Do Police Unions Increase Misconduct?

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Abstract

I evaluate the impact of police unions on deaths by police and occupational license decertifications. My empirical strategy exploits the staggered rollout of unionization across departments nationwide and union certification elections in Florida, where I compare winning and losing elections. I find impacts that are small and statistically insignificant, and most specifications rule out more than a 10% positive impact. These results are robust to accounting for error in union status and under-reporting of outcomes. While the evidence does suggest that unions reduce civilian oversight and increase legal protection for officers, these impacts do not translate into elevated misconduct.

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Police use of force and misconduct have become growing policy concerns in the United States. In 2019, US police officers are reported to have killed 1,096 individuals, 117 of whom were unarmed.[1] The increasing availability of footage recording police fatal incidents has raised the salience of police violence and led to reductions in public trust in the police. In a recent Gallup poll, a record low 52% of respondents stated they trusted law enforcement, with this figure at 30% for African Americans.[2] This growing distrust has developed into a widespread movement calling for reforms to how officer violence and misconduct are regulated and, more generally, how policing and its institutions are structured.

Unions are often blamed as impediments against firing problem officers and implementing wider reforms of police departments and are, as a consequence, cited as a cause of elevated levels of officer misconduct. Numerous news articles have brought attention to the power of police unions to lobby for protections for repeat-offender officers and for state-level legislation to enshrine job security measures for officers (Friedersdorf, 2014; Kelly *et al.*, 2017; Scheiber *et al.*, 2020; Finnegan, 2020). The academic literature has also stressed that the typical collective bargaining agreement, which stipulates a breadth of terms regulating the handling of misconduct investigations and discipline, may act as a barrier to a department's ability to regulate problematic officer behavior (Fisk and Richardson, 2017; Rushin, 2017).

From a theoretical perspective, the impact of unions on misconduct is ambiguous. While a decrease in the probability of discipline may cause a rise in misconduct, research has shown that unions also typically increase the earnings and fringe benefits of its members (Freeman, 1984; Card, 1996; Frandsen, 2014). An increase in officer pay may lead to an improvement in on-the-job behavior, since the potential loss from being fired is now greater (Becker and Stigler, 1974). Increases in salary have also been shown to improve morale and performance among officers (Mas, 2006), suggesting that unionization may reduce malfeasance by officers. This ambiguity is evident in the broader literature on labor unions, which has found across various occupations both positive (Sojourner *et al.*, 2015; Dube *et al.*, 2016) and negative (Hoxby, 1996; Kinia *et al.*, 2018; Lovenheim and Willén, 2019) impacts of collective bargaining on employee productivity. As recently summarized by Dube *et al.* (2016), "economists have long recognized the possibly contradictory effects of trade unions on worker produc-

[1] Calculation from https://mappingpoliceviolence.org. I exclude from unarmed individuals those who are reported to have been using a vehicle as a weapon, which comprises 66 individuals.

[2] http://news.gallup.com/poll/183704/confidence-police-lowest-years.aspx

tivity and product quality." Ultimately, the relationship between unionization and police misconduct is an empirical question.

There is currently little research that evaluates the impact of unionization on officer behavior, an effort that is hampered by the data challenges in documenting officer behavior. Recent years have seen various federal agencies initiating programs aimed at improving the reporting of civilian deaths by the police (Rushin, 2016),[3] and while these efforts are valuable for measuring current levels of use of force, they are limited in providing information on past cases. Since the current police unionization rate is nearly 80% (Figure 1), any analysis of the impact of unions must rely largely on historical changes in unionization as a source of statistical variation.

In this paper, I estimate the impact of police unionization on proxies for officer misbehavior. In particular, I evaluate its effect on fatal incidents by police and occupational license revocations by state-level certification agencies. My empirical strategy will be a difference-in-differences design, comparing unionizing police departments to those whose statuses do not change over the same time period.

I measure unionization from two sources. First, I use a national survey of police departments, the Law Enforcement Management and Administrative Statistics (LEMAS), which provides information on all large police departments (more than 100 sworn officers) roughly every three years since 1987. Second, I use a hand-collected database of Florida elections to unionize rank-and-file officers in municipal police departments, covering the time period 1972 to 2015. These elections are open to all officers that would be covered by the union and require a greater than 50% vote share to certify the union. My empirical strategy for these data will compare departments that win their election to those that held an election but lost. In both settings, a crucial assumption is that the outcomes for treated and control departments are on parallel trends, and I test this assumption by including union impacts over time in graphical format, including years prior to the change in union status.

To measure fatal incidents by police, I utilize records from FatalEncounters.org, a civilian-

[3] In 2014, Congress passed the Death in Custody Reporting Act (DCRA), requiring police departments to report to the Department of Justice any deaths of individuals who are "detained, under arrest, or [...] in the process of being arrested." The FBI recently announced an expansion of its program for tracking police shootings, which will include recording use-of-force incidents that may not lead to civilian death. And in 2015, the Bureau of Justice Statistics announced a program for collecting information on police fatal incidents using public information such as online news articles.

compiled database drawn from news reports and public records requests, and two federal government databases, maintained by the Federal Bureau of Investigation and the Centers for Disease Control, which are based on police department self-reports and Vital Statistics death records, respectively. To measure license revocations, I utilize data from public records requests made to twenty-six state-level certification agencies. These decertifications typically occur in cases where an officer is found guilty of a "moral conduct violation" and prohibit the officer from continuing work in law enforcement in that state. Decertification records in Florida include all state investigations for misconduct, regardless of whether the officer is found guilty, and I consider impacts on both sustained and unsustained cases from a Florida union election victory.

There is disagreement in the literature over how to properly define police misconduct (Frank, 2009). The outcomes considered here do not represent all cases of misconduct, nor do they necessarily consist solely of cases reflecting clear misbehavior. In particular, there is a heated debate over the degree to which many fatal incidents by police were justifiable. However, the outcomes under consideration merit attention in themselves and, in contrast to other measures of officer behavior, offer a wide geographic coverage and many years of data availability. Therefore, while I describe these outcomes as proxies for misconduct, it is worth noting these important caveats.

Several features of the current setting and approach are ideal for studying the impact of unionization on police officer behavior. First, because the data I examine cover multiple states, and the treatment in question varies at the agency level, the setting enables me to perform regressions that include state-by-year fixed effects. These specifications absorb any idiosyncratic statewide variation in the outcomes and help to rule out that the findings are due to changes in state-level reporting practices over time. The span of data across many states also offers the chance to investigate whether the union impact varies based on state-level labor laws, specifically whether the state is right-to-work or has a law enforcement officer's bill of rights (LEOBR). Second, the outcomes I examine are collected through various sources, providing corroboration that the results are not due to measurement error in any single database.

The evidence suggests that unionization does not lead to meaningful increases in fatal incidents by police or officer decertifications in either the national or Florida sample. Cross-sectionally, unionized departments have higher rates of fatal incidents conditional on number of officers and the crime rate, but the inclusion of department fixed effects significantly reduces the magnitude of the relationship for two of the three measures. Specifications that additionally include state-by-year fixed effects have small and insignificant coefficients for all three outcomes. Conversely, decertifications are less frequent cross-sectionally for unionized departments, though all difference-in-differences regressions have smaller and insignificant point estimates. For the majority of specifications and outcomes, the 95% confidence intervals reject a greater than 10% positive union impact. All the fatal incident measures are relatively infrequent, occurring less than once a year for every four hundred officers, indicating that the results rule out fairly small positive effects. The event study results corroborate these findings and confirm the absence of pre-trends for the treated departments. Analysis of the Florida sample similarly shows a small and statistically insignificant impact of a union election victory on investigations against a department's officers.

Several measurement issues arise with the data I employ. The records on fatal incidents by police are potentially underreported, and though the reported number of incidents for each agency correlates strongly across data sets, the frequency of reporting differs significantly (Loftin *et al.*, 2017; Renner, 2019). The non-standard error induced by under-reporting and the fact that unions may themselves change reporting practices suggest that estimates of the union impact will be biased without accounting for errors in the fatal incident measures. To address this issue, I exploit the availability of multiple estimates for the same underlying count and, assuming that the probability of reporting is independent across data sources, directly estimate the degree of underreporting from each record and construct an estimate of the true count of fatal incidents in each location. This exercise confirms the fact that these data substantially under-report fatal incidents and, perhaps surprisingly, finds that unionized departments tend to have *higher* reporting rates. Using the reporting results to construct estimates of the true number of incidents in place of the reported counts, I continue to find no positive impact of unionization, and the estimated coefficients are if anything more negative.

Decertifications are also susceptible to changes in reporting. The inclusion of state-by-year fixed effects in the national sample accounts for statewide changes in reporting over time but does not address potential variation in department-level reporting effects. One manner for state agencies to initiate investigations is to receive information from the officer's agency about an incident, and this reporting may change under unionization. I examine this issue by exploiting the fact that the Florida decertification records include information on how each case initiated. I conduct a specification in the Florida sample that restricts attention to cases that begin with a news story or civilian complaint to avoid changes in how agencies may report cases to the state certification agency. I find that the results continue to be insignificant with this restricted set of investigations.

I also examine possible measurement error in union status. The national survey occurs every three years or less frequent, leading to a lag in possible changes. Previous research has also found substantial misreporting of union status in survey data (Card, 1996; Lovenheim, 2009). I show that the results are robust to different specifications for union status. Further, I use the availability of two union status measures in Florida to estimate the degree of measurement error in the national data. After adjusting the main specifications for errors in union status, I continue to reject reasonably small positive impacts of unionization on fatal incidents and misconduct.

While the evidence does not confirm the commonly-argued claim that unions lead to elevated officer misconduct, this finding does not strictly conflict with media accounts of unions lobbying for officer protections. Indeed, I find that unionization is associated with a reduced likelihood of a department having a civilian oversight board and a formal internal affairs board, though this second association is statistically insignificant. Further, I find in my sample of Florida misconduct investigations that officers from unionized departments are more likely to have legal representation during their cases. In conjunction with the finding of increased reporting rates among unionized departments, the evidence does indicate that unions change several misconduct-related institutions within their departments. However, I notably find that unions do not decrease the likelihood an investigated officer is found guilty, challenging a central mechanism for a union impact on misconduct. I also find suggestive evidence of an increase in starting and median earnings for unionized officers in Florida (but not in the national sample), highlighting the potential for unions to provide benefits that actually deter misconduct.

Two other recent studies also consider the relationship between collective bargaining and police violence and misbehavior. Dharmapala *et al.* (2018) examine a 2003 Florida court ruling that extended collective bargaining rights to sheriff's officers. Using the comparison group of city police officers, who already had the right to collective bargaining, they find that investigations for misconduct by sheriff's officers increase in the ensuing period. In an ongoing study, Cunningham *et al.* (2020) evaluate the impact of the passage of public-sector collective bargaining rights in the 1960s and 70s. They find that these laws lead to increases in deaths of non-white individuals at the hands of police and no change for white individuals. While these studies focus on the *right* to unionize, my study estimates the impact of unionization in a time period when most officers have the right to unionize.

The rest of the paper proceeds as follows. Section 1 provides institutional background on police unions. Section 2 presents data and summary statistics for both the national and Florida samples. Section 3 through 5 present the empirical strategy, results, and robustness checks. Section 6 considers the union impact on other department outcomes, and Section 7 interprets the results and concludes.

1 Institutional Background

In most states, public sector labor law allows unions to bargain over wages, employment, and the conditions of work. The interpretation of "wages" has generally allowed unions to bargain over all pecuniary and non-pecuniary benefits given to workers, including overtime pay, sick leave, and rules for promotion, and "conditions of work" has been broadly interpreted to include all questions of discipline against officers (Bula, 2005).

In an extensive review of collective bargaining agreements for the largest departments in the country, Rushin (2017) documents the remarkable breadth of terms stipulated in the typical contract. In particular, contracts focus heavily on the terms of disciplinary investigations. Among the departments he considers, the most common disciplinary protections afforded by an agreement are delays in the interrogations of officers suspected of misconduct, officer access to evidence before a misconduct interview, limits to the consideration of disciplinary history, limits to the length of an investigation or a statute of limitations on misconduct, restrictions to making anonymous complaints, limits to civilian oversight, and the requirement of arbitration of disciplinary disputes.

Despite the substantial scope of issues over which unions can bargain, there are statutory limits on the parts of police work which may fall under negotiation. While courts vary across counties and states in their interpretation of what falls under the purview of labor contract negotiation, unions are generally prohibited from bargaining over the details of police work beyond investigations, such as patrol behavior, deployment patterns, or general policing strategy.

Unions also impose restrictions on the types of records that are publicly available, limiting the ability of citizens to be responsive to less-publicized instances of misbehavior. In many instances, strict public records laws have also kept the public from knowing the misconduct history of officers involved in fatal incidents. After Eric Garner was placed in a chokehold and killed by an NYPD officer in July 2014, journalists were unable for several years to acquire any information on how many previous complaints and investigations the officer had received (Baker and Mueller, 2017).

While the majority of the work of union leaders concerns the rights and benefits of officers they represent, unions have also been known to lobby for legislative changes at the local and state level, potentially impacting officers beyond those who are unionized. In 2016, the Philadelphia police union, in response to departmental attempts to disclose the identities of officers involved in civilian shootings, lobbied the state legislature to legally prohibit the release of officer identities (Sullivan *et al.*, 2016). Other examples of state lobbying by officers have been reported in the media (Fang, 2015; Perkins, 2020). Partly in response to such lobbying, many states have passed Law Enforcement Officer Bill of Rights (LEOBRs), which mandate statewide protections for officers in all disciplinary investigations (Rushin, 2017).

While the majority of the literature posits that unions generally function as impediments to departmental reform, Fisk and Richardson (2017) argue that, historically, many interventions aimed at reducing the prevalence of misconduct were only successful because of active cooperation between policy makers and rank-and-file unions. Police departments are hierarchically organized, with limited contact between rank-and-file officers and top management (Fyfe, 2010). The presence of unions, which often act as a go-between for these groups, may improve the ability of patrolmen and sergeants to share their views of a department's challenges and potentially improve the department's functioning.

Police unionization is part of a larger historical trend towards professionalization of public sector workers and a move from a spoils system to a merit system of hiring and firing employees, which began in the late 1800s and continued through the 1970s. The prevailing belief of scholars of government bureaucracy is that the professionalization of civil service has improved its functioning (Rauch, 1995), and the evidence for policing indicates that professionalization has led to a reduction in crime rates (Ornaghi, 2019). Unions provide similar protections for officers, shielding them from at-will firings that may be politically motivated. Therefore, these protections may allow for reduced officer turnover and greater expertise. As with other civil service innovations, the tradeoff with unionization is the possibility of improved expertise versus reduced responsiveness to civilian oversight.

Because of the prevalence of state-level civil service rules, many job protections already exist for public sector workers regardless of collective bargaining status. The Supreme Court has provided some additional protection to officers, ruling in two cases that city managers cannot fire government workers for political reasons (Elrod v. Burns, 1976; Branti v. Finkel, 1980). Because of these protections, it is unclear whether unions are able to guarantee meaningful improvements in job security beyond what is already available to most government employees.

Prior to widespread collective bargaining rights, it was not uncommon for officers to be members of a "union" that collected dues for legal representation and personal insurance but did not have the power to negotiate with the city or county (Burpo, 1971). Even today, some public sector workers in states without collective bargaining are members of unions that engage in "meet-and-confer" discussions with the government over employee contracts (Freeman and Han, 2013). For the purpose of this study, I consider workers in these categories as not unionized, and I define a department as unionized if their rank-and-file officers (i.e. those at the entering rank) are represented by an organization that has the power to collectively bargain over the officers' contracts.

2 Data and Summary Statistics

National Sample – The national data on union status comes from the Law Enforcement Management and Administrative Statistics (LEMAS) survey, which the Bureau of Justice Statistics has conducted in 1987, 1990, 1993, 1997, 2000, 2003, 2007, and 2013.[4] LEMAS contacts the universe of departments with at least 100 sworn officers (comprising 500-700 departments each survey) and a sample of smaller departments. The annual response rate is slightly over 90%. 1700-3000 departments answer each year, and 7600 departments appear in at least one survey.

In order to have sufficiently many survey years for each department, I focus attention on departments that have at least 100 sworn officers in one of the 1987, 1990, or 1993

[4]A shorter survey was conducted in 1999 which did not ask about department union status.

survey years, comprising 752 agencies. While a large share (35.9%) of these departments are surveyed in every year, the majority have at least one survey year missing, which requires that I impute the value for union status. In practice, the majority of departments with a year missing have the same union status for the two adjacent surveys, which I set to be its union status in the missing year. A full description of the imputation procedure is presented in Appendix A. For years between LEMAS surveys, I set union status to be the same as the two most adjacent surveys when they are the same. If union status changes between the two surveys, I set all interim dates to have the later union status. For example, if a department is not unionized in 1997 and is unionized in 2000, I set it as unionized in 1998 and 1999. In Table 5, I show that my primary results do not vary when using a different definition for union status in these interim years.

For the majority of years, the question posed by the LEMAS survey is a variant of "Does your agency authorize or provide any of the following for sworn personnel?" where a list of options includes "collective bargaining rights." This question is unfortunately less precise than ideal. Departments whose officers are not unionized but whose state-level laws permit unionization may check off this option, despite them not being unionized by my definition. Further, the question does not distinguish between department union status for different officer ranks, where the goal is to identify union status for rank-and-file officers. In Section 5, I show that this measure of union status correlates well with the measure constructed in Florida from union elections, which suffers from less measurement error and specifies when rank-and-file officers are covered in the bargaining unit.

LEMAS also records various other information on each department, including starting salary, number of sworn officers, officer demographics, technology usage, and types of specialized units. In later years, they also record whether the department has a civilian complaint process or internal affairs bureau. I supplement this information with data from the FBI Uniform Crime Reports, which report annual measures of total reported crime and violent crime for participating departments.

My primary outcome measures are police fatal incidents and license decertifications. Data on fatal incidents are collected from three sources. FatalEncounters.org is a civilian-run site that records fatal incidents involving police since 2000. These data record the date of the incident, the agencies of the officers involved, and the known names of any involved individuals. The owner of the site collects information from new stories and public records and therefore does not guarantee a complete collection of all shootings.

From the FBI Uniform Crime Reports (UCR), I use the Supplemental Homicide Report (SHR), which reports the number of "felons killed by police" for each department and covers all years of the LEMAS survey. These cases are incidents that are deemed legally justified killings by the department of the involved officers and therefore cover only a subset of cases. I use this data for the years 1985 to 2015.

The third data set on fatal incidents comes from the Centers for Disease Control's Vital Statistics, which provide county-level annual counts of deaths by cause of death and record police killings of civilians under the category of "legal intervention." I collect these data for 1985-2017. For simplicity, in places I refer to Fatal Encounters, Felons Killed by Police, and Vital Statistics by "FE," "FKP," and "VS," respectively.

All three measures of fatal incidents are imperfect. The Fatal Encounters data are collected only as far back as 2000, after about half of the 1987-2013 nationwide increase in unionization. While the Felons Killed by Police measure from the SHR covers a longer range of years, this information does not cover all instances in which an officer may kill an individual. In the department-years for which I observe both variables, there are more than twice as many observations where Fatal Encounters reports an incident than where Felons Killed by Police reports an incident, though the correlation between the variables is 0.77. The Vital Statistics data are only available at the county level, which requires that all regressions using them as an outcome cluster standard errors at the county level.

My additional proxy for misconduct is a record of state-issued license revocations. All states require that individuals receive certification or licensing prior to working as a police officer, and in forty-four states the licensing agency has the power to revoke an officer's license under certain circumstances (Goldman and Puro, 2001). These agencies hold hearings to investigate cases of misconduct or "moral turpitude", which can include incidents that occur when an officer is off-duty. A range of policy prescriptions are available for disciplining officers, the strongest being a full decertification that bars the individual from working as a police officer in the state. These data were received by the Invisible Institute, which placed a public records request with each state licensing agency for their records on decertifications.[5]

[5]These data are now available from USA Today: https://www.usatoday.com/in-depth/news/investigations/2019/04/24/usa-today-revealing-misconduct-records-police-cops/3223984002/

The original data are at the decertification level and report the year of the incident and the agency where the officer worked during the incident.

Florida Sample – To supplement my analysis of the national data, I use records of unionization elections and misconduct in Florida, which provide better measurement of union status and greater detail into each misconduct case. Information on union elections comes from the Public Employee Relations Commission. I collected election tallies for nearly every Florida law enforcement representation election for the years 1975-2015.[6] For each election, these records contain information on the petitioning union, the city/county being petitioned, the unit being petitioned (e.g. corrections officers, rank-and-file, supervisory), the number of eligible members, and the vote counts for each side.

I match the elections to officer-level data from the Florida Department of Law Enforcement (FDLE), the licensing agency for all Florida law enforcement and corrections officers. The FDLE maintains a database of all officer employment spells since 1984,[7] reporting the start and end date of each spell, the reason for separation, the employing agency, the officer's name, birth date, gender, race, and education level.

As the licensing agency for officers, the FDLE also conducts investigations into "moral conduct violations" committed by officers in order to determine whether to decertify them. These data are part of the FDLE database and record the date of each investigation, infraction charged, officer charged, the agency where they worked, and the final disposition of the department (i.e. whether to de-license the officer). If the agent is represented by an attorney, the attorney's information is also provided. These investigations will be my primary measure of misconduct, and I construct counts of all investigations and all investigations with a sustained finding of misconduct.

Crucially, the FDLE data also record how each investigation originated. Many cases begin with the department reporting an incident to the FDLE. Because of this channel for case origination, there may be a concern that unions impact the reporting of incidents and thus may mask any increase in misconduct. To address this concern, I construct a separate count of investigations that originate outside of the officer's department, specifically cases that

[6]Early records were maintained on microfilm, and some documents were simply not possible to attain. Beginning in 1986, I have a record of all elections.

[7]It is not clear exactly when the data begin covering all officer spells. There are officers in the data who begin and end work as early as the late 1960s. The frequency of end-dates for employment spells jumps substantially in 1984, so I take this year as the starting point for my sample.

begin with either a civilian complaint or a news story, which I call "external" investigations.

My initial sample of elections comprises 655 cases. I exclude all elections for state-level departments, such as the Highway Patrol and the Department of Corrections. Because election records do not report the agency being petitioned, only the municipality, I am unable to identify which state agency is being petitioned. I further restrict attention to elections for unions representing rank-and-file patrol officers. These restrictions reduce the sample of elections to 591 cases, 416 of which are to unionize the department and 175 to de-unionize, as described below.

There are four types of union elections in the data: (1) A department is not represented by a union, and a union petitions to represent it; (2) a department has a union, and an individual petitions to remove it; (3) a union exists, and a competing union petitions to replace it; and (4) no union currently exists, and two unions petition to represent it. In case (4), sometimes the unions petition to represent jointly, but much more often the unions are competing to be the sole representative.[8] In case (3), it is possible for voters to choose neither union, which removes any union from the department.

I divide the elections into those to unionize, (1) and (4), and those to de-unionize a department, (2) and (3). In the cases where there are two unions, (3) and (4), the tallies themselves often do not specify whether one is an incumbent union. To address this issue, I link elections within a department across time to determine whether there was already a union prior to the election.

The analysis sample will consist of two types of departments: 1) departments that hold an election to unionize and have a balanced panel of data around the date of the election, where I require five years prior and ten years after the election; and 2) departments that never hold an election and have a full panel of data for the sample period. While the empirical strategy will evaluate the impact of unionization rather than de-unionization, I will use the de-unionization elections to construct an estimate of each department's annual union status. For department with no recorded elections, I measure their union status using the Florida Criminal Justice Agency Profile (CJAP), which has surveyed the state's departments since 2000. The annual union status of departments will be used to validate the national LEMAS measure of unionization in Section 5.

[8] I do not observe whether the unions are competing or cooperative, but according to election officials I spoke with, the latter comprises only $\sim 10\%$ of these cases.

The total collection of elections does not represent the full set of union status changes in the state. There are some departments that only appear in the data with an election to de-unionize, suggesting there was a unionization event that occurred outside of the data available. There are also departments that report in CJAP records from the 2000s to be unionized for which there are no election records. These events are likely unionizations prior to 1975, where the department voluntarily allowed a union to be implemented. For these cases, we have no election tallies. Because of these departments, the unionization rate in 1975 begins at 18% rather than 0%.[9]

2.1 Summary Statistics

Figure 1 plots the share of officers unionized over time in the two settings. The Florida sample covers nearly the entire rise in unionization, beginning with an 18% rate in 1975 and 76% in 2016. The rate exceeded zero before the legalization of collective bargaining since department chiefs could allow unionization among their officers, though they had the right to unilaterally prohibit unionization. Large departments in Florida exhibit a similar rate of unionization throughout the entire period, with slightly higher rates in the late 70s and mid-2000s. The national sample covers a much narrower range of the change in unionization, beginning at 65% in 1993 for the full sample and rising to 70% in 2013. Large departments are asked about unionization in 1987 and 1990 as well, and the unionization rate increases from 69% to 75% in 2013. For all ensuing analysis, my national sample restricts attention to the 752 large departments.

Summary statistics for the national setting are presented in Table 1. The unit of observation is departments in 2013, where departments are presented in separate columns by whether they are always unionized, change in union status, or are never unionized. Agency characteristics vary significantly by unionization, and those who change union status are located in smaller cities (pop. 541,000) than always- and never-unionized agencies (544,000 and 802,000 respectively), employ more officers (540 v. 519 and 360), and employ a larger share of minority officers (0.25 v. 0.24 and 0.20). Departments that are always unionized pay significantly higher salaries than non-unionized and changing-status departments (50,000 v.

[9] It is possible these departments unionized in the sample period but their election records are missing. The data I have available are what PERC could collect from their archives, and it is possible the records are incomplete for early years.

43,000 and 37,700). Never-unionized agencies are more likely to be sheriff's offices, whose jurisdictions are counties rather than cities, explaining why they represent a larger population but employ fewer officers.

The frequency of the misconduct proxies are also different across department types. Never-unionized departments have 0.12 decertifications per 100 officers, compared to 0.11 for changing-status and 0.07 for always-unionized. Across all three measures of fatal incidents, never-unionized departments have fewer reported incidents than changing-status and always-unionized departments: 0.05 FKP cases per 100 officers, compared to 0.13 and 0.08, respectively; 0.28 FE cases, compared to 0.18 and 0.21; and 0.26 VS cases, compared to 0.66 and 1.05.

The overall rates of FE and FKP are fewer than one fatal incident per 400 officers annually for the average department in the sample. This figure is much larger for VS, around one per 128 officers. However, the VS data record all incidents in the department's county and therefore over-counts for each specific department. Nationally, the VS data report roughly half as many cases as FE,[10] suggesting that the true rate of VS cases for individual departments is actually less frequent than FE and FKP.

For the Florida sample, Table 2 presents summary statistics for the departments in 2014, separately by whether the department has always, never, or sometimes been represented by a union. Relative to the overall Florida population, officers are more likely to be white (79% v. 55%) and male (89% v. 49%) and similarly likely to have a bachelor's degree or higher (27% v. 28%).[11] Never-unionized departments are smaller on average than varying and always-unionized departments, both in terms of full-time officers and city population. Similarly, departments that are never unionized tend to have fewer minority officers, female officers, and less educated officers. Departments that never unionize are more likely to be sheriff's offices, representing counties rather than cities.

The final two rows indicate the number of unionizing and de-unionizing elections a department has ever held. The average department that changes union status at some point holds 1.62 elections to unionize and 0.75 to de-unionize. Departments that never unionize still have on average 0.26 elections, and always unionized department have on average 0.18

[10]I make the national comparison between FE and VS because the FKP data only cover a subset of departments.

[11]https://www.census.gov/quickfacts/FL

elections to unionize and 0.04 elections to de-unionize. Note that our designation of always and never unionized depend on our usage of the Florida CJAP survey, which reports union status for the years 2000 onwards. The reason we observe elections to unionize departments which are designated as always-unionized is because no cases in the election records change the department's union status, but CJAP reports them as unionized. This discrepancy may occur because, as noted above, a small subset of elections are missing from the records provided by PERC and because departments sometimes unionize without an election (with the consent of either city leaders or the county sheriff), which would go unreported in the election records.

Table 3 reports summary statistics at the level of an election rather than department. As can be seen by the share victorious, elections to unionize are much more likely to be successful (68%) than those to de-unionize (21%). Unionizing elections are held to represent fewer officers (64) than de-unionizing elections (121).

The frequency of union elections over time has been relatively constant, as shown in Figure 3. Two large increases in the number of elections and unionizations occurred in 1976, shortly after public sector unionization was legalized, and 2003, the year deputy sheriffs were ruled eligible for unionization. While there are some years where all elections resulted in a victory or defeat in elections, Figure 3 indicates that the rate of union victory is roughly consistent throughout the sample period.

If a department holds a unionizing election but does not receive sufficient votes to unionize, they have the opportunity to hold a new election in 12 months. Therefore, there is the possibility that losing department unionize in the near future. This fact is shown in Figure 4, which plots the unionization rate over time, separately for winning and losing department. By ten years after an election, 10% of unionized departments have removed their union. Conversely, more than 50% of departments that failed to unionize at first have successfully voted for and certified a union within 10 years of the original election. Note that the unionization rate for departments is not zero in the years prior to the election. This fact arises because, as noted above, departments can de-unionize without an election. Therefore, the departments who are listed as being unionized in the pre-period likely de-unionized without an election.

3 Empirical Strategy

National Sample – For the national data, I first conduct a difference-in-differences regression of the impact of unionization, exploiting variation within departments over time,

$$Y_{it} = \beta \text{Union}_{it} + \mu_i + \gamma_{s(i)t} + X_{it}\alpha + \epsilon_{it} \tag{1}$$

where the object of interest is β, the effect of unionization. I include in X_{it} the logarithm of the department's employment and municipal population and the log of crime, violent crime, crime clearances, and violent crime clearances from the UCR.[12]

My preferred specification will include agency fixed effects, μ_i, and state-by-year fixed effects, $\gamma_{s(i)t}$. The state-time dummy variables allow for statewide changes in reporting standards, though it does not correct for any potential within-department changes in reporting correlated with unionization. My primary tables will also present specifications with 1) year fixed effects alone (to document cross-sectional union "impacts"), 2) year effects and agency effects, and 3) state-by-year effects, agency effects, and agency time trends.

The variable Union_{it} will be defined as a department having ever been unionized by a certain date. In other words, a department will be considered treated if they were unionized in the past but are currently not unionized. Relative to using current union status as the treatment variable, this choice has two benefits. First, unionization can impact many department characteristics that are unlikely to disappear if the officers remove their union, such as the establishment of a civilian complaint review board, work hour restrictions, protections during internal affairs investigations, etc. If de-unionization has a smaller absolute impact on our outcomes than the initial unionization event, a coefficient that captures both changes will be small relative to the true coefficient for unionization. Second, initial unionization is much better measured than de-unionization. As discussed in Appendix Section A, there are several instances of departments in the LEMAS data apparently de-unionizing in a single survey year and otherwise reporting as unionized. Regardless, in Table 5 shows the main difference-in-differences result for current union status, and the coefficients are unchanged.

The primary threat to identifying β is that the timing of unionization may be correlated with a time-varying shock to the outcome measures. For example, officers may choose to

[12] For departments that do not report crime data to the UCR, I set those values to zero and include indicators for these variables missing.

unionize at times when deaths at the hands of police are particularly high to seek out greater employment protections. I will assess this concern by plotting the time path of changes in police behavior around unionization in an event study design:

$$Y_{it} = \sum_{\substack{\tau \in [-\underline{\tau}, \overline{\tau}], \\ \tau \neq -1}} \beta_\tau \text{Union}_{it}^{\tau} + \mu_i + \gamma_{s(i)t} + X_{it}\alpha + \epsilon_{it} \tag{2}$$

where Union_{it}^{τ} is a dummy for department i in year t having unionized τ years ago. For $\tau \geq 0$, the coefficients β^{τ} estimate the effect τ years after unionization. For $\tau < -1$, the coefficients β^{τ} provide placebo tests of the validity of the empirical strategy. If departments with different union statuses are on parallel trends and changes in unionization are uncorrelated with time-varying shocks to Y_{it}, then the pre-period coefficients should be statistically insignificant. The dummy variables for $\tau = \overline{\tau}$ and $\tau = \underline{\tau}$ include all years past those dates, so that the coefficients are identified relative to the year prior to unionization.

Since LEMAS is not conducted every year, I set Union_{it}^{0} equal to one for all years between the surveys where a department switches to unionized. For example, a department that goes from not unionized in 1997 to unionized in 2000 will have $\text{Union}_{it}^{0} = 1$ in 1998, 1999, and 2000, $\text{Union}_{it}^{1} = 1$ in 2001, and $\text{Union}_{it}^{-1} = 1$ in 1997.

As described above, each misconduct proxy is available for a unique time window and set of states. For each outcome, I restrict attention to the set of departments that have a full panel of observations for the years $-\underline{\tau}$ to $\overline{\tau}$ around the unionization event and all departments that do not change their union status. This restriction reduces the concern that differences across β^{τ} are due to compositional changes in the departments observed for each year after unionization. For all specifications, the outcome I consider is the log of the department-level count of incidents or decertifications, plus one to avoid dropping cases with no incidents. In Section 5, I consider specifications that instead use counts of the data.

Florida Sample – To understand the impact of a unionization election on misconduct among Florida departments, I employ two complementary empirical strategies. First, I perform event study regressions that compare all winners and losers over time around the election date:

$$Y_{et\tau} = \alpha + \delta_\tau + \beta_\tau \cdot \text{UnionWin}_e + \eta_t + \gamma_e + \epsilon_{et\tau} \tag{3}$$

The unit of observation is an election e in year t that is τ years from the election date. The object of interest is β_τ which indicates the impact of winning the election on outcome $Y_{et\tau}$ relative to losing the election. The coefficients for years prior to the election result serve as specification tests and should be insignificant if winning and losing elections are on parallel trends. As before, all outcomes are the log of the department-level counts of incidents, plus one to avoid dropping cases with no incidents. I restrict the sample to elections for which there are observations of the outcome variable for 5 years prior and 10 years after the election. Departments that have no elections to unionize are also included in the sample.

Because these regressions are at the level of each election, department-years that appear near multiple elections will be duplicated. I address the impact this duplication may have on standard errors by clustering at the agency level. Some research has pointed to concerns with using a "stacked" approach as above to deal with units that have multiple events. Sandler and Sandler (2014) argue that a better approach to achieving an unbiased estimate of the time path of the impact of an event is to use a "summed" specification, where each agency has one panel where all election variables are added together. This approach is the second empirical strategy I use. I again focus attention on elections for which I have data 5 years prior and 10 years after the date of the election, which I call "focal" elections, while also controlling for changes in future union status:

$$\begin{aligned} Y_{it} &= \alpha + \sum_{\tau}\sum_{k} \delta_\tau \text{ElectionHeld}_{it}^{\tau k} + \sum_{\tau}\sum_{k} \beta_\tau \text{ElectionWon}_{it}^{\tau k} \\ &\quad + \mu \sum_{k} \text{OtherUnionize}_{it}^{k} + \lambda \sum_{k} \text{OtherDe-Unionize}_{it}^{k} + \eta_t + \nu_i + \epsilon_{it} \qquad (4) \end{aligned}$$

$\text{ElectionHeld}_{it}^{\tau k}$ is an indicator that department i's "focal" election k was held τ years prior to year t (or after year t if τ is negative). Similarly, $\text{ElectionWon}_{it}^{\tau k}$ is an indicator that department i's focal election k was won τ years prior to year t. For elections where there is not a balanced panel of data around them, indicators $\text{OtherUnionize}_{it}^{k}$ and $\text{OtherDe-Unionize}_{it}^{k}$ control for changes in union status caused by these non-focal elections. η_t is a year fixed effect, and ν_c is an agency fixed effect.

The coefficients of interest are β_τ, which indicate how departments with union victories vary over time from departments with union losses. As a placebo test, I check that the coefficients for the years prior to the election are not statistically different from zero.

As with the stacked panel, the sample of observations include all departments with an election with a balanced panel of data around it. To better estimate the year fixed effects, I similarly include in the sample the set of departments which never hold an election and for which I observe the outcome variable for all years.

While I use an event study design as the primary empirical strategy, an alternative approach would be to exploit the discontinuity in union status around the cutoff in votes to unionize. This approach is presented in Appendix B. I focus on a comparison between all elections because the sample of elections where the vote share is close to 50% is somewhat small, leading to issues of statistical power.

4 Results

I present here the results for both samples. Across both settings and all outcomes, my estimates of union impacts are small and statistically insignificant in my preferred specifications.

National Results – The primary difference-in-differences results are presented in Table 4. Each panel presents a different outcome variable, the first three being the fatal incident measures and the bottom panel being the decertification records, and the columns progressively include additional fixed effects in the specification. All regressions control for the department's log of employment and crime level and violent crime level from the UCR.

The first column shows the cross-sectional relationship between unionization and the proxy measures, and we see that unionized departments have significantly higher rates of all measures of fatal incidents, on the order of seven percent. These relationships, however, disappear for FKP and VS when adding agency-level fixed effects in Column (2). The reduction in significance occurs not from an increase in standard errors but from a significant reduction in the point estimates. When controlling for state-by-year fixed effects in Column (3), the coefficient for FE becomes insignificant, and the coefficients for FKP and VS become negative. The coefficients and standard errors in Column (3) rule out more than a 11.5% increase for FE, a 1.6% increase for FKP, and a 2.9% increase for VS deaths. Column (4) includes agency-level time trends, and the coefficients and standard errors are very similar to Column (3), indicating that unionizing departments are likely not on significantly different time trends.

Panel D shows the regression coefficients for decertifications. Cross-sectionally, unionized departments tend to have *fewer* incidents, but this relationship also becomes insignificant with the addition of agency fixed effects. Similar to the fatal incident measures, we can rule out small positive union impacts, greater than 2.8% and 3.9% increases in decertifications in Columns (2) and (3), respectively.

Figure 5 documents the estimation coefficients from Equation 2, which includes agency and state-by-year fixed effects. For each outcome, I include four years after the year of unionization and three years prior, where the year prior to unionization is the excluded coefficient. In all cases, we continue to see no significant increases in our proxy measures. Because we now use a balanced panel of observations and are identifying each coefficient from fewer department-years, the standard errors are substantially larger. In all cases, we continue to reject increases of greater than 20% for every outcome. The pre-trends for all outcome are also close to zero. Of the eight pre-period coefficients across outcomes, only one is statistically significant (period -3 for FE), suggesting that departments who are treated do not have significantly different trends from untreated departments.

To further probe the robustness of these results, Figure 6 presents estimates of the union coefficient separately by year of unionization. Specifically, I rerun Equation 1, where I only include agencies that do not change union status and those that unionize in a particular year. The results continue to be small and insignificant across years, though some years have few observations and thus very large standard errors. The only year whose unionizing departments have an estimated significant increase is 1993 for the Vital Statistics data.

Florida Results – The results for the stacked panel approach are presented in Figure 7. The outcomes are the log of all incidents, incidents that begin with an external origin, and all sustained incidents.

The top left panel shows the impact of unionization on all investigations. Similar to the national sample, the estimates are small and statistically insignificant. Though the standard errors are somewhat large, we can reject a greater than 20% increase in the frequency of investigations. Investigations occur approximately once for every 93 officers each year, again indicating that we can rule out fairly small impacts.

The top right panel considers the union impact on "external" investigations, which originate with a news story or civilian complaint. These cases avoid the concern that investigations originating with the department may decline from reduced reporting. We find here small and statistically insignificant point estimates, and we can reject a greater than 10%

increase in the frequency of cases. Since we are finding a similar result as the specification with all investigations, there does not appear to be any evidence that union manipulation of reporting is biasing our estimates. The bottom left panel presents the estimates for the impact on sustained investigations and also finds no significant positive impacts.

The results of the summed panel approach of Equation 4, which addresses the concern from the stacked approach of accounting for follow-up changed to union status, are presented in Figure 8. Again, we find no evidence of an increase in any of our measures of misconduct. The impacts become perhaps more negative, with the coefficients for total investigations becoming significantly negative eight to ten years after the union election.

5 Robustness Checks

One of the primary challenges in evaluating the union impact on police misconduct is that both treatment and outcome variables are likely to suffer from mis-measurement. Misreporting of union status in surveys is a prevalent concern in the labor economics literature on unions, present for both individual-level (Card, 1996) and agency-level (Lovenheim, 2009) surveys. The criminology literature has similarly found consistent evidence of under-reporting of fatal incidents by police in all three databases utilized in my nationwide analysis (Loftin *et al.*, 2017; Renner, 2019). In this section, I consider various robustness checks to evaluate the strength of my findings, with a particular focus on addressing these measurement error issues.

Union Status Definition – The LEMAS survey is not conducted in every year, so the available union status variable is only updated in survey years. For the primary specifications, I set union status to change the earliest year possible. For example, if a department reports to be non-unionized in 1997 but is unionized in 2000, I set the department as unionized in 1998 and 1999. However, it is clearly uncertain when the department's union status changed. This type of problem is common for variables that are only periodically updated, such as population counts (Freedman *et al.*, 2018).

I evaluate the effect these errors may have on my estimates by redoing the main analysis, where union status is only changed in the year a survey occurs. In other words, a department who first reports to be unionized in 2000 will have union status equal to zero in 1998 and 1999. The results of this analysis are presented in Table 5, where Columns (1) and (2) present the baseline specifications, and Columns (3) and (4) use the later union status change. The findings remain similar for all outcomes. The point estimates continue to be negative or insignificant, and we can rule out relatively large positive impacts. The point estimates for FKP and VS become insignificant and more negative in the specifications with state-year fixed effects.

Another assumption made in the baseline specification is that there is no effect from de-unionization. If we imagine that there is a positive impact of unionization on misconduct and that this effect disappears if a department removes its union, the baseline specification which uses a variable for post-unionized may be biased towards finding no effect. To address this concern, in Columns (5) and (6) I present the main regression results with current union status as the treatment variable. The results do not change meaningfully. For the specification without state-by-year fixed effects, the FE regression coefficient actually *decreases* from 0.068 to 0.043 and loses significance, and the coefficient with state-by-year fixed effects is also decreased from 0.045 to 0.019. The magnitude and significance of the coefficients for all other outcomes are essentially unchanged.

Florida Union Status as a Validation Check – While the national LEMAS surveys do not provide the exact date of union status changes, and may suffer from additional measurement error, the Florida unionization data provide a more precise measure of a department's annual union status. Lewbel (2007) shows that, with misclassification in a binary regressor, the expectation of the regression coefficient is $\mu = m \cdot \theta$, where in our case $m = Pr(Union* = 1|Union = 1) - Pr(Union* = 1|Union = 0)$, $Union*$ is the mis-measured union status, $Union$ is true union status, and θ is the regression coefficient for true union status. To correct for misclassification, I use a two-step approach. First, I calculate m using the union status inferred from the Florida elections data, where I presume that the latter is measured without error. I then adjust my main regression estimates using the inflated estimate $\hat{\mu}/\hat{m}$.

In Table 6, I present the results of regressing union status from LEMAS on union status from the Florida elections data. The first and second columns present the relationship with no fixed effects and with year fixed effects, showing coefficients of 0.701 and 0.708. The third column is the preferred specification and shows the relationship with the addition of agency fixed effects. The coefficient is 0.338. The fourth column restricts attention to only years where we know the LEMAS survey, and the coefficient increases to 0.379. The fact that the coefficient does not substantially increase in these years suggests that the disparity is not predominately driven by lags in updating union status from the LEMAS survey. Instead, the disparity is likely due to differences in union status definition. The elections-based measure is constructed explicitly for rank-and-file officers, whereas the LEMAS survey question does not allow departments to specify which officers are represented. Indeed, among Florida departments that are not unionized based on the elections data, 24% report to be unionized in the LEMAS survey. This figure is 94% among departments that are unionized based on the election data, suggesting that much of the discrepancy is due to the LEMAS measure capturing unionization for more than just rank-and-file officers.

Using the relationship between the LEMAS and elections-based union status measures in Florida, I adjust my estimates of the main diff-diff regression coefficients in Table 7. Specifically, I inflate the estimates to $\theta = \mu/m$, where m corresponds to the coefficients in Columns (2) and (3) of Table 6, depending on whether the diff-diff regression includes agency-level fixed effects or not. Despite the increased magnitude of the coefficients, we can continue to reject large union impacts for all outcomes. For the FE, FKP, and VS outcomes, the top of the 95% confidence intervals are 0.22, 0.04, 0.06, respectively, and the analogous figure for decertifications is 0.10. With the exception of the Fatal Encounters data, we can still rule out small impacts of unionization despite accounting for a substantial degree of misclassification.

Adjusting Misconduct Proxies for Under-reporting – While the above two robustness checks address imprecision in my measure of union status, there are also issues with the available measures of misconduct. Measurement error in reports of fatal incidents may bias the results in two ways. First, counts are very likely *downwards* biased, because cases are more likely to go unreported than for false cases to be reported. Second, unionization may change the degree of reporting for an agency. While a police union may pressure a department to underreport fatal incident figures or indirectly do so by strengthening privacy protections for officers and their misconduct records, they may also professionalize certain procedures, such as disciplinary investigations, and cause an increase in the reporting rate.

Here I present a procedure for estimating the degree of under-reporting in fatal incidents that exploits the availability of more than one measure for the same statistic. This approach exploits the fact that, under the assumption that the probability a given incident is reported is independent across data sets, the discrepancy in counts across data allows us to identify the degree of under-reporting.

Let Y_{it} be the number of fatal incidents in a department-year, which is drawn from some discrete probability distribution $f(y)$. I assume that each fatal incident has an independent and identical probability p_k of being reported in data k. Therefore, conditional on $Y = y$, the probability that data k reports x incidents is $\binom{y}{x} p_k^x \cdot (1 - p_k)^{y-x}$. I further assume that the probability an incident is reported in data k is independent of whether it is reported in data $k' \neq k$.

Because we do not see the true number of occurrences, we want to calculate the likelihood of observing the reported counts summed over all possible realizations of Y:

$$\begin{aligned} Pr(N_{it}^1 = n_1, N_{it}^2 = n_2) &= \sum_y Pr(N_{it}^1 = n_1, N_{it}^2 = n_2 | Y = y) \cdot f(y) \\ &= \sum_{y \geq \max\{n_1, n_2\}} \binom{y}{n_1} p_1^{n_1} (1 - p_1)^{y-n_1} \binom{y}{n_2} p_2^{n_2} (1 - p_2)^{y-n_2} \cdot f(y) \end{aligned} \tag{5}$$

The objects of interest are the probabilities of reporting in each data set, p_1, p_2, and the underlying distribution of true incidents, $f(y)$. For estimation tractability, I model the true incidents as being drawn from a mixture distribution between a probability θ of having zero incidents and a geometric distribution with parameter ρ conditional on having any incidents: $f(y) = \theta \cdot \mathbb{I}(y = 0) + (1-\theta)(1-\rho)^{y-1}\rho \cdot \mathbb{I}(y > 0)$. I estimate these parameters using maximum likelihood on the probability distribution of observed counts from the Fatal Incidents and Felons Killed by Police data, which report their counts by department.

Of course, the reporting rates and frequency of incidents may vary across department types and union status. I allow for all model parameters to be a function of department size, which I break into above or below the 75th percentile of number of officers, and whether the department is unionized, and I estimate all parameters separately by year.

Figure 9 presents the estimates of reporting probabilities, with the left two panels displaying Fatal Encounters estimates and the right two panels displaying Felons Killed by Police estimates. Three insights arise from this figure. First, both data sources appear to suffer from significant under-reporting, with the estimates of reporting probability ranging 20-60%. Second, the reporting rate fluctuates over time. With the exception of FKP for unionized departments, all other groups and outcomes exhibit an increase in reporting probabilities throughout the sample period. Third, there are marked differences in reporting between unionized and non-unionized departments, with estimates for unionized departments typically higher than for non-unionized departments. Crucially, the union gap in reporting appears to decline over time. Without accounting for these differences in reporting between departments and years, estimates of the union impact may confound changes in the frequency of incidents with mere changes in the reporting of incidents.

I now take the parameter estimates from both approaches and construct estimates for the true number of incidents, $E(Y|N_{it}^1, N_{it}^2)$. In the place of the reported data, I run the baseline regression on these estimates of "expected incidents," and the results are presented in the third row of Table 8. The first two rows present FE and FKP regressions for the restricted sample where both outcomes are observed. The results become more negative with the adjustment for reporting. For the specification with state-by-year fixed effects, the point estimate is a 6.2% decline in adjusted incidents. These estimates provide further evidence that reporting measurement concerns are not masking a true positive impact of unions on misconduct.

State-level Union Impacts – While the impact of unionization on one measure of deaths by police, from Fatal Encounters, is positive with agency and year fixed effects, all impacts are insignificant and close to zero with the inclusion of state-by-year fixed effects. One concern with the state-by-year fixed effects specification is the fact that unions may play a role in shaping state-level law related to policing and protections for officers. This influence is due to significant resources spent on lobbying state legislators (Perkins, 2020). If union lobbying efforts lead to changes in police behavior that impact all departments regardless of union status, the state-by-year fixed effects may mask some of the union's impact.

I probe this concern in two ways. First, I directly consider the impact of the state unionization rate on the main outcomes. Second, I evaluate how the union impact varies across states with different state-level police labor laws.

Table 10 presents a modification of the baseline specifications with agency and year fixed effects that includes a measure of statewide police unionization rates. This measure is calculated by aggregating union status from all departments reporting in each LEMAS year (weighted by number of sworn officers), where non-LEMAS years are filled in with the union status from the following survey, as described in Section 2.1. If we believe that unions have an impact on misconduct through lobbying for state-level legislation protecting officers, we should expect a positive coefficient on the state unionization rate.

The first three columns present the regressions for fatal incidents. The coefficient on the state union rate is 0.1 (s.e. 0.06) for FE, 0.17 (0.05) for FKP, and -0.16 (0.13) for VS. Only the impact on FKP is significant, suggesting that statewide unionization leads to heightened deaths by police. However, the magnitude of the coefficient is somewhat small. The standard deviation in the state unionization rate (after residualizing agency and year fixed effects) is 0.08. So a two standard deviation increase in the state union rate would lead to a 2.7% increase in the FKP outcome. This positive and significant relationship does not persist using the "expected incidents" outcomes that adjust for reporting, shown in Column (4) with a coefficient of -0.28 (0.09). The state union rate coefficient for the decertification regression, shown in Column (5), is -0.139 (0.09). These findings suggest that, while the state-level union rate may play a role in elevating misconduct, the impact is at most small in magnitude and therefore unlikely to explain why the inclusion of state-year fixed effects lead to insignificant union impacts. Further, the significance of coefficient does not persist after accounting for reporting effects.

I next consider how the effect of unionization varies with state-level laws. As discussed in Section 1, union representatives often lobby for legislation that guarantees protections for officers during disciplinary cases, protections which are often codified in a Law Enforcement Officer's Bill of Rights (LEOBR). While these state protections may mute the impact of unions by already providing some of the guarantees typically provided in a collective bargaining agreement, states with these protections may be more open to allowing for increased officer protections during collective bargaining. Another important institutional factor is whether a state is right-to-work, which allows employees to join a unionized company or agency without formally joining the union (Lumsden and Petersen, 1975; Ellwood and Fine, 1987).[13] While the presence of a right-to-work law may reduce a union's bargaining strength, either through reduced funding or a reduction in its perceived legitimacy, unions may be more responsive to its membership if employees have the option to stop paying dues. Therefore, the impact on eventual policing outcomes is similarly ambiguous.

To study the variation in the union impact across states, I rerun the main difference-in-differences analyses separately by whether a state has a LEOBR and whether it is a right-

[13] In right to work states, employees who are not dues-paying members of the union are still covered by any collective bargaining agreement between the union and the municipality. However, they forgo other union benefits, such as legal representation in misconduct cases.

to-work state. The results are presented in Table 9. Each row presents the union impact for a different subsample of departments, and each column presents a different outcome. The estimates for each fatal incident measure separately suggests that the union impacts on fatal incidents do not vary significantly with state institutions. Across all three measures, the only positive and significant coefficient is for FKP in the states that are right-to-work and have a LEOBR. Similarly, I do not find significant differences in the union impact on decertifications, which is insignificant for all subsamples. These findings suggest that the lack of a positive union impact is robust to these paticular state-level institutional factors.

Specification Robustness – All the above analysis has been conducted with specifications that use a logged count of the outcome variables. One concern is that this estimation approach does not appropriately account for the skewness in the distribution of the outcomes or the fact that the data are counts of discrete events. In Tables A.1 and A.2, I consider whether the above results are sensitive to instead using the count of incidents as the outcome or using a poisson regression.

The first three columns present the regressions on counts of incidents, and the final three columns present poisson regressions, with progressively more saturated fixed effects. Columns (1) and (4), which include only year controls, offer the same conclusion as the baseline models, that unionized departments have more reported fatal incidents. The inclusion of agency controls in Columns (2) and (5) reduce the magnitude of all coefficients and render them insignificant. The inclusion of state-by-year fixed effects in Columns (3) and (6) also provide statistically insignificant coefficients, with the exception of a negative and statistically significant coefficient for the poisson regression on FKP.

The regressions of reporting-adjusted measure of fatal incidents are presented in Table A.2 and provide more negative coefficients, consistent with the baseline results. The final row presents the decertification models. Similar to the baseline estimates, we observe a cross-sectional negative relationship between unionization and decertifications which becomes smaller with the inclusion of agency fixed effects and insignificant with state-by-year fixed effects. The only discrepancy with the baseline estimates is that the poisson specification with agency fixed effects has a significant negative coefficient. Overall, these estimates confirm the robustness of the main conclusion that we can rule out relatively small positive union impacts on fatal incidents and decertifications.

6 Other Outcomes

The estimates so far suggest that unions do not lead to an increase in deaths by police or officer license revocations. These effects are robust to accounting for measurement error in union status and the outcomes and are robust to using various regression specifications. In contrast, the estimation of reporting effects in Section 5 suggests that unions are associated with higher rates of fatal incidents reporting for both the FE and FKP measures. In this section, I consider whether unions change other features of a police department related to officer performance and discipline.

Figure 11 presents estimates from the national sample on the impact of unionization on various other department-level characteristics related to performance and misconduct. The first row asks whether unionization is associated with a department's general adoption of computer technology, where the measure of computer usage is a summation across LEMAS questions on computer storage of various types of criminal records, data access during patrol, and types of criminal analyses conducted on computers. A full description of the construction of the measure is provided in Appendix Section A and follows closely the approach of Garicano and Heaton (2010). Across all specifications, the relationship between unionization and computer usage is small and statistically insignificant. The largest coefficient indicates only 0.28 more forms of computer usage among unionized departments, off a base of 8.2. The second row examines department organization complexity, where the variable is similarly constructed from a sum of LEMAS questions about various types of specialized units (further described in Appendix Section A). While unionized departments have fewer specialized units when comparing departments cross-sectionally, this association is small (-0.18 off a base of 3.2), and the coefficient becomes statistically insignificant in all specifications which include department fixed effects.

The third row evaluates the union impact on the usage of community policing. Beginning in 1997, LEMAS asks what share of a department's officers have received at least eight hours of training in some form of community policing. I find that unionization has a small and positive but significant relationship with community policing. While the coefficient increases with the inclusion of department fixed effects, increased standard errors lead to insignificant coefficients for all other specifications.

I next consider the impact of unionization on institutions for misconduct oversight. The

2000, 2003, and 2007 LEMAS surveys ask departments about the presence of a civilian complaint review board and internal affairs bureau, which I examine in rows four and five. Civilian complaints have been shown to strongly correlate with other measures of officer misbehavior (Rozema and Schanzenbach, 2019), and the public availability of information on complaints has been found to lead to reductions in officer misbehavior (Rivera and Ba, 2019). Cross-sectionally, unionized departments are more likely to have a civilian complaint review board. However, the relationship becomes negative and significant with the inclusion of department fixed effects. Off a base of 16%, unionization has a -11 percentage point effect on the likelihood of having a civilian complaint board. In contrast, internal affairs bureaus are less common cross-sectionally in unionized departments. The coefficient continues to be negative and the same magnitude with the inclusion of department fixed effects, though the increased standard errors lead to insignificance. These results must be interpreted with caution because of the large standard errors and short panel from which the effects are identified. However, these findings lend some support to media accounts of unions acting as impediments to internal reforms of misconduct oversight.

In Table 12, I evaluate the impact of unionization on whether an attorney is present to represent the officer in an investigation and whether an investigation is sustained and leads to discipline for the officer, measures which are available in the Florida investigations data. Because we no longer have a balanced panel of observations, and to increase statistical precision, the unionization variable is collapsed to an indicator for whether a department is unionized in the year in which an investigation occurs.[14] Consistent with the fact that unions tend to provide legal representation to their members, the likelihood an officer has an attorney representing them during an investigation increases after unionization. Surprisingly, however, I found no impact of unionization on whether a case leads to discipline for the officer conditional on being investigated. This null finding stands in contrast with the belief that unions are better able to shield officers from scrutiny and may help to explain the small and insignificant union impacts on fatal incidents and decertifications.

Economic and Demographic Impacts – While institutional characteristics that shape how officers are monitored and disciplined could play a role in the likelihood an officer engages in misconduct, the economics literature has naturally focused on the role of economic

[14] I run $Y_{it} = \beta_1 \text{Union}_{d(i)t} + \mu_{d(i)} + \gamma_t + \epsilon_{it}$, where the unit observation i is an investigation, and $d(i)$ denotes the department of the investigation.

incentives in shaping employee malfeasance. Officers who are paid better will suffer more from the loss of employment, and thus increased earnings may act as a deterrent from misbehavior (Becker and Stigler, 1974). Unionization may also change the composition of workers, which can have an ambiguous impact on officer misbehavior. Private sector unions have typically been found to provide increases in earnings on the order of 5-15% for its members (Freeman, 1984; Blanchflower and Bryson, 2004), but the literature on public-sector employees has been more mixed (Bartel and Lewin, 1981; Lovenheim, 2009; Frandsen, 2016). Here, I ask whether unionization impacts officer earnings, employment, and department-level demographics.

For my national sample, I measure earnings through the LEMAS survey, which provides departments' starting salaries for officers. To measure earnings impacts in Florida, I gathered salary data on all individuals partaking in the state pension system for the years 1973-2015 from the Florida Retirement System (FRS). These data cover each individual's annual earnings, department of hire, and starting date. I use these data to construct a measure of the annual starting earnings of officers. More information on these data are provided in Appendix Section A, including how I construct the measure of starting earnings.

Estimates for the national sample are presented in Appendix Table A.3, where each row presents a different outcome and each column progressively adds more saturated fixed effects. The first row shows that, while unionized departments pay officers starting salaries that are substantially higher cross-sectionally than non-unionized departments, this apparent union salary premium becomes insignificant and *negative* with the inclusion of agency fixed effects and then state-by-year fixed effects. The inclusion of agency time trends leads to a marginally significant negative union impact on earnings of 0.06. The next two rows show impacts on employment from LEMAS and LEOKA, respectively, and do not show a consistent impact in either direction. Police administrative data are known to contain significant reporting errors in manpower figures (Chalfin and McCrary, 2017), so these estimates suffer from some imprecision and are unable to rule out either large union impacts in either direction. The bottom two panels show a small and marginally significant positive union impact on share female and small and marginally significant negative union impact on share minority.

Economic and demographic impacts for the Florida sample are presented in Figures A.1-A.3. In contrast to the national sample, the Florida sample shows evidence of a significant union impact on starting and median earnings by six to ten years after unionization, on the order of a 5-10% increase. Similar to the national sample, I find limited evidence of a significant change in employment. Unionization does not appear to change department separation and new hire rates, officer experience, or share minority, share female, and share with a bachelor's degree.

In line with the existing literature on public sector collective bargaining, I find conflicting evidence of a union pay premium, present in the Florida sample but not nationally. Much further investigation is needed to clearly understand the economic and compositional impacts of police unions, but this suggestive evidence indicates that earnings increases may act as a counterbalancing union impact that deters rather than promotes misconduct.

7 Conclusion

The United States is undergoing a heated debate about the appropriate limits of police use of force and the proper policy tools for addressing excessive force and other forms of officer misconduct. Many critics and scholars have pointed to police unions as a potential culprit for heightened levels of police misconduct, but there has been limited evidence on the impact of police unions on officer behavior.

In this paper, I collect records on unionization from large police departments nationwide and all departments in Florida and link them to measures of fatal incidents by police and state-level license decertifications for misconduct. In contrast to public perception, the empirical evidence suggests that unions have small and statistically insignificant impacts on misconduct. I further show that my results are robust to accounting for various forms of measurement error in both union status and the outcome measures of fatal incidents and misconduct.

There are several potential explanations for why the empirical evidence points to at most a small union impact on misconduct. The primary outcome measures are relatively rare, and the lack of an impact of unionization on deaths by police or decertifications does not rule out impacts on lower levels of misconduct such as excessive use of non-fatal force. However, the crucial presumption that unions are able to successfully protect problem officers is not borne out among Florida misconduct investigations. The question remains whether officers would respond to policies that substantially impact the probability of discipline for misconduct.

Because of the challenges in the reporting of deaths by police, there is precious little known about its determinants (Zimring, 2017). While I consider here the impact of a particular institutional characteristic, the presence of a union, there is still uncertainty about the importance of more general features of the criminal environment, such as the number of officers and the crime rate. In Appendix Table A.4, I show an expanded version of Table 4 to directly examine the relationship between my main outcome measures and these variables. Surprisingly, the magnitude of the coefficients on the number of officers and various crime measures are small and largely insignificant, and the within-fixed-effects R^2 are very small. While these relationships are only observational and do not necessarily correspond to causal impacts, recent work by Chalfin *et al.* (2020) also finds that the causal impact of the number of sworn officers on deaths by police is small and statistically insignificant. There is much more work needed examining the determinants of deaths by police.

The present analysis highlights that a central challenge in studying unionization and police misconduct is that both treatment and outcome variables suffer from measurement issues. Additional studies compiling detailed data on union status and measures of misconduct, perhaps from individual states' records, would provide valuable insight into this question. While this study presents a strategy to correct for under-reporting in existing databases on fatal incidents, the current execution of this approach is hopefully only a start in exploring methods to account for under-reporting in this context. Going forward, state and national policy could play a valuable role in mandating better reporting for policing outcomes. Evaluation of any policy is only as good as the measurements available, and improvements in policing data are much needed.

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Table 1: Summary Statistics – National Sample

	(1) Always Unionized	(2) Variation	(3) Never Unionized	(4) All
Full-Time Officers	518.72	539.64	359.86	482.08
City Population / 1000	543.84	541.24	802.00	597.77
Sheriff's Office	0.23	0.35	0.51	0.33
Share Minority Officers	0.24	0.25	0.20	0.24
Share Female Officers	0.24	0.27	0.28	0.25
Starting Salary / 1000	49.98	43.00	37.70	45.88
Decertifications per 100 Officers	0.07	0.11	0.12	0.10
Felons Killed by Police per 100 officers	0.08	0.13	0.05	0.09
Fatal Encounters per 100 Officers	0.21	0.28	0.18	0.22
Vital Stat Deaths per 100 Officers	1.05	0.66	0.26	0.78
Share	0.555	0.195	0.246	1
N	417	147	185	752

Notes: Observations are at the level of the police department, recorded in 2013.

Table 2: Summary Statistics – Florida Sample

	(1) Always Unionized	(2) Variation	(3) Never Unionized	(4) All
Full-Time Officers	112.20	136.71	57.58	115.61
City Population / 1000	53.44	71.55	32.45	60.91
Sheriff's Office	0.18	0.15	0.39	0.21
Share Minority	0.29	0.22	0.15	0.21
Share Female	0.12	0.12	0.09	0.11
Share w Bachelor's or Higher	0.28	0.29	0.19	0.27
Starting Monthly Earnings	3206.42	3736.03	2733.77	3386.90
Number of Investigations	1.74	1.33	0.82	1.24
Number of External Investigations	0.48	0.37	0.20	0.34
Number of Sustained Investigations	0.26	0.20	0.17	0.20
Unionize Elections	0.18	1.62	0.26	1.17
De-Unionize Elections	0.04	0.75	0.00	0.51
Share	0.083	0.676	0.241	1
N	27	219	78	324

Notes: This table presents summary statistics at the agency level. Data on department demographics are taken from the FDLE employment spells records, and starting monthly earnings are taken from the Florida Retirement System, as described in Section 6 and Appendix Section A. Union status comes from Florida Public Employees Relations Commission. Monthly starting earnings are reported only for departments that take part in the Florida Retirement System.

Table 3: Summary Statistics for Florida Elections

	Unionization	De-Unionization	Total
Win	0.750	0.246	0.601
Vote Share	0.682	0.212	0.538
Number Eligible	64.26	120.9	81.89
Share Dept. Eligible	0.829	0.828	0.829
Observations	416	175	591

Notes: Unionization election-level data. Records are taken from the Florida Public Employees Relations Commission. In de-unionization elections, vote share is for removing the union, and for unionization elections the vote share is for instating the union. De-Unionization elections consist of both petitions from individuals explicitly to de-unionize and petitions from competing unions, in which case members are given the option to vote for neither union.

Table 4: Difference-in-Differences Results

A. Fatal Encounters	(1)	(2)	(3)	(4)
Unionized	0.064***	0.067*	0.044	0.024
	(0.019)	(0.037)	(0.036)	(0.054)
R-Squared	0.301	0.594	0.624	0.661
Outcome Mean	0.323	0.323	0.323	0.323
N	12008	12008	11975	11975

B. Felons Killed By Police	(1)	(2)	(3)	(4)
Unionized	0.044***	0.021	-0.029	0.018
	(0.014)	(0.022)	(0.023)	(0.025)
R-Squared	0.364	0.604	0.636	0.676
Outcome Mean	0.184	0.184	0.184	0.184
N	20694	20694	20566	20566

C. Vital Statistics	(1)	(2)	(3)	(4)
Unionized	0.234**	-0.018	-0.014	0.044
	(0.094)	(0.026)	(0.022)	(0.028)
R-Squared	0.077	0.698	0.753	0.775
Outcome Mean	0.488	0.488	0.489	0.489
N	23101	23101	23023	23023

D. Decertifications	(1)	(2)	(3)	(4)
Unionized	-0.106***	-0.037	-0.030	0.012
	(0.025)	(0.033)	(0.035)	(0.042)
R-Squared	0.072	0.478	0.537	0.586
Outcome Mean	0.205	0.205	0.205	0.205
N	10215	10215	10163	10163
Year Controls	X	X	X	X
Agency Controls		X	X	X
State X Year Controls			X	X
Agency Time Trends				X

Notes: Observations are at the level of the department-by-year, and the outcome is the log of the count of incidents (plus one). The vital statistics regressions are also at the department-by-year level, but the outcome is at the county-by-year level. Accordingly, Panels A, B, and D regressions cluster standard errors at the agency level, and Panel C regressions cluster standard errors at the county level.

Table 5: Alternative Specifications of Difference-in-Difference Regressions

A. Fatal Encounters	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.067*	0.044	0.034	-0.018	0.041	0.017
	(0.037)	(0.036)	(0.039)	(0.040)	(0.028)	(0.028)
R-Squared	0.594	0.623	0.593	0.623	0.593	0.623
Outcome Mean	0.324	0.324	0.325	0.325	0.325	0.326
N	11976	11943	11928	11895	11900	11867
B. Felons Killed By Police	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.022	-0.029	0.021	-0.031	0.007	-0.030
	(0.022)	(0.023)	(0.026)	(0.030)	(0.019)	(0.021)
R-Squared	0.604	0.636	0.604	0.635	0.604	0.636
Outcome Mean	0.184	0.184	0.185	0.185	0.185	0.185
N	20694	20566	20595	20467	20552	20424
C. Vital Statistics	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	-0.024	-0.017	-0.022	-0.045*	-0.026	-0.017
	(0.026)	(0.022)	(0.026)	(0.025)	(0.022)	(0.018)
R-Squared	0.699	0.754	0.698	0.753	0.699	0.754
Outcome Mean	0.488	0.489	0.490	0.490	0.490	0.491
N	23069	22991	22976	22898	22926	22848
D. Decertifications	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	-0.037	-0.031	-0.051	-0.016	-0.038	-0.032
	(0.033)	(0.035)	(0.040)	(0.041)	(0.032)	(0.033)
R-Squared	0.478	0.537	0.479	0.537	0.479	0.537
Outcome Mean	0.205	0.205	0.203	0.204	0.204	0.205
N	10215	10163	10182	10130	10160	10108
Union Definition	Baseline	Baseline	Late	Late	Non-Cumulative	Non-Cumulative
State X Year Controls		X		X		X

Notes: Columns (1) and (2) report the same specifications as Columns (2) and (3) in Table 4. Columns (3) and (4) change union status to only adjust in years when the LEMAS survey occurs. Columns (5) and (6) use indicator for whether the department has ever been unionized as union status.

Table 6: LEMAS Union Status Compared to Florida Election-Based Union Status

LEMAS Union	(1)	(2)	(3)	(4)
Florida Union	0.701**	0.708**	0.338**	0.379**
	(0.053)	(0.059)	(0.072)	(0.081)
Outcome Mean	0.622	0.622	0.622	0.588
N	2210	2210	2210	520
Year Controls		X	X	X
Agency Controls			X	X
All Years	X	X	X	
LEMAS Years				X

Notes: Regressions are at the department-year level and cover all large departments in Florida. The outcome variable is union status as measured in the LEMAS data, and the regressor is union status as inferred from the Florida elections data. Union status for departments who never appear in the elections data are drawn from the Florida Criminal Justice Agency Profile (CJAP).

Table 7: Difference-in-Differences Results, Adjusting for Union Status Measurement Error

A. Fatal Encounters	(1)	(2)	(3)	(4)
Unionized	0.074**	0.120	0.049	-0.032
	(0.029)	(0.087)	(0.084)	(0.119)
Outcome Mean	0.325	0.325	0.326	0.326
N	11900	11900	11867	11867
B. Felons Killed By Police	(1)	(2)	(3)	(4)
Unionized	0.054**	0.022	-0.088	-0.011
	(0.021)	(0.057)	(0.064)	(0.053)
Outcome Mean	0.185	0.185	0.185	0.185
N	20552	20552	20424	20424
C. Vital Statistics	(1)	(2)	(3)	(4)
Unionized	0.295**	-0.076	-0.051	0.034
	(0.130)	(0.067)	(0.055)	(0.069)
Outcome Mean	0.490	0.490	0.491	0.491
N	22926	22926	22848	22848
D. Decertifications	(1)	(2)	(3)	(4)
Unionized	-0.154***	-0.112	-0.093	0.008
	(0.037)	(0.097)	(0.099)	(0.126)
Outcome Mean	0.204	0.204	0.205	0.205
N	10160	10160	10108	10108
Year Controls	X	X	X	X
Agency Controls		X	X	X
State X Year Controls			X	X
Agency Time Trends				X

Notes: Regression coefficients are adjusted for measurement error in union status, as explained in Section 5. The first column of coefficients are inflated by the coefficient in Column (2) of Table 6, and the coefficients in Columns (2) to (4) are inflated by the coefficient in Column (3) of Table 6. Standard errors are constructed using the delta method. Observations are at the level of the department-by-year, and the outcome is the log of the count of incidents (plus one). The vital statistics regressions are also at the department-by-year level, but the outcome is at the county-by-year level. Panels A, B, and D regressions cluster standard errors at the agency level, and Panel C regressions cluster standard errors at the county level.

Table 8: Difference-in-Differences Results For Reporting-Adjusted Estimates

A. Fatal Encounters, 2000-2015	(1)	(2)	(3)	(4)
Unionized	0.026	0.029	0.002	-0.060
	(0.025)	(0.033)	(0.033)	(0.051)
R-Squared	0.354	0.635	0.663	0.694
Outcome Mean	0.346	0.346	0.345	0.345
N	7545	7545	7481	7481
B. Felons Killed By Police, 2000-2015	(1)	(2)	(3)	(4)
Unionized	0.033	0.038*	0.014	0.004
	(0.021)	(0.023)	(0.026)	(0.042)
R-Squared	0.396	0.670	0.696	0.724
Outcome Mean	0.214	0.214	0.214	0.214
N	7513	7513	7433	7433
C. Reporting Adjusted Incidents, 2000-2015	(1)	(2)	(3)	(4)
Unionized	-0.097**	-0.121***	-0.104**	-0.133*
	(0.039)	(0.047)	(0.049)	(0.078)
R-Squared	0.450	0.684	0.712	0.740
Outcome Mean	0.646	0.646	0.646	0.646
N	7545	7545	7481	7481
Year Controls	X	X	X	X
Agency Controls		X	X	X
State X Year Controls			X	X
Agency Time Trends				X

Notes: Observations are at the level of the department-by-year, and the outcome is the log of the count of incidents (plus one). The FKP incidents are aggregated to the county level, and "Reporting Adjusted Incidents" are constructed as discussed in Section 5. The sample is restricted to department-years where measures of FKP and Vital Statistics are available.

Table 9: Difference-in-Differences Results Interacted With State-Level Laws

	(1) FE	(2) FKP	(3) Vital Stats	(4) Expected Incidents	(5) Decertifications
No Right-to-Work, No LEOBR	-0.015	0.026	-0.008	-0.136	-0.020
	(0.033)	(0.037)	(0.034)	(0.083)	(0.083)
No Right-to-Work, LEOBR	0.000	-0.121	-0.021	.	0.000
	(0.000)	(0.088)	(0.084)	()	(0.000)
Right-to-Work, No LEOBR	0.078	-0.019	-0.000	-0.118	-0.072
	(0.065)	(0.036)	(0.051)	(0.104)	(0.054)
Right-to-Work, LEOBR	0.032	0.089**	-0.016	-0.008	0.002
	(0.064)	(0.043)	(0.043)	(0.162)	(0.041)

Notes: Observations are at the level of the department-by-year, and the outcome is the log of the count of incidents (plus one). The vital statistics regressions are also at the department-by-year level, but the outcome is at the county-by-year level. The "Expected Incidents" outcome is constructed as described in Section 5 using FE and FKP. Columns (1), (2), (4), and (5) regressions cluster standard errors at the agency level, and the Column (3) regression clusters standard errors at the county level. "LEOBR" stands for "Law Enforcement Officers' Bill of Rights."

Table 10: Difference-in-Differences with State-level Union Control

	(1) Fatal Encounters	(2) Felons Killed by Police	(3) Vital Statistics	(4) Expected Incidents	(5) Decertifications
Union	0.0511	-0.00461	0.00714	-0.0831	-0.0306
	(0.0379)	(0.0229)	(0.0247)	(0.0674)	(0.0341)
State Union	0.101	0.170***	-0.161	-0.284***	-0.139
	(0.0617)	(0.0519)	(0.129)	(0.0927)	(0.0913)
Observations	12008	20694	23101	7545	10215
Mean	0.323	0.184	0.488	0.646	0.205
R-squared	0.594	0.604	0.698	0.684	0.475

Notes: State-level union rate is constructed by averaging reported union status in the LEMAS, weighted by number of sworn officers. All departments are used to calculate the state-level rate, not just large departments included in the analysis sample. Each regression includes agency fixed effects and year fixed effects.

Table 11: Union Impact on Other Department Policies

Computer Usage	(1)	(2)	(3)	(4)
Unionized	0.189*	0.062	0.071	0.283
	(0.098)	(0.176)	(0.195)	(0.292)
R-Squared	0.507	0.642	0.675	0.747
Outcome Mean	8.179	8.180	8.199	8.199
N	4921	4916	4878	4878
Special Units	(1)	(2)	(3)	(4)
Unionized	-0.113	0.111	-0.007	-0.263
	(0.074)	(0.140)	(0.147)	(0.213)
R-Squared	0.282	0.516	0.555	0.653
Outcome Mean	3.230	3.234	3.240	3.240
N	4720	4702	4666	4666
Community Policing	(1)	(2)	(3)	(4)
Unionized	0.028*	0.046	0.043	0.015
	(0.015)	(0.035)	(0.039)	(0.069)
R-Squared	0.070	0.385	0.436	0.636
Outcome Mean	0.598	0.601	0.601	0.601
N	3033	2989	2967	2967
Civilian Complaint Board	(1)	(2)	(3)	(4)
Unionized	0.084***	-0.117*	-0.109**	-0.115
	(0.022)	(0.059)	(0.056)	(0.108)
R-Squared	0.152	0.766	0.783	0.911
Outcome Mean	0.162	0.161	0.161	0.161
N	1926	1870	1858	1858
Internal Affairs	(1)	(2)	(3)	(4)
Unionized	-0.041*	-0.042	-0.084	-0.064
	(0.022)	(0.066)	(0.078)	(0.160)
R-Squared	0.105	0.668	0.692	0.859
Outcome Mean	0.837	0.841	0.843	0.843
N	1909	1856	1844	1844
Year Controls	X	X	X	X
Agency Controls		X	X	X
State X Year Controls			X	X
Agency Time Trends				X

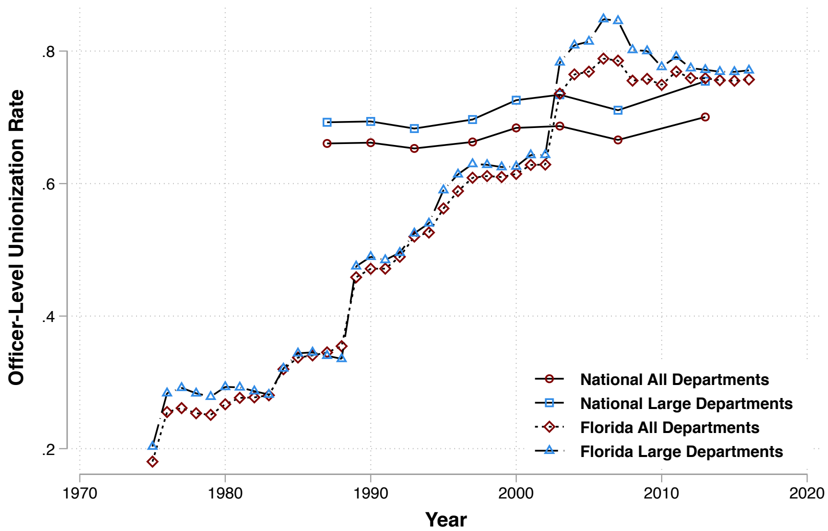
Notes: The unit of observation is agency by year. All outcomes are measured from the LEMAS survey. Computer usage and special units variable construction are described in Appendix Section A.

Table 12: Attorney Representation Effect of Unionization

	(1) Attorney Present	(2) Attorney Present	(3) Disciplined	(4) Disciplined
Unionized	0.0352***	0.0395***	-0.00976	-0.00910
	(0.0105)	(0.0135)	(0.0207)	(0.0243)
Mean	0.073	0.073	0.301	0.301
Observations	13039	13039	13039	13039
Agency Time Trend		X		X

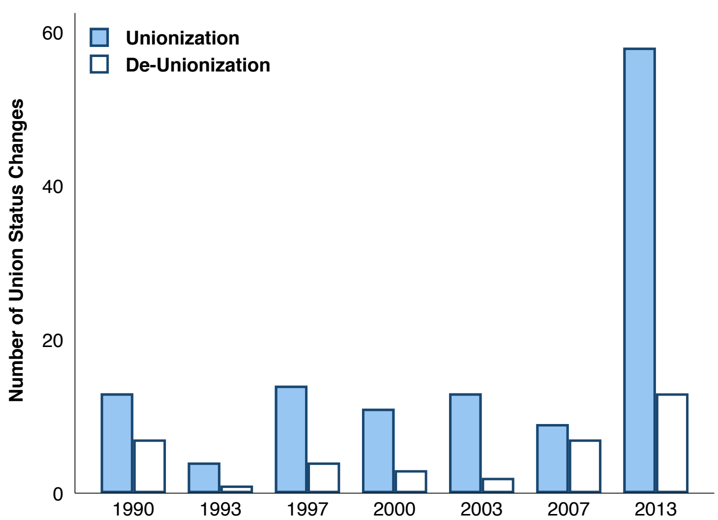
Notes: Regressions estimates coinciding with the equation in Footnote 14. The unit of observation is an officer investigation. Because misconduct investigations do not occur every year for every department, I do not require that the panel be balanced around the date of an election for each department.

Figure 1: Unionization Rate Over Time



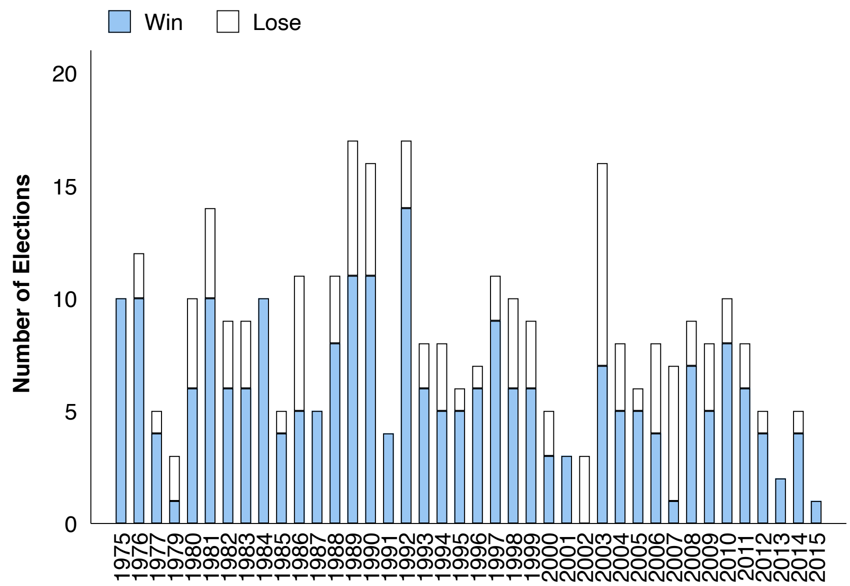
Notes: Unionization rate over time nationally and in Florida. Large departments are those with at least 100 sworn officers in 1987. The national unionization rates are taken from the LEMAS data. The Florida unionization rates are calculated from the PERC election records and the CJAP data. These shares are at the officer level and represents officers who are covered by a collective bargaining agreement, not who are in a union themselves.

Figure 2: Number of Unionization Changes



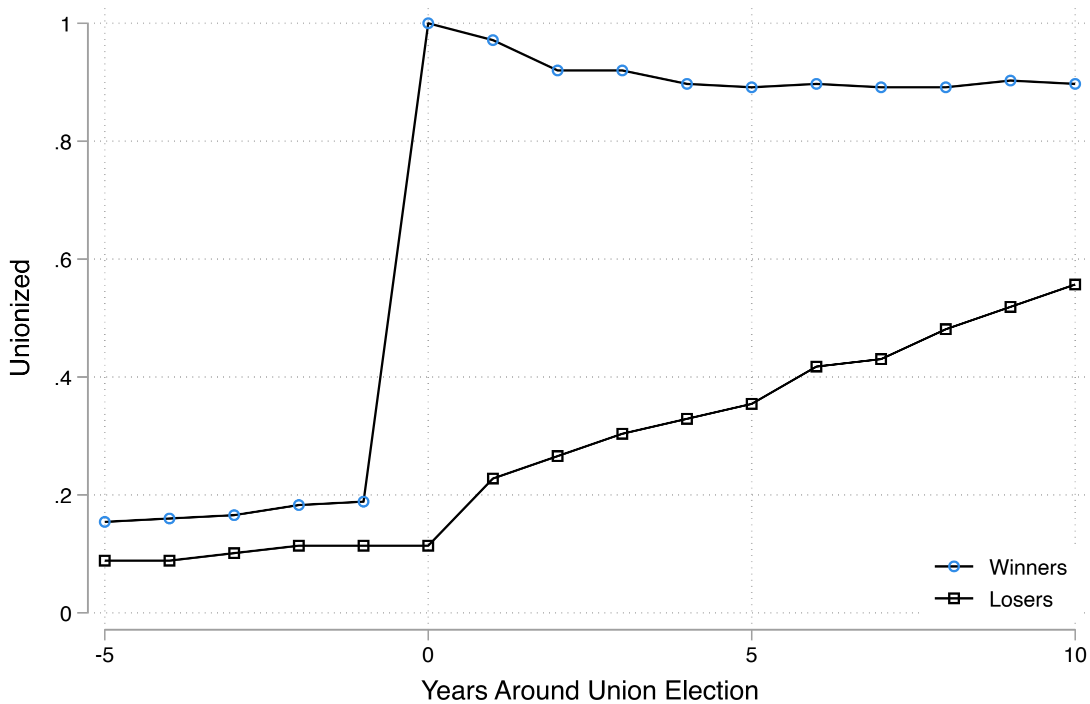
Notes: Counts of unionization changes in the LEMAS data.

Figure 3: Florida Elections Over Time



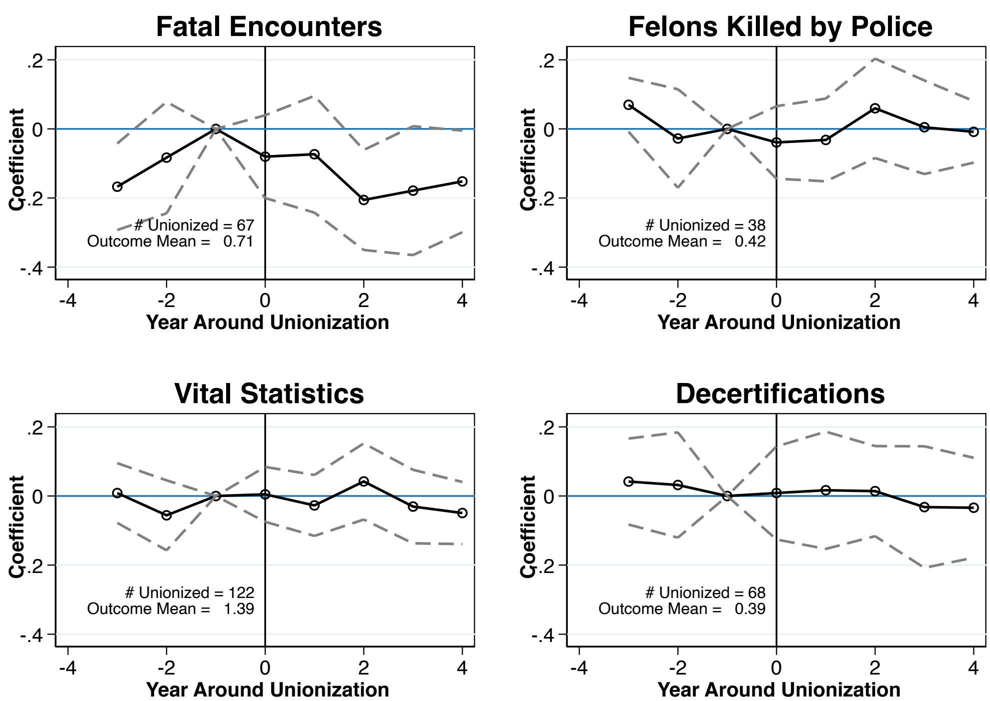
Notes: Reports the frequency of winning and losing elections over time, for all elections to unionize. Does not elections to de-unionize.

Figure 4: Event Study of Unionization Rate Around Elections



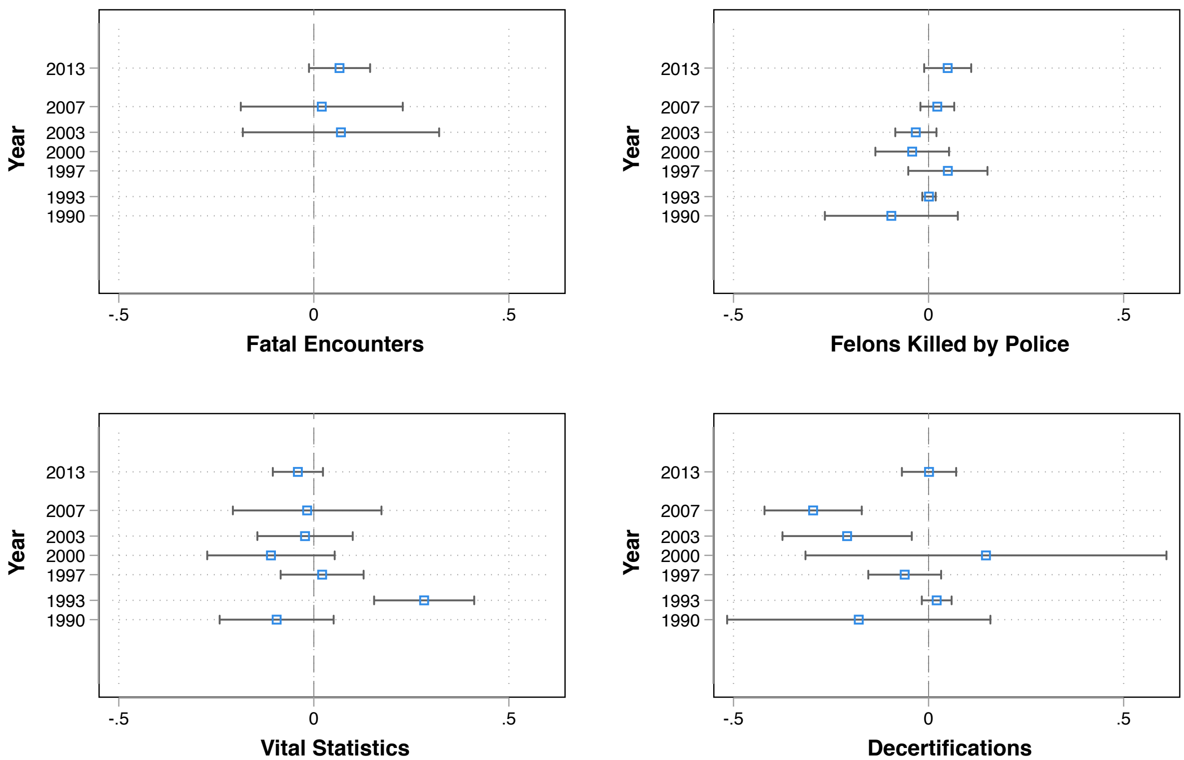
Notes: This figure shows the rate of unionization in the years around an election to unionize, separately by whether the election is won or lost.

Figure 5: Event Study Regression Results



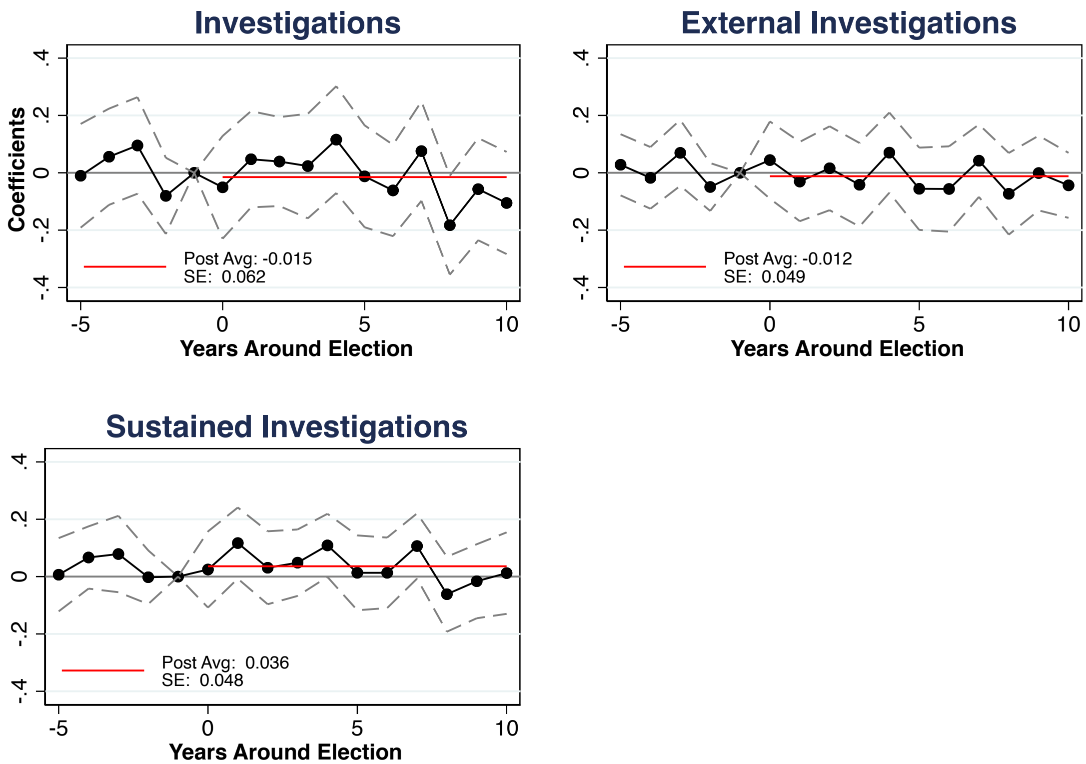
Notes: Figures report the event study regressions from Equation 2. Each regression restricts attention to departments with a full set of observations around the date of unionization or that have no change in union status.

Figure 6: Difference-in-Differences Results by Year of Unionization



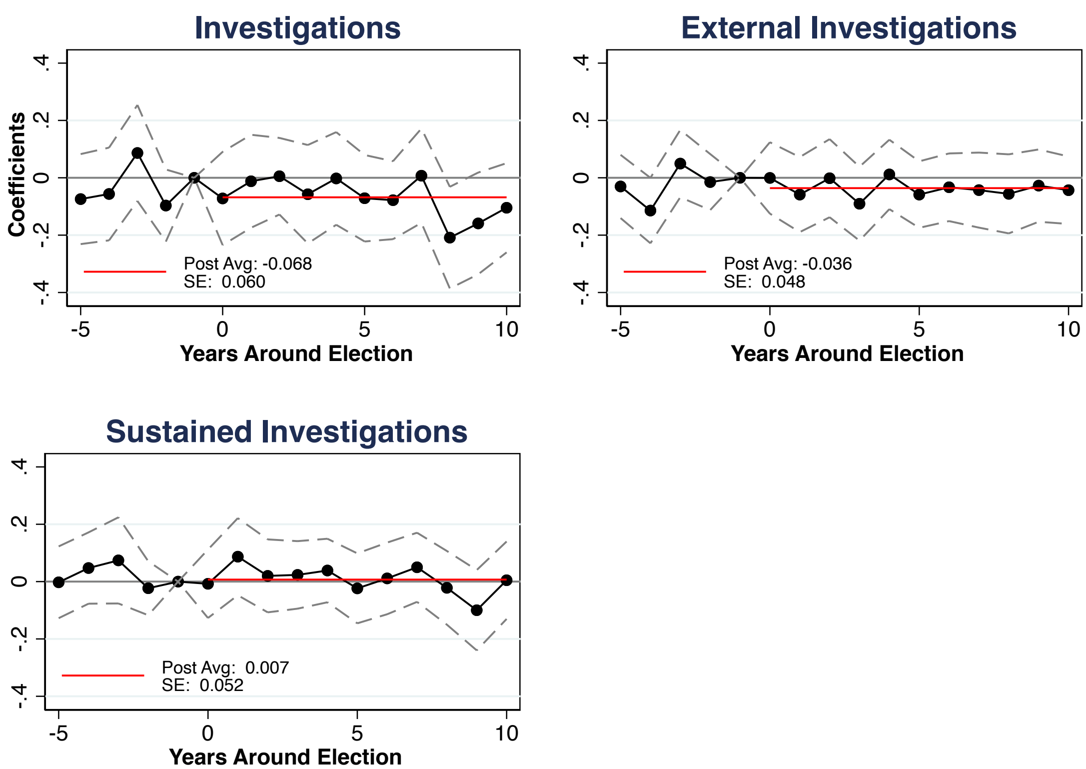
Notes: Each panel presents the difference-in-differences regressions for a separate outcome, separate by year of unionization. For each year, I include departments that unionized that year and the departments whose union status does not change.

Figure 7: Investigations Event Study Coefficients, "Stacked" Approach



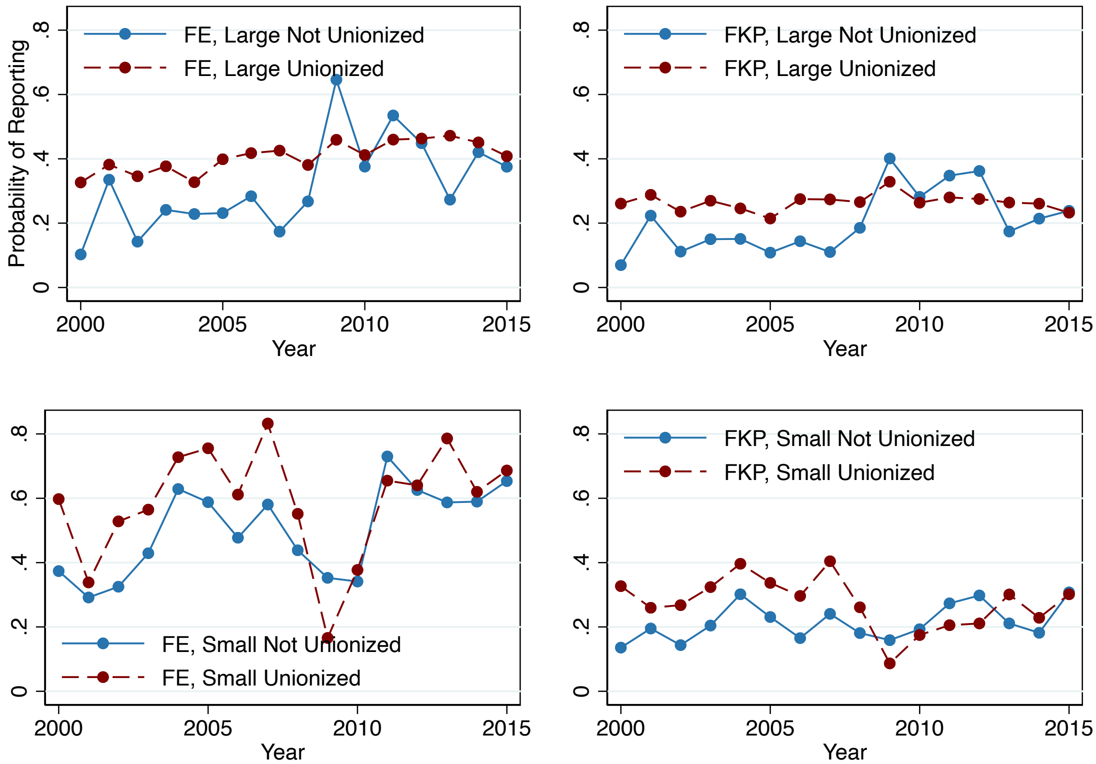
Notes: Regression coefficients $\hat{\beta}_\tau$ presented from the regression equation 3, where the outcome is number of investigations of officers, and the unit of observation is department-year. The red line presents the average value for all post-period coefficients.

Figure 8: Investigations Event Study Coefficients, "Summed" Approach



Notes: Regression coefficients $\hat{\beta}_\tau$ presented from the regression equation 4, where the outcome is number of investigations of officers, and the unit of observation is department-year. Red line presents the average of post-period $\hat{\beta}_\tau$.

Figure 9: Probability of Reporting Fatal Incident, 2000-2015



Notes: Figure presents the probability of reporting a fatal incident in the Fatal Encounters and Felons Killed by Police data, estimated from Equation 5. Estimation uses data at the department-year level.

Appendix

A Data Appendix

Law Enforcement Management and Administrative Statistics

The LEMAS surveys does not provide an identifier that allows perfect linkage across all years. In some years, the UCR ORI identification number is provided. For all other years, I use agency name and state to link to the UCR Agency crosswalk. I then use the ORI identification number to link across years.

The questions on union status changed between the 2007 and 2013 surveys. Up through 2007, the questionnaire asked some variant of the question "Does your agency authorize or provide any of the following for sworn personnel?" where a list of options includes "collective bargaining rights." In 2003, the questionnaire asked "Is collective bargaining authorized for your agency's employees?" where there are boxes for both sworn and non-sworn employees.

In 2013, departments were asked two questions about collective bargaining. First, "As of January 1, 2013, were the interests of SWORN personnel represented by a COLLECTIVE BARGAINING ORGANIZATION?" and "As of January 1, 2013, what was the status of the COLLECTIVE BARGAINING AGREEMENT between your agency and its SWORN personnel?" For the second question, the options are "active", "expired", or "No collective bargaining agreement."

The natural choice for measuring 2013 union status is the yes or no answer to the first question. However, the rate of reporting "yes" is extremely high. Based on conversations with an employee at the Bureau of Justice Statistics, I decided to use the second measure, where union status is reported as one if "active" or "expired" is chosen. The results do not, however, change meaningfully using the alternative measure of union status.

The LEMAS union status measure is cleaned to correct for various instances of apparent measurement error. If a department is (not) unionized in all but one survey which is not the first or last survey, then the status is changed to (not) unionized in the exception year. If a department reports that is (not) unionized in all years that it is surveyed, I fill in that it is (not) unionized in all non-surveyed years. If a department is not surveyed in a given year but reports to be (not) unionized in the nearest previous and succeeding years in which it is surveyed, I fill that year to be (not) unionized. Finally, I replace union status in state-years where the state law contradicts the reported union status, where state laws are collected from Reuben (1996). For example, Iowa requires unionization beginning in 1992. Therefore, all departments are set to unionized in the years following.

LEMAS - Computer Usage

The LEMAS survey asks various questions about computer usage each year, though the specific questions have evolved over time. Beginning in 1987, the survey asks whether computer files are used to store various forms of information: crime reports, arrests, calls for service, traffic stops, warrants, stolen property, and criminal histories.[15] In addition, it asks whether computers are used in the following operations: call dispatch, criminal investigations, and crime analysis. Beginning in 1997, the survey asks whether any of the following databases are available during patrol: motor vehicle records, driving records, and criminal history records. In 2013, the survey dropped all questions about which operations use computers and restricted its questions on computer files to whether criminal incidents are recorded on a computer. My measure for a department's computer usage is the summation of all indicators for the available questions for each year. While the range of possible values of the measure varies by year, the inclusion of year fixed effects will account for this variation.

LEMAS - Special Units

My measure for department specialization is a count of the number of special units the department reports to have in LEMAS. I use the following units, which the surveys list for 1987 to 2007: child abuse, community crime prevention, family/domestic violence, drug education, driving under the influence, missing children, repeat offenders, and victim assistance. This list is based on the choice of Garicano and Heaton (2010), with the removal of police/prosecutor relations because it is missing in 2007.

For most years, the survey asks the following question: "How does your agency address the following problems/tasks?" One option is "Agency has special unit with full-time personnel."

In 1997, the survey asks for the number of sworn officers who are assigned full time to each unit, and we denote the department as having a unit if at least one officer is assigned. However, the overall rates for each unit are much higher in this year, suggesting that depart-

[15]The survey asks about other types of files and computer usages. I only list here the variables I use in my measure of computer usage.

ments interpreted the question differently from other years. The inclusion of fixed effects at the year and state-by-year will account for this variation in the overall rate of reporting.

Vital Statistics

The Center for Disease Control maintains records of all deaths in the United States and their cause. Their public records provide information on the frequency of deaths by various causes at the county-by-year level.

These data were collected from the WONDER online database, `https://wonder.cdc.gov/`. Deaths by police fall under the code "legal intervention." The 1983-1998 (1999-2017) records used the ICD-9 (ICD-10) code for cause of death, for which legal interventions are Y70-Y79 (Y35).

"Legal intervention" deaths do not perfectly overlap with deaths by police. A fatal incident by an on-duty military officer would fall under this category. Further, an individual may have died during a police incident where the proximate cause is another death code (e.g. automobile accident).

The linkage of these data to each agency is at the county level, so any two agencies in the same county have the same vital statistics values. Therefore, all regressions using vital statistics cluster at the county level.

Fatal Encounters

These data were collected from `https://fatalencounters.org`. The data are at the level of the incident and record the deceased's name, age, gender, and race (which is often missing), the date and location of the incident, the agency or agencies involved, the cause of death, a brief description of the encounter, an official disposition of the case (which is often missing), and a link to the original article used to identify the case.

These data include all encounters where police are involved, even when the fatality is not caused by the officers. Therefore, I remove cases where the reported cause of death is suicide. I then aggregate the incidents to the agency-year level and match to the LEMAS data using a fuzzy merge on agency name and state. In cases where more than one agency is involved, each agency has the incident added to their count for that year.

Felons Killed by Police

Data were collected from UCR Supplementary Homicide Reports: `https://www.icpsr.umich.edu/icpsrweb/NACJD/series/57#`. These data are voluntarily reported by each police agency to the FBI and are meant to provide information on the circumstances of every homicide in the jurisdiction of the reporting agency.

I restrict attention to cases where the reported circumstance of the homicide is a "felon killed by police." These homicides are those deemed to be justified and do not necessarily restrict attention to individuals who were committing an offense that would be classified as a felony (Federal Bureau of Investigation, 2004). I collapse the data to the agency-year level and link to the LEMAS using UCR ORI.

Decertifications

The data on officer decertifications were received by the Invisible Institute, which made public records requests to every state police certification agency in the country. These data are publicly available from USA Today: https://www.usatoday.com/in-depth/news/investigations/2019/04/24/usa-today-revealing-misconduct-records-police-cops/3223984002/. As discussed in the body of the text, each state requires officers to be licensed by the state certification agency, and the majority of the states have the ability to revoke this license in cases of moral conduct violations. These violations can either be infractions on-the-job or off-duty.

The data record the officer name, agency where they worked when de-certified, and the year of the incident. I aggregate these data to the agency-year level and keep the count of incidents. Agencies are linked to the LEMAS data using a fuzzy merge on agency name and state.

Florida Union Certification Elections

Records from the Florida PERC comprise election certification documents ("Verification of Election Results") for the years 1975 to 1986 and certification documents ("Tally of Ballots") for 1987 onwards. These records were acquired through a public records request and through the PERC website, http://perc.myflorida.com.

A primary objective in cleaning and filtering these records is to restrict attention to elections that represent the department's rank and file, as opposed to representing non-sworn employees or officers in management positions (e.g. lieutenants, captains). In some election certifications, the document reports the units of the department which would be covered by collective bargaining and those that are excluded. I include as rank and file any case that lists patrolmen, police officers, or sheriff's deputies. For the 1987 documents onwards, I observe a separate document which reports the units represented in each election. Some elections do not appear in this document and therefore are unclear with regards to the represented unit.

I drop all elections where I observe information on the represented employees and rank-and-file officers are not included. I also drop all elections for which I do not have the listed unit and the election is immediately preceded by a successful election to represent the department's rank-and-file. As I note below, these restrictions may both remove some elections for rank-and-file officers and include some elections which do not represent rank-and-file officers.

In some elections, two unions are listed on the ballot, one as a petitioner and the other as a "respondent." This typically occurs in cases where a union is challenging to replace an incumbent union. It can also be the case that two unions are competing to represent an unrepresented department, but according to PERC staff this occurrence is quite rare. While the records often do not report whether an incumbent is represented in the election, I presume that a competitive election has no incumbent when it is the agency's first rank-and-file election in my database.

The measurement of union status from the elections data is not perfect. Some election records are ambiguous such that it is not clear whether rank and file officers are being petitioned for representation or whether there already exists an incumbent union. In Section 5, I use the elections-based measure of union status to estimate the degree of measurement error in the LEMAS measure of union status. The imperfection in the elections-based union measure likely means that the estimated relationship between the LEMAS and elections estimates are a lower bound for the relationship between the LEMAS estimate and true union status.

Some de-unionization cases are also not in the election certifications record. For example, in some instances a city has two adjacent successful elections to unionize its rank and file officers. These occurrences are due to the fact that a union can choose to not challenge a city's petition to remove the union's representation. In those cases, PERC grants a de-unionization, but no election occurs. Because the main measure of unionization we focus on is post-unionized rather than currently unionized, these errors are not a concern.

Florida Department of Law Enforcement Misconduct Investigations

The records of FDLE investigations list the date a case is opened and the dates of all subsequent case related events. However, it does not list the date of the original incident. I use the date the case opened as the focal date with the caveat that this date should be treated as the best proxy for the true incident date.

Florida Retirement System

To measure earnings impacts in Florida, I gathered salary data on all individuals partaking in the state pension system for the years 1973-2015 from the Florida Retirement System (FRS). These data cover each individual's annual earnings, department of hire, starting date, risk class, and whether they are in a management position. Reported earnings include all forms of overtime and other pay in addition to base salary. I use these data to construct a measure of the annual starting earnings of officers.

Every department has the choice of whether to use the state's defined-benefit pension plan or organize their own private pension system, and all individuals within a participating department have a choice of whether to join. Beginning in 2002, the state created an alternative defined-contribution plan, which does not appear in the data. If individuals make a request to the state, their records are removed from the public domain and do not appear in my data either. Therefore, my sample consists of individuals in participating agencies who partake in the defined-benefit program and have not elected to make their records private.

There are some observations that appear to be significant outliers within their department, even after accounting for tenure. For each department-year, I regress individual earnings on tenure and construct error terms, $\hat{\epsilon}_i = Y - X\hat{\beta}$. I then drop observations in the top and bottom 1% of values of $\epsilon_i/X\hat{\beta}$.

Measuring Starting Earnings

The effect of unionization on earnings will be at the department level. However, the earnings data are at the individual level. For each department-year, I regress earnings of officers on tenure:

$$E_{ijt} = \beta^0_{jt} + \beta^1_{jt}T_{ijt} + \epsilon_{ijt}$$

In these regressions, the objects of interest are β^0_{jt}, the predicted starting salary. To account for estimation error, my earnings regressions use weighted least squares, where the inverse of the standard error of the estimate are the weights for each observation.

The pay data report total earnings rather than salaries. There also appear to be some variation across years in the reporting practices of the FRS, since earnings jump systematically in certain years of the data. To account for this concern, I consider a version of the starting earnings estimate that is smoothed: for each department and year, I regress starting earnings estimates of all other years β^0_{jt} on a third-degree local polynomial. I then construct "smoothed" values of starting earnings by predicting the value of $\hat{\beta}^0_{jt}$. I also do this procedure for my estimates of median earnings.

The primary regression estimates in Appendix Figure A.1 use the baseline estimates of β^0_{jt} and median earnings, and Figure A.3 compare these estimates to those using the smoothed values. The point estimates are similar in magnitude, but the time path and significance is much clearer using the smoothed values.

B Florida Regression Discontinuity Strategy

The main empirical strategy used with the Florida elections data is an event study design, where a balanced panel of departments with winning and losing elections are compared around the date of the election. However, the election records also provide the specific vote tallies for and against unionization. Therefore, an alternative approach would be to exploit the discontinuity in outcomes when a majority of votes are for unionization.

To do so, I use a regression discontinuity design that compares departments with a small vote difference in favor of unionization compared to those with a small vote difference against unionization, with controls linear in vote difference on each side of the discontinuity,

$$\begin{aligned}
Y_{it} \quad = \quad & \alpha \cdot \mathbb{I}\big(VoteDiff_{e(i)t} > 0\big) \\
& + \beta_k^+ \cdot \mathbb{I}\big(VoteDiff_{e(i)t} > 0\big) \cdot VoteDiff_{e(i)t} \\
& + \beta_k^- \cdot \mathbb{I}\big(VoteDiff_{e(i)t} <= 0\big) \cdot VoteDiff_{e(i)t} \\
& + \phi_{e(i)} + \kappa_t + \psi_k + U_{e(i)t}
\end{aligned}$$

and where $k = t - ElectionYear$.

The top panel of Figure A.4 documents the histogram of vote differences between officers in favor of unionizing and those against. The bottom panel shows the vote *share* in favor of unionization. Because many departments are very small, there are spikes at certain vote shares that are common for small elections. I therefore choose to use the vote difference approach, with the awareness that the comparison will be local to smaller departments that are more likely to have close elections.

Figure A.5 presents the raw averages of the log of investigations for departments with votes around the cutoff for unionization along with a smooth polynomial regression line above and below the cutoff. The left panel plots the data for the one to five years prior to the election, and the right panel plots the data for the unionization year to ten years after the election. While there appears to be no pre-period difference in outcomes between winning and losing departments, there also is no significant difference after the election. If anything, the difference in frequency of investigations appears to become more negative though insignificant.

These results are confirmed by the reported regression results in Table A.5. Each panel presents a different outcome measure for investigations, and the columns represent different bandwidths, with the columns further to the right increasing the bandwidth. Consistent with the event study results, none of the coefficients are significantly positive, and the majority of the point estimates are negative. The 95% confidence intervals for all coefficients can reject a greater than 10% increase in investigations.

As mentioned above and presented in Figure 4, a unionization election victory relative to not unionized is only an imprecise indicator of whether a department is unionized in future years. Therefore, in an additional approach, I use the discontinuity in vote difference as an instrument for whether the department is unionized in every future year:

$$\begin{aligned} Union_{it} \quad = \quad & \alpha_{rho} \cdot \mathbb{I}\big(VoteDiff_{e(i)t} > 0\big) \\ & + \beta^{+}_{k\rho} \cdot \mathbb{I}\big(VoteDiff_{e(i)t} > 0\big) \cdot VoteDiff_{e(i)t} \\ & + \beta^{-}_{k\rho} \cdot \mathbb{I}\big(VoteDiff_{e(i)t} <= 0\big) \cdot VoteDiff_{e(i)t} \\ & + \phi_{e(i)} + \kappa_t + \psi_k + U_{e(i)t} \end{aligned} \tag{6}$$

where $k = t - ElectionYear$

The results of this analysis are in Table A.6. Relative to the RD design, the standard errors are substantially larger. However, the results continue to be insignificant, and the point estimates tend to be negative. For all investigations, in Panel A, the estimated impacts with a bandwidth of 10 or larger all reject a greater than 17% increase in investigations due to unionization. The upper bounds are similar for externally-initiated investigations and sustained investigations. For all three outcomes, the upper bounds are significantly higher when the bandwidth is 5, though the estimates are still insignificant and the number of observations are reduced significantly.

Table A.1: National Difference-in-Differences Regressions with Alternative Specifications

	Levels			Poisson		
A. Fatal Encounters	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.244***	0.137	0.065	0.188**	0.114	0.073
	(0.067)	(0.114)	(0.119)	(0.077)	(0.107)	(0.090)
Outcome Mean	0.731	0.731	0.732	0.731	0.902	0.926
N	12008	12008	11975	12008	9725	9439
R-Squared	0.275	0.715	0.716	0.343	0.452	0.477
B. Felons Killed By Police	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.130**	0.043	-0.107	0.214**	-0.023	-0.253**
	(0.053)	(0.072)	(0.091)	(0.091)	(0.133)	(0.127)
Outcome Mean	0.433	0.433	0.432	0.433	0.608	0.676
N	20694	20694	20566	20694	14751	13072
R-Squared	0.307	0.653	0.653	0.496	0.525	0.558
C. Vital Statistics	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.909*	-0.125	-0.035	0.807**	-0.070	0.035
	(0.477)	(0.081)	(0.076)	(0.355)	(0.079)	(0.056)
Outcome Mean	1.377	1.377	1.379	1.377	1.395	1.521
N	23101	23101	23023	23101	22791	20859
R-Squared	0.056	0.783	0.812	0.086	0.583	0.616
Year Controls	X	X	X	X	X	X
Agency Controls		X	X		X	X
State X Year Controls			X			X

Notes: First three columns present an analogous version to those in Table 4, where the outcome is the count of number of incidents. The final three columns run a poisson specification, with the outcome also the count of number of incidents.

Table A.2: National Difference-in-Differences Regressions with Alternative Specifications, Continued

	Levels			Poisson		
D. Expected Incidents	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	0.001	-0.404	-0.543	-0.118	-0.165	-0.173*
	(0.209)	(0.405)	(0.376)	(0.141)	(0.157)	(0.105)
Outcome Mean	1.703	1.703	1.710	1.703	1.909	1.943
N	7545	7545	7481	7545	6732	6583
R-Squared	0.377	0.781	0.785	0.472	0.611	0.639
E. Decertifications	(1)	(2)	(3)	(4)	(5)	(6)
Unionized	-0.258***	-0.081	-0.069	-0.686***	-0.346***	-0.088
	(0.058)	(0.093)	(0.094)	(0.126)	(0.121)	(0.099)
Outcome Mean	0.406	0.406	0.407	0.406	0.623	0.845
N	10215	10215	10163	10215	6657	4875
R-Squared	0.067	0.426	0.461	0.102	0.339	0.312
Year Controls	X	X	X	X	X	X
Agency Controls		X	X		X	X
State X Year Controls			X			X

Notes: First three columns present an analogous version to those in Table 4, where the outcome is the count of number of incidents. The final three columns run a poisson specification, with the outcome also the count of number of incidents.

Table A.3: Economic and Demographic Impacts, National Sample

A. Log Starting Earnings	(1)	(2)	(3)	(4)
Unionized	0.226***	-0.026	-0.033	-0.062*
	(0.016)	(0.017)	(0.020)	(0.037)
R-Squared	0.391	0.609	0.631	0.681
Outcome Mean	10.319	10.320	10.321	10.321
N	4955	4949	4916	4916

B. Employment, LEMAS	(1)	(2)	(3)	(4)
Unionized	-0.008	0.007	-0.066	-0.017
	(0.057)	(0.070)	(0.070)	(0.029)
R-Squared	0.006	0.926	0.935	0.972
Outcome Mean	5.510	5.510	5.511	5.511
N	6016	6016	6000	6000

C. Employment, LEOKA	(1)	(2)	(3)	(4)
Unionized	0.025	0.197**	0.093	0.060
	(0.076)	(0.095)	(0.092)	(0.083)
R-Squared	0.010	0.881	0.896	0.936
Outcome Mean	5.383	5.384	5.382	5.382
N	5959	5958	5937	5937

D. Share Female	(1)	(2)	(3)	(4)
Unionized	-0.036***	0.005	0.008**	0.002
	(0.005)	(0.004)	(0.004)	(0.005)
R-Squared	0.073	0.838	0.855	0.910
Outcome Mean	0.245	0.245	0.245	0.245
N	5867	5865	5841	5841

E. Share Minority	(1)	(2)	(3)	(4)
Unionized	0.018	-0.032**	-0.026*	-0.024
	(0.011)	(0.013)	(0.014)	(0.020)
R-Squared	0.061	0.754	0.802	0.879
Outcome Mean	0.204	0.204	0.204	0.204
N	4937	4930	4896	4896
Year Controls	X	X	X	X
Agency Controls		X	X	X
State X Year Controls			X	X
Agency Time Trends				X

Notes: All outcomes except for Row C. come from the LEMAS survey. No covariates are included in any regression with the exception of the fixed effects noted at the bottom of the table.

Table A.4: Main Diff-Diff Impacts With Covariates Shown – National Sample

	(1) Log FE	(2) Log FKP	(3) Log VS	(4) Log Decertifications
Log Sworn Officers	0.0437	0.00667	0.0272	0.125***
	(0.0303)	(0.0200)	(0.0181)	(0.0358)
Log Population	0.00519	0.00389	-0.00251	0.0165
	(0.00663)	(0.00206)	(0.00410)	(0.00949)
Log Crimes	0.00115	0.00155	0.0165	0.00963
	(0.0204)	(0.0115)	(0.0160)	(0.0244)
Log Violent Crimes	-0.00619	0.0134	-0.0187	0.0120
	(0.0227)	(0.0118)	(0.0176)	(0.0246)
Log Clearances	-0.0783	0.177	-0.0174	-0.0912
	(0.130)	(0.116)	(0.140)	(0.165)
Log Violent Clearances	0.0407	-0.0178	0.0282	-0.00221
	(0.0327)	(0.0170)	(0.0324)	(0.0420)
Log Other Clearances	0.0328	-0.141	-0.00512	0.0856
	(0.109)	(0.0943)	(0.113)	(0.133)
Union	0.0442	-0.0292	-0.0142	-0.0306
	(0.0361)	(0.0230)	(0.0246)	(0.0351)
Observations	11975	20566	23023	10163
Mean	0.323	0.184	0.489	0.205
Agency FE	X	X	X	X
State-Year FE	X	X	X	X
R-squared	0.569	0.594	0.727	0.481
Within-FE R-squared	0.000	0.003	0.000	0.007

Notes: Expansion of estimates from Table 4.

Table A.5: Regression Discontinuity Table

Investigations	(1)	(2)	(3)	(4)
Vote Share > .5	-3.631	-0.795	0.277	0.087
	(5.248)	(1.931)	(0.453)	(0.111)
Outcome Mean	0.505	0.512	0.527	0.470
N	30	59	100	231
Externally-initiated Investigations	(1)	(2)	(3)	(4)
Vote Share > .5	-0.743	-0.748	-0.349	-0.085
	(4.084)	(1.799)	(0.356)	(0.096)
Outcome Mean	0.200	0.215	0.227	0.201
N	30	59	100	231
Sustained Investigations	(1)	(2)	(3)	(4)
Vote Share > .5	-3.104	0.789	0.293	-0.062
	(3.963)	(1.557)	(0.354)	(0.081)
Outcome Mean	0.198	0.214	0.214	0.190
N	30	59	100	231
Bin Width	5%	10%	20%	∞

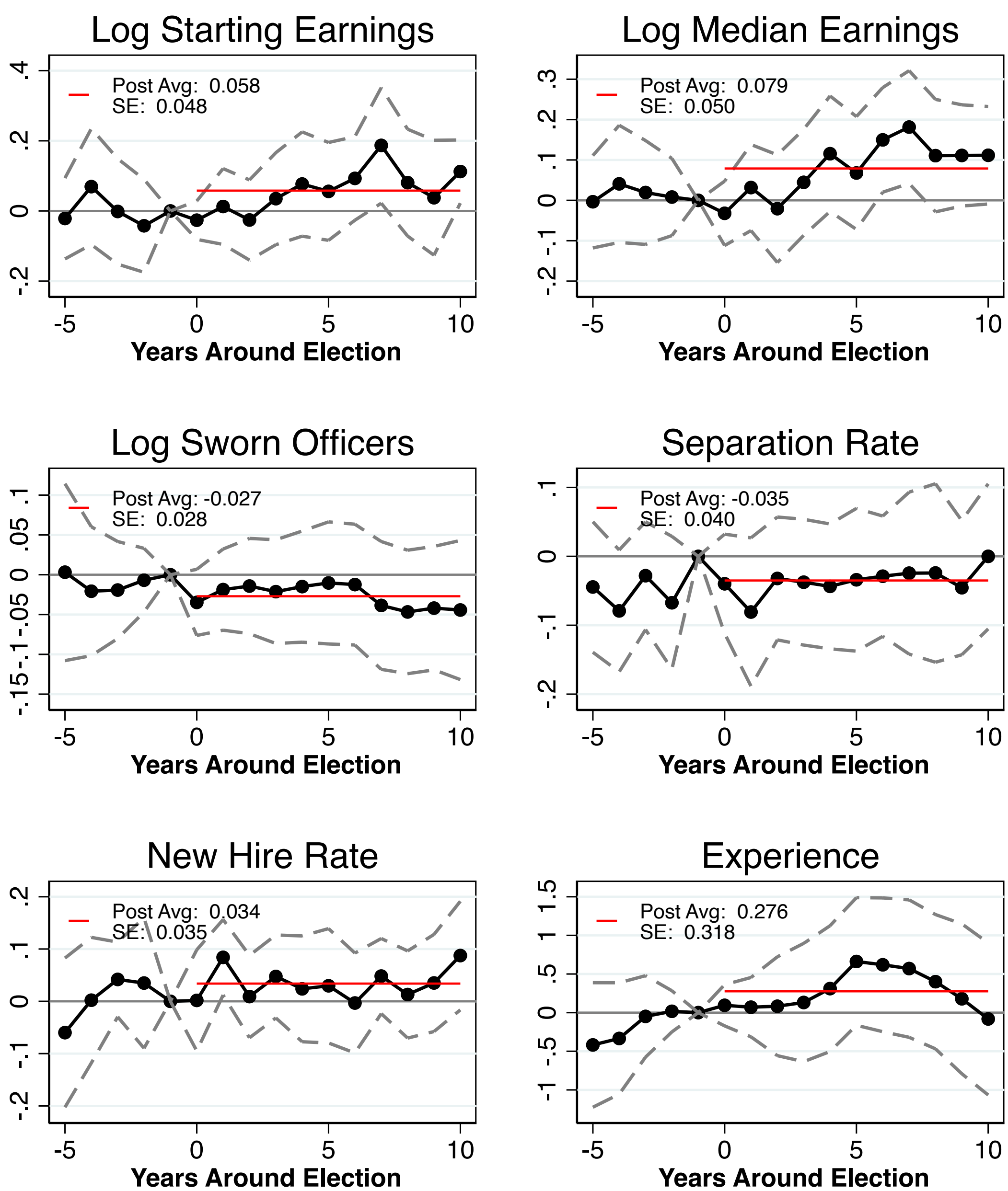
Notes: Each panel presents a different dependent variable, and the columns represent different bandwidths, with the columns further to the right increasing the bandwidth. Data are for five years prior to an election to ten years after, and attention is restricted to departments with a balanced panel of observations.

Table A.6: Fuzzy Regression Discontinuity Table

Investigations	(1)	(2)	(3)	(4)
Unionized	-0.011	-0.052	0.061	0.061
	(0.200)	(0.128)	(0.102)	(0.102)
Outcome Mean	0.505	0.512	0.527	0.527
N	30	59	100	100
Externally-initiated Investigations	(1)	(2)	(3)	(4)
Unionized	-0.054	-0.133	-0.063	-0.063
	(0.161)	(0.116)	(0.086)	(0.086)
Outcome Mean	0.200	0.215	0.227	0.227
N	30	59	100	100
Sustained Investigations	(1)	(2)	(3)	(4)
Unionized	0.030	-0.082	0.013	0.013
	(0.124)	(0.083)	(0.059)	(0.059)
Outcome Mean	0.198	0.214	0.214	0.214
N	30	59	100	100
Bin Width	5%	10%	20%	∞

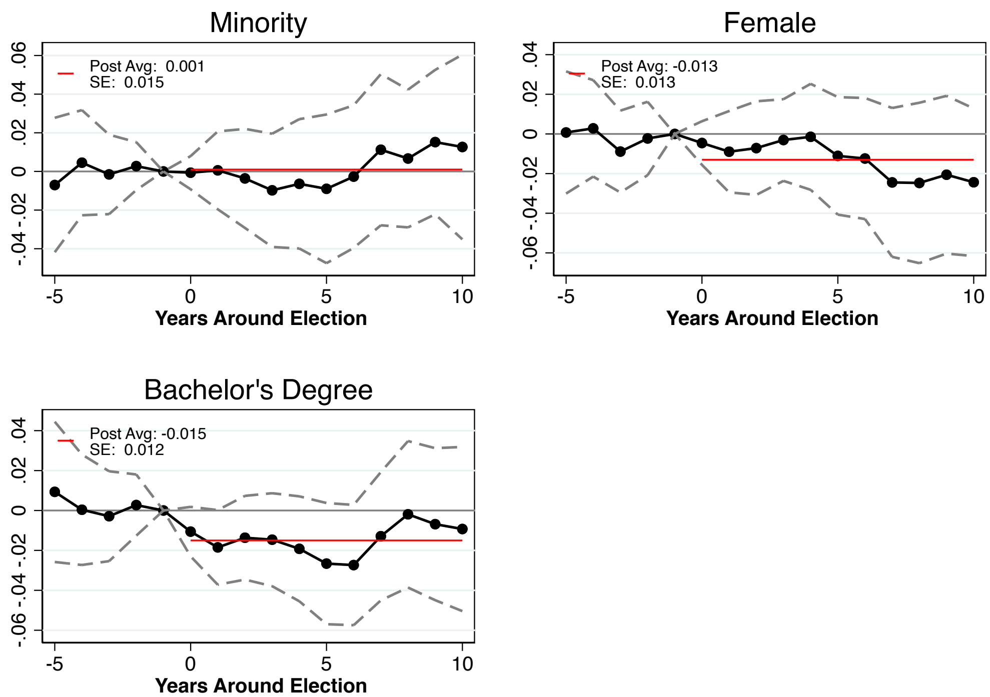
Notes: Results are from instrumental variable regressions, where "Unionized" is instrumented for as in Equation 6. Each panel presents a different dependent variable, and the columns represent different bandwidths, with the columns further to the right increasing the bandwidth. Data are for five years prior to an election to ten years after, and attention is restricted to departments with a balanced panel of observations.

Figure A.1: Economic and Demographic Impacts, Florida Sample



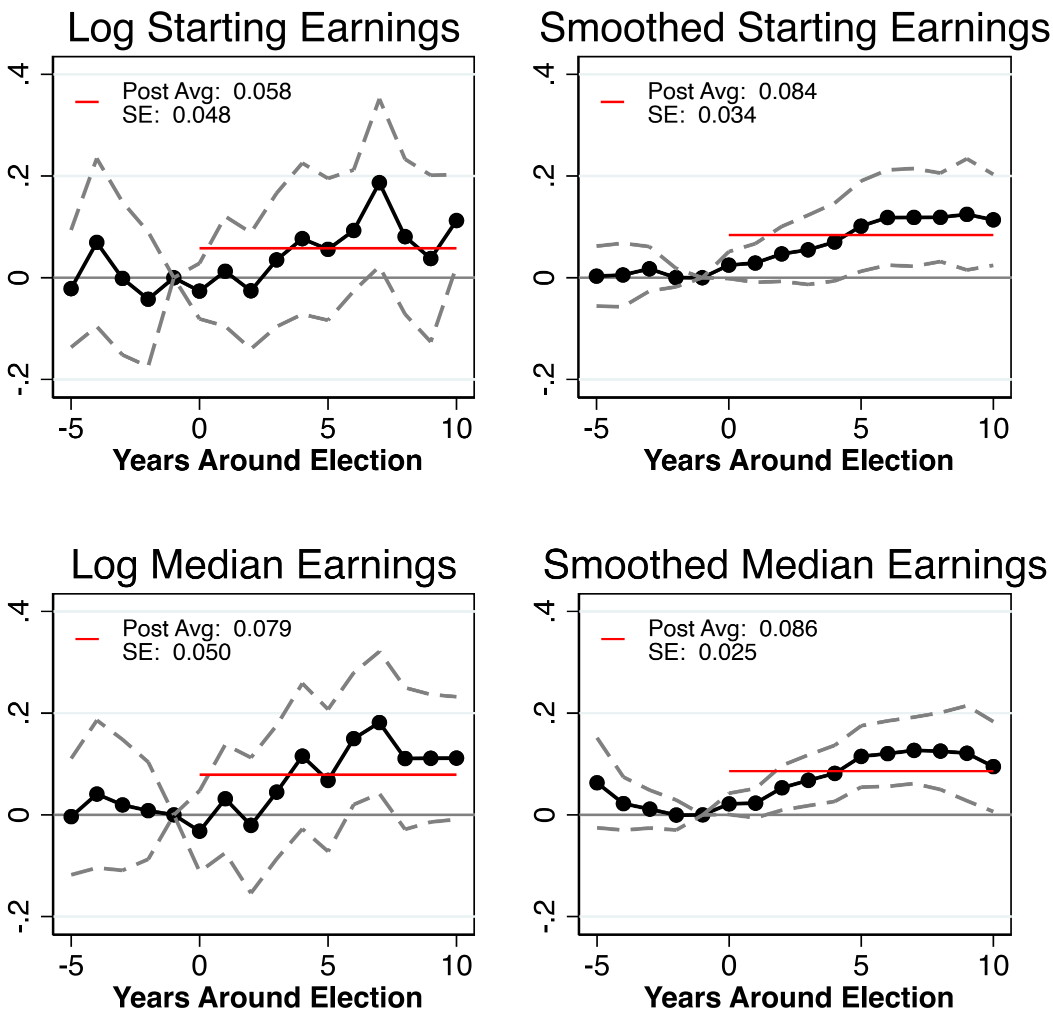
Notes: Measures of starting and median earnings are described in Appendix Section A. The outcomes in bottom four panels are constructed from the FDLE officer database.

Figure A.2: Economic and Demographic Impacts, Florida Sample (Cont.)



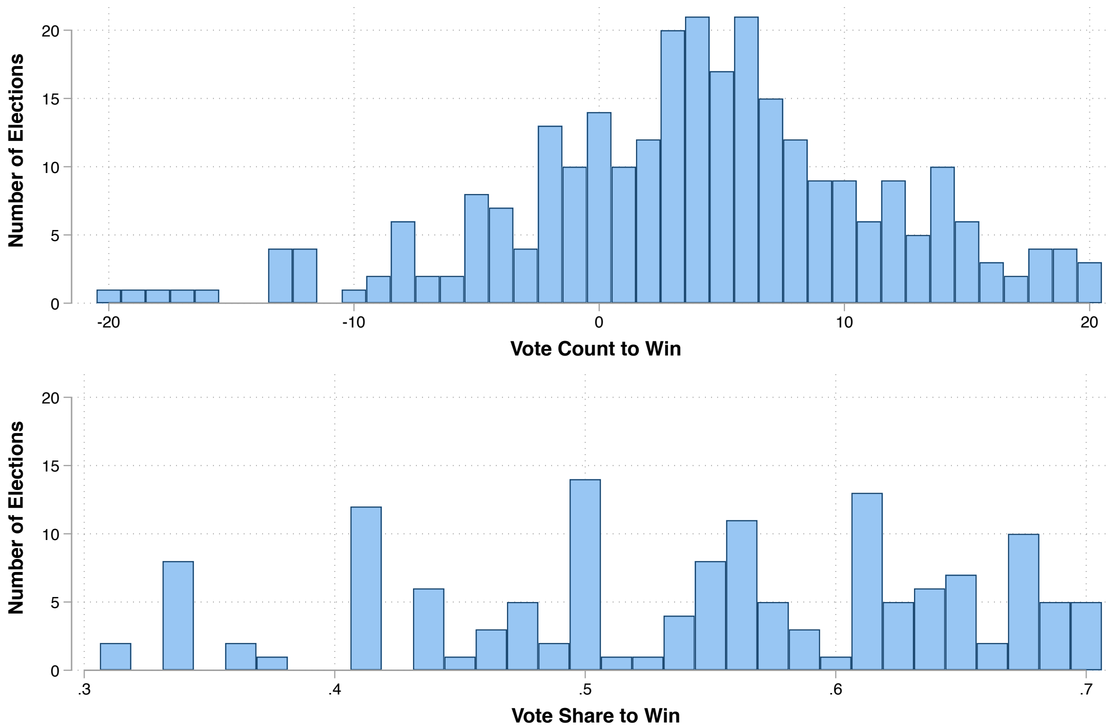
Notes: Outcomes are constructed from the FDLE officer database.

Figure A.3: Economic and Demographic Impacts, Florida Sample (Cont.)



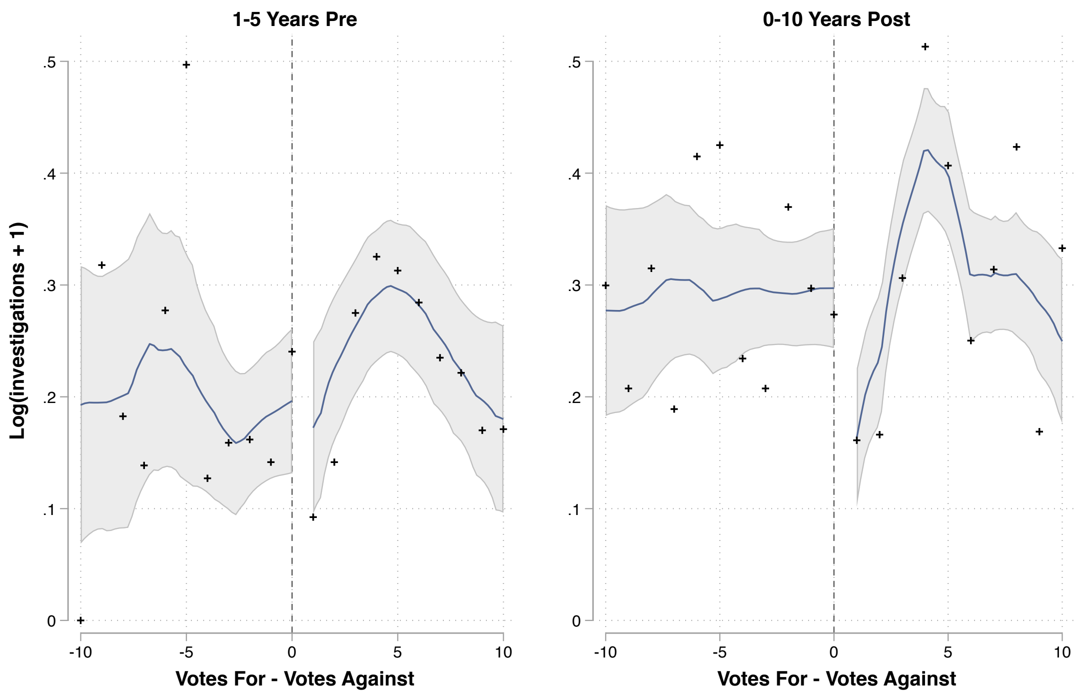
Notes: Measures of starting earnings, median earnings, and smoothing procedure are described in Appendix Section A

Figure A.4: Histogram of Vote Shares



Notes: The top panel plots the distribution of vote differences for and against in elections to unionize. The bottom panel plots the distribution of vote shares in favor of unionization.

Figure A.5: Regression Discontinuity Raw Data Plot



Notes: Both panels plot the log of investigations plus one, where department-years are averaged within vote difference for and against unionization. The left panel plots data for one to five years prior to unionization, and the right panel plots data for the year of unionization to ten years after. Data is restricted to departments with a balanced panel of observations around their election.