with county fixed effects. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms median income in their tract of residence. After the exam, the individuals who pass live in neighborhoods with lower median household incomes. The pooled effect estimated from equation (1) is also reported in this figure.

Figure A15: Estimates of County-Level Treatment Effects

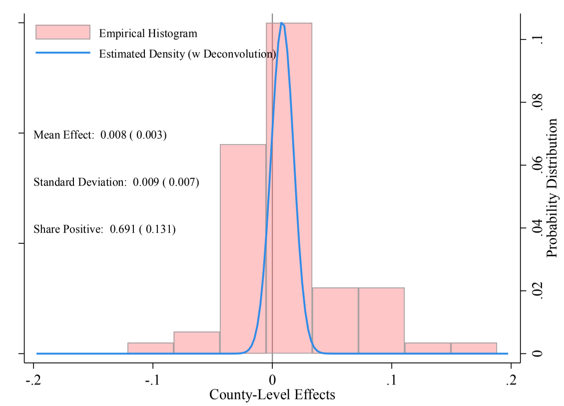
A. All



B. Not Republican Before Exam



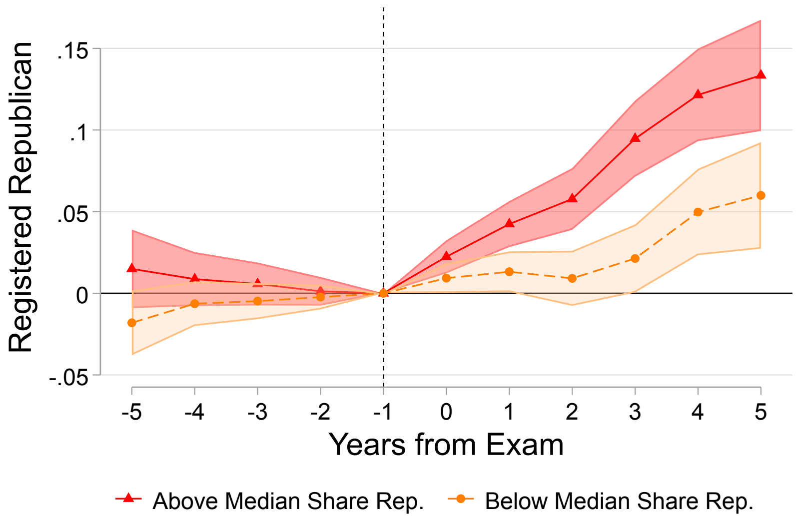
C. Republican Before Exam



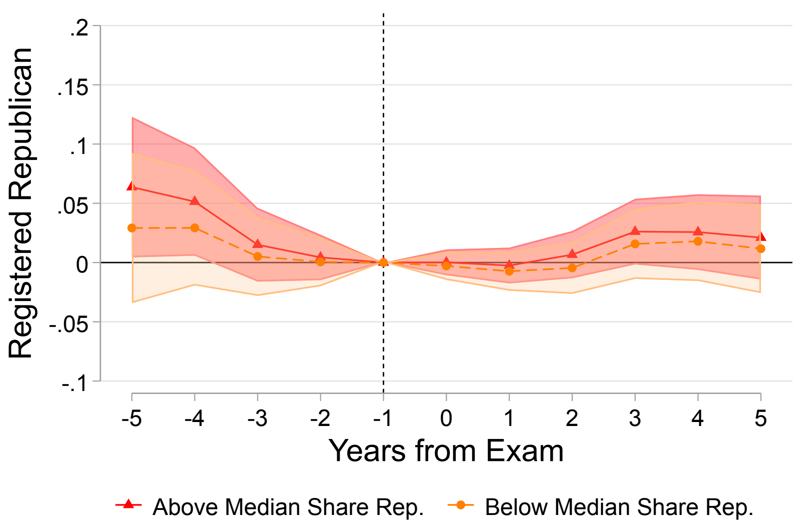
Notes: These figures plot the effect of passing the police civil service exam on Republican party affiliation, separately by county of residence in the year prior to taking the exam. Panel A estimates the effect for all test takers, Panel B restricts to individuals not registered as Republican in the year prior to the test, and Panel C restricts to individuals registered as Republican in the year prior. The density lines plot estimates of the true distribution of effects across counties, estimated using a deconvolution procedure.

Figure A16: Heterogeneity by Agency Share Republican, Florida

A. Not Affiliated with Republican Party Before Exam

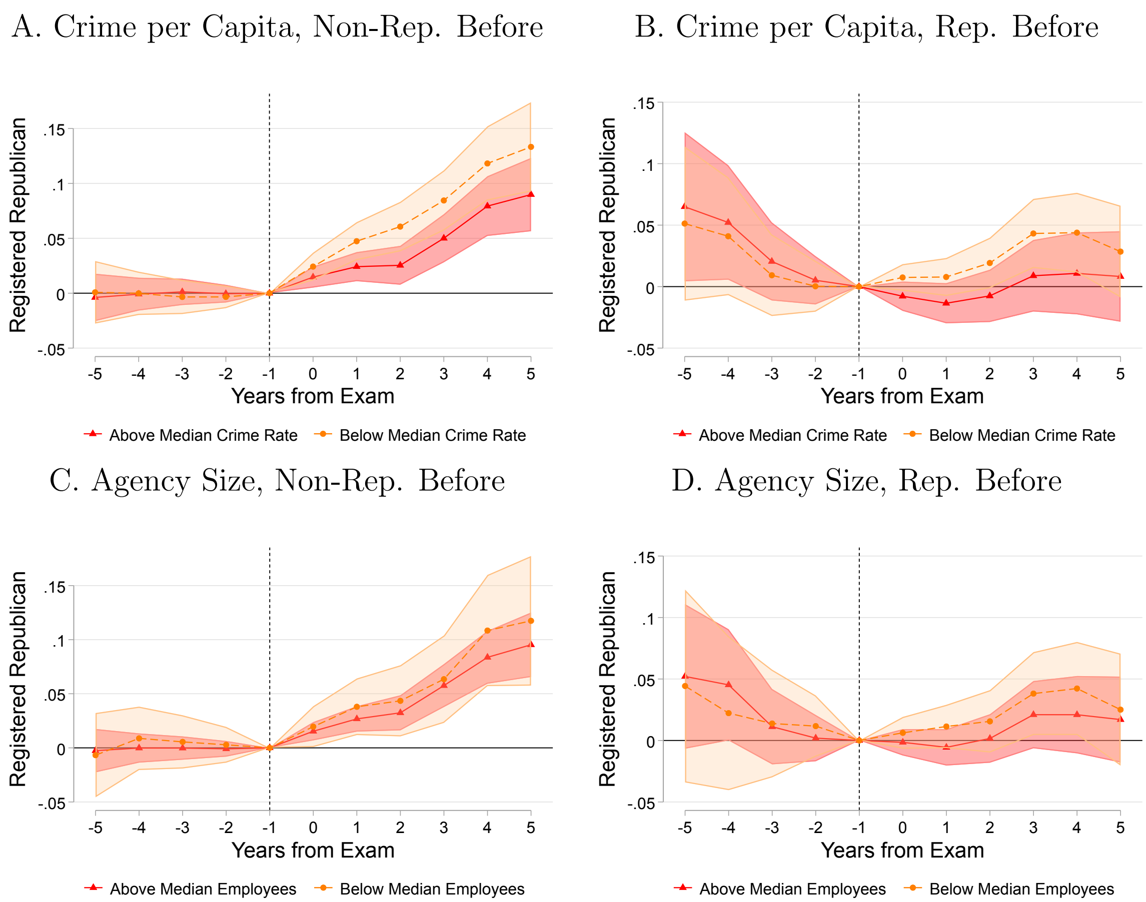


B. Affiliated with Republican Party Before Exam



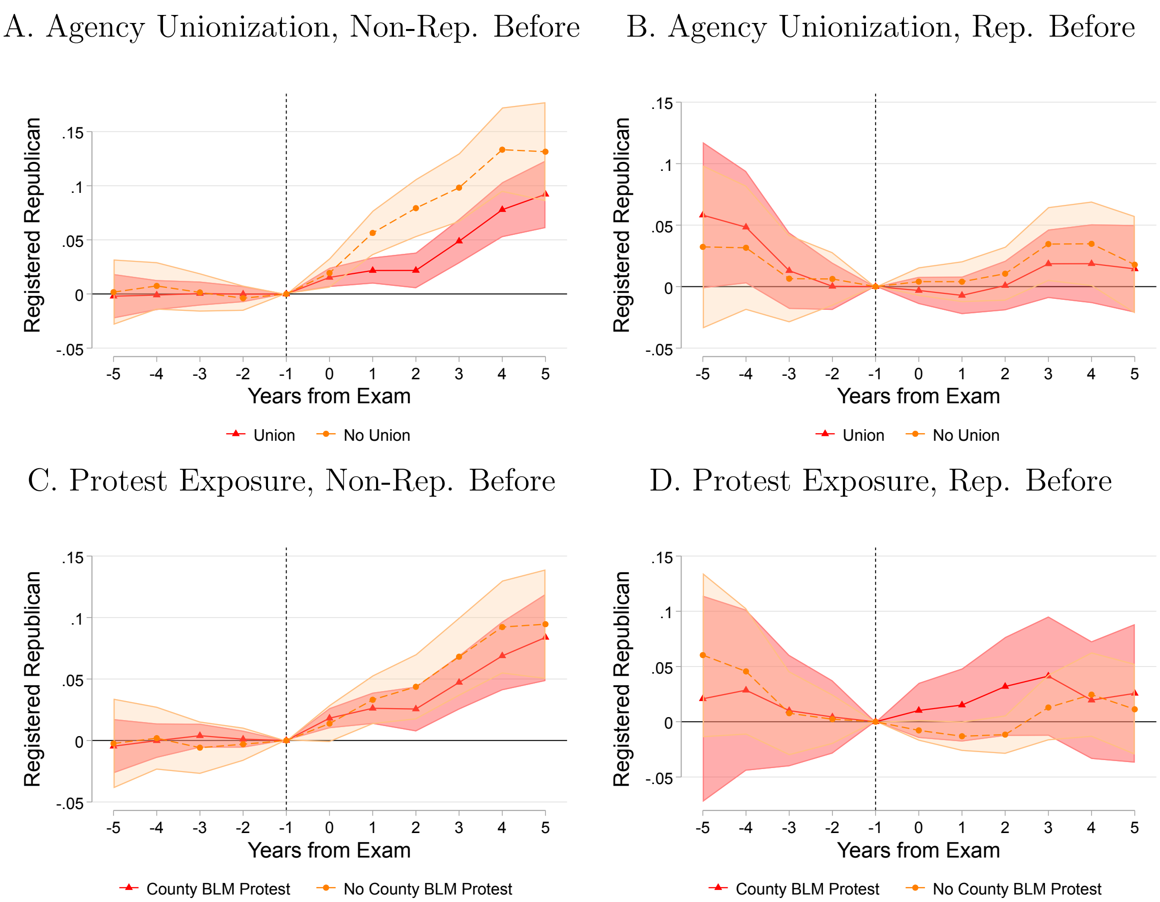
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panels (a) shows this analysis for individuals who are not affiliated with the Republican party in the year before the exam. Panel (b) shows this for individuals who are affiliated with the Republican party in the year before the exam. Both panels explore heterogeneity by share of employees in the agency who are affiliated with the Republican party in the year before the exam. Panel (a) suggests that the effects on party affiliation are stronger in agencies with a higher share of Republican employees.

Figure A17: Heterogeneity by Other Agency Characteristics, Florida



Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panels (a) and (c) show this analysis for individuals who are not affiliated with the Republican party in the year before the exam. Panels (b) and (d) show this for individuals who are affiliated with the Republican party in the year before the exam. Panels (a) and (b) show heterogeneity by agency crime rate per capita from 2000-2010. Panels (c) and (d) show heterogeneity by agency size (i.e., number of employees in 2012). In general, we do not find significant heterogeneity on these dimensions.

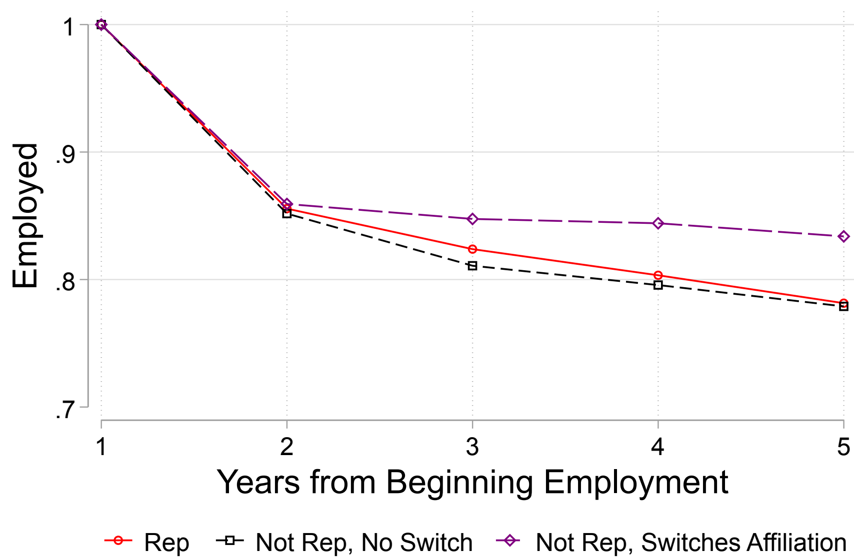
Figure A18: Heterogeneity by Other Agency Characteristics, Florida



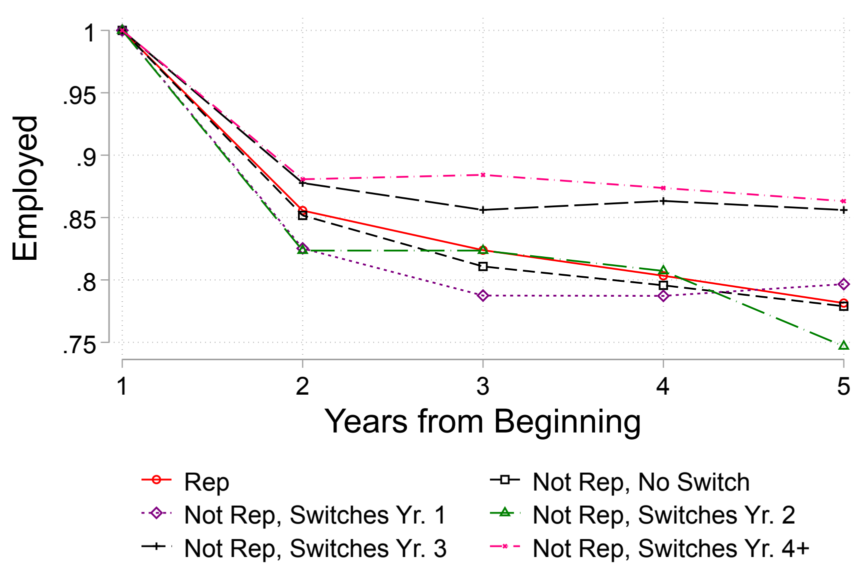
Notes: These figures plot the effect of passing the police civil service exam on likelihood of affiliating with the Republican party. They display coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Panels (a) and (c) show this analysis for individuals who are not affiliated with the Republican party in the year before the exam. Panels (b) and (d) show this for individuals who are affiliated with the Republican party in the year before the exam. Panels (a) and (b) show heterogeneity by agency unionization status. Panels (c) and (d) show heterogeneity by exposure to county-level Black Lives Matter (BLM) protests. In general, we do not find significant heterogeneity on these dimensions. Panel (a) shows stronger effects in agencies without unions, however this results is not robust to the inclusion of other agency characteristics (see Table **??**).

Figure A19: Employment Attrition by Party Affiliation, Florida

A. By Party Affiliation and Switching Status

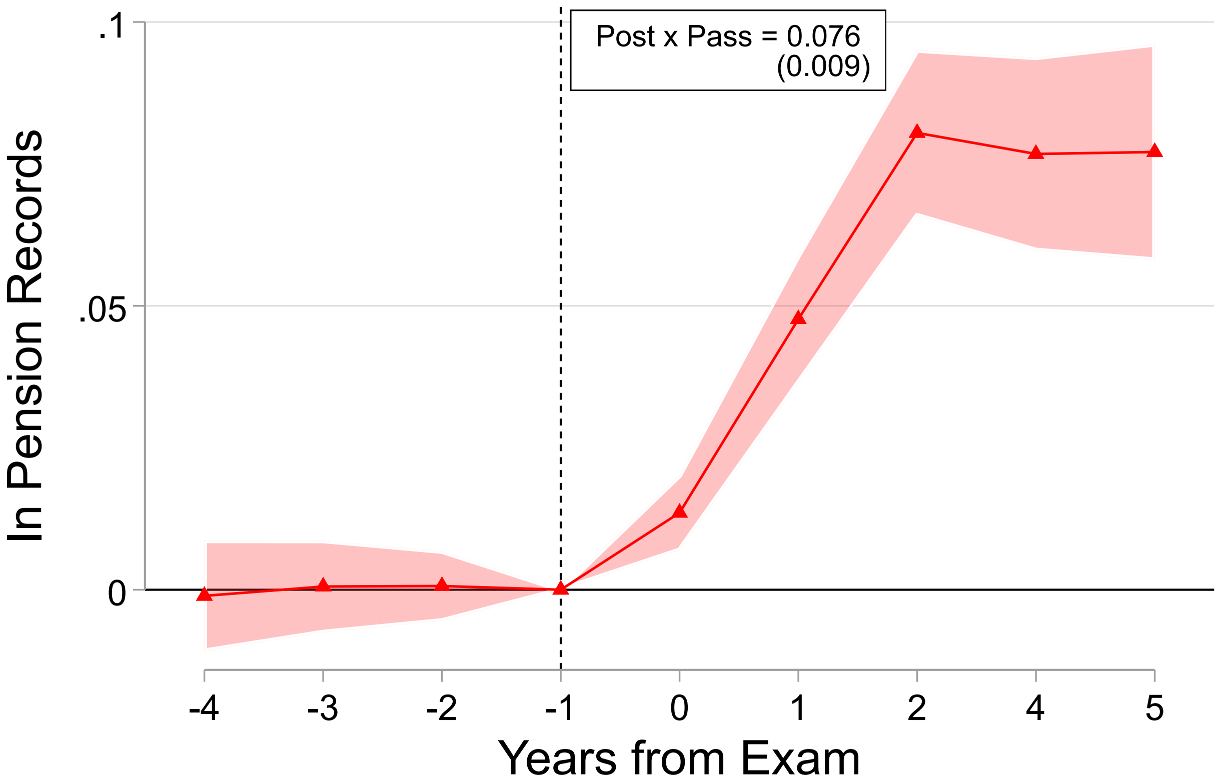


B. By Timing of Affiliation Switch



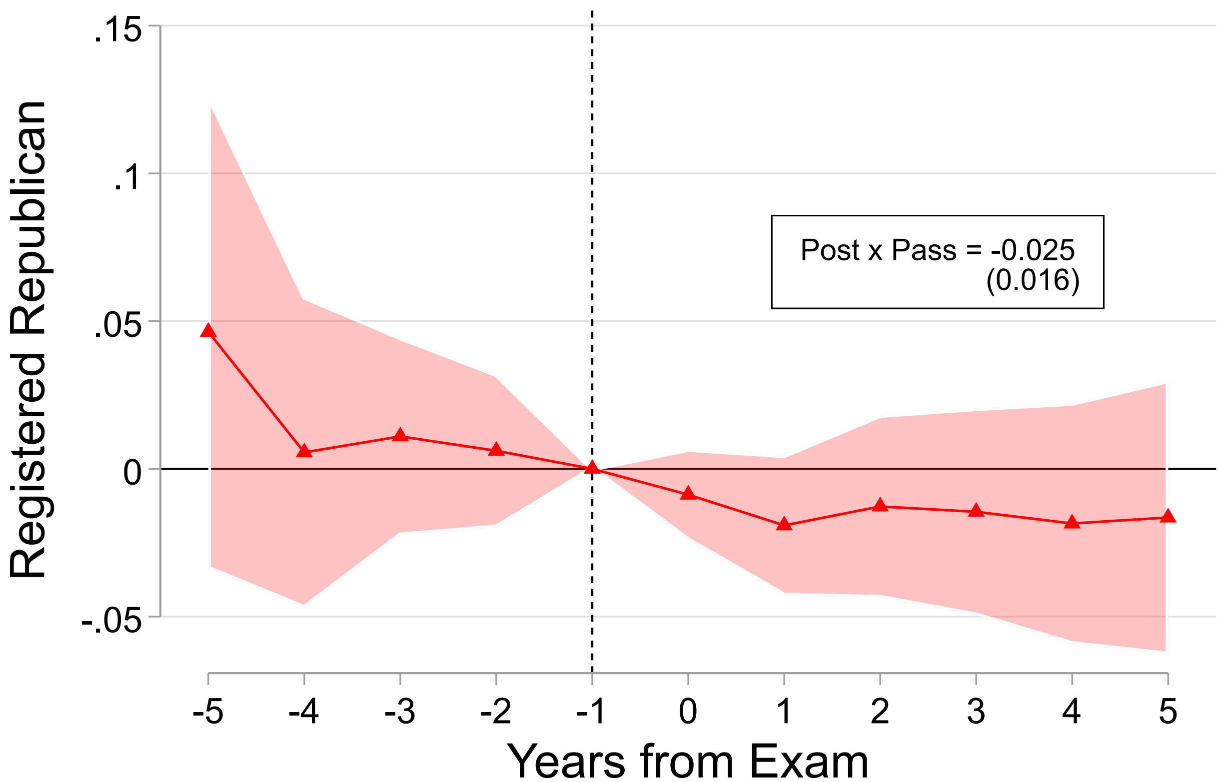
Notes: These figures plot share of police in our sample remaining employed in each year since employment start. Panel (a) shows this by party affiliation prior to the exam, breaking out the series for non-Republicans who do not switch their affiliation post-employment and those that do. Republicans and non-Republican who do not switch party are equally likely to attrit over time. Non-Republicans who switch party affiliation are more likely to remain employed. Panel (b) breaks out those who switch their affiliation based on the timing of the switch. For example, the dashed black line and the dashed pink line show attrition rates for people who are non-Republican before the exam but switch in years 3+ post-employment. These individuals have lower attrition rates even in year 2.

Figure A20: Effect of Passing Exam on Employment for Firefighters, Florida



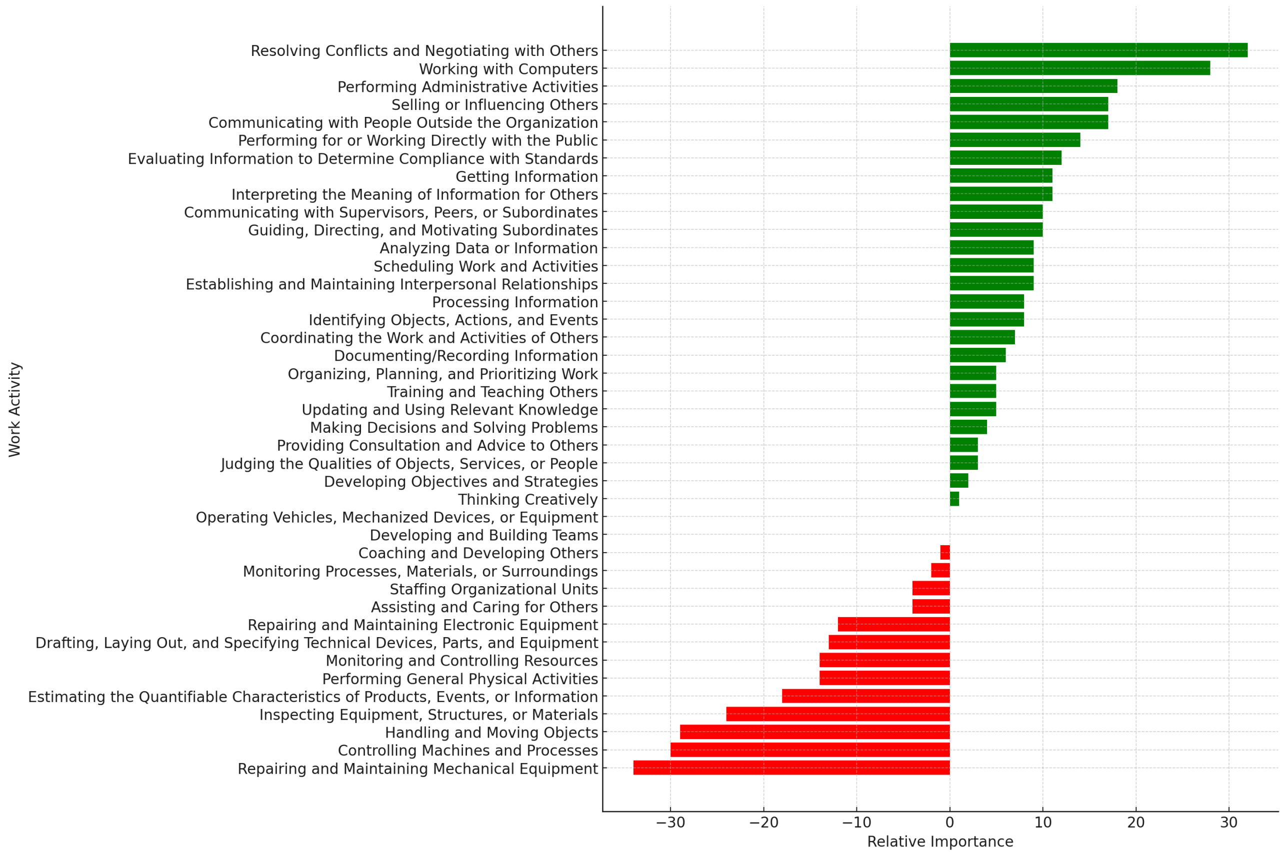
Notes: This figure plots the effect of passing the firefighter civil service exam on likelihood of having a Florida pension record as a "special risk class" employee, but not for a police or corrections agency. This designation will thus capture employment as a firefighter or emergency medical services employee. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of employment. After the exam, we estimate an increase in likelihood that applicants become employed. The pooled effect estimated from equation (1) is also reported in this figure. We use exam firefighter exam cohorts from 2005-2014 to create this figure since the pension data ends in 2015. We expect the first stage is similar for exam cohorts from 2013-2019.

Figure A21: Effect of Passing Exam on Affiliation for Firefighters, Florida



Notes: This figure plots the effect of passing the firefighter civil service exam on likelihood of affiliating with the Republican party. It displays coefficients estimated from equation (2). With each coefficient, we plot the 95 percent confidence interval, based on standard errors clustered at the person-level. Prior to the exam, individuals who pass and individuals who fail follow similar trends in terms of affiliation. After the exam, we estimate an imprecise decrease in the likelihood that applicants who pass affiliate with the Republican party. The pooled effect estimated from equation (1) is also reported in this figure. N=39,274 and *Unique Individuals*=5,840.

Figure A22: Relative Importance of Work Activities for Police versus Firefighters



Notes: This figure plots the relative importance of various work activities for police versus firefighters from the O*NET survey. For each work activity, we calculate the importance rating (from 1-100) given by police and firefighters. Then, we calculate relative importance by substracting the rating for firefighters from the rating for police. Positive relative importance (in green) represents activities that police report as relatively more important than firefighters. Negative relative importance (in red) represents activities that firefighters report as relatively more important than police.

Table A1: Previous Occupations of New Police

Previous Occupation	Share of New Police
sheriffs, bailiffs, correctional officers, and jailers	.1242
security guards and gaming surveillance officers	.0851
managers, nec (including postmasters)	.0398
private detectives and investigators	.0352
compliance officers, except agriculture	.0247
social workers	.0231
first-line supervisors of sales workers	.0207
first-line supervisors of office and administrative support workers	.0155
driver/sales workers and truck drivers	.0146
construction laborers	.0129
office clerks, general	.0124
law enforcement workers, nec	.0122
dispatchers	.012
retail salespersons	.012
customer service representatives	.0119
secretaries and administrative assistants	.0107
human resources, training, and labor relations specialists	.0104
laborers and freight, stock, and material movers, hand	.0102
janitors and building cleaners	.0088
chief executives and legislators/public administration	.0086
computer scientists and systems/network analysts/web developers	.0086
accountants and auditors	.0084
carpenters	.0081
registered nurses	.0079
community and social service specialists, nec	.0079
elementary and middle school teachers	.0078
cashiers	.0078
office and administrative support workers, nec	.0076
lawyers, and judges, magistrates, and other judicial workers	.0074
stock clerks and order fillers	.007
tax examiners and collectors, and revenue agents	.0068
chefs and cooks	.0067
operations research analysts	.0067
first-line supervisors of production and operating workers	.0064
software developers, applications and systems software	.0064
data entry keyers	.0062
sales representatives, wholesale and manufacturing	.0061
claims adjusters, appraisers, examiners, and investigators	.0061
bookkeeping, accounting, and auditing clerks	.0059
supervisors, protective service workers, all other	.0058
management analysts	.0056
firefighters	.0055
nursing, psychiatric, and home health aides	.0054
computer support specialists	.005
financial managers	.005
medical assistants and other healthcare support occupations, nec	.0049
managers in marketing, advertising, and public relations	.0049
education administrators	.0049
life, physical, and social science technicians, nec	.0048
computer programmers	.0046

Notes: Top 50 previous occupations among new police in CPS, 2000-2019. These occupations represent a combined share of 70% of new police. Shares are weighted by CPS sample weights.

Table A2: New Occupations of Former Police

Next Occupation	Share of Former Police
sheriffs, bailiffs, correctional officers, and jailers	.1222
security guards and gaming surveillance officers	.0825
private detectives and investigators	.044
managers, nec (including postmasters)	.0346
social workers	.0266
driver/sales workers and truck drivers	.0193
compliance officers, except agriculture	.0191
first-line supervisors of sales workers	.0178
secretaries and administrative assistants	.0168
retail salespersons	.0154
law enforcement workers, nec	.0147
office clerks, general	.0141
dispatchers	.014
computer scientists and systems/network analysts/web developers	.012
first-line supervisors of office and administrative support workers	.0113
human resources, training, and labor relations specialists	.011
accountants and auditors	.0104
supervisors, protective service workers, all other	.01
janitors and building cleaners	.0096
construction laborers	.0096
customer service representatives	.009
other teachers and instructors	.009
laborers and freight, stock, and material movers, hand	.0085
carpenters	.008
registered nurses	.0074
other production workers including semiconductor processors...	.0073
tax examiners and collectors, and revenue agents	.0073
secondary school teachers	.0072
office and administrative support workers, nec	.0067
management analysts	.0067
waiters and waitresses	.0066
life, physical, and social science technicians, nec	.0066
community and social service specialists, nec	.0063
elementary and middle school teachers	.0063
general and operations managers	.0062
inspectors, testers, sorters, samplers, and weighers	.0057
chief executives and legislators/public administration	.0056
financial managers	.0056
lawyers, and judges, magistrates, and other judicial workers	.0056
firefighters	.0055
food service and lodging managers	.0054
bookkeeping, accounting, and auditing clerks	.0054
stock clerks and order fillers	.0053
sales representatives, services, all other	.0052
chefs and cooks	.0051
first-line supervisors of construction trades and extraction workers	.0051
data entry keyers	.0051
claims adjusters, appraisers, examiners, and investigators	.005
first-line supervisors of correctional officers	.005
counselors	.0047

Notes: Top 50 new occupations among former police in CPS, 2000-2019. These occupations represent a combined share of 70% of former police. Shares are weighted by CPS sample weights.

Table A3: Comparing Police to Pre-Police Occupations, General Social Survey

	(1)	(2)	(3)	(4)	(5)
	Strong Rep.	Rep. or Lean Rep.	Other	Dem. or Lean Dem.	Strong Dem.
Police	0.058***	0.098***	–0.045**	–0.062**	–0.048***
	(0.021)	(0.027)	(0.019)	(0.025)	(0.019)
Constant	0.11***	0.24***	0.17***	0.31***	0.17***
	(0.0024)	(0.0033)	(0.0029)	(0.0036)	(0.0030)
Observations	54,982	54,982	54,982	54,982	54,982

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A3**: Robust standard errors in parentheses. This table compares party affiliation for police to a weighted average of pre-police occupations (derived from the CPS, see Table A1). We find police who respond to the GSS report stronger Republican identification than people who respond from common alternative occupations. For comparison, police are 15 percentage points more likely to report a Republican affiliation than respondents from these alternative occupations, and respondents from these alternative occupations are 1.3 percentage points more likely to report a Republican affiliation than the average respondent.

Table A4: Comparing Police to Post-Police Occupations, GSS

	(1)	(2)	(3)	(4)	(5)
	Strong Rep.	Rep. or Lean Rep.	Other	Dem. or Lean Dem.	Strong Dem.
Police	0.058***	0.097***	–0.042**	–0.064**	–0.049***
	(0.021)	(0.027)	(0.019)	(0.025)	(0.019)
Constant	0.11***	0.24***	0.17***	0.31***	0.17***
	(0.0023)	(0.0033)	(0.0028)	(0.0035)	(0.0030)
Observations	54,351	54,351	54,351	54,351	54,351

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A4**: Robust standard errors in parentheses. Table compares party affiliation for police to a weighted average of post-police occupations (derived from the CPS, see Table A2). We find police who respond to the GSS report stronger Republican identification than people who respond from common alternative occupations.

Table A5: Difference-in-Differences Coefficients for Yearly Outcomes, Florida

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Employed	Has Match	Republican	Republican	Republican	Tract Income	Tract Income	Republican
Years From Exam = –7	–0.013***	–0.017	0.026	0.045***	0.068	153	51	0.021
	(0.0045)	(0.032)	(0.018)	(0.015)	(0.051)	(1,381)	(1,338)	(0.020)
Years From Exam = –6	–0.0059	–0.041*	0.016	0.023**	0.076*	–525	–623	0.0067
	(0.0039)	(0.022)	(0.013)	(0.0093)	(0.039)	(710)	(689)	(0.015)
Years From Exam = –5	–0.0029	–0.0028	–0.0053	–0.0035	0.049*	216	32	–0.016
	(0.0031)	(0.018)	(0.011)	(0.010)	(0.030)	(552)	(529)	(0.013)
Years From Exam = –4	–0.0017	0.0076	0.00063	0.00053	0.040*	147	49	8.4e-06
	(0.0022)	(0.015)	(0.0080)	(0.0068)	(0.023)	(423)	(409)	(0.0097)
Years From Exam = –3	–0.000088	–0.0024	–0.0052	–0.00035	0.0088	–1.0e+02	–124	–0.0044
	(0.00082)	(0.012)	(0.0059)	(0.0054)	(0.016)	(349)	(333)	(0.0071)
Years From Exam = –2	–0.00017	0.0097	–0.0029	–0.00042	0.0025	336	256	–0.00048
	(0.00081)	(0.0083)	(0.0038)	(0.0036)	(0.0095)	(219)	(213)	(0.0045)
Years From Exam = 0	0.062***	–0.00030	0.0048	0.015***	–0.0016	–493**	–457**	0.0071*
	(0.0060)	(0.0029)	(0.0033)	(0.0042)	(0.0054)	(203)	(201)	(0.0041)
Years From Exam = 1	0.32***	0.0047	0.0074	0.028***	–0.0037	–333	–284	0.014**
	(0.011)	(0.0038)	(0.0046)	(0.0058)	(0.0074)	(285)	(276)	(0.0057)
Years From Exam = 2	0.31***	0.010*	0.0071	0.032***	0.0022	–389	–203	0.012
	(0.012)	(0.0053)	(0.0064)	(0.0080)	(0.0099)	(389)	(370)	(0.0077)
Years From Exam = 3	0.29***	0.0099	0.021**	0.053***	0.021	–1,019**	–917**	0.029***
	(0.013)	(0.0063)	(0.0080)	(0.0098)	(0.014)	(451)	(430)	(0.0094)
Years From Exam = 4	0.29***	0.00022	0.029***	0.076***	0.021	–590	–627	0.038***
	(0.013)	(0.0072)	(0.0097)	(0.012)	(0.016)	(571)	(543)	(0.011)
Years From Exam = 5	0.27***	0.0053	0.029**	0.084***	0.015	–1,457**	–1,348**	0.039***
	(0.015)	(0.0083)	(0.012)	(0.015)	(0.018)	(703)	(663)	(0.013)
Years From Exam = 6	0.27***	0.0058	0.021	0.084***	0.0079	–1,010	–930	0.040**
	(0.016)	(0.011)	(0.015)	(0.020)	(0.021)	(859)	(810)	(0.017)
Years From Exam = 7	0.26***	0.016	0.038*	0.13***	0.0011	–834	–669	0.052**
	(0.018)	(0.015)	(0.020)	(0.026)	(0.029)	(1,176)	(1,107)	(0.022)
N	173,892	130,419	119,919	72,207	47,712	109,740	109,740	100,332

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A5**: Standard errors clustered at the person level in parentheses. This table reports estimates from equation (2). Column 1 and 2 includes all exam-takers, and columns 3-7 include only people who match to the voter file. Column 8 includes people who match in at least 2012 but do not necessarily match in other years, coding the outcome as zero when the individual does not match in a given year. Column 7 is the same as Column 6 but includes county fixed effects.

Table A6: Pooled Difference-in-Differences Estimates, Relaxed Matching Requirement, Florida

	(1)	(2)	(3)
		Registered Republican	
Post x Pass	0.045***	–0.0095	0.0053
	(0.0075)	(0.018)	(0.054)
Constant	0.020***	0.74***	0.29***
	(0.0019)	(0.0047)	(0.023)
Observations	76,923	51,201	2,295

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A6**: Standard errors clustered at the person level in parentheses. This table reports estimates from equation (1). The sample includes individuals even in years when they do not match to the voter file. Column 1 includes individuals who are in the voter file and not registered as Republican in the year before the exam, column 2 includes individuals who are in the voter file and registered as Republican in the year before the exam, and column 3 includes individuals who are not in the voter file in the year before the exam. This last sample is small because we still require a match in at least one year prior to the exam. Overall, we find that effects are driven by those individuals who are in the voter file but not registered as Republican in the year before the exam.

Table A7: Testing Continuity of Exam-Taker Characteristics, Columbus

	(1)	(2)	(3)
	Black	Male	Predicted P(Hired)
Above Cutoff	0.0157	0.00607	–0.000684
	(0.0174)	(0.0154)	(0.000850)
Constant	0.203***	0.841***	0.0730***
	(0.0150)	(0.0131)	(0.000731)
Observations	11,132	11,132	11,132

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A7**: Robust standard errors in parentheses. This table reports estimates from equation (3) with exam-taker characteristics as the dependent variables. Column 1 shows that, at the cutoff, there is no discontinuous break in the probability that the exam-taker is Black. Column 2 shows that there is no discontinuous break in the probability that the exam-taker is male. For Column 3, we estimate a logistic model in which we predict hiring probability based on the interaction between Black and male. Then, we include that predicted probability of hire as the dependent variable. Using this approach, we also find that there is no discontinuous change in exam-taker characteristics at the cutoff. All specifications use a bandwidth of +/- 15 points.

Table A8: First Stage Effect of Passing on Police Hiring and Other Hiring, Columbus

	(1)	(2)
	Hired in Columbus Police	Hired in Other Govt. Agency
Above Cutoff	0.0463***	–0.00562
	(0.00745)	(0.00794)
Constant	0.0236***	0.0387***
	(0.00442)	(0.00682)
Observations	11,132	11,132

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A8**: Robust standard errors in parentheses. This table reports estimates from equation (3) with hiring outcomes as the dependent variables. Column 1 shows that, at the cutoff, there is a discontinuous break in the probability that the exam-taker is hired as a police officer in Columbus, OH from 2011-2019. Column 2 shows that there is not a discontinuous break in the probability that the exam-taker is hired into another government agency in Columbus, OH from 2011-2019. All specifications use a bandwidth of +/- 15 points.

Table A9: Effect of Passing on Party Affiliation, Columbus

	(1)	(2)	(3)	(4)	(5)	(6)
	Voted in Republican Primary Post-Exam					
Above Cutoff	0.0334**	0.0256*	0.0426***	0.0340**	0.0435*	0.0326
	(0.0145)	(0.0137)	(0.0165)	(0.0152)	(0.0224)	(0.0209)
Constant	0.145***	0.0768	0.166***	0.160**	0.235***	0.0697
	(0.0118)	(0.0511)	(0.0134)	(0.0644)	(0.0184)	(0.0796)
Observations	11,132	11,132	9,657	9,657	6,785	6,785
Covariates Included	No	Yes	No	Yes	No	Yes
Sample Restriction	None	None	Any Match	Any Match	Unique Match	Unique Match

	(1)	(2)	(3)	(4)	(5)	(6)
	Voted in Democrat Primary Post-Exam					
Above Cutoff	–0.00522	–0.0113	–0.00253	–0.0103	–0.00594	–0.0118
	(0.0139)	(0.0132)	(0.0159)	(0.0147)	(0.0219)	(0.0204)
Constant	0.145***	0.175***	0.166***	0.158**	0.231***	0.248**
	(0.0116)	(0.0669)	(0.0132)	(0.0806)	(0.0184)	(0.0970)
Observations	11,132	11,132	9,657	9,657	6,785	6,785
Covariates Included	No	Yes	No	Yes	No	Yes
Sample Restriction	None	None	Any Match	Any Match	Unique Match	Unique Match

Notes: * $p<0.1$, ** $p<0.05$, *** $p<0.01$. **Table A9**: Robust standard errors in parentheses. This table reports estimates from equation (3) with party affiliation (based on primary voting) outcomes as the dependent variables. Columns 1-2 include all exam-takers, even those who do not match to the voter file. Columns 3-4 include only exam-takers who match to the voter file. In the event of duplicate matches, the dependent variables takes on the minimum value across all matches. Columns 5-6 include only exam-takers who match to a unique person in the voter file. Columns 1, 3, and 5 do not include additional covariates. Columns 2, 4, and 6 include: pre-exam party affiliation, recruitment period fixed effects, and demographic controls. All specifications use a bandwidth of +/- 15 points.

Table A10: Effect of Passing on Party Affiliation Pre-Exam, Columbus

	(1)	(2)	(3)	(4)	(5)	(6)
	Voted in Republican Primary Pre-Exam					
Above Cutoff	0.000646	0.00303	0.00189	0.00447	–0.00194	0.00174
	(0.00804)	(0.00804)	(0.00925)	(0.00915)	(0.0133)	(0.0132)
Constant	0.0394***	–0.0349	0.0452***	–0.133***	0.0660***	–0.202***
	(0.00643)	(0.0282)	(0.00737)	(0.0387)	(0.0107)	(0.0494)
Observations	11,132	11,132	9,657	9,657	6,785	6,785
Covariates Included	No	Yes	No	Yes	No	Yes
Sample Restriction	None	None	Any Match	Any Match	Unique Match	Unique Match

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A10**: Robust standard errors in parentheses. This table reports estimates from equation (3) with party affiliation before the exam (based on primary voting) outcomes as the dependent variables. Columns 1-2 include all exam-takers, even those who do not match to the voter file. Columns 3-4 include only exam-takers who match to the voter file. In the event of duplicate matches, the dependent variables takes on the minimum value across all matches. Columns 5-6 include only exam-takers who match to a unique person in the voter file. Columns 1, 3, and 5 do not include additional covariates. Columns 2, 4, and 6 include: recruitment period fixed effects and demographic controls. All specifications use a bandwidth of +/- 15 points.

Table A11: Effect of Passing on Political Contributions, Columbus

	(1)	(2)	(3)	(4)	(5)
	Republican, Post-Exam	Democrat, Post-Exam	Other, Post-Exam	Trump, Post-Exam	Republican, Pre-Exam
Above Cutoff	0.0121**	0.00114	–0.00216	0.00398***	0.000658
	(0.00575)	(0.00541)	(0.00557)	(0.00132)	(0.00309)
Constant	0.0137***	0.0114**	0.0142***	–0.0000888	0.00150
	(0.00439)	(0.00465)	(0.00466)	(0.0000915)	(0.00281)
Observations	8,260	8,260	8,260	8,260	8,260

	(1)	(2)	(3)	(4)	(5)
	Republican, Post-Exam	Democrat, Post-Exam	Other, Post-Exam	Trump, Post-Exam	Republican, Pre-Exam
Above Cutoff	0.0104*	–0.000594	–0.00378	0.00381***	0.000911
	(0.00574)	(0.00538)	(0.00565)	(0.00128)	(0.00308)
Constant	–0.00814	0.0174	–0.0141	–0.0132**	–0.0138***
	(0.0201)	(0.0262)	(0.0169)	(0.00549)	(0.00516)
Observations	8,260	8,260	8,260	8,260	8,260

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A11**: Robust standard errors in parentheses. This table reports estimates from equation (3) with federal political contributions outcomes as the dependent variables. The sample includes all exam-takers who do not have a duplicate match in the voter file. This is analogous to the restriction in our contributions analysis in Florida. The specifications in the first panel do not include covariates. The specifications in the second panel include: pre-exam contributions (when possible), recruitment period fixed effects, and demographic controls. All specifications use a bandwidth of +/- 15 points.

Table A12: Placebo Test Based on Strength of First Stage by Year, Columbus

	(1)	(2)	(3)	(4)
		Voted in Republican Primary Post-Exam		
Above Cutoff	0.0640**	0.0407*	–0.00699	0.0123
	(0.0261)	(0.0244)	(0.0433)	(0.0405)
Constant	0.216***	0.0200	0.285***	–0.118
	(0.0214)	(0.0907)	(0.0358)	(0.151)
Observations	4,795	4,795	1,990	1,990
Covariates Included	No	Yes	No	Yes
Sample Restriction	First Stage>0.01	First Stage>0.01	First Stage<0.01	First Stage<0.01

	(1)	(2)	(3)	(4)
		Republican Contributions Post-Exam		
Above Cutoff	0.0176***	0.0150**	–0.000549	–0.000394
	(0.00681)	(0.00677)	(0.0108)	(0.0107)
Constant	0.0117**	0.00720	0.0183**	–0.0549*
	(0.00509)	(0.0212)	(0.00871)	(0.0317)
Observations	5,802	5,802	2,458	2,458
Covariates Included	No	Yes	No	Yes
Sample Restriction	First Stage>0.01	First Stage>0.01	First Stage<0.01	First Stage<0.01

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A12**: Robust standard errors in parentheses. This table reports the results from equation (3). In the first panel, the dependent variable is party affiliation after the exam (based on primary voting). In the second panel, the dependent variable is federal political contributions. In columns 1-2, we restrict the data to years in which there is a strong first stage relationship between passing the exam and being hired as a police officer. In columns 3-4, we restrict the data to years in which there is not a strong first stage relationship between passing the exam and being hired as police officer.

Table A13: Effect of Passing on Party Affiliation Separately by Election Pre/Post-2014, Columbus, Exam Cohorts with First-Stage>0.01

	(1) Republican Primary	(2) Republican Primary	(3) Republican Primary	(4) Republican Primary
Pass Exam	0.013**	0.010*	0.011**	0.006
	(0.006)	(0.006)	(0.005)	(0.005)
Elections	2002-2012	2002-2012	2014-2020	2014-2020
Covariates		X		X
Outcome Mean	0.044	0.044	0.053	0.053
Observations	18395	18395	29505	29505

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A13**: Robust standard errors in parentheses. The unit of observation is an exam applicant-by-election year. The outcome is whether the individual votes in a Republican primary. We restrict attention to elections that occur on or after each cohorts' exam year. Columns (1) and (2) focus on elections that occur in the years 2002 to 2012, and Columns (3) and (4) focus on elections that occur in the years 2014 to 2020. Covariates included in Columns (2) and (4) are Black, male, and indicators for cohort year.

Table A14: Relationship Between Police Employment and Earnings, CPS

	(1)	(2)	(3)
		Weekly Earnings	
Police	256.01***	96.729***	–29.119**
	(6.2559)	(5.8228)	(11.404)
Constant	623.46***	–16.121*	552.39***
	(3.0041)	(9.0014)	(5.2765)
Observations	2,349,972	2,349,972	2,349,972
Year & Month FEs	Yes	Yes	Yes
Individual Controls	No	Yes	No
Individual FEs	No	No	Yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A14**: Standard errors clustered at the person-level in parentheses. This table reports the results from regressions of weekly earnings in the CPS on a binary indicator that is equal to one when the person is a police officer and equal to zero if not. Column 1 only includes survey month and year fixed effects. Column 2 adds demographic controls, a control for education, and a control for veteran status. Column 3 includes individual fixed effects, effectively leveraging variation in policing from within-individual job transitions. We find police are positively selected on earnings. Once we control for individual fixed effects, we find that police earn approximately $30 less per week than non-police.

Table A15: Comparing Police Republicans to Non-Police Republicans, General Social Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Too much spending:	Environment	Health	Big Cities	Education	Blacks	Welfare	Military	Crime	Index
Police	–0.042	0.060	0.057	–0.0091	0.083	0.069	–0.067*	–0.056***	0.0020
	(0.035)	(0.054)	(0.059)	(0.028)	(0.063)	(0.059)	(0.034)	(0.0098)	(0.021)
Constant	0.10***	0.072***	0.18***	0.14***	0.32***	0.67***	0.32***	0.042***	0.31***
	(0.018)	(0.016)	(0.024)	(0.022)	(0.028)	(0.028)	(0.027)	(0.012)	(0.011)
Observations	16,545	16,681	15,183	16,850	15,653	16,712	16,642	16,677	17,174

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Gov't Does Too Much	No Asistance for Poor	Anti-Redistribute	Racial Attitudes Index	Gender Atittudes Index	Abortion Index	Oppose Gun Permits	Favor Capital Punish-ment	Pr(Repub.)
Police	–0.028	0.043	–0.11	–0.0068	0.028	–0.063	–0.023	0.13***	0.028
	(0.077)	(0.077)	(0.075)	(0.033)	(0.062)	(0.061)	(0.068)	(0.018)	(0.019)
Constant	0.41***	0.34***	0.43***	0.34***	0.30***	0.30***	0.28***	0.78***	0.22***
	(0.030)	(0.028)	(0.028)	(0.022)	(0.026)	(0.017)	(0.025)	(0.025)	(0.0031)
Observations	9,369	9,486	9,939	16,609	11,141	12,049	11,837	15,340	17,951

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A15**: Robust standard errors in parentheses. This table compares reported beliefs for police who respond to the GSS and identify as Republican to non-police who respond to the GSS and identify as Republican. Columns 1-8 in the first panel test whether police and non-police Republicans hold different views on whether there is too much spending on: the environment, healthcare, big cities, education, improving the conditions of Blacks, welfare, the military, or crime. Column 9 creates an index of "too much spending" across all categories, including many not reported individually in this table and excluding "military" and "crime". Columns 1-8 in the second panel test whether police and non-police Republicans hold similar views: that the government does too many things that should be left to individuals and private businesses, that assistance to the poor is not the government's responsibility, that government should not reduce income differences with redistribution, on an index of conservative racial attitudes, on an index of conservative gender attitudes, on an index of "pro-life" attitudes, on opposing gun permits, or on favoring capital punishment. We then use all variables from this second panel and all questions on government spending to predict whether the respondent identifies as Republican. We test whether police and non-police Republicans differ on this index of ideology in column 9. Ultimately, we find that, in general, police and non-police Republicans hold similar views on a wide range of issues unrelated to policing or crime.

Table A16: Comparing Police Democrats to Non-Police Democrats, General Social Survey

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Too much spending:	Environment	Health	Big Cities	Education	Blacks	Welfare	Military	Crime	Index
Police	0.0022	0.022	0.019	–0.013	0.022	0.049	–0.096*	–0.072***	0.016
	(0.029)	(0.032)	(0.052)	(0.026)	(0.055)	(0.056)	(0.056)	(0.0044)	(0.018)
Constant	0.071***	0.039***	0.12***	0.067***	0.18***	0.44***	0.42***	0.049***	0.29***
	(0.011)	(0.0086)	(0.015)	(0.011)	(0.017)	(0.022)	(0.022)	(0.0099)	(0.0069)
Observations	23,755	24,201	21,949	24,305	22,852	23,993	23,589	23,956	24,740

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Gov't Does Too Much	No Asistance for Poor	Anti-Redistribute	Racial Attitudes Index	Gender Atittudes Index	Abortion Index	Oppose Gun Permits	Favor Capital Punish-ment	Pr(Repub.)
Police	–0.11***	–0.10***	0.037	0.062	0.088*	0.015	0.079	0.16***	0.033**
	(0.039)	(0.037)	(0.074)	(0.049)	(0.051)	(0.043)	(0.072)	(0.045)	(0.013)
Constant	0.22***	0.16***	0.22***	0.36***	0.26***	0.39***	0.26***	0.63***	0.22***
	(0.019)	(0.016)	(0.018)	(0.018)	(0.017)	(0.014)	(0.017)	(0.022)	(0.0021)
Observations	12,836	13,148	13,829	23,904	16,382	18,165	17,768	21,837	26,081

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A16**: Robust standard errors in parentheses. This table compares reported beliefs for police who respond to the GSS and identify as Democrat to non-police who respond to the GSS and identify as Democrat. Columns 1-8 in the first panel test whether police and non-police Democrats hold different views on whether there is too much spending on: the environment, healthcare, big cities, education, improving the conditions of Blacks, welfare, the military, or crime. Column 9 creates an index of "too much spending" across all categories, including many not reported individually in this table and excluding "military" and "crime". Columns 1-8 in the second panel test whether police and non-police Republicans hold similar views: that the government does too many things that should be left to individuals and private businesses, that assistance to the poor is not the government's responsibility, that government should not reduce income differences with redistribution, on an index of conservative racial attitudes, on an index of conservative gender attitudes, on an index of "pro-life" attitudes, on opposing gun permits, or on favoring capital punishment. We then use all variables from this second panel and all questions on government spending to predict whether the respondent identifies as Republican. We test whether police and non-police Democrats differ on this index of ideology in column 9. Ultimately, we find that, in general, police and non-police Democrats hold similar views on a wide range of issues unrelated to policing or crime.

Table A17: Party Affiliation of Firefighters versus Police, Florida

	(1)	(2)	(3)	(4)	(5)	(6)
	Republican	Republican	Democrat	Democrat	Other	Other
Firefighter	–0.020**	–0.036***	–0.025***	–0.0091	0.045***	0.045***
	(0.0083)	(0.0087)	(0.0075)	(0.0077)	(0.0082)	(0.0085)
Constant	0.40***	0.42***	0.28***	0.26***	0.32***	0.32***
	(0.0040)	(0.0043)	(0.0036)	(0.0038)	(0.0038)	(0.0040)
Observations	19,806	17,470	19,806	17,470	19,806	17,470
Sample	All Exam-Takers	Passed Exam	All Exam-Takers	Passed Exam	All Exam-Takers	Passed Exam

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A17**: Robust standard errors in parentheses. Columns 1, 3, and 5 of this table estimate differences in party affiliation (prior to the exam) among people taking the firefighter exam versus people taking the police exam. Columns 2, 4, and 6 of this table estimate differences in party affiliation (prior to the exam) among people who pass the firefighter exam versus people who pass the police exam. Overall, we find that individuals who take the firefighter exam and individuals who pass the firefighter exam are less likely to register as Republican in the year before the exam and are more likely to register as Independent or Other.

Table A18: Party Affiliation of Firefighters versus Police, GSS

	(1)	(2)	(3)	(4)	(5)
	Strong Rep.	Rep. or Lean Rep.	Other	Dem. or Lean Dem.	Strong Dem.
Firefighter	–0.11***	–0.0067	0.046	0.064	0.011
	(0.043)	(0.057)	(0.043)	(0.059)	(0.041)
Constant	0.20***	0.34***	0.11***	0.23***	0.11***
	(0.033)	(0.031)	(0.021)	(0.026)	(0.020)
Observations	434	434	434	434	434

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A18**: Robust standard errors in parentheses. This table estimates differences in party ID among police who respond to the GSS and firefighters who respond to the GSS. We find broadly similar patterns in this table. People employed as firefighters are less likely to identify as Republican, specifically "strong Republican." Part of that difference is due to a greater prevalence of Other/Independent among firefighters.

Table A19: Heterogeneity by Agency Characteristics, Florida

	(1)	(2)	(3)	(4)	(5)
	Registered Republican				
Post x Pass	0.028***	0.023**	0.041*	0.019	–0.0042
	(0.0080)	(0.0096)	(0.022)	(0.022)	(0.037)
... x Agency Rep. 2012	0.044***	0.039***	0.027***	0.030***	0.021**
	(0.0079)	(0.0083)	(0.0086)	(0.0094)	(0.0093)
... x Crime Per Cap. 2000–10		–0.024**	–0.0098	–0.014	–0.0093
		(0.010)	(0.011)	(0.011)	(0.011)
... x Num. Emps. 2012			0.0038	0.023	0.033**
			(0.015)	(0.015)	(0.015)
... x Union			–0.024*	–0.018	–0.016
			(0.012)	(0.013)	(0.013)
... x County BLM Protests			–0.0089	–0.015	–0.015
			(0.016)	(0.017)	(0.016)
Constant	0.031***	0.058***	0.066***	0.059***	0.073***
	(0.0025)	(0.0067)	(0.011)	(0.0099)	(0.017)
Observations	63,730	63,730	63,730	63,730	63,730
County x Year x Exam Year FEs	No	No	No	Yes	Yes
Post x Pass x Off. Demographics	No	No	No	No	Yes

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A19**: Standard errors clustered at the officer level in parentheses. This table estimates modified versions of equation (1) for individuals who are not affiliated with the Republican party in the year before the exam. We limit the sample to individuals who pass the exam and join an agency or individuals who fail the exam. Then, we interact the post-by-pass indicator with various agency characteristics, which are coded as zero for individuals who fail the exam. Column 1 includes Republican share in the agency in 2012 (dichotimized to indicate above/below median). Column 2 adds agency crime per capita in 2000-2010 (dichotimized to indicate above/below median). Column 3 adds number of employees in the agency in 2012 (dichotimized to indicate above/below median), agency unionization status, and agency exposure to county-level Black Lives Matter protests in 2014-15. Column 4 adds county of residence before the exam by year by exam year fixed effects. Finally, Column 5 includes interactions between the post-by-pass indicator and officer demographics (i.e., age at time of exam, sex, and race). We find that individuals who join agencies with a higher share Republican in 2012 are more likely to register as Republican in the years after joining than individuals who join agencies with a lower share Republican in 2012.

Table A20: Heterogeneity by Agency Characteristics, Florida

	(1)	(2)	(3)
	Registered Republican		
Post x Pass	0.026	0.059***	–0.030
	(0.029)	(0.022)	(0.047)
... x Agency Rep. 2012	0.018**		0.046**
	(0.0089)		(0.022)
... x Agency Pre-Exam Rep., 2013–14 Cohorts		0.0099**	
		(0.0048)	
Observations	45,990	45,990	45,990
Estimator	OLS	OLS	2SLS

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A20**: Standard errors clustered at the officer level in parentheses. Table **??** estimates heterogeneity in our main effect by agency characteristics, including the share Republican in the agency in 2012. We interpret this result as evidence that agencies with more Republican officers shift new hires toward the Republican party more than agencies with fewer Republican officers. However, an alternative interpretation is that agencies with more Republican officers in 2012 are agencies that have some unobserved characteristic which makes officers Republican and that characteristic also influences new hires in our sample. To account for this possibility, we use the share Republican **prior to the exam** among the 2013 and 2014 hires as an instrument for share Republican in the agency in 2012. Since we are using 2013 and 2014 hires to construct the instrument, we limit the analysis sample to exam cohorts from 2015 to 2019. Column 1 re-estimates our main heterogeneity test from Table **??** on the 2015-2019 exam cohorts. Column 2 uses the same specification as column 1, but uses the pre-exam share Republican among the 2013-2014 hires. Finally, column 3 instruments for share Republican in the agency in 2012 with the pre-exam share Republican among the 2013-2014 hires. Column 3 confirms our main result in Table **??**.

Table A21: Changing Views, On-the-job Behavior, and Ethics, Survey of Philadelphia Police

Dependent Variable	Coefficient (1)	Standard Error (2)	Dep. Var. Mean (3)
On-the-job Behavior (Have you had a...?)			
Formal Complaint	0.104	(0.045)	0.475
Disciplinary Violation	0.099	(0.038)	0.234
Internal Affairs Investigation	0.124	(0.043)	0.377
Disciplinary Hearing	0.039	(0.034)	0.174
Guilty Hearing	0.035	(0.028)	0.114
Use of Force Incident	0.213	(0.040)	0.315
Ethics Questions (It's okay for police to...)			
Accept Gifts	0.077	(0.036)	0.202
Use Prohibited Methods	0.176	(0.036)	0.224
Break Rules	0.072	(0.028)	0.112
Protect Fellow Officer Misconduct	0.062	(0.030)	0.128
Use Verbal Abuse	0.182	(0.038)	0.248
Bend Facts in Court	0.082	(0.024)	0.080
Exaggerate Probable Cause	0.121	(0.031)	0.150
Complete Errand while Working	0.151	(0.037)	0.228
Want 'Street Justice' for Hurting Officer	0.085	(0.026)	0.098
Go on Strike	–0.027	(0.042)	–0.335
Excuse DWI for Fellow Officer	0.173	(0.035)	0.202
Use Force as Punishment	0.090	(0.030)	0.134
N = 499			

Notes: * p<0.1, ** p<0.05, *** p<0.01. **Table A21**: Robust standard errors in parentheses. This table reports results from an existing survey of 499 patrol officers in the Philadelphia Police Department in 2000 (Greene and Piquero, 2004). On that survey, approximately half of officers agree or strongly agree with the view, "police officers have a different view of human nature because of the misery and cruelty of life which they see everyday." We examine how that view, that the work of policing can alter one's view of human nature, correlates with a range of on-the-job behaviors and response to questions about police ethics. Specifically, we regress a series of dependent variables on a binary variable equal to one if the officer agrees or strongly agrees that policing changes one's views and equal to zero otherwise. The dependent variables fall into two broad categories: on-the-job behavior and ethics questions. For on-the-job behavior, the survey asks officers if they have ever been: subject to a formal complaint, charged with a disciplinary violation, subject to an internal affairs investigation, subject to a disciplinary hearing with the PA Bureau of Investigation (PBI), found guilty by PBI, or involved in a use of force incident. We code the answer "yes" as one and the answer "no" as zero. For ethics questions, the survey asks officers if they strongly disagree, disagree, are neutral, agree, or strongly agree with a range of behaviors for police officers. We dichotomize these responses as one if the officer agrees or strongly agrees and zero otherwise. The full text of these questions can be found in Greene and Piquero (2004). The abridged versions are: (i) it's not wrong to accept small gifts from the public, (ii) sometimes an officer has to use prohibited methods to enforce the law, (iii) officers need to bend/break rules to be productive, (iv) officers should protect each other when misconduct is alleged, (v) sometimes it is necessary to be verbally disrespectful or abusive, (vi) officers sometimes have to bend facts in court to get a criminal convicted, (vii) sometimes officers have to exaggerate probable cause to get a crook off the street, (viii) taking care of errands while working is generally okay, (ix) some people should get "street justice" for hurting an officer, and (x) go on strike if there are unfair working conditions. For the last two questions we report, officers where behavior falls on a range of "not at all serious" to "very serious." We dichotimize these responses as one if the officer reports 1 ("not at all serious") or 2 and zero otherwise. These scenarios are: (xi) an officer does not report a DWI if the offender is a fellow officer and (xii) an officer punches a man who tried to flee as punishment for fleeing.

B Model of Police Preferences and Selection

We have shown that police work impacts officers' partisanship and that partisanship correlates with officer enforcement behavior. In this section, we consider the implications of our results for the optimal selection of bureaucrats. We will do so by building on the principal-agent model of Prendergast (2007). In this model, a police department (the principal) delegates enforcement activity to a police officer (the agent), whose preferences may differ from the department's. The officer observes suspects and provides a recommendation of whether to arrest the individual. We will first apply the insight from the original model that it may be desirable for bureaucratic preferences to deviate from the department's preferences. Second, we will model a department's choice of the optimal wage and oversight of officer arrest decisions, and we will consider how these choices are impacted by the presence of a treatment effect of work on preferences. This treatment effect creates a wedge between individuals' preferences at the point of selecting into the job and their preferences while working on the job. As part of this second analysis, we will show that, for a sufficiently large treatment effect of work, the optimal choice of who to select into policing may actually flip.

Model Setup

We being with the exact model setup from Prendergast (2007). The model has three individuals: a criminal suspect, the officer, and the police department. The officer makes the decision of whether to arrest the suspect, $A \in \{0, 1\}$. This choice of arrest affects the (a) suspect, but it also affects (b) the rest of society. Individuals in society differ in how they weight these two outcomes, and the department's job is to maximize the utility of the median individual in society.

The suspect receives benefit $B(A, \alpha)$ based on whether they are arrested and their guilt status, $\alpha \in \{0, 1\}$.[41] For simplicity, we assume that guilt and innocence are equally likely. We will also assume that, when a suspect is not arrested, their personal benefit is 0. We normalize the benefit of arrest of an innocent suspect to $B(1, 0) = -1$, and we denote the benefit of arrest for a guilty suspect to $B(1, 1) \equiv -\beta$. For now, we do not impose any assumption on β other than to assume that it is weakly positive, $\beta \geq 0$, i.e. guilty suspects are harmed from arrest.

Separate from the direct impact to the suspect, society receives a benefit from the allocation to the suspect of $\Sigma(A, \alpha)$. In addition, members of society value the direct effect on the suspect, $B(A, \alpha)$. Each individual in society places a different relative weight on these two considerations and receives utility $U = \Sigma(A, \alpha) + v \cdot B(A, \alpha)$. The department's goal is to maximize the utility of the median individual in society, $S(A, \alpha) = \Sigma(A, \alpha) + v_m \cdot B(A, \alpha)$. We will assume this

[41] Prendergast (2007) defines A and α as whether an arrest is *not* made and whether an individual is innocent, respectively. For ease of exposition in our setting, we use the inverse indicator for each.

function takes the simple form of $S(A, \alpha) = \mathbb{I}(A = \alpha)$. Crucially, note that there is a divergence in interests between the department and the suspect: the department would like to match arrests to guilt status, whereas the suspect would always like to not be arrested.[42]

The department delegates to the officer the arrest decision for the suspect. The officer's key decision is of how much effort e to exert, which is also the probability of a correct decision, $e \in [1/2, 1]$. The officer faces a personal cost $d(e)$ of exerting effort, which has the following properties: $d(1/2) = 0$, $d'(\cdot) > 0$, $d''(\cdot) > 0$, and $d(1) \to \infty$.

Like all individuals in society, the officer values the social benefit of their allocation decision, $\Sigma(A, \alpha)$, as well as the impact on the suspect, $B(A, \alpha)$, but may not have the median weight on the suspect's outcome, so the non-wage component of their utility is

$$U_a = S(A, \alpha) + (v - v_m) \cdot B(A, \alpha) - d(e)$$

We will refer to $(v - v_m)$ as the officer's "bias" towards or against the suspect. We will refer to an officer with $v - v_m < 0$ as "hostile" to the suspect and an officer with $v - v_m > 0$ as "sympathetic" to the suspect.

To map this model to our empirical application, we will consider political ideology as being reflected in $(v - v_m)$. Evidence from the General Social Survey shows that Republicans and Democrats differ in their preferences for punitiveness in criminal justice and in their average tradeoff between Type-I and Type-II errors. Specifically, Republicans are approximately 30% more likely to believe that courts do not deal with criminals harshly enough and 33% more likely to say that allowing a guilty person to go free is a worse mistake than convicting an innocent person. For this reason, we treat officer alignment with the suspect as the model's analogue to officer partisanship.

After the officer's allocation decision, the department has some mechanism for correcting mistakes. Let ρ_0 and ρ_1 be the probabilities that the officer's decision is overturned when the suspect is innocent and guilty, respectively.

The model timing is as follows: the department offers a wage w to the candidate officer and announces the level of oversight ρ_0 and ρ_1 (which may or may not be a choice to the department).[43] The individual is employed if they accept. Nature assigns guilt status α to the suspect. The officer exerts effort e, announces his arrest choice a, and is paid w. The department then possibly receives information on a mistake by the officer, and decides on final arrest choice A.

[42] Prendergast (2003) highlights this divergence as a fundamental feature of government "services" allocated by bureaucrats; if interests were aligned, the service would be more efficiently provided through consumer choice.

[43] We will allow the wage to possibly be negative, in which case the individual pays the department for the opportunity to work on the job.

Which Officers are Preferred by the Department?

The first insight from the model comes directly from Prendergast (2007), which is about which officer type is most desired by the department.

Note that the department's utility net of wage and oversight cost can be written as $e + (1-e)\frac{\rho_0+\rho_1}{2}$. Therefore, when we abstract from wage setting and oversight cost and allow the department to directly observe and select officer motivation v, the department prefers the individual who will exert the highest effort on the job.

To see who will exert the most effort, we will write out the officer's non-wage utility as a function of effort:

$$U_a = e + (1-e)\frac{\rho_0+\rho_1}{2} + (v - v_m)\cdot\Big[-\frac{1}{2}(1-e)\cdot(1-\rho_0) - \frac{\beta}{2}(e + (1-e)\rho_1)\Big] - d(e) \quad \text{(A1)}$$

The optimal choice e^* satisfies the first-order condition

$$(1-\rho_0) + (1-\rho_1) + (v - v_m)\cdot\Big[(1-\rho_0) - (1-\rho_1)\beta\Big] \leq 2d'(e^*) \quad \text{(A2)}$$

In cases where $e^* > 1/2$, the first order condition binds. This condition tells us how effort will vary with officer motivation. The left-hand side is the marginal value of an increase in effort, and the right-hand side is the marginal cost. If $\big[(1-\rho_0) - (1-\rho_1)\beta\big] > 0$, then the marginal value of effort increases with bias towards the suspect. Therefore, the officers with greater $v - v_m$ will exert higher effort. If, instead, this expression is negative, then officer effort increases with bias *against* the suspect.

Proposition 1 (Prendergast 2007)**.** *When $\beta > \frac{1-\rho_0}{1-\rho_1}$, officer effort and department utility decrease with officer bias v. If $\beta < \frac{1-\rho_0}{1-\rho_1}$, they both increase with officer bias.*

This condition states that officer bias against the suspect (more negative v) is preferred when: corrections of wrongful arrests of innocent suspects is more probable (higher ρ_0), corrections of wrongful non-arrests of guilty suspects is less probable (lower ρ_1), and the cost to guilty suspects of being arrested is large (higher β).

Henceforth, we will assume $\beta > 1$, which simplifies our ensuing analysis. This assumption is consistent with the idea that a truly guilty suspect suffers more from an arrest than an innocent suspect, who is less likely to experience criminal conviction, sentencing, etc.

Self-Selection into Police Work

The next question is how individuals select into policing and how that can be shaped by department policy. Here we deviate from the benchmark Prendergast (2007) model by allowing for an effect of police employment on preferences. We will model this effect as generating a gap between the type that is induced into police work and the type that is observed in the job. To simplify our analysis here, we are going to assume that the government has a single probability ρ of correcting an officer mistake for both innocent and guilty suspects. The department has a choice of which oversight level to implement, and they pay a cost $c(\rho)$, with similar properties to the officer's cost of effort: $c(0) = 0$, $c'(\cdot) > 0$, $c''(\cdot) > 0$, and $c(1) \to \infty$.

We will also assume that the department is not able to observe anything about individuals at the point of hiring. Individuals observe their type v at the point of hiring, though this may differ from their type once they are employed (which we describe in more detail below). All selection into the department is thus due to self-selection.

We are going to assume that all officers have an outside option utility of $u = 0$, but they also internalize the outcome of the arrest allocation in their absence. We assume the following about the job: among those who are interested in the position, one is randomly offered the job. If they turn down the position, the department randomly gives an allocation, with their usual oversight applied. So the officer's utility if they turn down the offer, denoted by $u_0(v)$, is

$$u_0(v) = \frac{1}{2} + \frac{1}{2}\rho + \frac{v - v_m}{2} \cdot [-\frac{1}{2} \cdot (1-\rho) - \beta \cdot (\frac{1}{2} + \frac{1}{2}\rho)],$$

and on-the-job utility relative to the outside option is

$$u(v, w, \rho) \equiv w + U_a - u_0(v) = w + (e - \frac{1}{2})(1-\rho) + \frac{v - v_m}{2} \cdot \Big[(e - \frac{1}{2})(1-\rho)(1-\beta)\Big] - d(e)$$

To identify the patterns of who selects into the department, we will first ask how $U_a - u_0(v)$ varies with v, for which we use the envelope theorem:

$$\frac{du(v,w,\rho)}{dv} = \frac{\partial u(v,w,\rho)}{\partial v}\Big|_{e^*} = \frac{1}{2}(e^* - \frac{1}{2})(1-\rho)(1-\beta)$$

We know this expression is negative as we are assuming $\beta > 1$. Since utility is monotonically decreasing in v, there will be a cutoff value of v below which an individual joins the department and above which they do not.

How does the officer's relative utility of working on the job change with ρ? We can again use the envelope theorem:

$$\frac{du(v,w,\rho)}{d\rho} = \frac{\partial u(v,w,\rho)}{\partial \rho}\Big|_{e^*} = -(e^* - \frac{1}{2}) - \frac{v - v_m}{2} \cdot [(e^* - \frac{1}{2})(1-\beta)] \tag{A3}$$

Notice that, with $\beta > 1$, the marginal value for the officer of an increase in oversight is increasing in bias v. For values of v that satisfy $v > v_m + \frac{2}{\beta - 1}$, this expression is positive. Officers for whom this inequality is reversed, including all officers biased against the suspect ($v - v_m < 0$), dislike oversight.

We will first consider the department's maximization problem absent a treatment effect of work. We will assume that v is bounded by the support $[\underline{v}, \overline{v}]$. We will also assume that, for at least the most biased officer $\underline{v}$, Equation A2 is an equality and the officer exerts effort $e^* > 1/2$. Since effort is decreasing in v, this assumption is equivalent to saying that not all officers would exert minimal effort.

Because departments desire officers with lower values of v, and reducing w lowers the cutoff v for individuals desiring to enter, the optimal choice of w is the one that, for a given ρ, sets $u(\underline{v}, w, \rho) = 0$. In other words, the department induces only the most biased officer to join the department, and they are paid exactly enough to satisfy their participation constraint.

Then, the department sets w and ρ to solve

$$\max_{w,\rho} \underbrace{e(\underline{v}, \rho) + (1 - e(\underline{v}, \rho))\rho}_{\text{Social Benefit}} - \underbrace{w}_{\text{Wage}} - \underbrace{c(\rho)}_{\text{Oversight Cost}} \quad \text{s.t.} \quad \underbrace{u(\underline{v}, w, \rho) = 0}_{\text{Participation Constraint}}$$

We can characterize the optimal w and ρ by first noting that the participation constraint rearranges to $w = -U_a + u_0(\underline{v})$, so the department's problem can be written as an unconstrained maximization of $S(A, \alpha) + U_a - u_0(v) - c(\rho)$ with respect to ρ. The first order condition is

$$\frac{d\big(S(A, \alpha) + U_a - u_0(v)\big)}{d\rho} = c'(\rho)$$

We will now incorporate a treatment effect of police work on preferences. At the point of considering whether to join the department, an individual believes their preference to be $v + \Delta$, and preferences are drawn from a distribution with support $[\underline{v} + \Delta, \overline{v} + \Delta]$. Once they join the department and when setting their effort level e, the officer will have preference v. A positive value of Δ indicates that the job causes a bias against suspects.

The department's constrained maximization problem is now

$$\max_{w,\rho} \underbrace{e(\underline{v}, \rho) + (1 - e(\underline{v}, \rho))\rho}_{\text{Social Benefit}} - \underbrace{w}_{\text{Wage}} - \underbrace{c(\rho)}_{\text{Oversight Cost}} \quad \text{s.t.} \quad \underbrace{u(\underline{v} + \Delta, w, \rho) = 0}_{\text{Participation Constraint}}$$

and first order condition is

$$\frac{d\big(S(A, \alpha) + U_a(\underline{v} + \Delta) - u_0(\underline{v} + \Delta)\big)}{d\rho} = c'(\rho)$$

We are interested in asking how the optimal choice of ρ and w change with Δ, and we will consider deviations from $\Delta = 0$. For a given value of Δ, we will denote the optimal oversight and wage by the firm by $\rho^*(\Delta)$ and $w^*(\Delta)$.

Proposition 2. *When $\beta > 1$, for every $\Delta' > 0$, $w^*(\Delta') > w^*(0)$ and $\rho^*(\Delta') > \rho^*(0)$. When*

$\beta < 1$, $w^*(\Delta') < w^*(0)$ *and* $\rho^*(\Delta') < \rho^*(0)$.

We can prove this proposition by showing that, when $\beta > 1$, $\frac{dw}{d\Delta} \geq 0$ and $\frac{d\rho}{d\Delta} \geq 0$ for all values of Δ, with strict inequalities at $\Delta = 0$. To calculate $\frac{d\rho}{d\Delta}$, we will differentiate the first order condition with respect to Δ and rearrange:

$$\frac{d\rho}{d\Delta} = -\frac{d^2\big(U_a(\underline{v}+\Delta) - u_0(\underline{v}+\Delta)\big)}{d\rho d\Delta} \Big/ \left(\frac{d^2 S(A,\alpha)}{d\rho^2} + \frac{d^2\big(U_a(\underline{v}+\Delta) - u_0(\underline{v}+\Delta)\big)}{d\rho^2} - c''(\rho)\right)$$

The denominator is the expression for the second-order condition of the optimal ρ, which we know is negative. So the sign of $\frac{d\rho}{d\Delta}$ is the same as the sign of the cross-derivative of the rents to the officer. From Equation A3 above, we know $\frac{du}{d\rho}$. Notice that multiplying that expression by $(1-\rho)$ gives us $-u(v,w,\rho) + w$. So we can write

$$\frac{du}{d\rho} = \frac{-u(v+\Delta, w, \rho) + w}{1-\rho}$$

Taking the derivative with respect to Δ, and again using the envelope theorem, we get

$$\begin{aligned}\frac{d^2u}{d\rho d\Delta} = \frac{-du/d\Delta}{1-\rho} &= \frac{-1}{1-\rho}\Big[\frac{\partial u(\underline{v}+\Delta, e)}{\partial v}\Big|_{e=e^*}\Big] \\ &= \frac{-1}{1-\rho}\Big[\frac{1}{2}(e^* - \frac{1}{2})(1-\rho)(1-\beta)\Big] \\ &\geq 0\end{aligned}$$

This proves that ρ weakly increases in Δ. Since we assume that the most biased officer, $\underline{v}$, exerts non-minimal effort $e^* > 1/2$, this inequality is strict at $\Delta = 0$, guaranteeing that $\rho^*(\Delta) > \rho^*(0)$.

The reason for an increase in ρ is that, as officers increase their effort, they dislike oversight more because it reduces the return to their effort. So as officers become less biased at the point of entry (as $\underline{v} + \Delta$ increases with Δ), they expect a less negative effect on utility from an increase in oversight.

To prove the effect on the offered wage, remember that the wage is set to achieve zero utility for the entering officer, so $w = -(U_a - r_0(v))$. Using this formulation, we calculate $\frac{dw}{d\Delta} = -\frac{\partial U_a}{\partial v} - \frac{\partial U_a}{\partial \rho} \cdot \frac{d\rho}{d\Delta}$.[44] Both terms in this expression are positive, and the first is strictly positive at $\Delta = 0$, again because the most biased officer exerts non-minimal effort. So the wage increases as Δ increases from 0.

The logic of this result is that, for a given value of wage and oversight, the less biased officers receive lower rents for police work. To continue to satisfy the participation constraint

[44]Note that we do not take the derivative with respect to e because of the Envelope Theorem.

as Δ increases, the department needs to increase pay.

With $\beta < 1$, the implications of the presence of $\Delta > 0$ are flipped. The department desires an officer who is maximally biased *towards* the suspect, $\overline{v}$. As Δ increases, the optimal oversight level ρ decreases, as does the optimal wage w. Here the logic is reversed: with $\beta < 1$, officers become less desirous of oversight as their bias towards the suspect increases, because effort now increases in bias towards the suspect. However, for a given wage and oversight level, an increase in bias towards the suspect now raises the officer's expected job rents, so the department can lower the offered wage and still satisfy the participation constraint.

Can Δ Change the Optimal Selection of Officers?

The first insight of the model, from Proposition 1, is that the department's preference over officer bias will depend on the relative magnitude of harm to the suspect from an arrest, β, as well as the degree of oversight, ρ_0 and ρ_1. In cases where the department has a sufficiently elastic supply of oversight, they may be able to choose between values of oversight where the most desirable officers are those biased against the suspect (when $\beta > \frac{1-\rho_0}{1-\rho_1}$) or those most biased in favor of the suspect (when $\beta < \frac{1-\rho_0}{1-\rho_1}$). In this section, we show that the presence of a treatment effect of police work on preferences, $\Delta > 0$, can actually change which type of officer the department optimally chooses to induce into the job.

To see this, we will return to the case of asymmetric oversight, where ρ_0 and ρ_1 potentially differ. The officer's on-the-job rents are now

$$w + (e - \frac{1}{2})(1 - \frac{\rho_0 + \rho_1}{2}) + \frac{v - v_m}{2}\Big[(e - \frac{1}{2})(1 - \rho_0) - \beta(e - \frac{1}{2})(1 - \rho_1)\Big] - d(e). \tag{A4}$$

To simplify the analysis, we will let ρ_0 be fixed, and ρ_1 can take on two possible values, $\rho_1 \in \{\underline{\rho_1}, \overline{\rho_1}\}$. We will also allow effort to take on two values, $e \in \{\underline{e}, \overline{e}\}$. Building from Equation A1, officers will exert high effort if the following condition is satisfied:

$$\big[\overline{e} - \underline{e}\big] \cdot \big[1 - \frac{\rho_0 + \rho_1}{2}\big] + \big[\overline{e} - \underline{e}\big] \cdot \frac{v - v_m}{2}[(1 - \rho_0) - \beta(1 - \rho_1)] > d(\overline{e}) - d(\underline{e}) \tag{A5}$$

Note that, if the coefficient on $\frac{v - v_m}{2}$ is negative, officers with lower $v - v_m$ will exert high effort. This condition requires that $\rho_1 < 1 - \frac{1-\rho_0}{\beta}$. If this inequality is reversed and $\rho_1 > 1 - \frac{1-\rho_0}{\beta}$, then officers with higher $v - v_m$ will exert high effort. To make the problem here interesting, we will thus assume that $\underline{\rho_1} < 1 - \frac{1-\rho_0}{\beta}$ and $\overline{\rho_1} > 1 - \frac{1-\rho_0}{\beta}$. Notice that, from Equation A4, these assumptions also imply that job rents are decreasing in v for $\underline{\rho_1}$ and increasing in v for $\overline{\rho_1}$.

Note also that, for each level of oversight, there is a value of bias $\tilde{v}(\rho_1)$ where Equation A5 is an equality and the officer is indifferent between levels of effort. We will assume that $\underline{v} < \tilde{v}(\rho_1) < \overline{v}$ for both values of ρ_1, which guarantees that under either oversight level there are officer types who will pick both high and low effort.

The department has the choice between two options, setting $\rho_1 = \underline{\rho_1}$ and inducing the most hostile officers, or setting $\rho_1 = \overline{\rho_1}$ and inducing the most sympathetic officers. In both cases, the wage is set to satisfy the participation constraint. We will define $\rho^*(\Delta)$ to be the optimal level of oversight for a given treatment effect of work. The next proposition shows that the choice of oversight may change with Δ and, as a result, the induced officer may change.

Proposition 3. *For every set of parameters where $\rho^*(0) = \underline{\rho_1}$ and only officers of type $\underline{v}$ are hired, there exists a $\Delta > 0$ such that $\rho^*(\Delta) = \overline{\rho_1}$ and only officers of type $\overline{v}$ are hired.*

We will first consider the case when $\Delta = 0$ and write the condition where the optimal choice is $\rho_1 = \underline{\rho_1}$:

$$\begin{aligned}
&2\big[\overline{e} + (1-\overline{e})\frac{\rho_0 + \underline{\rho_1}}{2}\big] + (\underline{v} - v_m) \cdot \Big[\frac{1}{2}(\overline{e} - \frac{1}{2}) \cdot (1-\rho_0) - \frac{\beta}{2}(\overline{e} - \frac{1}{2})(1-\underline{\rho_1})\Big] - d(\overline{e}) - c(\underline{\rho_1}) \\
&> 2\big[\overline{e} + (1-\overline{e})\frac{\rho_0 + \overline{\rho_1}}{2}\big] + (\overline{v} - v_m) \cdot \Big[\frac{1}{2}(\overline{e} - \frac{1}{2}) \cdot (1-\rho_0) - \frac{\beta}{2}(\overline{e} - \frac{1}{2})(1-\overline{\rho_1})\Big] - d(\overline{e}) - c(\overline{\rho_1})
\end{aligned}$$

By assumption, officer $\underline{v}$ exerts high effort when $\rho = \underline{\rho_1}$. If, however, Δ is sufficiently large such that $\underline{v} + \Delta > \tilde{v}(\underline{\rho_1})$, the most hostile officer at the point of selection will expect to exert low effort once employed. We will consider values of Δ sufficiently large that this condition is satisfied.

The condition on Δ for the department's optimal choice to flip is now:

$$\begin{aligned}
&\big[\overline{e} + (1-\overline{e})\frac{\rho_0 + \underline{\rho_1}}{2}\big] + \big[\underline{e} + (1-\underline{e})\frac{\rho_0 + \underline{\rho_1}}{2}\big] \\
&+ (\underline{v} + \Delta - v_m) \cdot \Big[\frac{1}{2}(\underline{e} - \frac{1}{2}) \cdot (1-\rho_0) - \frac{\beta}{2}(\underline{e} - \frac{1}{2})(1-\underline{\rho_1})\Big] - d(\underline{e}) - c(\underline{\rho_1}) \\
&< 2\big[\overline{e} + (1-\overline{e})\frac{\rho_0 + \overline{\rho_1}}{2}\big] + (\overline{v} + \Delta - v_m) \cdot \Big[\frac{1}{2}(\overline{e} - \frac{1}{2}) \cdot (1-\rho_0) - \frac{\beta}{2}(\overline{e} - \frac{1}{2})(1-\overline{\rho_1})\Big] - d(\overline{e}) - c(\overline{\rho_1})
\end{aligned}$$

This inequality reduces to a condition $\Delta > K$ for some K. Therefore, any treatment effect of work sufficiently large that $\Delta > \max\{\tilde{v}(\underline{\rho_1}) - \underline{v}, K\}$ will give a department optimal oversight level of $\rho^*(\Delta) = \overline{\rho_1}$. In this case, only the most sympathetic officers of type $\overline{v}$ will be hired.

Consider a concrete example with numbers for each parameter. Let $\rho_0 = 0$, $\underline{\rho_1} = 0$, $\overline{\rho_1} = \frac{2}{3}$, $\beta = 2$, $\underline{e} = 2/3$, $\overline{e} = 5/6$, $d(\underline{e}) = 0$, $d(\overline{e}) = 1/6$, $c(\underline{\rho_1}) = 0$, $c(\overline{\rho_1}) = 1/4$, $v_m = 1$, $\underline{v} = -4$, and $\overline{v} = 6$, so the support of preferences on the job is $[-4, 6]$. When $\Delta = 0$, the optimal choice is for the department to choose oversight $\underline{\rho_1}$ and induce the most hostile officer into the job. If instead $\Delta = 4$, so the support of preferences at the point of hiring is $[0, 10]$, the department's optimal choice switches to $\overline{\rho_1}$, and they induce the most sympathetic officer into the job.

The intuition behind this result is that, as Δ increases, it becomes increasingly expensive for the department to induce the most hostile officer, since at the point of selection officer

$\underline{v} + \Delta$ receives smaller on-the-job rents as Δ increases. In contrast, it becomes increasingly less expensive to induce the most sympathetic officer, since at the point of selection officer $\overline{v} + \Delta$ receives larger on-the-job rents as Δ increases. There is a value Δ where the department's utility is higher from increasing the oversight level to $\overline{\rho_1}$ and inducing the most sympathetic officer.

Notice that, with $\Delta > 0$, the department may switch from desiring a hostile officer to desiring a sympathetic officer, but there will not be a switch in the opposite direction. The logic would be reversed if $\Delta < 0$, i.e. the job makes officers more sympathetic to suspects. In that case, the presence of a work treatment effect may induce departments to switch from inducing the most sympathetic officer to instead inducing the most hostile officer.