The Effect of Collective Bargaining Rights on Law Enforcement: Evidence from Florida

Dhammika Dharmapala
dharmap@uchicago.edu
University of Chicago Law School

Richard H. McAdams
rmcadams@uchicago.edu
University of Chicago Law School

John Rappaport
jrappaport@uchicago.edu
University of Chicago Law School

Initial Version – December 2017

PRELIMINARY AND INCOMPLETE – PLEASE DO NOT CITE WITHOUT AUTHORS' PERMISSION

Abstract

Growing controversy surrounds the impact of labor unions on law enforcement behavior. Critics allege that unions impede organizational reform and insulate officers from discipline for misconduct. The only evidence of these effects, however, is anecdotal. We exploit a quasi-experiment in Florida to estimate the effects of collective bargaining rights on law enforcement misconduct and other outcomes of public concern. In 2003, the Florida Supreme Court's *Williams* decision extended to county deputy sheriffs collective bargaining rights that municipal police officers had possessed for decades. We construct a comprehensive panel dataset of Florida law enforcement agencies starting in 1997, and employ a difference-in-difference approach that compares sheriffs' offices and police departments before and after *Williams*. Our primary result is that collective bargaining rights lead to about a 27% increase in complaints of officer misconduct for the typical sheriff's office. This result is robust to the inclusion of a variety of controls. The time pattern of the estimated effect, along with an analysis using agency-specific trends, suggests that it is not attributable to preexisting trends. The estimated effect of *Williams* is not robustly significant for other potential outcomes of interest, however, including the racial and gender composition of agencies and training and educational requirements.

Acknowledgments: We thank Morgen Miller and Rafeh Qureshi of the Coase-Sandor Institute for Law and Economics at the University of Chicago Law School for their outstanding work in constructing the dataset used in this paper, and Whittney Barth, Jeremy Chen, Alan Hassler, Isabella Nascimento, Eileen Prescott, and Christopher Walling for excellent research assistance. We also thank Terry Baker and Stacey Price of the Florida Department of Law Enforcement (FDLE) for providing the data and for patiently answering our questions. Margaret Schilt of the University of Chicago Law Library kindly helped us with background research on Florida law enforcement agencies. We also thank William Hubbard, Vic Khanna, Anup Malani, Kyle Rozema, Kim Rueben, Juan Carlos Suarez-Serrato, Andrew Verstein, and Eric Zwick for helpful conversations. Dharmapala acknowledges the financial support of the Lee and Brena Freeman Faculty Research Fund at the University of Chicago Law School. Rappaport acknowledges the Darelyn A. and Richard C. Reed Memorial Fund. Any remaining errors or omissions are, of course, our own.

1) Introduction

In January of 2017, the United States Department of Justice (DOJ) released a report on the Chicago Police Department (CPD) based on extensive investigation and access to CPD files and videos. The DOJ found that CPD had engaged in a "pattern or practice" of the unconstitutional use of lethal and non-lethal force against suspects. It explained the problem in part by the failures of the CPD discipline system, which "lacks integrity and does not effectively deter [police] misconduct" (U.S. Department of Justice and U.S. Attorney's Office 2017, p. 80). The next month, CPD altered its discipline system by replacing an informal and unreliable process of penalty selection with a 47-page matrix that recommended a penalty for each type of misconduct. In response, the Fraternal Order of Police Lodge #7, a union representing CPD officers, filed a charge with the Illinois Labor Relations Board, calling the matrix an unfair labor practice because it was instituted without collective bargaining. In November, an administrative law judge ruled in favor of the union, placing the new matrix in legal jeopardy unless and until the Board intervenes. (Anna Hamburg-Gal, Admin. Law Judge, Recommended Decision and Order, Illinois Labor Relations Board Local Panel, Nov. 8, 2017). The judge observed that, "[w]hile the . . . Matrix seeks to eliminate such misconduct by permanently removing the offending officer from service, the Union can offer suggestions on how to rehabilitate officers" (p. 30). The DOJ had concluded in its report that CPD generally failed to discipline officers sufficiently and had particular difficulties in discharging officers who merited discharge, and also that the proposed matrix was an "important" step but often provided for sanctions that were too low. (U.S. Department of Justice and U.S. Attorney's Office 2017, pp.80-89), Nonetheless, the police union may succeed in blocking this modest reform on the grounds that it goes too far.

This anecdote illustrates one way in which collective bargaining rights may make it more difficult for local governments to deter misconduct by law enforcement officers and terminate misbehaving officers: collective bargaining may impede efforts at the necessary reform. In addition, some scholarship identifies collective bargaining agreements (CBAs) as a key reason that reform is necessary in the first place. Huq and McAdams (2016), Keenan and Walker (2005), and Rushin (2017) document how many law enforcement CBAs create procedural rights for officers that make it difficult for departments to investigate and discipline misconduct, including the excessive use of force. Unions have other forms of leverage as well, as they may successfully lobby for state and local legislation that provides the same kind of procedural protections against investigation and discipline. At the same time, it is possible that unionization may reduce misconduct by providing a sense of empowerment and increased job satisfaction. To the extent that collective bargaining improves wages and benefits, the opportunity cost of misconduct may increase.[1] Thus, the impact of collective bargaining rights on law enforcement misconduct is ultimately an empirical question.

There has been significant scholarly attention recently to the issue of excessive law enforcement violence (Fryer 2016; Legewie and Fagan 2016; Shjarback 2015; Shane, Lawton, and Swenson 2017; Stickle 2016), some of which has trained on the role of collective bargaining (Huq and McAdams 2016; Rushin 2017). No previous work, however, has offered empirical evidence of the causal role that unionization or collective bargaining play in the performance of law enforcement. We offer such evidence by exploiting a January 2003 change in Florida labor law. By a judicial decision that month (*Williams*), county sheriffs' deputies won for the first time the right to organize for collective bargaining. Before and after that date, municipal police officers had the right to engage in collective bargaining. We examine how *Williams* affected complaints of misconduct against law enforcement personnel at these two types of agencies.

Our analysis uses a dataset on Florida law enforcement agencies – covering both county sheriffs' offices (SOs) and city police departments (PDs) – that begins in 1997 (our primary tests use data for 1997-2010). This dataset combines annual Criminal Justice Agency Profile (CJAP) surveys conducted by the Florida Department of Law Enforcement (FDLE) with administrative data from the FDLE on law enforcement officers and on complaints and disciplinary actions against officers. We analyze in particular the number of complaints at the agency-year level against officers affiliated with each agency. Our empirical strategy involves the use of a difference-in-difference framework, in which the treatment group consists of SOs (that were affected by *Williams*) and the control group consists of PDs (that were unaffected). Officers assigned to agencies in the treatment and control groups perform similar job functions (Pynes and Corley 2006,

[1] This is analogous to the idea that higher compensation can deter malfeasance among law enforcers (Becker and Stigler 1974). There is also some evidence that police performance is affected by changes in wages relative to a reference point. Mas (2006) finds that police performance in New Jersey (as measured primarily by arrest rates) declines when police unions lose in wage arbitration. Mas (2006) does not analyze police misconduct, however. Unionization may also foster collective solidarity among police and interact with officers' intrinsic motivation. Dharmapala, Garoupa, and McAdams (2016) develop a theoretical model of intrinsic motivation among law enforcement agents but do not address the impact of unionization.

p. 299).[2] Likewise, similar pools of applicants seek employment with SOs and PDs, and there is lateral movement by officers between the agency types (Baker 2017a).

We begin by showing that *Williams* led to substantial unionization among SOs. This occurred over a three-year period following the decision, and then stabilized; thus, our primary specifications use a three-year lag of the variable of interest (an interaction between the post-2003 years and an indicator for SOs). Our baseline analysis uses a Poisson maximum-likelihood model in order to accommodate count data on complaints. We control for agency and year fixed effects and for an extensive set of controls (including demographic variables, local economic conditions, and local crime rates). The central result is that collective bargaining rights led to about a 27% increase in complaints of officer misconduct for a typical SO. We also find some evidence that, for agency-years with positive numbers of complaints, the number of state disciplinary actions against officers also increased. We argue in Section 5 below that this suggests that the increased complaints against SOs were not accompanied by a decrease in the seriousness of these complaints.

When adding an extensive set of leads and lags to the basic model, we find that the difference-in-difference estimate is small and statistically insignificant for "false experiments" (or placebo tests) in years prior to 2003, suggesting that the results are not attributable to a preexisting trend toward more complaints against SOs. In a linear framework, the result is robust to adding linear agency-specific time trends and county-by-year fixed effects. Taken together, the results on prior years and on agency-specific time trends suggest that the parallel trend assumption that is crucial in the difference-in-difference framework is satisfied here. This supports a causal interpretation of our findings on the impact of collective bargaining rights, despite numerous factors we describe below that create a bias against our results.

Complaints are of course an imperfect measure of underlying misconduct. They are potentially affected by the strength of internal mechanisms for detection and by the propensity of civilians to report misconduct, as well as by the level of misconduct. All of the complaints in the FDLE's database meet a specified threshold of seriousness, however. Most, moreover, were already sustained by a local agency's internal investigation and then reported, as required by law, to the FDLE. Thus, our complaints measure is better viewed as consisting substantially of adjudicated instances of misconduct. The FDLE also has access to independent sources of

[2] One minor distinction between sheriff deputies and police officers is that only deputies serve court papers, such as injunctions (Baker 2017a).

information (beyond the reports of local agencies), allaying concerns about biases in local agencies' internal investigation processes. These factors mitigate the concerns about using complaints as a proxy for misconduct.

We also examine a number of other outcome variables that are potentially of interest in ongoing scholarly and public debates about policing. For instance, we explore how collective bargaining affected the racial and gender composition of law enforcement agencies, which may be one mechanism by which such bargaining affects police behavior and citizen complaints. The estimates suggest a decline in the racial diversity of law enforcement personnel, but this is not robustly significant across specifications. The estimated effect on gender diversity is small and statistically insignificant. We also find some evidence for increases in training and education requirements, but these results too are not robustly significant across specifications.

Our paper proceeds as follows. Section 2 reviews the literature. Section 3 describes the relevant legal developments relating to collective bargaining rights under Florida law. Section 4 describes the dataset and our empirical strategy. Section 5 discusses the results, while Section 6 concludes.

2) Literature Review

Most prior studies on the effects of collective bargaining by law enforcement officers examine the relationship between the bargaining environment and officer remuneration. Unionization is consistently and positively associated with officer wages and benefits (Briggs et al. 2008; Delaney and Feuille 1985; Doerner and Doerner 2010; Feuille and Delaney 1986; Feuille, Hendricks, and Delaney 1983; Freeman and Valletta 1988; Trejo 1991; Wilson et al. 2006; Zhao and Lovrich 1997). Interestingly, this relationship holds even in states that forbid collective bargaining, where many officers still join unions (Freeman and Han 2013). Likewise, strong labor laws are associated with higher wages even for nonunionized officers (Freeman and Valletta 1988; Ichniowski, Freeman, and Lauer 1989).

The existing literature has mostly exploited either variation in the timing of state laws relating to public sector unions or variation in the timing of agencies' unionization. A recent example of the former is Frandsen (2014), which uses state-level panel data to determine that collective bargaining rights increased wages and shortened the workweek for law enforcement officers (for an earlier example, see Ichniowski, Freeman, and Lauer 1989). A recent example of the latter approach is Anzia and Moe (2014), which uses city-level panel data and finds that unionization has had a strong positive effect on police departments' wages, employment levels, and payroll expenditures. Anzia and Moe (2014) also find that, relative to nonunionized agencies, unionized agencies spend more on salaries and health benefits, but have lower levels of employment.

To draw causal inferences, the former approach assumes that state legislation is exogenous with respect to the outcomes of interest, while the latter approach assumes that agencies' unionization decisions are exogenous. Our approach uses quasi-experimental variation in collective bargaining rights across agencies in the same state, and so does not require either of these assumptions. In addition, the past literature on the effects of labor laws on nonunionized workers – and the related idea of bargaining in the shadow of collective bargaining rights (whether or not an agency is actually unionized) – motivate our focus on collective bargaining *rights*, as distinct from collective bargaining *agreements*.

Most pertinent to our study, many scholars, drawing heavily on case studies, argue that law enforcement unions impede progressive policy reform and innovation (Bies 2017; Epp 2009; Fisk and Richardson 2016; McCormick 2015; Walker 2008). The small empirical literature on this question is mixed, however. Some studies find that unionization is negatively associated with the adoption of particular reforms (Nowacki and Willits 2016) or modern accountability mechanisms more generally (Epp 2009). Perhaps related, Magenau and Hunt (1996) find that unionized agencies place significantly more emphasis on their "law enforcement" function relative to order maintenance or service delivery. Other work finds no relationship (Wilson and Buckler 2010) or even a positive association between unionization and reform (Morabito 2014).

Many collective bargaining agreements provide significant procedural protections and rights of appeal to officers accused of misconduct. Using Chicago data, Iris (1998) finds that disciplinary orders are frequently overturned during arbitral review. Others argue that the special procedural rights afforded law enforcement officers make it unduly difficult to discipline them in the first place (Huq and McAdams 2016; Keenan and Walker 2005; Rushin 2017). Collectively, scholars worry, these contractual provisions threaten to undermine the ability of management to deter misconduct, and thus may promote its commission.[3]

Although most collective bargaining agreements do not address recruitment and hiring of new officers (DeCarlo and Jenkins 2015), qualitative evidence suggests that unions have affected law enforcement hiring practices nonetheless. Unions have brought or supported reverse discrimination lawsuits challenging affirmative action hiring plans, fought to maintain entry requirements that depress female and minority hiring, and, in some cases, contributed to a work environment intolerant of racial and gender difference (Kearney and Mareschal 2014; McCormick 2015; Piven 1973; Riccucci 1990; Walker 2008).

Here, too, however, the empirical literature is small and conflicted. Mladenka (1991) finds that unionization is negatively correlated with black employment in protective service jobs, which include law enforcement as well as firefighting and corrections. Sass and Troyer (1999) conclude that the evidence of union effects on the hiring of female law enforcement officers is mixed. There is some evidence, they find, that unions boost the hiring of female officers, yet unions are also associated with adoption of fitness examinations to screen new recruits, a potential barrier for female applicants. Finally, Morabito and Shelley (2015) find that law enforcement unionization is positively correlated with female employment but uncorrelated with racial diversity.

3) Collective Bargaining Rights in Florida Law Enforcement Agencies

We begin with two general points of Florida law. First, Florida is a right-to-work state, meaning that employees cannot be compelled to join or pay dues to the union that represents and collectively bargains for their workforce (Fla. Const., Art. I, Sec. 6). Second, Florida provides by statute a Law Enforcement Officer Bill of Rights ("LEOBOR"), which includes a variety of procedural protections for officers facing disciplinary investigations. One provision gives such an officer the right to "be informed of the nature of the investigation before any interrogation begins," and to receive "all witness statements . . . and all other existing evidence, including, but not limited to, incident reports, GPS locator information, and audio or video recordings relating to the incident under investigation, . . . before the beginning of any investigative interview of that officer" (F.S.A.

[3] Hickman and Piquero (2009) find that citizen complaints are sustained at a lower rate in unionized agencies than in nonunionized agencies, but later work (Hickman and Poore 2016) casts doubt on their measure of complaints, and thus their findings.

§ 112.532(1)(d)). That is particularly generous given another requirement that "[a]ll identifiable witnesses shall be interviewed, whenever possible, prior to the beginning of the investigative interview of the accused officer" (id.).

This background ought to dull the effects of collective bargaining on law enforcement behavior. First, unions are generally weaker in right-to-work states than in non-right-to-work states. According to Putchinski (2007, p. 71), "[u]nions in Florida, including police unions, experience relatively lower membership rates with subsequent lower resources and funds as a result of existing right-to-work legislation." Second, Florida's statutory procedural protections leave less for unions to accomplish through collective bargaining, so bargaining might be expected to have only a weak effect on law enforcement behavior. As we discuss below, these are two of several reasons that our study is biased against finding effects from collective bargaining. Now we turn to the specific change in Florida labor law that is the basis for our quasi-experiment.

3.1) The Florida Supreme Court's **Williams** *Decision of 2003*

To test the causal relationship between collective bargaining rights and the behavior of law enforcement officers, we seek to exploit a 2003 change in Florida's public sector labor law. Before 2003, with a few exceptions,[4] sheriff deputies in Florida (employed at the county level) were not allowed to engage in collective bargaining. When the issue first arose in 1978, the Florida Supreme Court unanimously held in *Murphy v. Mack* (358 So.2d 822) that state law did not grant deputies such bargaining rights because deputies were "appointees" rather than "employees" of the sheriff and therefore not covered by a statute granting collective bargaining rights to employees. That changed in January of 2003, when the Florida Supreme Court held by a 4-3 vote in *Coastal Florida Police Benevolent Association v. Williams* ("*Williams*") (838 So.2d 543) that deputies have the right to engage in collective bargaining.[5] The court held that Article I, Section 6 of the Florida *Constitution* granted deputies this right, invalidating any contrary statute.

[4] Deputies in a few sheriffs' offices managed to obtain county-specific legislation allowing them to engage in collective bargaining before 2003, as we explain further below.

[5] The change was foreshadowed by the Florida Supreme Court's decision in *Service Employees International Union Local 16, AFL-CIO v. Public Employees Relations Commission* in 2000 (752 So.2d 569). The court there decided a statutory issue parallel to the one in *Mack*: whether deputy court clerks could unionize, which also depended on whether they were "employees." In holding that appointed deputies of court clerks were employees under the statute granting collective bargaining rights, the court criticized its prior decision in *Mack* for "appear[ing] to have exalted form over substance in contravention of the plain language and broad purpose of the [labor] Act" (p. 573). Soon after this decision, the Coastal Florida Police Benevolent Association, Inc. sought certification as the collective bargaining agent for employees of the Brevard County Sheriff's Office, and the litigation over that matter resulted in the Supreme Court decision in *Williams*.

Immediately after that, sheriffs' deputies began to organize for collective bargaining in substantial numbers. Doerner and Doerner (2010, p. 368) report that, by the end of 2008, a total of 28 sheriffs' offices had a collective bargaining agreement (CBA), representing 15,581 sworn personnel or 76% of sheriff deputies in Florida. We document a similar pattern using our dataset, as discussed in Section 4 below.

The significance of *Williams* for our research question stems from the fact that, by contrast, Florida *police officers* (employed at the municipal level) were able to bargain collectively starting in 1968 (Pynes and Corley 2006, p. 300; *Dade County Teachers' Association v. Ryan* (225 So.2d 903, 905 [Fla. 1969])). They have done so to a significant extent before and after 2003. As we document in Section 4 below, slightly over half of Florida police departments had CBAs in the period around 2003, and this fraction was quite stable over the time period that we examine. Thus, sheriff deputies after *Williams* experienced the impact of the *introduction* of collective bargaining rights, whereas police officers (whether they had chosen to unionize or not) did not. In this sense, police departments can serve as a control group in a quasi-experimental setting in which sheriffs' offices, whose deputies were awarded collective bargaining rights by the *Williams* decision in 2003, are the treatment group. We elaborate on this empirical design in Section 4 below.

3.2) Subsequent Developments: The "Legislative Body" Question

One legal issue remained unresolved after 2003. Parties to collective bargaining sometimes reach a bargaining impasse, in which they cannot agree on the terms of the employment contract. Florida law provides that a public entity's "legislative body" will resolve any impasse between the entity and one of its public employee unions (F.S.A. § 447.403). Once deputies began to organize in Florida, there was disagreement as to who or what the pertinent "legislative body" was. Deputy unions claimed it was the county commissioners, but sheriffs claimed *they* were the legislative body, meaning they could unilaterally resolve their own bargaining impasses. After several years of legal wrangling and uncertainty (see, for example, Ellman 2004; Moorhead 2008; Cravey 2008, 2009), a Florida Court of Appeals twice held, consistent with the position of Florida's Public Employee Relations Commission, that the county commission was the appropriate impasse-resolving body (see, for example, *Sheriff of Pasco County v. Florida State Lodge* (53 So.3d 1073 [Fla. Dist. Ct. App. 2010])). The Supreme Court of Florida effectively resolved the issue by denying discretionary review of the first of these decisions.

This change is potentially significant to our project. According to conventional thinking, deputy unions have more bargaining power when the county's commissioners resolve impasses than when the sheriff does, which explains the parties' litigation positions. Thus, it is possible that the effect of the 2003 legal change on police misconduct did not fully manifest until after the 2011 resolution of the "legislative body" issue in favor of the unions. This constitutes another bias against our finding any result for the earlier period. However, additional tests do not find a statistically significant effect of the 2011 resolution of legal uncertainty on the number of complaints at SOs relative to PDs.

4) Data and Empirical Strategy

4.1) Data

The dataset used in this analysis combines information from various sources. One of these is the annual CJAP survey conducted by the FDLE. The FDLE is a state-level agency that, among other things, collects data on the activities of local law enforcement agencies. The CJAP data is universal, covering all law enforcement agencies in the state; thus, it covers both SOs and PDs. Importantly, the available surveys cover the period 1997-2016,[6] spanning the *Williams* decision and its aftermath. Note that, while data is available through 2016, our primary tests use data for 1997-2010, in order to maintain roughly balanced pre-*Williams* and post-*Williams* time periods, and to separate out the impact of *Williams* from the effects of subsequent legal developments (as summarized in Section 3.2 above).

The CJAP data records whether a CBA existed for each agency in each year for years after 2000. It also reports extensive information about each agency. This information includes the number and demographic composition of the agency's sworn officers, the length of the training period required of new officers (under a Field Training Officer (FTO)), the types of firearms (handguns, shotguns, and rifles) the agency issues to each officer, and the minimum education requirements for new officers (typically, this is a high school diploma or equivalent, but for a minority of agency-years requires some college education). Some salary information is reported but the coverage is quite limited.

[6] There is limited coverage for some variables in 1996. However, as extensive coverage begins in 1997, our analysis generally begins in 1997.

Although, in principle, CJAP reports the race and gender composition of agencies' sworn officers, this data is missing for many years, including most years prior to 2003. Thus, we use an employment database maintained by the FDLE, called the Automated Training Management System (ATMS), to match officers to agencies. We thereby construct, for each year in our sample period (1997-2016), variables for the total number of officers in an agency, the fraction of officers who are non-Hispanic Whites, and the fraction of officers who are male. We use these ATMS variables, rather than the corresponding variables reported in CJAP, in our analysis. However, for the years where they overlap, the ATMS-based variables are virtually identical to those in CJAP.

As foreshadowed in Section 3, our empirical strategy involves comparing SOs and PDs before and after the *Williams* decision. Pynes and Corley (2006, p. 299) highlight "the similarities in job duties" between sheriff deputies and police officers, which suggests that PDs are in general a good comparison group. Moreover, any differences in job duties that may exist are not likely to have changed at the time of the *Williams* decision. In one respect, however, SOs and PDs do differ. Peace officers in Florida are generally certified in "law enforcement," "corrections," or both (known as "concurrent" certification). SOs employ a greater proportion of certified corrections officers than do PDs. Thus, we restrict our analysis to officers who are certified in law enforcement, either with or (more typically) without concurrent certification in corrections.[7] The total counts of officers that we use, as well as the race and gender variables and the complaints and disciplinary actions data, all exclude officers certified solely in corrections.

The ATMS database also records complaints against officers. These include complaints that are initiated or investigated by an officer's local agency (i.e., the employing SO or PD). If a local agency has cause to believe an officer has committed a felony or a misdemeanor involving dishonesty, or is not of "good moral character" (as defined by regulation), the agency must investigate. If the agency sustains the allegation, it must submit its findings to the FDLE, which triggers the FDLE's disciplinary process (F.S.A. § 943.13(4), (7); F.A.C. Rule 11B-27.0011). However, the FDLE also has information channels that are independent of the local agencies – for instance, newspaper reports of officer misconduct.

[7] Anecdotally, it is believed that most officers with concurrent certification primarily perform law enforcement activities (Baker 2017b).

The complaints dataset records for each complaint the source of the complaint, the officer, the officer's agency, the date on which the case was opened, and the disposition of the complaint.[8] The most common source is investigations by local agencies. The complaint-source categories "Internal Investigation" and "Affidavit of Separation" both comprise complaints that originate with the local agency; the latter category is used when the officer's employment has been terminated. "Verifiable Complaints" include complaints from members of the public, while "Newspaper" includes incidents brought to the attention of the FDLE through media reports.

From this database, we extract information on the number of complaints and aggregate this to the agency-year level – for instance, we compute the number of complaints faced by officers employed at the Broward County SO in 2002. The majority of these observations are zeroes, indicating that the agency had no complaints of officer misconduct reported in the FDLE complaints database in that year. Over 1997-2016, about 38% of agency-year observations have a positive number of complaints.

As previously discussed, complaints are an imperfect measure of underlying police misconduct. Complaints are potentially affected by the strength of internal mechanisms for detection and by the propensity of civilians to report misconduct, as well as by the level of misconduct. As noted earlier, however, all of the complaints in the ATMS database meet a specified threshold of seriousness, and most were already sustained by a local agency. These state-level complaints, therefore, may be a better proxy for misconduct than are agency-level complaints, including civilian complaints.[9]

The FDLE has the power to impose additional sanctions on officers, beyond those imposed by the local agency. For each complaint in the database, we observe whether the FDLE imposed any disciplinary action. We extract information on the number of disciplinary actions, aggregated to the agency-year level. Again, there are a large number of zeroes. Over 1997-2016, about 18% of observations have a positive number of disciplinary actions. Among agency-years with a

[8] As we aggregate this data to the agency-year level, we can only use complaints with nonmissing data on the officer's agency and on the year in which the case was opened. About 15% of complaints have missing data on the date on which the case was opened, while 9% of complaints have missing data on the officer's agency.

[9] Rozema and Schanzenbach (2016) find a strong relationship between civilian complaints against police officers and actual misconduct (as proxied by litigation), using data from the Chicago Police Department. In some respects, their finding is supportive of our use of complaints as a proxy for misconduct. Note, however, that our state-level measure of complaints is actually closer to being a measure of adjudicated instances of misconduct, and so is likely to be an even better proxy for underlying misconduct than are civilian complaints.

positive number of complaints, about 47% have a positive number of disciplinary actions by the state.

Control variables for the analysis are obtained from a number of additional sources. We use Census Bureau estimates of county population size, the fraction of the county population aged 20 to 24, and the racial and ethnic composition of the county population (U.S. Census Bureau, "Population and Housing Unit Estimates," at https://www.census.gov/programs-surveys/popest.html). Unemployment rates at the county level are obtained from the Bureau of Labor Statistics (at https://www.bls.gov/data/#unemployment). Crime rates by law enforcement agency are from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) system (at https://www.bjs.gov/ucrdata/abouttheucr.cfm). The UCR data also provides an estimate of the population of the area corresponding to the law enforcement agency's jurisdiction. This has somewhat less coverage than the county population data from the Census Bureau, but the results are robust to using the UCR measure rather than the Census Bureau measure of population.

Table 1 reports summary statistics for the primary variables used in the analysis, separately for the treatment and control groups before and after *Williams*. The control group consists of all PDs for which data is available. As noted in Section 3, one by one, nine of the 67 SOs in Florida had obtained county-specific legislation before 2003 allowing them to engage in collective bargaining.[10] These SOs were thus unaffected by *Williams*, and so we exclude them from our baseline analysis. This leaves 58 SOs in our treatment group. Note, however, that reclassifying these nine SOs as part of the control group (rather than excluding them from the analysis) leads to very similar results.

The descriptive statistics in Table 1 suggest a relative increase in complaints and in state disciplinary actions for the 58 SOs in our treatment group, when compared to PDs. It also suggests, albeit less clearly, the possibility of post-*Williams* increases for SOs in the fraction of non-Hispanic White officers, the fraction of male officers, the length of training required for new recruits with an FTO, the fraction of agencies imposing educational requirements beyond a high school degree, and the fraction of agencies issuing all officers weapons more powerful than a handgun (i.e., shotguns or rifles). These outcomes are all analyzed further in the regression analysis described below.

[10] These SOs are Broward, Charlotte, Escambia, Flagler, Jacksonville, Miami-Dade, Monroe, Nassau, and Volusia (Doerner and Doerner 2010, pp. 382-83).

Table 1 also reports summary statistics for the control variables. The treatment and control groups differ along some dimensions. PDs tend to be smaller on average, in terms of the number of sworn law enforcement and concurrent officers employed. Relative to SOs, PDs are more likely to be located in areas with higher crime rates and with larger and more diverse populations. However, these differences in characteristics do not appear to change substantially in the post-*Williams* period. Moreover, the regression analysis described below controls for changes in these variables.

A first step in the study is to verify that the treatment (i.e., the *Williams* decision) did indeed have an impact on the unionization status of SOs, as has been documented in the past (see, for example, Doerner and Doerner 2010). Figure 1 plots the fraction of SOs and PDs with CBAs, as reported in the CJAP data over 2001-2010. As the treatment group excludes those SOs that had previously obtained collective bargaing rights, this fraction is initially zero for the treatment SOs. After *Williams* was decided in January 2003, union activity begins among SOs within the same year. However, there is a clear lag in the impact of *Williams*, in the sense that the fraction of SOs with CBAs keeps rising for about three years, before stabilizing after about 2006. This pattern suggests that any impact of collective bargaining rights (whether directly through CBAs or indirectly through bargaining in the shadow of unionization) may be similarly delayed. Thus, our primary analysis uses a three-year lag of the treatment variable.[11]

Another important point to note from Figure 1 is that the fraction of PDs with CBAs remained quite stable throughout this period (at a little over a half). This suggests that, while outcomes for PDs were potentially affected throughout this time by collective bargaining rights, this impact is unlikely to have changed over this period. Moreover, the baseline result is robust to redefining the control group as consisting only of those PDs without CBAs (as well as to restricting the control group to PDs with CBAs). It is also robust to comparing SOs without CBAs to PDs without CBAs.

Figure 2 plots the mean number of complaints for the treatment and control groups over 1997-2010. The two groups follow what appear to be parallel trends prior to *Williams*. Following *Williams*, complaints against officers at SOs seem to follow an upward trend, while those for PDs

[11] There are, of course, additional reasons to expect that any effect would occur with some lag. For instance, Putchinski (2007, p. 46) notes that, "[a]s collective bargaining progresses over time, police unions tend to become more influential within the local host government." However, we use Figure 1 to choose a specific lag to use in our empirical specification.

remain quite stable. Most dramatically, there is a large increase in complaints about three years after *Williams* (corresponding to the period over which SOs transitioned to CBAs, as shown in Figure 1). This is partially reversed the following year, although the magnitude of the decline is substantially smaller than the prior year's increase. Thereafter, the upward trend resumes. Overall, the post-*Williams* period exhibits an upward trend for SOs, relative to PDs, even ignoring the 2006 spike.

Moreover, this increase in complaints among SOs appears most pronounced among SOs that adopted CBAs. Figure 3 reports the mean number of complaints for SOs only, divided between SOs with and without CBAs. Initially in 2003, those SOs that adopt CBAs look very similar to other SOs. Over subsequent years, and especially in 2006, their complaints increase substantially. Figure 3 suggests that the spike in complaints among SOs in 2006 (and the upward trend in the post-*Williams* period) is concentrated among those SOs with CBAs. Indeed, our baseline regression result is robust (and the magnitude of the estimated effect is somewhat larger) when restricting the treatment group to only those SOs with CBAs.

Given the unusual nature of the spike in complaints in 2006, it is important to determine whether it may be attributable to some extraneous factor (unrelated to *Williams*). Our searches of news sources and our communications with the FDLE have not uncovered any alternative factor that would account for this pattern in the data.[12] This pattern does not seem attributable to outliers, as the regression results are robust to excluding the SO with the largest number of complaints in 2006. Moreover, the increase in complaints appears to have been quite widely distributed among SOs – about half of SOs experienced an increase in complaints in 2006, including 10 out of 19 SOs with CBAs and 17 out of 38 SOs without CBAs.[13]

The distribution of sources of complaints also does not seem to have changed dramatically in 2006 relative to prior years. Figure 4 shows the fraction of complaints originating from each of the sources recorded in the FDLE data in 2006 and 2005. The fraction attributable to the most common source – investigations by the local agency (including both "Internal Investigation" and "Affidavit of Separation" categories) – did not change substantially. The "Newspaper" category –

[12] In 2008, news sources reported that corrections officers at Palm Beach SO were alleged to have been involved in a scheme to fraudulently claim overtime pay (see, for example, Byrd 2008). Our analysis does not include corrections officers, but it is possible that some concurrent officers may have been accused (and, although the reports emerged in 2008, some of the alleged conduct occurred in past years, potentially including 2006). However, our regression results are robust to omitting Palm Beach SO from the analysis.
[13] Note that only 12 SOs (4 with CBAs and 8 without CBAs) experienced decreases in the number of complaints.

incidents brought to the attention of the FDLE through media reports – declined. This suggests that the increased number of complaints is unlikely to result from greater zeal on the part of the FDLE. There was an increase in “Verifiable Complaints” (which include complaints from members of the public), consistent with an increase in underlying misconduct. The “Other” category also grew, but it is difficult to interpret the implications of this, as there is limited information about what this category includes.

Overall, the figures discussed above suggest that complaints increased for SOs after *Williams*. Figure 5 plots the corresponding numbers for state disciplinary actions against officers. Here, too, there is evidence of an increase in 2006, with disciplinary actions stabilizing at a higher level than previously existed. Moreover, in a manner analogous to the pattern in Figure 3, the increase in disciplinary actions after 2003 is more pronounced among SOs with CBAs. However, the number of disciplinary actions for SOs exhibits substantially greater volatility than that for PDs prior to *Williams*. Thus, while there appears to be an upward trend following *Williams* for SOs relative to PDs, it is difficult to reach any firm conclusion.

Figure 6 shows the fraction of non-Hispanic White officers at SOs and PDs over this time period. This fraction is relatively stable for SOs, while it has declined significantly for PDs. The regression analysis below seeks to analyze whether a causal impact from *Williams* can be detected. Figure 7 shows the corresponding plot for the fraction of officers who are male. There is very little variation in this fraction over time, either for SOs or for PDs, making it difficult to reach any firm conclusions about the impact of *Williams*.

4.2) Empirical Strategy

As foreshadowed in Section 3, our empirical strategy involves comparing outcomes such as complaints for SOs and PDs before and after the *Williams* decision. This approach has a number of advantages over those in the existing literature. Prior studies of the impact of police unions use potentially endogenous unionization decisions (Anzia and Moe 2014) or changes in state law with respect to public sector unions (Frandsen 2014; Ichniowski, Freeman, and Lauer 1989). The latter approach requires the assumption that state legislation is exogenous with respect to outcomes of interest. Our approach holds state-level factors constant by focusing on quasi-experimental variation across agencies in the same state.

Pynes and Corley (2006, p. 299) highlight the “unusual history of collective bargaining” rights in Florida, but the *Williams* decision has not previously been used to construct a quasi-experimental framework. Doerner and Doerner (2010) refer to the case, but their empirical analysis uses only data on Florida SOs to examine wage and benefits outcomes for SOs that (potentially endogenously) choose to unionize after *Williams*. Doerner and Doerner (2013) extend their analysis to Florida PDs, but do not use PDs as a control group for SOs; the source of variation is again derived from potentially endogenous unionization decisions.[14]

While we observe CBA status at the agency-year level in the CJAP data, a major concern with using this as our variable of interest is that the factors that lead officers in a given agency to unionize and form a collective bargaining unit may be correlated with the outcome variables (such as the number of complaints). For instance, officers who anticipate engaging in higher levels of misconduct in the future may form a union in order to obtain greater procedural safeguards against being disciplined. A regression of complaints on CBA status would then yield a coefficient that may be biased upwards, relative to the causal impact of CBA status on complaints. Our approach of using an exogenous conferral of collective bargaining rights to a subset of agencies avoids this potential bias.[15] It also allows us to analyze the impact of bargaining in the shadow of collective bargaining rights (irrespective of CBA status).

The main outcome variable (complaints) takes on only non-negative integer values, and thus is an example of "count" data. Moreover, it includes many zero observations, as noted above. Although linear specifications are in general highly flexible and robust, there are a number of problems with a standard linear model in these circumstances. Due to the skewness of the data and the large number of zeroes, the normality of errors assumption is difficult to satisfy with any feasible transformation. It is thus common to use a specification that better accommodates count data, such as the Poisson maximum-likelihood model:

$$Y_{it} = \exp(\beta_1 (Post_t * SO_i)_{i,t-3} + \beta_2 Officers_{it} + \gamma \mathbf{X}_{it} + \mu_i + \delta_t)\epsilon_{it} \qquad (1)$$

Y_{it} represents an outcome variable (such as the number of complaints) at agency *i* in year *t*. $Post_t$ is an indicator variable equal to one for the years (2003-2010) after *Williams*.[16] SO_i is an indicator

[14] Bulman (2017) uses an empirical strategy that identifies the impact of the race of the sheriff on the racial composition of arrests, controlling for the race of police department chiefs in the same county. While his approach also compares SOs and PDs, the identification strategy and research question are very different from ours.

[15] Note, however, that in our setting a "naïve" regression of the number of complaints on an indicator variable for the presence of a CBA yields a positive and significant coefficient on the CBA variable. The magnitude is fairly similar to our baseline difference-in-difference estimate. This suggests that this type of bias is not very large in our particular context. Even so, in the absence of a quasi-experiment such as *Williams*, it would be impossible to be confident of the credibility of a causal interpretation of this effect.

[16] Note that $Post_t$ includes 2003 because the decision was made in January of that year.

variable equal to one if agency i is part of the treatment group. The interaction term $(Post_t * SO_i)_{i,t-3}$ is our variable of interest; as noted earlier, we use a 3-year lag to reflect the delayed response evident in Figure 1. $Officers_{it}$ is the number of sworn officers (certified in law enforcement or concurrently certified in both law enforcement and corrections) employed at agency *i* in year *t*. μ_i is an agency fixed effect and δ_t is a year fixed effect, while ϵ_{it} is the error term.

$\mathbf{X}_{it}$ is a vector of control variables, which fall into three categories. The first category captures the demographic characteristics of the county in which agency *i* is located. These include the size of the resident population, the fraction of the resident population aged 18-24, the fraction of the resident population that is Hispanic, and the fraction of the resident population that is African American. Second, local economic conditions are captured by the county's unemployment rate. The third category of controls is for crime rates, and includes the number of murders, property crimes, and violent crimes in agency *i*'s jurisdiction in year *t*.

Crime rates are important controls as they indicate the extent and nature of law enforcement contacts with civilians. We include aggregate measures of property and violent crimes. As these may be mismeasured due to underreporting (and may indeed be influenced by the agency's policies and likelihood of solving crimes), we also include murders, which are generally thought to be measured with greater accuracy. The unemployment rate in part serves as a proxy for incentives to commit crime, but also provides a measure of officers' outside options in the local area (and hence the opportunity cost of misconduct). The demographic variables capture aspects of the characteristics of the local area that may affect the number of complaints and the various other outcome variables that we study. For instance, the fraction of the resident population aged 18-24 is often thought to be correlated with crime rates. The racial composition of the resident population is of particular importance when testing for effects of *Williams* on the racial diversity of agencies.

The Poisson model in Equation (1) has a number of potential limitations. The Poisson distribution assumes that the variance is equal to the mean, although it is somewhat robust to violations of this assumption. To address possible "over-dispersion" (where the variance exceeds the mean) in our data, we compute robust standard errors that are clustered at the agency level. We also use the alternative negative binomial model for count data. Unfortunately, the baseline specification (with agency fixed effects along with robust standard errors clustered at the agency level) cannot be precisely replicated in the negative binomial framework. However, using alternative negative binomial specifications that allow for robust standard errors leads to generally similar results.

A variant of the Poisson model, known as the zero-inflated Poisson (ZIP) specification, is designed to accommodate situations where the data includes a particularly large number of zeroes. However, available implementations of the ZIP model do not allow for fixed effects, and thus we would be unable to control for unobserved heterogeneity across agencies. Moreover, the ZIP model requires an exclusion restriction – a variable that affects the probability of a zero observation, while not affecting the magnitude of nonzero observations – that is not readily available in our setting.

For other potential outcome variables such as the racial and gender composition of agencies and the length of FTO training, our baseline analysis uses a linear fixed-effects specification:

$$Y_{it} = \beta_1(Post_t * SO_i)_{i,t-3} + \beta_2 Officers_{it} + \gamma \boldsymbol{X}_{it} + \mu_i + \delta_t + \epsilon_{it} \qquad (2)$$

where Y_{it} is a relevant outcome variable and the other variables are as defined above in Equation (1). For robustness checks, we add to Equation (2) linear agency-specific trends of the form $(A_i * t)_{it}$, where A_i is an indicator variable equal to one for agency i and t is the year. In other robustness checks, we add to Equation (2) county-by-year fixed effects of the form η_{ct} - i.e., an indicator variable for county *c* (which typically contains multiple agencies) in year *t*. Note also that, while the baseline analysis of complaints uses the Poisson specification in Equation (1), the results are very similar – and indeed stronger in some respects – when using the linear specification in Equation (2).

A key assumption of our difference-in-difference approach is that the treatment and control groups experience parallel trends in the period prior to the treatment. That is, it is important to establish that any estimated effect for complaints against SOs is not the result of a preexisting increasing trend in complaints for SOs, relative to PDs. We follow three different approaches to addressing this issue. The first is the visual inspection of the graph in Figure 2, which does not suggest that SOs and PDs experienced different trends in complaints prior to *Williams* (see the discussion in Section 4.1 above). Second, we add to our regression specification (Equation (1)) a series of leads and lags of the variable of interest, in order to determine whether a differential "effect" for SOs is detectable in years prior to *Williams* (see the discussion in Section 5.1 below). Finally, we add linear agency-specific time trends to our model (see the discussion in Section 5.2 below).

5) Results

5.1) Basic Results

The results from the specification in Equation (1) for the number of complaints is reported in Table 2. The time period used is 1997-2010, in order to maintain approximate balance in the lengths of the pre-*Williams* and post-*Williams* periods, and to focus on the effects of *Williams* rather than those of the subsequent legal developments.[17] The first column includes agency and year fixed effects, but no controls apart from the number of officers (which is included in all specifications in order to scale the number of complaints by agency size). The variable of interest is the third lag of the interaction of a post-*Williams* dummy with a dummy for SOs. This has a positive coefficient that is statistically significant at the 5% level. This estimate is virtually identical in magnitude and significance when adding the extensive set of demographic, economic, and crime-rate controls described previously (Column 2).

As the Poisson specification takes an exponential form (see Equation (1)), the percentage impact of *Williams* on complaints is given by $100(e^{0.24}-1)$, holding all other independent variables fixed. Thus, the estimated coefficient implies that collective bargaining rights lead to about a 27% increase in complaints of officer misconduct. As the mean number of complaints per year for SOs prior to *Williams* is a little over 2, this implies an increase of about 0.6 of a complaint for a typical SO (holding all other independent variables fixed). The implied magnitude is thus quite substantial.

In Column 3 of Table 2, we add a series of leads and lags of the variable of interest, following an approach proposed by Autor (2003). Figure 8 plots these leads and lags of the difference-in-difference estimate, with 95% confidence intervals also being shown. There are a number of reasons for undertaking this exercise. First, it enables us to verify the time pattern of the estimated effects, in particular whether the three-year lagged effect that we assumed in Columns 1 and 2 (based on the time pattern of SOs adopting CBAs in Figure 1) is borne out by this more flexible specification. Indeed, the results in Column 3 indicate that the effect is concentrated in the third year after *Williams*. As might be expected from Figure 2, there is a partial reversal of this effect in the subsequent year; however, this is substantially smaller in magnitude

[17] Note that the results are similar, albeit somewhat weaker, when using the full 1997-2016 period. It is possible, however, that the longer post-*Williams* period reduces the precision with which the treatment effect can be estimated.

than the prior year's increase, and is not statistically significant. Similarly, Figure 8 shows that the only statistically significant difference-in-difference effect is in 2006, with the other estimates being statistically insignificant and mostly close to zero.

The leads and lags in Column 3 also provide evidence for a causal interpretation of the estimated effect. In particular, there are no apparent differential effects for SOs relative to PDs for years prior to *Williams*. These estimates can be viewed as "false experiments" (or placebo tests) showing the absence of any effects prior to the actual experiment (i.e. *Williams*). They imply that SOs did not experience a preexisting upward trend in complaints that merely continued in the post-*Williams* period. Figure 2 does not suggest the existence of such a preexisting trend, and the results in Column 3 confirm this, thereby reinforcing a causal interpretation of our findings as an effect of the collective bargaining rights *Williams* conferred.

In Columns 1-3 of Table 2, we include both zero and nonzero observations of the number of complaints (note, however, that the Poisson model omits any agency for which complaints are zero in all years). In Column 4, we restrict the analysis to nonzero observations only. The result is robust, and indeed larger in magnitude than in Column 2. Admittedly, excluding zeroes potentially creates bias in the estimates. However, the similarity of the findings with and without zeroes suggests that the basic results in the prior columns are not attributable to any bias associated with the presence of an excess number of zeroes in our data. Moreover, it implies that our finding holds along the "intensive" margin – i.e., SOs that were already experiencing complaints tended to experience more of them after *Williams*.

A potential alternative explanation for a post-*Williams* increase in complaints against SOs is that unionization may result in an increased degree of bureaucratization of the investigation process. This may entail that – rather than there being an increase in the level of actual misconduct – a larger number of less serious complaints end up being recorded. To address this point about the "quality" (or seriousness) of complaints, we examine the number of disciplinary actions by the state, conditional on the presence of complaints. Column 5 of Table 2 (like the previous column) restricts the sample to agency-years with a nonzero number of complaints, while the dependent variable is now the number of state disciplinary actions. The estimates imply an increase in the number of disciplinary actions following *Williams*, although this is only of borderline statistical significance.

The imposition of discipline by the state (in addition to penalties imposed at the local level) can arguably be viewed as a proxy for complaints of greater seriousness. In principle, it is possible that the absence of state disciplinary action results not from the lack of severity of the infraction, but rather is due to the local agency imposing sufficiently strong sanctions. In such circumstances, the FDLE issues a Letter of Acknowledgement (LOA), acknowledging that the disciplinary action already taken by the agency is sufficient. However, under the FDLE's rules, an LOA cannot be issued in cases where the officer has faced discipline in the past eight years, when the officer has resigned, or when the penalty guideline for the misconduct specifies a penalty ranging from prospective suspension of certification to revocation (F.A.C. Rule 11B-27.005). As a result, most serious violations are subject to state disciplinary action, irrespective of the local agency's penalty. Thus, the increase in disciplinary actions conditional on complaints in Column 5 of Table 2 suggests that the substantially higher number of complaints against SO officers in and after 2006 was not accompanied by a decline in the seriousness of these complaints.

5.2) Robustness Checks

Figure 2 does not suggest an obvious differential time trend for complaints among SOs relative to PDs prior to *Williams*. Moreover, Figure 8 shows that our difference-in-difference estimate is small and statistically insignificant in years prior to *Williams*, as would be expected in the absence of a preexisting trend. A more formal approach to accounting for prior trends is to add linear agency-specific trends to the model. This allows for each agency to have an arbitrary growth rate in complaints, so that the difference-in-difference estimate captures the change *from prior trends* for SOs relative to PDs. This involves adding up to 366 new variables of the form $(A_i * t)_{it}$, where A_i is an indicator variable equal to one for agency i and t is the year. Unfortunately, maximum-likelihood estimation for the Poisson model (Equation (1)) does not converge when linear agency-specific trends are included. Thus, notwithstanding the limitations of the linear model with data of this nature (as discussed in Section 4.2 above), we use a linear framework to assess whether linear agency-specific trends change the baseline findings.

Column 1 of Table 3 reports a simple linear specification for the number of complaints (similar to Equation (2)), while Column 2 adds the standard set of control variables. In each case, the *Williams* effect on SOs is substantial and statistically significant. The magnitude implies that collective bargaining rights lead to about an additional 0.6 complaints per year (relative to a mean among SOs prior to *Williams* of a little over 2). This is very close to the magnitude estimated for a typical SO using the Poisson model (Columns 1 and 2 of Table 2). While caution must be exercised in interpreting a linear specification in view of the nature of our data, it is reassuring that the Poisson and linear models lead to very similar conclusions.

Column 3 of Table 3 adds linear agency-specific trends to the model. This does not affect the statistical significance of the result, relative to Columns 1 and 2. Indeed, the magnitude is larger, implying that collective bargaining rights lead to about one additional complaint per year. The main implication here is that linear agency-specific trends do not undermine the basic result in a linear framework. This provides some indication that the baseline result using the Poisson model is unlikely to be driven by preexisting trends.

The specification we have used controls for a wide variety of demographic, economic, and crime-related variables. However, it is still possible that unobserved changes at the local level may affect the number of complaints. It is possible to take account of these by including county-by-year fixed effects of the form η_{ct} - i.e., an indicator variable for county *c* in year *t*. These county-by-year fixed effects can in principle be estimated even in the presence of agency and year fixed effects (and linear agency-specific trends), as each county typically includes multiple agencies. However, as Florida has 67 counties, we need to include 938 county-by-year fixed effects over 1997-2010, and maximum-likelihood estimation for the Poisson model (Equation (1)) does not converge in these circumstances. Adding county-by-year fixed effects to the linear specification (Column 4 of Table 3), however, shows that the basic result remains robust. Note here that the demographic variables and unemployment are omitted because they are defined at the county-year level (and would thus be absorbed by county-by-year fixed effects); the crime variables are included because they are defined at the agency-year level, and so vary within a county-year cell. The estimated magnitude again implies that collective bargaining rights lead to about one additional complaint per year.

As noted at various prior points in the paper, we conduct a number of other robustness checks that are not reported in the tables. For instance, the results are robust to using the UCR measure of city population, rather than the Census Bureau measure of county population. The baseline result is robust to redefining the control group as consisting of only those PDs without CBAs, or to redefining the treatment group as consisting of only those SOs with CBAs. The nine SOs that obtained legislative dispensation to engage in collective bargaining prior to 2003 are excluded from the baseline analysis, but treating them as part of the control group instead leads to similar results. The result is also robust to excluding Palm Beach SO – the SO with the largest number of complaints in 2006 – from the analysis.

5.3) Other Outcome Variables and Tests

While our primary results relate to complaints, there are a number of other outcome variables that are potentially of interest in relation to ongoing scholarly and public debates about policing. We thus report in Table 4 the analysis of various other outcomes, although none of these results is robustly significant across specifications.

An important – and largely unresolved – issue in the literature on police unions is the impact of collective bargaining rights on racial and gender diversity in law enforcement agencies. Indeed, Morabito and Shelley (2015, p. 338) notes that, "[t]hus far, the empirical relationship between diversity and unionization is unclear." The first two columns of Table 4 report results from running the linear specification in Equation (2) on a variable capturing the racial composition of agencies – the fraction of officers who report their race/ethnicity as non-Hispanic White.[18] Column 1 finds what appears to be a substantial and statistically significant increase in the fraction of officers who are non-Hispanic Whites at SOs, relative to PDs, after *Williams*. However, adding the control variables (Column 2) leads to a smaller and statistically insignificant effect. In particular, it appears that the control variable for the Hispanic fraction of the resident population – which is highly significant in Column 2 – substantially absorbs the apparent *Williams* effect. We also test for any impact on the fraction of African American officers, but the estimated impact of *Williams* is small and statistically insignificant.

Thus, any effect of *Williams* on the racial composition of law enforcement agencies is not robustly significant across specifications. It should be remembered, however, that there is limited variation in the racial composition of agencies, as illustrated in Figure 6. Similarly, as shown in Figure 7, there is very limited variation over our sample period in the fraction of male officers for either SOs or PDs (and the (unreported) difference-in-difference estimate of the effect of *Williams* on the fraction of male officers is very close to zero and is statistically insignificant). The limited

[18] This fraction is computed as the number of officers in an agency-year reporting that they are non-Hispanic White divided by the total number of officers employed (excluding those officers with a missing value for the race variable in the ATMS database). In each case, only law enforcement and concurrently certified officers are included (with corrections officers being excluded). The results are similar when dividing by the total number of officers employed (including those officers with a missing value for the race variable in the ATMS database).

variation makes it difficult to detect any impact that collective bargaining rights may have on racial and gender diversity.

Column 3 of Table 4 reports results on the length of the FTO training period. The difference-in-difference estimate implies that collective bargaining rights increase the training period by about 6 days (relative to a mean of about 10 weeks for SOs in 2002). However, the estimate is of only borderline statistical significance. Column 4 of Table 4 reports a conditional fixed effects logit model for a binary variable equal to one if the minimum education requirement exceeds a high school diploma. The logit model omits agencies that do not change their minimum education requirement during our sample period, accounting for the small sample size. The estimate implies an increase in the probability of higher minimum education requirements. This is also the case when using a linear model, but the estimate is of only borderline statistical significance.

Taken together, the results on training and education are not robustly significant across specifications. To the extent that there is any positive impact of collective bargaining rights on these requirements, this may be another factor creating a bias against our basic finding on complaints. In particular, it might be expected that better-trained and better-educated officers would, other things equal, be less likely to engage in misconduct. (On training, see Eitle, D'Alessio, and Stolzenberg (2014); on education, see Chapman (2012); Kane and White (2009); Lersch and Kunzman (2001).)[19] However, firm conclusions are difficult to reach because the estimates are not robustly significant.[20]

We described in Section 3.2 above the legal developments with respect to the "legislative body" question that lingered subsequent to the *Williams* decision. To the extent that the definition of the legislative body matters for outcomes such as police misconduct, it is possible that the resolution of legal uncertainty about this issue (which occurred around 2010 or 2011) may generate additional effects. We test for any such effects by running a difference-in-difference model analogous to Equations (1) and (2), with the variable of interest being an interaction between an indicator for post-2010 years and an indicator for SOs. This is run over the sample period 2004-

[19] An alternative hypothesis, in line with our results, is that bad training causes bad behavior (Getty, Worrall, and Morris 2016). If most of the new increment of training were of the wrong kind, it could make police behavior worse. It is also possible that heightened minimum education requirements reduce the pool of eligible officer candidates who otherwise present a low risk of misconduct.

[20] Estimates of the impact of *Williams* on the types of firearms issued to officers (not reported here) are small and statistically insignificant.

2016, to focus on the post-*Williams* period. These tests do not reveal any additional effect on the number of complaints: the interaction term of interest is small and statistically insignificant. This suggests that the impact of *Williams* has largely been captured by our earlier results (in Tables 2 and 3).

6) Discussion and Conclusion

The determinants of law enforcement misconduct have become a question of wide interest to scholars, policymakers, and the public. We provide what we believe to be the first quasi-experimental evidence on the impact of collective bargaining rights on misconduct by law enforcement officers. Using the Florida Supreme Court's *Williams* decision of 2003, we show that the conferral of collective bargaining rights on officers at SOs led to an increase in complaints of misconduct, relative to a control group of PDs that were unaffected by *Williams*.

While some of these points have been raised earlier, it is worth reiterating that there are several sources of potential bias against this finding. First, Florida is a right-to-work state, which limits the ability of employees to organize effectively. Second, Florida has a statutory LEOBOR that applies to all law enforcement officers, leaving less for collective bargaining to accomplish. Relatedly, the existence of a state-level FDLE disciplining mechanism leaves less room for collective bargaining to reduce the expected sanctions for misconduct. Our result may be attributable to those CBA provisions that exceed the LEOBOR in extending procedural protections to officers (we give examples below).

Third, throughout the post-treatment observation period, sheriffs continued to claim the authority, as the pertinent "legislative body," to resolve any bargaining impasse, creating uncertainty about the strength of the *Williams* right (although we do not find any detectable additional effect of the resolution of uncertainty about this question). Fourth, our measure of complaints consists mostly of misconduct claims that have been sustained by the local agency. To the extent that unionized agencies are less likely to sustain complaints (because of the strong procedural protections they afford), it is possible that fewer complaints will be initiated. Even if the same number of complaints are initiated, the number of sustained complaints reported to the FDLE would fall. These potential effects would create a bias against our finding.

Finally, we noted earlier that the duties of sheriff deputies and police officers are quite similar, and that the agencies draw upon similar pools of applicants. If the labor market for SO and

PD officers were perfectly integrated and frictionless, then the procedural protections (and other benefits) of unionized PDs would form part of SO officers' outside option. Their ability to seamlessly switch employers to a unionized PD would have resulted in their receiving these benefits even prior to *Williams*; thus, *Williams* would not have had any impact. In reality, labor markets are not frictionless; the accumulation of agency-specific human capital and the costs of moving may limit the mobility of officers across agencies, even if the initial applicant pool is very similar. Nonetheless, to the extent that SO and PD labor markets are integrated, that would constitute another form of bias against our result.

Our quasi-experimental results are consistent with the observation made earlier that CBAs contain provisions (beyond those in Florida's LEOBOR) that make investigations more difficult and less likely to result in significant sanctions. For example, some Florida CBAs give law enforcement officers the right to challenge any discipline the local government seeks to impose through arbitration or other administrative review,[21] thus depriving the government of the power to make independent disciplinary decisions. Other rights include a time limitation on internal disciplinary investigations, expungement of old records even when the officer is found to have engaged in misconduct, and inspection of investigation files prior to a disciplinary hearing. Like the example in the introduction, all of these additional procedural rights raise the cost of terminating misbehaving officers and thereby lower deterrence.[22] Our results are also potentially consistent with the idea that the political influence of unions leads to local legislation that embodies similar procedural protections. It would be valuable to narrow down the causal mechanisms that are responsible for our results. In ongoing research, we are collecting and analyzing the text of CBAs from Florida law enforcement agencies, with the aim of identifying specific provisions

[21] See, e.g, Agreement Between City of Coral Springs and Fraternal Order of Police I, Law Enforcement Officers, Lodge #87 (valid through Sept. 30, 2018) (Article 47(c): "After the imposition of discipline, the affected employee shall have the right to challenge the discipline per Article 37, the Grievance Procedure and Article 38 Arbitration."); Agreement Between City of Hialeah. and Dade County Police Benevolent Ass'n (Oct. 1, 2013 – Sept. 30, 2016) (Art. 25, sec. 3(b): "No employee shall serve a suspension without pay until an Arbitrator or the Personnel Board has rendered a decision, whichever procedure is applicable.").

[22] See, e.g., Agreement Between Fraternal Order of Police, Coral Gables Lodge #7 and The City of Coral Gables (Oct. 1, 2013 – Sept. 30, 2016) (Art. 10(n): "No records will be saved, for any reason, beyond three years from the date that they were first eligible for destruction with the exception of noticed litigation.") Agreement Between City of Hialeah, Fla. and Dade County Police Benevolent Ass'n (Oct. 1, 2013 – Sept. 30, 2016) (Art. 25, sec. 2(o): "Any internal investigation, except where criminal charges are being investigated, shall be completed within sixty (60) days from the date the officer is informed of the initial complaint. No officer may be subjected to any disciplinary action as a result of any investigation not completed within that time period.").

found in these CBAs that provide procedural protections for officers that go beyond those in Florida's LEOBOR.

There are also some possible channels that would not be found in the CBAs. For instance, it is possible that certain LEOBOR rights may be partly ignored by some sheriffs before there is collective bargaining but not afterwards. It is also possible that collective bargaining increases the group cohesion of officers and strengthens pro-worker norms, perhaps including the code of silence that reduces the probability of the detection of misconduct. Agencies may also be more reluctant to take actions that alienate officers (independently of specific CBA provisions) when officers have greater bargaining power. Our ongoing work seeks to distinguish among these and other potential channels to the extent possible with the available data.

References

Anzia, Sarah F., and Terry M. Moe. 2014. Public Sector Unions and the Costs of Government. *Journal of Politics* 77:114-27.

Autor, David H. 2003. Outsourcing at Will: The Contribution of Unjust Dismissal Doctrine to the Growth of Employment Outsourcing. *Journal of Labor Economics* 21:1-42.

Baker, Terry. 2017a. Email correspondence with John Rappaport, 30 October.

_____. 2017b. Email correspondence with John Rappaport, 29 November.

Becker, Gary S., and George J. Stigler. 1974. Law Enforcement, Malfeasance, and Compensation of Enforcers. *Journal of Legal Studies* 3:1-18.

Bies, Katherine J. 2017. Let the Sunshine In: Illuminating the Powerful Role Police Unions Play in Shielding Officer Misconduct. *Stanford Law & Policy Review* 28:109-49.

Briggs, Steven J., Jihong Zhao, Steve Wilson, and Ling Ren. 2008. The Effect of Collective Bargaining on Large Police Agency Supplemental Compensation Policies: 1990-2000. *Police Practice and Research: An International Journal* 9: 227-38.

Byrd, Allison. 2008. Deputy's Complaint Spurs Probe of Officers. *Palm Beach Post*, May 14.

Bulman, George. 2017. Law Enforcement Leaders and the Racial Composition of Arrests: Evidence from Overlapping Jurisdictions. Working paper.

Chapman, Christopher. 2012. Use of Force in Minority Communities Is Related to Police Education, Age, Experience, and Ethnicity. *Police Practice and Research: An International Journal* 13:421-36.

Cravey, Beth Reese. 2008. Board of Commissioners Backs Sheriff as Authority in Disputes. *Florida Times Union*, November 8.

_____. 2009. Clay Sheriff To Appeal Union's Win in Court. *Florida Times Union*, May 27.

DeCarlo, John, and Michael J. Jenkins. 2015. *Labor Unions, Management Innovation and Organizational Change in Police Departments*. Springer.

Delaney, John Thomas, and Peter Feuille. 1985. Collective Bargaining, Interest Arbitration, and the Delivery of Police Services. *Review of Public Personnel Administration* 5:21-36.

Dharmapala, Dhammika, Nuno Garoupa, and Richard H. McAdams. 2016. Punitive Police? Agency Costs, Law Enforcement, and Criminal Procedure. *Journal of Legal Studies* 45:105-41.

Doerner, William M., and William G. Doerner. 2010. Collective Bargaining and Job Benefits: The Case of Florida Deputy Sheriffs. *Police Quarterly* 13:367-86.

Doerner, William M., and William G. Doerner. 2013. Collective Bargaining and Job Benefits in Florida Municipal Police Agencies, 2000–2009. *American Journal of Criminal Justice* 38:657-77.

Eitle, David, Stewart J. D'Alessio, and Lisa Stolzenberg. 2014. The Effect of Organizational and Environmental Factors on Police Misconduct. *Police Quarterly* 17:103-26.

Ellman, Steve. 2004. A Gotcha from the Boss. *Miami Business Review*, September 9.

Epp, Charles R. 2009. *Making Rights Real: Activists, Bureaucrats, and the Creation of the Legalistic State*. Chicago, Ill.: University of Chicago Press.

Feuille, Peter, and John Thomas Delaney. 1986. Collective Bargaining, Interest Arbitration, and Police Salaries. *Industrial and Labor Relations Review* 39: 228-40.

Feuille, Peter, Wallace Hendricks, and John Thomas Delaney. 1983. *The Impact of Collective Bargaining and Interest Arbitration on Policing*.

Fisk, Catherine L., and L. Song Richardson. 2016. Police Unions. *George Washington Law Review*. 85:712-99.

Frandsen, Brigham R. 2014. The Effects of Collective Bargaining Rights on Public Employee Compensation: Evidence from Teachers, Firefighters, and Police. *Industrial and Labor Relations Review* 69:84-112.

Freeman, Richard B., and Eunice S. Han. 2012. Public Sector Unionism Without Collective Bargaining. Unpublished manuscript. Harvard University, Department of Economics, December.

Freeman, Richard B., and Robert G. Valletta. 1988. The Effects of Public Sector Labor Laws on Labor Market Institutions and Outcomes. Pp. 81-106 in *When Public Sector Workers Unionize*, edited by Richard B. Freeman and Casey Ichniowski. Chicago, Ill.: University of Chicago Press.

Fryer, Jr., Roland G. 2016. An Empirical Analysis of Racial Differences in Police Use of Force. NBER Working Paper No. 22399, at http://www.nber.org/papers/w22399.

Getty, Ryan M., John L. Worrall, and Robert G. Morris. 2016. How Far From the Tree Does the Apple Fall? Field Training Officers, Their Trainees, and Allegations of Misconduct. *Crime & Delinquency* 62:821-39.

Hickman, Matthew J., and Alex R. Piquero. 2009. Organizational, Administrative, and Environmental Correlates of Complaints About Police Use of Force: Does Minority Representation Matter? *Crime & Delinquency* 55:3-27.

Hickman, Matthew J., and Jane E. Poore. 2016. National Data on Citizen Complaints About Police Use of Force: Data Quality Concerns and the Potential (Mis)Use of Statistical Evidence to Address Police Agency Conduct. *Criminal Justice Policy Review* 27:455-79.

Huq, Aziz Z., and Richard H. McAdams. 2016. Litigating the Blue Wall of Silence: How To Challenge the Police Privilege To Delay Investigation. *University of Chicago Legal Forum* 2016: 213-53.

Ichniowski, Casey, Richard B. Freeman, and Harrison Lauer. 1989. Collective Bargaining Laws, Threat Effects, and the Determination of Police Compensation. *Journal of Labor Economics* 7: 191-209.

Iris, Mark. 1998. Police Discipline in Chicago: Arbitration or Arbitrary? *Journal of Criminal Law & Criminology* 89:215-44.

Kane, Robert J., and Michael D. White. 2009. Bad Cops: A Study of Career-Ending Misconduct Among New York City Police Officers. *Criminology and Public Policy* 8:737-69.

Kearney, Richard C., and Patrice M. Mareschal. 2014. *Labor Relations in the Public Sector*. 5th ed. Boca Raton, Fla.: CRC Press.

Keenan, Kevin M., and Samuel Walker. 2005. An Impediment to Police Accountability? An Analysis of Statutory Law Enforcement Officers' Bills of Rights. *Public Interest Law Journal* 14:185-244.

Legewie, Joscha, and Jeffrey Fagan. 2016. Group Threat, Police Officer Diversity and the Deadly Use of Police Force. Working Paper No. 14-512. Columbia University Law School, New York, NY.

Lersch, Kim Michelle, and Linda L. Kunzman. 2001. Misconduct Allegations and Higher Education in a Southern Sheriff's Department. *American Journal of Criminal Justice* 25:161-72.

Magenau, John M., and Raymond G. Hunt. 1996. Police Unions and the Police Role. *Human Relations* 49:1315-43.

Mas, Alexandre. 2006. Pay, Reference Points, and Police Performance. *Quarterly Journal of Economics* 121:783-821.

McCormick, Marcia L. 2015. Our Uneasiness with Police Unions: Power and Voice for the Powerful? *Saint Louis University Public Law Review* 35:47-65.

Mladenka, Kenneth R. 1991. Public Employee Unions, Reformism, and Black Employment in 1,200 American Cities. *Urban Affairs Quarterly* 26:532-48.

Moorhead, Molly. 2008. Union Skips Sheriff's Hearing on Contract Impasse. *St. Petersburg Times*, March 5.

Morabito, Melissa. 2014. American Police Unions: A Hindrance or Help to Innovation? *International Journal of Public Administration* 37:773-80.

Morabito, Melissa, and Tara O'Connor Shelley. 2015. Representative Bureaucracy: Understanding the Correlates of the Lagging Progress of Diversity in Policing. *Race and Justice* 5:330-55.

Nowacki, Jeffrey S., and Dale Willits. 2016. Adoption of Body Cameras by United States Police Agencies: An Organisational Analysis. *Policing and Society* 1-13.

Piven, Frances Fox. 1969. Militant Civil Servants in New York City. *Trans-action* 7:24-55.

Putchinski, Laurence J. 2007. *Union Influence and Police Expenditures*. New York, NY: LFB Scholarly Publishing LLC.

Pynes, Joan E. and Brian Corley. 2006. Collective Bargaining and Deputy Sheriffs in Florida: An Unusual History, *Public Personnel Management* 35:299-309.

Riccucci, Norma M. 1990. *Women, Minorities, and Unions in the Public Sector*. Westport, Conn.: Greenwood Press.

Rozema, Kyle, and Max M. Schanzenbach. 2016. Good Cop, Bad Cop: An Analysis of Chicago Civilian Allegations of Police Misconduct. Working Paper.

Rushin, Stephen. 2017. Police Union Contracts. *Duke Law Journal* 66:1191-1266.

Sass, Tim R., and Jennifer L. Troyer. 1999. Affirmative Action, Political Representation, Unions, and Female Police Employment. *Journal of Labor Research* 20:571-87.

Shane, Jon M., Brian Lawton, and Zoë Swenson. 2017. The Prevalence of Fatal Police Shootings by U.S. Police, 2015–2016: Patterns and Answers from a New Data Set. *Journal of Criminal Justice* 52:101-11.

Shjarback, John A. 2015. Emerging Early Intervention Systems: An Agency-Specific Pre-Post Comparison of Formal Citizen Complaints of Use of Force. *Policing: A Journal of Policy and Practice* 9:314-25.

Stickle, Ben. 2016. A National Examination of the Effect of Education, Training and Pre-Employment Screening on Law Enforcement Use of Force. *Justice Policy Journal* 13:1-15.

Trejo, Stephen J. 1991. Public Sector Unions and Municipal Employment. *Industrial and Labor Relations Review* 45:166-80.

U.S. Department of Justice, Civil Rights Division, and U.S. Attorney's Office, Northern District of Illinois. 2017. Investigation of the Chicago Police Department. https://www.justice.gov/opa/file/925846/download.

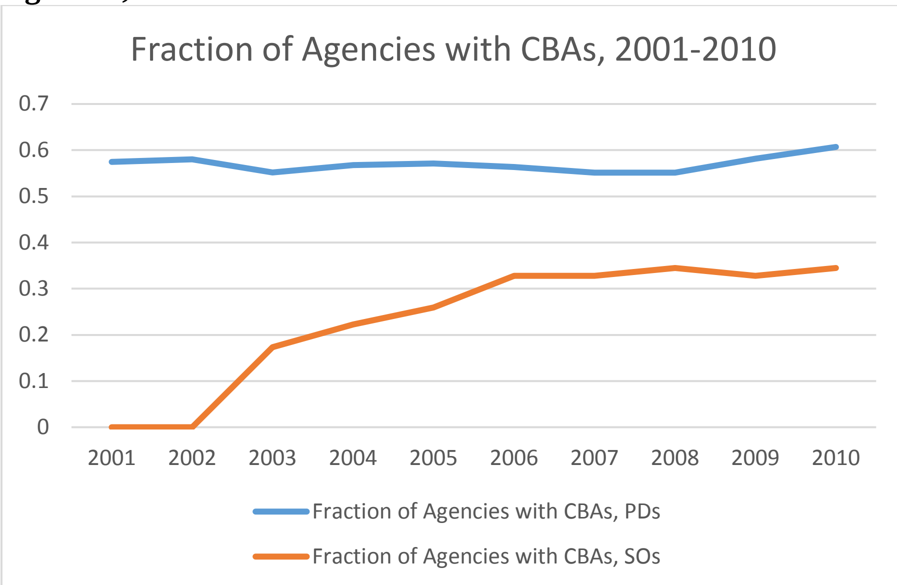
Walker, Samuel. 2008. The Neglect of Police Unions. *Police Practice and Research* 9:95-112.

Wilson, Steve, and Kevin Buckler. 2010. The Debate over Police Reform: Examining Minority Support for Citizen Oversight and Resistance by Police Unions. *American Journal of Criminal Justice* 35:184-97.

Wilson, Steve, Jihong Zhao, Ling Ren, and Steven Briggs. 2006. The Influence of Collective Bargaining on Large Police Agency Salaries: 1990-2000. *American Journal of Criminal Justice* 31:19-34.

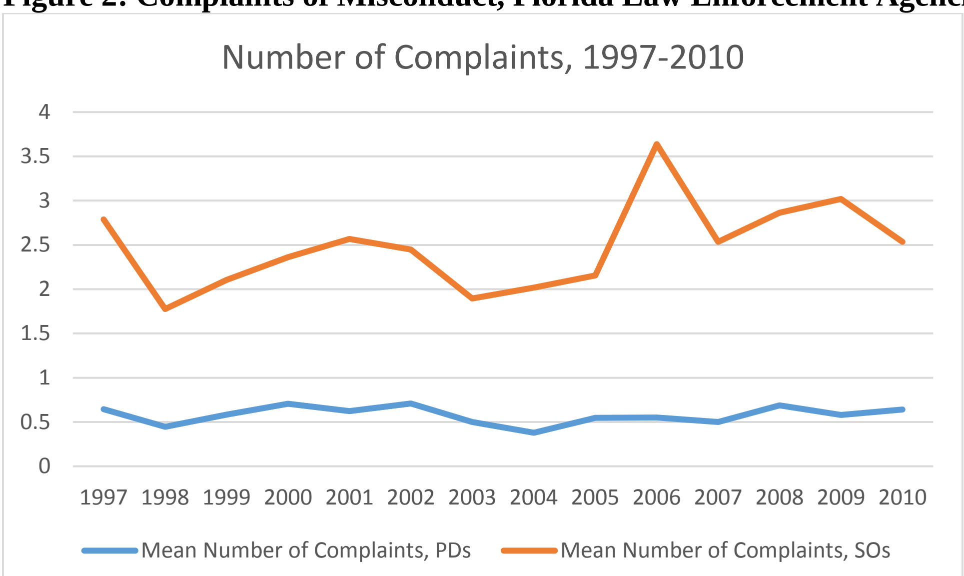
Zhao, Jihong, and Nicholas Lovrich. 1997. Collective Bargaining and the Police: The Consequences for Supplemental Compensation Policies in Large Agencies. *Policing: An International Journal of Police Strategies & Management* 20:508-18.

Figure 1: Collective Bargaining Agreements (CBAs) Among Florida Law Enforcement Agencies, 2001-2010



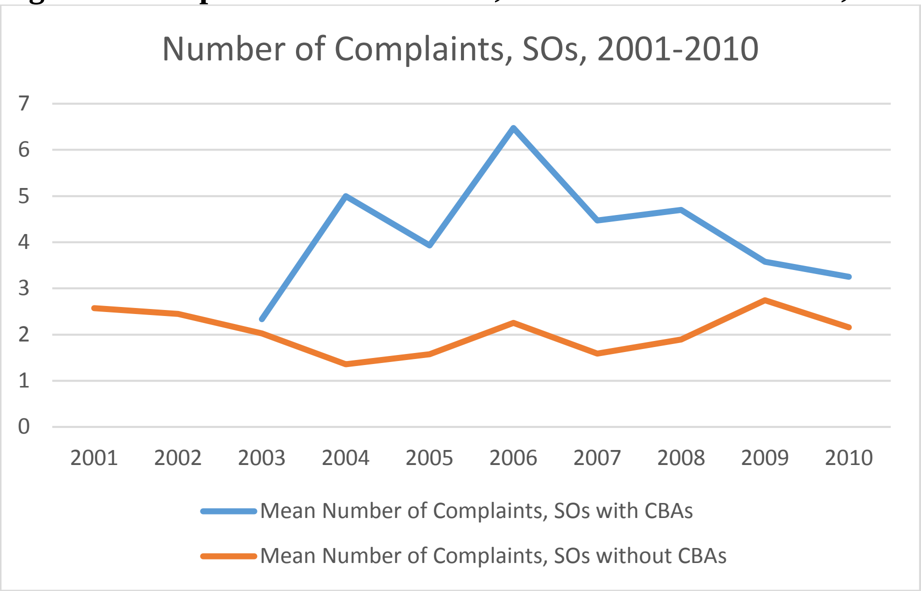
Note: This graph depicts the fraction of Florida law enforcement agencies for which the Criminal Justice Agency Profile (CJAP) data reports a collective bargaining agreement (CBA) being in place. This fraction is reported separately for the treatment group of Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) and the control group of Police Departments (PDs).

Figure 2: Complaints of Misconduct, Florida Law Enforcement Agencies, 1997-2010



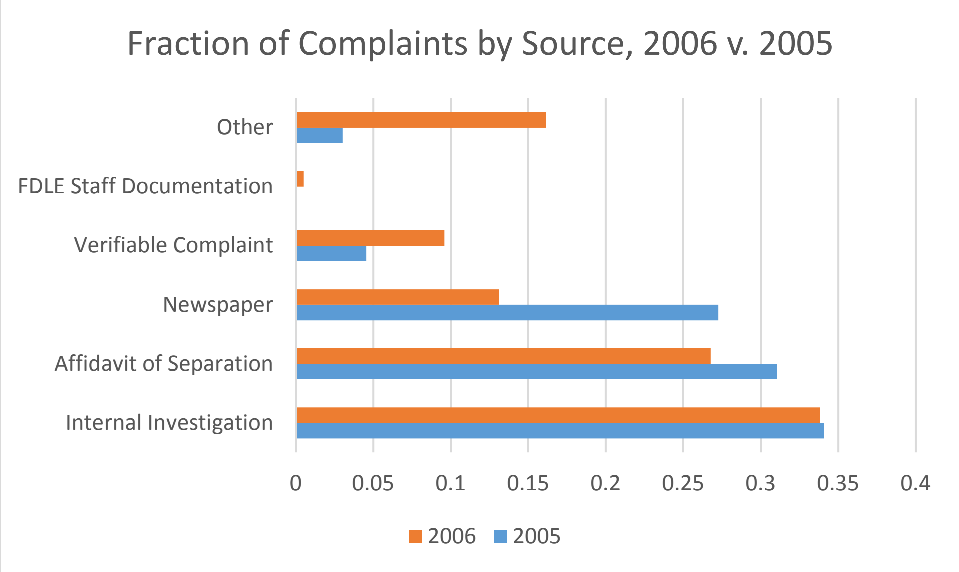
Note: This graph depicts the number of complaints in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database that can be matched to Florida law enforcement agencies. The mean number of complaints in each year is reported separately for the treatment group of Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) and the control group of Police Departments (PDs).

Figure 3: Complaints of Misconduct, Florida Sheriffs' Offices, 2001-2010



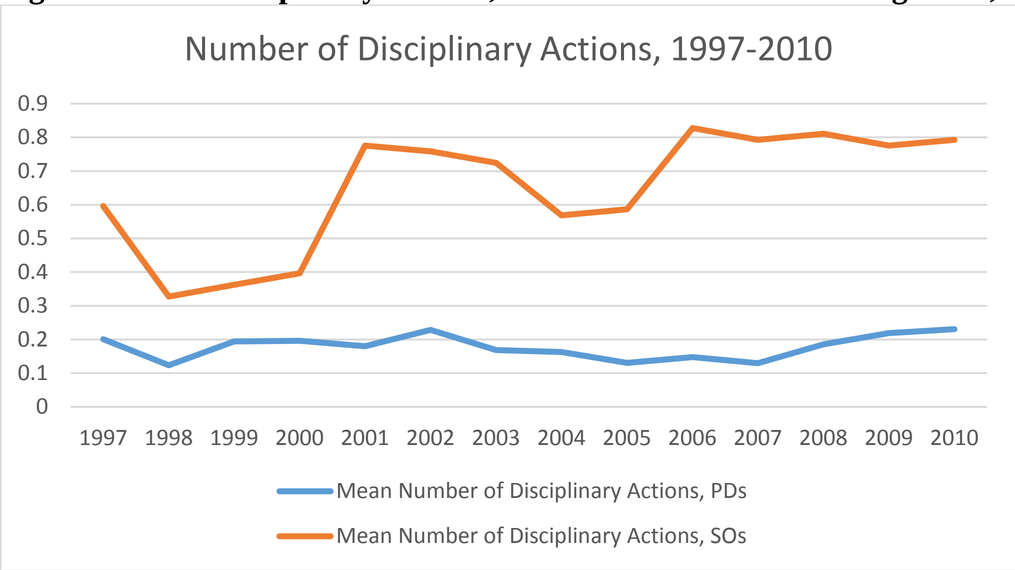
Note: This graph depicts the number of complaints in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database that can be matched to Florida Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003). The mean number of complaints in each year is reported separately for SOs for which the Criminal Justice Agency Profile (CJAP) data reports a collective bargaining agreement being in place and for SOs for which the CJAP data reports no collective bargaining agreement being in place.

Figure 4: Changes in the Sources of Complaints, Florida Sheriffs' Offices, 2005 to 2006



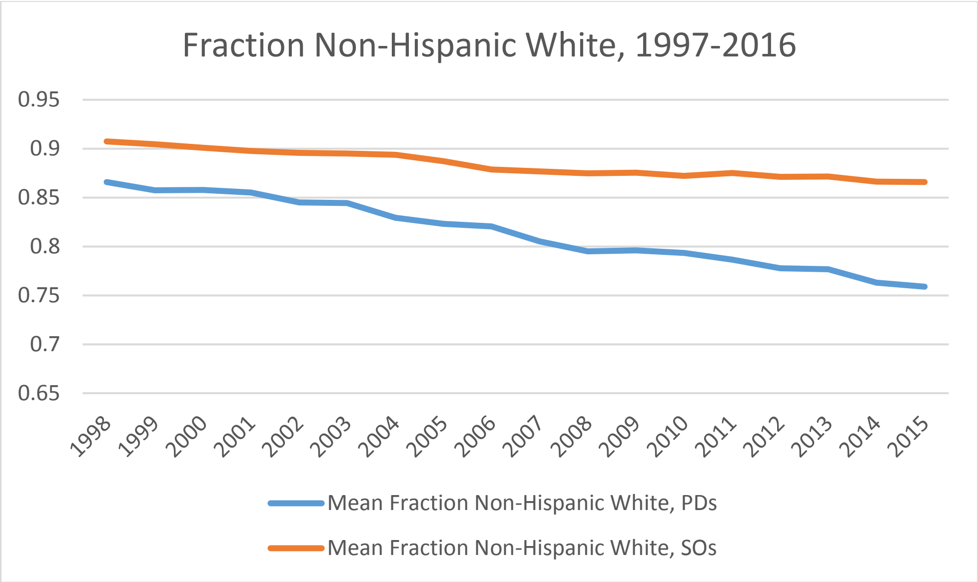
Note: This chart compares the fraction of complaints in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database that are recorded as originating from each type of reported source, for Florida Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) in 2005 and 2006. "Internal Investigation" and "Affidavit of Separation" both refer to complaints that originate with and were sustained by the local agency (with the officer's employment being terminated in the latter category but not in the former). "Verifiable Complaints" include complaints from members of the public; "Newspaper" includes incidents brought to the attention of the FDLE through media reports.

Figure 5: State Disciplinary Actions, Florida Law Enforcement Agencies, 1997-2010



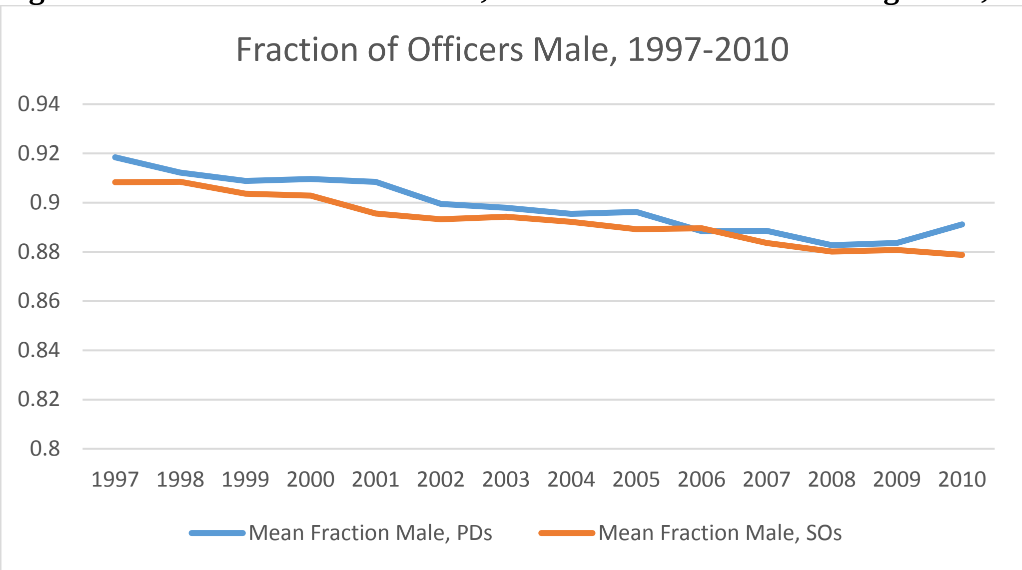
Note: This graph depicts the number of disciplinary actions in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database that can be matched to Florida law enforcement agencies. The mean number of disciplinary actions in each year is reported separately for the treatment group of Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) and the control group of Police Departments (PDs).

Figure 6: Fraction of Non-Hispanic White Officers, Florida Law Enforcement Agencies, 1997-2010



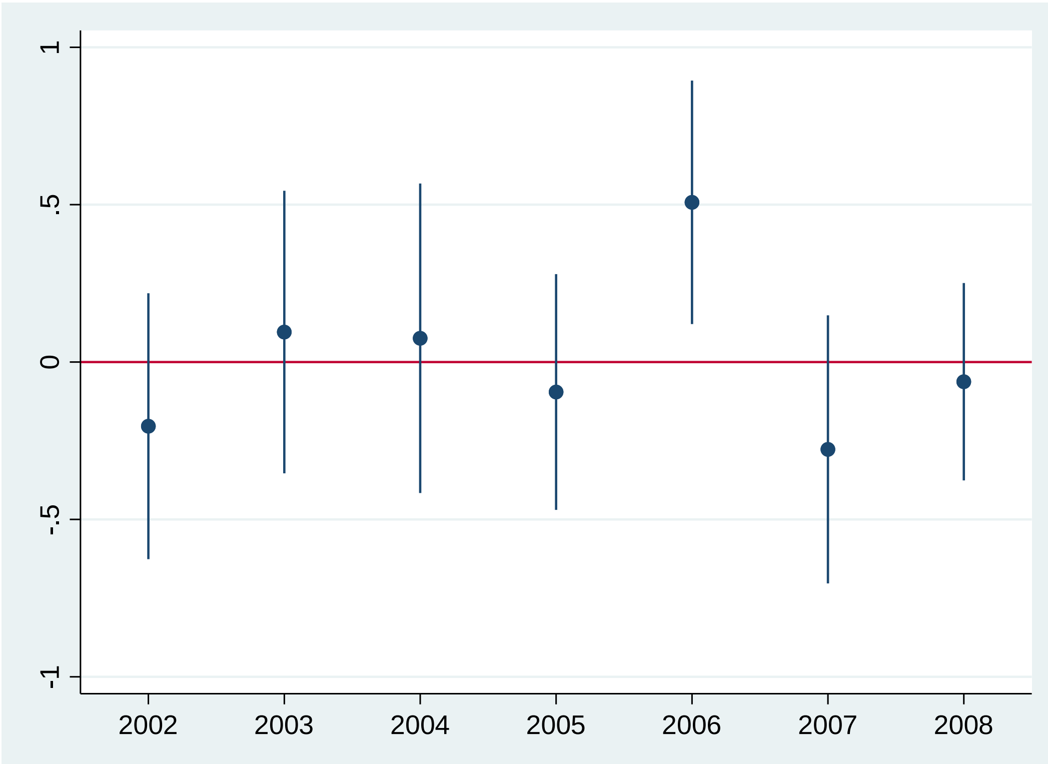
Note: This graph depicts the fraction of officers in Florida law enforcement agencies who are recorded in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database as being non-Hispanic White by race/ethnicity. The mean fraction of non-Hispanic White officers in each year is reported separately for the treatment group of Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) and the control group of Police Departments (PDs).

Figure 7: Fraction of Male Officers, Florida Law Enforcement Agencies, 1997-2010



Note: This graph depicts the fraction of officers in Florida law enforcement agencies who are recorded in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database as being male. The mean fraction of male officers in each year is reported separately for the treatment group of Sheriffs' Offices (SOs; excluding the 9 SOs that obtained collective bargaining rights prior to 2003) and the control group of Police Departments (PDs).

Figure 8: Leads and Lags of the Difference-in-Difference Estimate of the Impact of *Williams* on Complaints, 2002-2008



Note: This graph depicts the coefficient estimate and confidence intervals for the leads and lags (reported in Column 3 of Table 2) of the difference-in-difference estimate of the impact of *Williams* on complaints. The horizontal line represents a coefficient of zero. The vertical bars represent 95% confidence intervals.

Table 1: Summary Statistics

Panel A: Sheriffs' Offices (SOs)

Variable	**1997-2002**			**2003-2010**		
	Number	Mean	Standard Deviation	Number	Mean	Standard Deviation
CBA = 1	116	0	0	450	.2933333	.4557966
Complaints	347	2.340058	3.411081	464	2.581897	3.711778
Disciplinary Actions	347	.5360231	1.020455	464	.7349138	1.203302
Non-Hispanic White Fraction (Officers)	341	.9035744	.0671843	456	.8848981	.0870508
Male Fraction (Officers)	341	.901986	.0502473	456	.8860785	.0465631
FTO Training Length (weeks)	322	8.770186	6.517887	379	11.68338	5.303489
Some College Required = 1	332	.0271084	.1626445	423	.0425532	.2020865
Shotgun and/or Rifle = 1	174	.4425287	.4981195	464	.875	.3310759
Number of Officers	341	211.4545	282.3964	456	258.9956	338.504
Resident Population	347	176.5673	253.5601	464	204.4404	287.6289
Resident Population Aged 18-24	347	.060376	.0235786	464	.0666806	.0275992
Hispanic Fraction (Resident Pop)	347	.0761975	.0803359	464	.1050395	.099281
African American Fraction (Resident Pop)	347	.1421161	0.100818	464	.1445069	.0952992
Unemployment Rate (%)	347	4.920461	1.896502	464	5.936207	2.959325
Murders	347	7.063401	13.12265	464	7.512931	15.66385
Property Crimes (Thousands)	347	7.038153	13.26521	464	3.775573	6.116371
Violent Crimes (Thousands)	347	1.574179	2.636632	464	1.87175	2.668373

Note: This table reports summary statistics for the 58 SOs in the treatment group, separately for the pre-*Williams* period (1997-2002) and the post-*Williams* period. Florida has 67 SOs (one per county), but we exclude the 9 SOs that obtained collective bargaining rights through county-specific legislation prior to 2003. The CBA indicator = 1 if the agency is recorded as having a collective bargaining agreement (CBA) in the CJAP dataset. "Complaints" is the number of complaints (against law enforcement and concurrent officers) reported in the ATMS database. "Disciplinary actions" is the number of state disciplinary actions (against law enforcement and concurrent officers) reported in the ATMS database. The Non-Hispanic White fraction is calculated by dividing the number of law enforcement and concurrent officers reporting their race/ethnicity as Non-Hispanic White by the total number of law enforcement and concurrent officers employed at the agency whose race/ethnicity is reported in the ATMS database. The male fraction is calculated by dividing the number of male law enforcement and concurrent officers by the total number of law enforcement and concurrent officers employed at the agency, as reported in the ATMS database. FTO

(Field Training Officer) training length is from the CJAP dataset. The "Some College" indicator = 1 if the CJAP dataset reports that the agency requires officers to have an Associate's degree, some college education, or a Bachelor's degree. The "Shotgun and/or Rifle" indicator = 1 if the CJAP dataset reports that the agency issues shotguns, rifles, or both types of guns to all of its sworn officers (with the indicator = 0 otherwise). The number of officers (restricted to those with law enforcement or concurrent certification) is from the ATMS database. The resident population, the fraction of the resident population aged 18-24, and the Hispanic and African American fractions of the resident population are from Census Bureau estimates, and are at the county-year level. The unemployment rate is from the Bureau of Labor Statistics, and is at the county-year level. The crime rate variables are from the UCR dataset, and are at the agency-year level (i.e., pertain to the area under the jurisdiction of a given agency).

Panel B: Police Departments (PDs)

Variable	**1997-2002**			**2003-2010**		
	Number	Mean	Standard Deviation	Number	Mean	Standard Deviation
CBA = 1	540	.5685185	.4957422	1,953	.6026626	.4894722
Complaints	1,662	.6197353	1.282418	2,459	.5416836	1.079146
Disciplinary Actions	1,662	.1877256	.521327	2,459	.1708011	.4908701
Non-Hispanic White Fraction (Officers)	1,629	.8598418	.1781176	2,276	.815559	.2014977
Male Fraction (Officers)	1,629	.9094056	.0703634	2,283	.8905504	.0860073
FTO Training Length (weeks)	1,539	10.91553	5.540196	2,122	12.44392	5.458939
Some College Required = 1	1,590	.0962264	.2949943	1,990	.0994975	.299404
Shotgun and/or Rifle = 1	808	.4034653	.4908964	2,461	.7354734	.4411705
Number of Officers	1,632	50.10355	79.36338	2,388	51.3995	82.918
Resident Population	1,663	684.0151	697.9257	2,452	755.0828	756.6273
Resident Population Aged 18-24	1,663	.0583461	.0224166	2,452	.065785	.0291381
Hispanic Fraction (Resident Pop)	1,663	.1371811	.1608696	2,452	.1712227	.1718399
African American Fraction (Resident Pop)	1,663	.1505228	.0906936	2,452	.1572087	.0876413
Unemployment Rate (%)	1,663	4.717739	1.464289	2,452	5.955383	2.897567
Murders	1,663	32.06254	61.87605	2,452	21.91232	51.56144
Property Crimes (Thousands)	1,663	28.3086	46.63505	2,456	1.297978	4.799675
Violent Crimes (Thousands)	1,663	4.656373	7.949685	2,456	.4734279	1.256981

Note: This table reports summary statistics for the control group (PDs), separately for the pre-*Williams* period (1997-2002) and the post-*Williams* period. There are up to 308 PDs with the required data (although the number is smaller for some variables). The variables are defined as described in the notes to Panel A.

Table 2: Complaints and Disciplinary Actions – Poisson Specification, 1997-2010

	(1) Full Sample	(2) Full Sample	(3) Full Sample	(4) Complaints > 0	(5) Complaints > 0
	Dependent Variable: Number of Complaints				Dependent Variable: Number of Disciplinary Actions
$(Post*SO)_{t+1}$			-0.20385		
			(0.216)		
$(Post*SO)_{t}$			0.09530		
			(0.229)		
$(Post*SO)_{t-1}$			0.07553		
			(0.251)		
$(Post*SO)_{t-2}$			-0.09519		
			(0.191)		
$(Post*SO)_{t-3}$	**0.24670****	**0.23912****	**0.50745****	**0.28010*****	**0.28613***
	(0.109)	**(0.109)**	**(0.197)**	**(0.098)**	**(0.161)**
$(Post*SO)_{t-4}$			-0.27743		
			(0.217)		
$(Post*SO)_{t-5}$			-0.06248		
			(0.160)		
Number of Officers	0.00007	-0.00011	0.00082	-0.00012	-0.00007
	(0.000)	(0.000)	(0.001)	(0.000)	(0.001)
Resident Population		0.00078	0.00109	0.00056	-0.00088
		(0.001)	(0.002)	(0.001)	(0.002)
Resident Population		5.88306	6.55117	6.58619	7.52019
Aged 18-24		(4.849)	(5.617)	(5.180)	(9.772)
Hispanic Fraction		0.42729	0.67169	0.82275	-0.97837
(Resident Population)		(2.808)	(3.422)	(2.449)	(3.990)
African American		-1.05479	-0.41444	1.06359	8.56352
Fraction (Resident Pop)		(3.281)	(3.990)	(2.742)	(5.897)
Unemployment Rate		-0.03002	-0.11925**	-0.06424*	-0.01153
(%)		(0.038)	(0.051)	(0.034)	(0.058)
Murders		-0.00092	-0.00070	-0.00044	-0.00075
		(0.001)	(0.001)	(0.001)	(0.001)
Property Crimes		0.00013	0.01051	-0.00346	-0.02090***
		(0.004)	(0.025)	(0.004)	(0.007)
Violent Crimes		0.01210	0.08580	0.02469	0.11844***
		(0.021)	(0.060)	(0.022)	(0.035)
Agency and Year Fixed	Yes	Yes	Yes	Yes	Yes
Effects?					
Observations	3,483	3,478	2,668	1,480	1,349
Number of Agencies	314	313	289	263	220

Note: This table reports regression results for the number of complaints at the agency-year level. The primary variable of interest is the third lag of the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for SOs; this is the variable $(Post*SO)_{t-3}$ (which denotes the third lag). Other leads and lags are also reported in Column 3. The last two columns use only agency-years where the number of complaints is strictly positive. All other variables are as defined in Table 1. Robust standard errors clustered at the agency level are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 3: Complaints – Linear Specification, 1997-2010

	(1)	(2)	(3)	(4)
	Dependent Variable: Number of Complaints			
$(Post*SO)_{t-3}$	**0.55601*** **	**0.59640*** **	**1.18048*** **	**1.11086** **
	(0.207)	**(0.216)**	**(0.394)**	**(0.448)**
Number of Officers	0.00432	0.00384	-0.00659	-0.01231
	(0.003)	(0.003)	(0.008)	(0.009)
Resident Population		0.00152	-0.00364	
		(0.001)	(0.003)	
Resident Population		7.47165	3.95286	
Aged 18-24		(7.373)	(8.402)	
Hispanic Fraction		0.35120	2.12875	
(Resident Population)		(3.286)	(5.892)	
African American		-0.44691	-3.32917	
Fraction (Resident Pop)		(3.000)	(4.549)	
Unemployment Rate		-0.01546	-0.04569	
(%)		(0.030)	(0.044)	
Murders		-0.00064	-0.00006	-0.06649**
		(0.001)	(0.001)	(0.032)
Property Crimes		0.00321	-0.00347	-0.10797
		(0.013)	(0.012)	(0.129)
Violent Crimes		-0.00633	0.02236	0.36688
		(0.080)	(0.073)	(0.266)
Agency and Year Fixed Effects?	Yes	Yes	Yes	Yes
Linear Agency-Specific Trends?	No	No	Yes	Yes
County-by-Year Fixed Effects?	No	No	No	Yes
Observations	3,896	3,894	3,894	3,894
R-squared (within)	0.024	0.026	0.153	0.337
Number of Agencies	366	366	366	366

Note: This table reports regression results for the number of complaints at the agency-year level. The primary variable of interest is the third lag of the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for SOs; this is the variable $(Post*SO)_{t-3}$ (which denotes the third lag). All other variables are as defined in Table 1. Robust standard errors clustered at the agency level are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Table 4: Additional Outcomes, 1997-2010

	(1) Linear	(2) Linear	(3) Linear	(4) Logit
	Dependent Variable: Fraction of Non-Hispanic White Officers		Dependent Variable: Length of FTO Training	Dependent Variable = 1 if Some College Required
$(Post*SO)_{t-3}$	**0.02661*** **	**0.01019**	**0.86557***	**1.86039** **
	(0.008)	**(0.009)**	**(0.511)**	**(0.730)**
Number of Officers	-0.00014***	-0.00004	0.00288	-0.01053**
	(0.000)	(0.000)	(0.006)	(0.005)
Resident Population		-0.00007	-0.01174***	-0.00149
		(0.000)	(0.004)	(0.007)
Resident Population		-0.41796	-3.17682	3.08307
Aged 18-24		(0.274)	(13.519)	(22.910)
Hispanic Fraction		-1.00029***	11.42075	10.52319
(Resident Population)		(0.290)	(10.357)	(25.049)
African American		0.14045	15.43047	55.07970**
Fraction (Resident Pop)		(0.292)	(11.726)	(22.065)
Unemployment Rate		0.00542**	-0.08655	-0.20051
(%)		(0.003)	(0.136)	(0.207)
Murders		-0.00007*	0.00387	-0.00631
		(0.000)	(0.002)	(0.006)
Property Crimes		-0.00037	0.00884	0.01049
		(0.000)	(0.045)	(0.030)
Violent Crimes		0.00396*	-0.05631	-0.04836
		(0.002)	(0.278)	(0.153)
Agency and Year Fixed Effects?	Yes	Yes	Yes	Yes
Observations	3,847	3,845	3,477	508
R-squared (within)	0.123	0.166	0.051	
Number of Agencies	364	364	359	45

Note: This table reports regression results for the fraction of officers who are non-Hispanic whites (Columns 1-2) at the agency-year level. Column 3 reports regression results for the Field Training Officer (FTO) training period. Column 4 reports a conditional fixed-effects logit specification for an indicator capturing minimum educational requirements (specifically, a dummy for whether education beyond a high school diploma is required). The primary variable of interest is the third lag of the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for SOs; this is the variable $(Post*SO)_{t-3}$ (which denotes the third lag). All other variables are as defined in Table 1. Robust standard errors clustered at the agency level are in parentheses; *** p<0.01, ** p<0.05, * p<0.1