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Dharmapala, Dhammika; McAdams, Richard H.; Rappaport, John

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Collective Bargaining and Police Misconduct: Evidence from Florida

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Dhammika Dharmapala, Richard H. McAdams, John Rappaport

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Collective Bargaining and Policy Misconduct: Evidence from Florida

Abstract

Growing controversy surrounds the impact of labor unions on law enforcement behavior. Critics argue that unions impede organizational reform and insulate officers from discipline for misconduct. Yet collective bargaining tends to increase wages, which could improve officer behavior. We provide quasi-experimental empirical evidence on the effects of collective bargaining rights on violent incidents of misconduct. Our empirical strategy exploits a 2003 Florida Supreme Court decision (*Williams*), which conferred collective bargaining rights on sheriffs' deputies, resulting in a substantial increase in unionization among these officers. Using a Florida state administrative database of "moral character" violations reported by local agencies between 1996 and 2015, we implement a difference-in-difference approach in which police departments (which were unaffected by *Williams*) serve as a control group for sheriffs' offices (SOs). Our estimates imply that collective bargaining rights led to a substantial increase in violent incidents of misconduct among SOs, relative to police departments. The effect of collective bargaining rights is concentrated among SOs that subsequently adopted collective bargaining agreements, and the timing of the adoption of these agreements is associated with increases in violent misconduct. There is also some evidence consistent with a "bargaining in the shadow" effect among SOs that did not unionize.

JEL-Codes: J450, J500, K420.

Keywords: collective bargaining rights, police unions, police misconduct, law enforcement.

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

Richard H. McAdams
University of Chicago Law School / USA
mcadams@uchicago.edu

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

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1) Introduction

Police unions sometimes successfully resist the imposition of discipline on officers for misconduct. Huq and McAdams (2016), Keenan and Walker (2005), and Rushin (2017) show that many law enforcement collective bargaining agreements (CBAs) create procedural rights for officers that make it difficult for agencies to investigate and discipline misconduct, including the excessive use of force.[1] These scholars express concern that such contractual provisions undermine the ability of management to deter misconduct and thus may promote its commission. Unions may also successfully lobby for state and local legislation that provides the same kind of procedural protections against investigation and discipline, or lobby and litigate against reform efforts. At the same time, unionization might reduce misconduct by producing a sense of empowerment and increased job satisfaction.[2] Collective bargaining tends to improve wages and benefits;[3] Becker and Stigler (1974) argue that higher compensation can deter malfeasance among law enforcers by raising the opportunity cost of misconduct. Likewise, the theory of efficiency wages holds that paying wages above the market-clearing equilibrium may improve productivity, which, in the context of police, could entail decreased misconduct.[4] Thus, the impact of collective bargaining on law enforcement misconduct is ultimately an empirical question.

Numerous recent studies examine the issue of law enforcement violence (Fryer 2018; Legewie and Fagan 2016; Shjarback 2015; Shane, Lawton, and Swenson 2017; Stickle 2016), some focusing on the role of collective bargaining (Huq and McAdams 2016; Rushin 2017). Most pertinent to our study, many scholars, drawing upon case studies, argue that unions impede progressive policy reform and innovation (Bies 2017; Epp 2009; Fisk and Richardson 2016;

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[1] For instance, using Chicago data, Iris (1998) finds that disciplinary orders are frequently overturned during arbitral review.

[2] Unionization may also foster collective solidarity among police and interact with the intrinsic motivation of those who self-select into policing. Dharmapala, Garoupa, and McAdams (2016) develop a theoretical model of self-selection and intrinsic motivation among law enforcement agents but do not address the impact of unionization.

[3] Indeed, 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).

[4] 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, measured primarily by arrest rates, declines when unions lose in wage arbitration. Mas (2006) does not analyze police misconduct, however. Chandrasekher (2017) examines police misconduct using data from the unionized New York Police Department. She focuses not on the impact of unionization *per se*, but on the impact of lengthy negotiations that result in the expiration of union contracts (after which officers are "out of contract," with the terms of the expired contract continuing to apply in the interim). She finds evidence that incidents of misconduct increase with time spent out of contract.

McCormick 2015; Walker 2008).[5] No previous work, however, has offered empirical evidence of the causal role that collective bargaining rights play in the behavior of law enforcement.

This paper offers such evidence by exploiting a January 2003 change in Florida labor law. By a judicial decision that month (*Williams*), county sheriffs' deputies won the right to organize for collective bargaining. *Williams* led to substantial unionization among sheriffs' offices (SOs). Officers at municipal police departments (PDs), in contrast, had the right to bargain collectively both before and after that date. It is important to note that *Williams* is a source of exogenous variation in collective bargaining *rights*, rather than in unionization *per se*, as SOs' post-*Williams* decisions to adopt CBAs are potentially endogenous with respect to factors that may affect misconduct. Thus, *Williams* represents a "treatment" that involves collective bargaining *rights*, regardless whether those rights are exercised. This interpretation highlights the possibility of officers at agencies without CBAs bargaining "in the shadow of" collective bargaining rights.[6]

We examine how *Williams* affected incidents of misconduct by law enforcement personnel at these two types of agencies. The empirical strategy involves the use of a difference-in-difference framework, in which the treatment group consists of SOs, which were affected by *Williams*, and the control group consists of PDs, which were unaffected. As discussed more fully in Section 4, officers at agencies in the treatment and control groups perform similar job functions and are drawn from similar pools of applicants. The treatment and control groups also experienced similar trends in misconduct prior to *Williams*.

Our analysis uses a comprehensive administrative dataset on Florida law enforcement agencies – covering both SOs and PDs – over the period 1996-2015. Our 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 incidents of misconduct and

[5] 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 particular reforms (Morabito 2014).

[6] The empirical relationship between unionization and higher wages 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). This past literature on the effects of labor laws on nonunionized workers motivates our focus on collective bargaining *rights*, rather than on CBAs, though we test the impact of CBAs in supplementary analyses. An alternative interpretation of our empirical design regards CBAs as the treatment of interest and *Williams* as analogous to an "intent-to-treat" intervention.

disciplinary actions against officers, known as the Automated Training Management System (ATMS). The ATMS database records allegations of officer misconduct, most of which have been sustained by local agencies. These allegations typically begin as civilian or internal affairs complaints investigated by a local agency. If the local agency sustains the allegation (using a "preponderance of the evidence" standard) and the offense violates a "good moral character" requirement, the agency is required to report its findings to the FDLE, which opens its own "complaint" file and begins an independent disciplinary process. These state-level investigations form the basis for the misconduct data in the ATMS database. "Moral character" violations are defined by regulation to include the commission of any Florida felony or any of a substantial list of Florida misdemeanors, whether prosecuted or not, or excessive force or misuse of official position as defined by state statute (F.A.C. Rule 11B-27.0011). Within the universe of moral character violations, we focus on the subset involving express or implied violence.

Because the typical complaint in our dataset has been sustained at least once, we refer to this set of moral character violations as "violent incidents" rather than "complaints" or "allegations."[7] The processes generating the ATMS data are quite complex, however, as detailed in Section 3. For instance, a minority of complaints in the ATMS data originate from media sources, civilian allegations made directly to the FDLE, and other (unspecified) sources, rather than from allegations sustained by local agencies. In some instances, too, the FDLE does not sustain a complaint, possibly due to the higher "clear and convincing" evidence standard it applies. To account for these cases, we construct alternative measures of violent incidents, excluding potentially unverified complaints, which generate results very similar to the baseline results discussed below.

We employ a Poisson maximum-likelihood model for count data and control for agency and year fixed effects and an extensive set of local and agency characteristics. We find that violent incidents rose substantially among the SOs treated by *Williams* (relative to the control group of PDs) in the years after *Williams*. Our estimates imply that the right to bargain collectively led to about a 40% increase in violent incidents at SOs. While this effect may seem strikingly large, the baseline rate of violent incidents is low. The estimated effect implies an increase of 0.2 violent

[7] There is a literature that studies civilian complaints *per se*, as opposed to the incidents predominantly involving *sustained* complaints that we study. Rozema and Schanzenbach (2019) find a strong relationship between civilian complaints against police officers and misconduct as proxied by litigation, using data from the Chicago Police Department.

incidents per agency-year, relative to a pre-*Williams* mean among SOs of about 0.5. At a typical SO with 210 officers, this effect corresponds to *one* officer being involved in *one* additional violent incident every five years. So described, the estimated effect is not implausibly large, though it points nonetheless to a substantial divergence between SOs and PDs following *Williams*.

While our baseline estimate uses the entire 1996-2015 sample period, the result is robust to using shorter windows around 2003, such as 1999-2006. The basic effect also appears to persist over time – it is detectable (and similar in magnitude) in the 2011-2016 period as well as in the 2003-2010 period. The result does not seem to be attributable to differential prior trends in violent incidents at SOs and PDs: Plotting the mean residuals of our violent incidents measure (from a regression that controls for agency and year fixed effects and the number of officers) reveals closely parallel trends for SOs and PDs prior to *Williams*. The result is also robust to allowing for differential linear trends among SOs and PDs, and adding leads of the variable of interest does not reveal any differential effects for SOs and PDs in years prior to *Williams*.

The estimated effect appears to be more concentrated among SOs that adopted CBAs following *Williams*. The effect for these agencies is large and statistically significant at the 5% level. For SOs that did not adopt CBAs following *Williams*, the point estimate is smaller in magnitude and of only borderline statistical significance. Taken together, these results provide some reassurance that the mechanism driving the baseline result involves collective bargaining, as opposed to some extraneous factor that differentially affected SOs after 2003. Yet they also suggest the possibility that SOs that did not adopt CBAs nevertheless bargain in the shadow of their newfound collective bargaining rights.

This latter point, in turn, casts doubt on a potential alternative explanation for our finding: that unionization may increase "bureaucratization." This explanation would posit that management in a unionized agency is more likely to formalize complaints, and so the increase in violent incidents we detect may actually reflect changes in reporting behavior rather than officer behavior on the street. Yet this formalization effect is unlikely to apply within SOs that did not unionize. That these agencies also experience a positive (albeit weaker) effect on violent misconduct therefore undermines the bureaucratization explanation.

Unfortunately, it is difficult to distinguish statistically between this "shadow" effect and the effect of CBAs. This may be because virtually all CBAs among SOs were adopted in the immediate aftermath of *Williams* (during 2003-2006) and so were closely contemporaneous with

Williams' impact on collective bargaining rights. Controlling for the primary *Williams* effect, the point estimate suggests that the adoption of CBAs among SOs is associated with further increases in violent incidents; however, the latter effect is not statistically significant. It is noteworthy, though, that a simple regression of violent incidents on the adoption of CBAs by SOs yields a substantial positive and statistically significant association. This underscores that the basic result seems attributable to unions and collective bargaining, even though the precise delineation of the relative magnitudes of the "shadow" and CBA effects is elusive in our data.

To the best of our knowledge, this paper presents the first quasi-experimental evidence on the impact of collective bargaining rights on police misconduct. In a paper that is contemporaneous with ours, however, Goncalves (2019) uses a similar dataset from Florida (along with a national database of fatal incidents) to analyze the impact of unionization on police misconduct. His empirical strategy involves comparing Florida agencies in which unionization elections are successful to those with unsuccessful elections, and does not exploit the variation in collective bargaining rights created by *Williams*. Using this approach, Goncalves (2019) finds statistically insignificant and relatively small effects of unionization on misconduct. In Section 4 below, we discuss the relationship between this paper and Goncalves (2019) in detail and seek to reconcile the contrasting findings. While Goncalves (2019) has a different research question and empirical strategy, there are clearly some overlapping elements. We conclude that our results are fairly consistent with Goncalves' (2019) where they overlap (as in the CBA analysis described above), but that the *Williams* quasi-experiment provides a valuable source of variation for understanding the impact of collective bargaining rights.

Our paper proceeds as follows. Section 2 describes the relevant legal developments relating to collective bargaining rights under Florida law. Section 3 details our data sources. Section 4 presents our empirical strategy and results, along with a variety of robustness checks. Section 5 discusses these results, and Section 6 concludes.

2) 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). The U.S. Supreme Court's decision in *Janus v. AFSCME* (138 S.Ct. 2448 [2018]), issued after our study period ended,

essentially imposed a right-to-work rule on public sector unions nationwide. Nevertheless, during the pertinent period, unions were 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[d] relatively lower membership rates with subsequent lower resources and funds as a result of . . . right-to-work legislation."

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. § 112.532(1)(d)). This might be thought 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.). 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.

This legal background ought to dull the effects of collective bargaining 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. We now turn to the specific change in Florida labor law that is the basis for our quasi-experiment.

2.1) 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 exploit a 2003 change in Florida's public sector labor law. Before 2003, with a few exceptions detailed below, sheriff deputies in Florida, who are 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 collective bargaining rights because deputies were "appointees" rather than "employees" of the sheriff. They were 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.[8] The court held that Article I, Section 6 of the Florida Constitution granted deputies this right, invalidating any contrary statute.

Sheriffs' deputies immediately 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 (SOs) had a collective bargaining agreement (CBA). These offices represented 15,581 sworn personnel or 76% of sheriff deputies in Florida. We document a similar pattern using our dataset, as discussed in Section 3 below.

The significance of *Williams* for our research question stems from the fact that, by contrast to sheriff deputies, Florida *police officers*, who are 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 both before and after 2003. As we document in Section 3 below, slightly over half of Florida police departments (PDs) had CBAs around 2003, representing about two-thirds of the police officers in our principal dataset, 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 (regardless of whether they had chosen to unionize) did not. In this sense, PDs can serve as a control group in a quasi-experimental setting in which SOs, whose deputies were awarded collective bargaining rights by the *Williams* decision, are the treatment group. We elaborate on this empirical design in Section 4 below.

2.2) 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

[8] 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*.

in Florida, there was disagreement over the identity of the pertinent "legislative body." Deputy unions claimed it was the county commission. But sheriffs argued *they* were the legislative bodies, 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 (*White v. Florida State Lodge* (60 So.3d 236 [Fla. 2011])).

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. Additional tests, however, detect no statistically significant effect of the resolution of legal uncertainty in 2011 on the number of violent incidents at SOs relative to PDs. The coefficient is close to zero, though somewhat imprecisely estimated.

3) Data

3.1) The ATMS Database

Our dataset combines information from various sources. A particularly crucial data source for our analysis is the Automated Training Management System (ATMS) maintained by the FDLE. The FDLE is a state-level agency that, among other things, collects data on the activities of local law enforcement agencies and imposes discipline on officers in certain circumstances. The ATMS database contains extensive information on Florida law enforcement officers. Most important for our purposes, it records incidents of alleged officer misconduct. These allegations typically begin as civilian or internal affairs 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 (on or off the job) a felony or a misdemeanor involving dishonesty, or is not of "good

moral character" (in ways enumerated by regulation), the agency must investigate. If the agency sustains the allegation, it must submit its findings to the FDLE, which opens a "complaint" and begins a disciplinary process (F.S.A. § 943.13(4), (7); F.A.C. Rule 11B-27.0011).

Complaints in the ATMS database are classified by source. The complaint-source categories "Internal Affairs" and "Affidavit of Separation" both comprise complaints that, as just described, were sustained by the local agency; the latter category is used when the local agency has terminated the officer's employment. These two categories, taken together, supply the majority share of ATMS complaints. The FDLE also has information channels independent of the local agencies, however. "Verifiable Complaints" include signed complaints from members of the public; "Newspaper" includes incidents brought to the FDLE's attention by media reports; "Arrest Hit" captures incidents for which an officer was arrested and booked, alerting the FDLE; "FDLE" covers incidents revealed during FDLE staff audits of local agency documents; and "Other" captures any incidents not marked with one of the preceding codes.

Figure 1 presents a flowchart illustrating, in slightly simplified terms, the process by which the FDLE handles the complaints it receives. Regardless of a complaint's source, FDLE staff first screen out complaints that do not, on their face, allege a "moral character" violation. For complaints that pass this test, the process diverges depending upon the complaint's source: complaints that were already sustained by the employing agency's internal affairs division are usually forwarded to an FDLE "probable cause" panel, while complaints that originated through some other channel (e.g., media reports) are first sent to the local agency and then, typically, to the probable cause panel if the local agency sustains them. If the probable cause panel then finds probable cause to proceed with formal charges against the officer's certification to work in law enforcement, the complaint advances to full FDLE commission review. Finally, if the commission finds misconduct by "clear and convincing" evidence, it may discipline the officer pursuant to established disciplinary guidelines (even though the officer might have already been disciplined by the local agency).

Note that "probable cause" takes a meaning here different from in other legal settings, where it indicates only a "fair probability" of wrongdoing (*Illinois v. Gates* (462 U.S. 213, 238 [1983])). A finding of "no probable cause" does not suggest that there is no fair probability of misconduct; to the contrary, FDLE staff do not present a complaint to the probable cause panel unless they believe the evidence is sufficient to prove the misconduct by "clear and convincing"

evidence. The probable cause panel can enter a finding of "no probable cause" to proceed with formal charges for any number of reasons unrelated to the strength of the evidence. The panel may conclude, for example, that the officer has offered a reasonable explanation for his misconduct, that the employing agency has already imposed sufficient discipline, or that the misconduct, while qualifying as a "moral character" violation, is too minor to justify use of the formal disciplinary apparatus.

The ATMS database records, for each complaint, the nature of the misconduct, the source of the complaint, the officer, the officer's agency, the date on which the complaint was opened, and the disposition of the complaint. As we aggregate this data to the agency-year level, we can use only incidents for which we have, or can infer, the officer's agency and the year in which the complaint was opened. About 15% of complaints are missing the date on which the case was opened, while 9% of complaints are missing the officer's agency. In some instances, where the case reaches the FDLE only after a lengthy internal affairs process at the local agency, the FDLE may open its complaint in a year later than the year in which the underlying incident took place. That Florida law typically requires internal affairs investigations to be completed within 180 days (F.S.A. § 112.532(6)), however, suggests this problem is not too severe. Furthermore, where the FDLE learns of the incident through an officer's arrest or from media reports, it will open a complaint typically within days.

The database uses 275 different offense codes to characterize the nature of the misconduct. We focus on complaints that involve either express or implied violence, including sexual violence, as these incidents are of the greatest public concern and are also most distinctively characteristic of law enforcement activity. We thus separate out 66 of the 275 ATMS codes as involving violence, the threat of violence, or related attributes. We observe positive numbers of incidents for 47 of these 66 codes and find a total of 2158 violent incidents across all agencies over 1996-2015.[9] The 47 codes are listed in the Appendix, along with the corresponding number of incidents of that type for our baseline measure of violent incidents and for two alternative measures (described below).[10]

[9] Note that, as we drop 9 SOs that were not affected by *Williams*, the total number of violent incidents in the baseline estimation sample is slightly smaller.

[10] Incidents can have multiple offense codes if the officer's conduct falls within more than one of the 275 different offense types. For such incidents, one offense code is listed as the "major" offense code. Our classification of incidents treats them as "violent incidents" if *any* of the offense codes is among those listed in the Appendix.

While we include all 47 codes in the Appendix for completeness, most violent incidents fall into a small number of major categories. Assault or aggravated assault constitutes about 23% of violent incidents, while about 21% are in the "excessive force" category. About 20% of violent incidents are classified as "Battery – Domestic Violence" (although the results are quite similar when this category of violent incidents is omitted from the analysis). Another 17% of violent incidents involve sexual assault or other sexual offenses.

The ATMS data does not record the identity of the victim(s) of the officer's conduct but it seems reasonable to assume that most of the incidents in these 49 categories involve civilian victims. We refer to incidents falling within these 47 categories as "violent incidents." Our procedure was to include misconduct with any plausible violence, actual or threatened, express or implied, but to exclude those types of misconduct with no violence. Typical instances of excluded categories involve various forms of drug or alcohol abuse, corruption, theft, or embezzlement or other financial impropriety. While in many cases quite serious, these types of misconduct are less tied to the distinctive role of law enforcement officers than are violent incidents, and have been less often the subject of public debate.

As discussed in Section 4 below, officers at SOs and PDs perform generally similar duties. In one respect, however, SOs and PDs do meaningfully differ. Peace officers in Florida are generally certified in "law enforcement," "corrections," or both (known as "concurrent" certification). SOs employ a much greater proportion of certified corrections officers than do PDs. Moreover, violent incidents involving corrections officers may be driven by quite different factors from those involving law enforcement officers. Thus, we restrict our analysis to officers who are certified in law enforcement, either with or (more typically) without concurrent certification in corrections. Anecdotally, it is believed that most officers with concurrent certification in Florida primarily perform law enforcement activities (Baker 2017b). However, as a robustness check, we restrict our analysis to officers who are certified *only* in law enforcement and find very similar results.

We extract from the ATMS database information on the number of violent incidents involving law enforcement and concurrently certified officers. We aggregate this number to the agency-year level – for instance, we compute the number of violent incidents associated with officers employed at the Broward County SO in 2002. The vast majority of these observations –

about 82% – are zeroes. That is, for 82% of agency-years, the agency had no violent incidents reported in the ATMS database in that year.

In addition to its records of misconduct, the ATMS contains an employment database that reports the agency affiliation and demographic characteristics of all Florida law enforcement officers. We use this database to fill in missing agency data for some of the complaints. In addition, the employment database enables us to construct variables for the total number of officers in an agency-year and a number of demographic variables capturing the composition of officers at each agency-year in terms of race and gender. Consistent with the restrictions described earlier, the total counts of officers that we use, as well as the race and gender variables, exclude officers certified solely in corrections. While the demographic variables are not used in our primary analysis, we employ them for some supplemental tests.

3.2) Other Variables

Our dataset also includes information from 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, including both SOs and PDs. Importantly, the available surveys cover the period 1996-2015, spanning the *Williams* decision and its aftermath. The CJAP database reports extensive information about each agency at the agency-year level.[11] Most important for our purposes, it records whether a CBA existed for each law enforcement agency in Florida in each year over the period 2000-2015. Unfortunately, CBA status was not recorded in surveys prior to 2000. There is some missing data on the CBA variable over 2000-2015, amounting to about 10% of agency-year observations over that period. To address the problem of missing values, we impute CBA status for agency-years where it is missing but the agency had identical nonmissing values for both the prior year and the succeeding year. For example, suppose an agency reports having a CBA in 2005 and 2007 but the CBA variable is missing for that agency in 2006. We infer that the agency also had a CBA in 2006. This imputation procedure reduces the prevalence of missing

[11] This information includes, for instance, the length of the training period required of new officers under a field training officer, the types of firearms (handguns, shotguns, and rifles) the agency issues to each officer, and the minimum education requirements for new officers (typically a high school diploma or equivalent but occasionally some college credit). We find no robustly significant effects of collective bargaining rights on any of these variables. Some salary information is reported in CJAP but, unfortunately, the coverage is quite limited.

values from 10% to 2%. Moreover, all results reported below using the CBA variable are robust to omitting the imputed values.

Control variables for the analysis are obtained from a number of additional sources. We use U.S. 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. Department of Commerce 1996-2015). Unemployment rates at the county level are obtained from the Bureau of Labor Statistics (U.S. Department of Labor 1996-2015). The number of arrests by each law enforcement agency in each year is obtained from the Federal Bureau of Investigation's Uniform Crime Reporting (UCR) system (U.S. Department of Justice 1996-2015). The UCR system also provides data on crime rates, which are used as an alternative to arrests in our robustness checks.

3.3) Descriptive Statistics

Table 1 reports summary statistics for the 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. In defining SOs for purposes of this analysis, we account for the fact that, as briefly noted in Section 2, nine of the 67 SOs in Florida had obtained county-specific legislation before 2003 allowing them to engage in collective bargaining.[12] These SOs were unaffected by *Williams*. We thus exclude them from our baseline analysis, though the results are very similar if we reclassify them as part of the control group. The SO category used in Table 1 includes only the remaining 58 SOs; about 18% of our observations are on these SOs, while the rest are on PDs. As *Williams* was decided in January 2003, the post-*Williams* period (2003-2015) includes 2003.

The descriptive statistics in Table 1 suggest a relative increase after *Williams* in violent incidents for the 58 SOs in our treatment group, when compared to PDs. The mean number of violent incidents among SOs increased from 0.51 to 0.62 (about a 22% increase). The mean number of violent incidents among PDs fell from 0.24 to about 0.2 (about a 17% decrease).

Table 1 also reports summary statistics for the control variables. The SOs and PDs differ along some dimensions with respect to the control variables. PDs tend, on average, to employ fewer sworn law enforcement and concurrent officers. PDs are also more likely to be located in areas with larger and more diverse populations. These differences tend to hold both before and

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

after *Williams*, however. Moreover, the regression analysis described below controls for changes in these variables.

A first step in the study is to verify that *Williams* did indeed impact collective bargaining activity among SOs. Figure 2 plots the fraction of SOs and PDs with CBAs, as reported in the CJAP data over 2000-2015. As the treatment group excludes the nine SOs that had obtained collective bargaining rights before *Williams*, this fraction is initially zero for the treatment SOs. After *Williams* was decided in January 2003, collective bargaining activity begins among SOs within the same year. The fraction of SOs with CBAs keeps rising for about three years, before stabilizing around 2006. Another important point to note from Figure 2 is that the fraction of PDs with CBAs remains quite stable, at a little over a half, throughout this period. This suggests that, while collective bargaining may potentially affect outcomes for PDs, this impact is unlikely to have changed before and after *Williams*.

4) Empirical Analysis

4.1) Empirical Strategy

As outlined earlier, our empirical strategy involves comparing violent incidents at 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 law enforcement unions use potentially endogenous unionization decisions (Anzia and Moe 2014) or potentially endogenous changes in state law with respect to public sector unions (Frandsen 2014; Ichniowski, Freeman, and Lauer 1989), creating challenges to causal inference. Similarly, if we were merely to examine the effects of CBAs on misconduct, one important source of potential endogeneity is that agencies in which officers anticipate an increasing number of violent incidents, or an increasing probability of their detection, may be inclined to unionize. This would potentially bias the estimated coefficient upwards. But it is also possible that, in other circumstances, the estimated coefficient would be biased downwards. It may be, for example, that when unobserved morale is high, officers are more likely to resolve conflict with citizens without violence and also to succeed in winning unionization elections. Our approach avoids these challenges by focusing on the (exogenous) conferral of collective bargaining rights rather than the (potentially endogenous) adoption of CBAs. It also holds state-level factors constant by focusing on quasi-experimental variation across agencies in the same state.

This strategy requires comparability between SOs and PDs. Officers at agencies in the treatment and control groups perform similar job functions, with the exception of the larger fraction of corrections officers at SOs, which we address by excluding corrections officers from our dataset. Indeed, 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 for SOs.[13] Moreover, any minor differences in job duties are unlikely to have changed at the time of the *Williams* decision. Likewise, similar pools of applicants reportedly seek employment with SOs and PDs, and there is lateral movement by officers between the agency types (Baker 2017a).

Pynes and Corley (2006, p. 299) highlight the "unusual history of collective bargaining" rights in Florida, focusing on sheriffs' deputies. The *Williams* decision, however, has not previously been used to construct a quasi-experimental framework. Doerner and Doerner (2010) refer to the case but their empirical analysis uses data only on Florida SOs to examine wage and benefits outcomes for SOs that 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]

In implementing this empirical strategy in a regression framework, we bear in mind that the dependent variable (violent incidents) takes only non-negative integer values and thus is an example of "count" data. Moreover, it includes many zero-value observations, as noted above. Although linear specifications are generally highly flexible and robust, there are a number of problems with using 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:

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

Y_{it} represents the number of violent incidents matched to (law enforcement and concurrently certified) officers at agency i in year t. $Post_t$ is an indicator variable equal to one for the years

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

[14] Bulman (2019) 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.

after $Williams$. $Post_t$ includes 2003 because the decision was made in January of that year. SO_i is an indicator variable equal to one if agency i is part of the treatment group – i.e., any SO other than the 9 SOs that obtained collective bargaining rights by special dispensation prior to 2003 and that are excluded from the analysis. The interaction term $(Post_t * SO_i)_{it}$ is our variable of interest.

$Officers_{it}$ is the number of sworn officers (certified in law enforcement or concurrently certified) employed at agency *i* in year *t*. Equation (1) uses the number of violent incidents as the dependent variable while controlling for the number of officers, rather than using the violent incident rate. This specification is more flexible in many respects and the number of incidents tends to be less noisy than the rate. Using the number of violent incidents per 100 officers yields quite similar results, however.

$\mathbf{X}_{it}$ is a vector of control variables, which includes the demographic characteristics in year *t* of the county in which agency *i* is located. In particular, these are 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. Local economic conditions are captured by the county's unemployment rate in year *t*. 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 total number of arrests made by agency *i* in year *t* is included as a measure of the extent of contact officers in agency *i* in year *t* have with the civilian population. The basic results are robust, however, to using crime rates – the number of murders, property crimes, and violent crimes in agency *i*'s jurisdiction in year *t* – instead of arrests. μ_i is an agency fixed effect and δ_t is a year fixed effect, while ϵ_{it} is the error term.

The inclusion of these controls affects the interpretation of our results. In particular, the number of officers and the number of arrests may potentially be affected by collective bargaining rights or unionization. By controlling for these variables in our baseline analysis – and hence for the size of the agency and the scale and nature of its law enforcement activities – we seek to isolate the impact of collective bargaining rights *per se* (absent such aggregate impacts). It is possible that collective bargaining rights may also affect the aggregate amount of misconduct by changing the size of agencies and the scope of their activities. While this aggregate effect is not what we aim to estimate, it is worth noting that our basic result holds whether we include these controls or exclude them.

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" in our data, where the variance exceeds the mean, we compute robust standard errors that are clustered at the agency level.[15]

A crucial assumption of our difference-in-difference approach is that SOs and PDs experienced similar trends in violent incidents prior to *Williams*. To illustrate this and to further motivate the analysis, Figure 3 plots a natural representation of the mean number of violent incidents for the treatment and control groups over 1996-2015. We begin by running a Poisson regression of the number of violent incidents on agency fixed effects, year fixed effects, and the number of law enforcement and concurrently certified officers associated with each agency-year. This is a simplified version of Equation (1) that de-means the data and controls for common time shocks and for changes in the size of agencies. We then compute the residuals from this regression. Figure 3 shows the mean of these residuals, computed separately for SOs and PDs for each year. It is readily apparent that the mean residual of violent incidents rises substantially for SOs following *Williams*. Although the time series is quite noisy, the residuals are negative in most pre-*Williams* years and tend to be positive in post-*Williams* years. The mean residual of violent incidents for PDs, in contrast, is fairly stable and close to zero throughout the sample period. Moreover, the mean residuals for the two types of agencies follow what appear to be parallel trends prior to *Williams*, though there is also a certain amount of noise here as well.

There is a noticeable spike in violent incidents at SOs in 2006. Given the unusual nature of this spike, it is important to determine whether it may be attributable to some extraneous factor (unrelated to *Williams*) that occurred in 2006. 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. The distribution of sources of complaints also does not seem to have changed dramatically in 2006 relative to prior years. Reassuringly, the regression results are robust to excluding all observations for 2006 (as reported in Table 4, Column 1 and discussed below).

[15] 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. Available implementations of the ZIP model do not allow for fixed effects, however, 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.

To smooth out the noise apparent in Figure 3, we show in Figure 4 the lines of best fit for the residuals from the simple Poisson regression described above, separately for SOs and PDs, first over 1996-2002. Note that this uses the residuals at the agency level for each year rather than the mean residuals averaged over all SOs and over all PDs. The resulting lines represent time trends in violent incidents for SOs and PDs, controlling for agency fixed effects, year fixed effects, and the number of officers. As shown in Figure 4, these are closely parallel prior to *Williams*. For the post-*Williams* period, where a larger number of years is available, we use a more flexible local polynomial approach with a quadratic specification. Again, this shows a substantial increase in the residual number of violent incidents at SOs after *Williams*, relative to the fairly stable residual number for PDs. After the immediate post-*Williams* years, the residual number of violent incidents at SOs appears to fall and then stabilize. It is unclear from Figure 4 whether this results in a long-run level that is similar to or higher than the pre-*Williams* level. In the regression analysis, however, we show that the effect of *Williams* is persistent rather than transitory – violent incidents in the final years of the sample (excluding the immediate post-*Williams* years) are higher than in the pre-*Williams* period (see Table 2, Column 4).

4.2) Basic Regression Results

The results from the specification in Equation (1) are reported in Table 2. The maximal sample over 1996-2015 consists of 6320 observations at the agency-year level on 316 agencies (58 SOs and 258 PDs). Fixed-effects Poisson estimation, however, automatically omits agencies for which the number of violent incidents is always zero, as well as any agencies that appear in the dataset for only one year. The sample in Column 1 thus consists of 4760 observations on 238 agencies, and is slightly smaller in Column 2 when controls are added because of missing data on control variables. The reported standard errors are robust and clustered at the agency level, here and throughout the tables. Column 1 includes agency and year fixed effects but no controls apart from the number of officers, which we include in order to scale the number of violent incidents by agency size. The variable of interest is 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. The estimate (of about 0.34) is very similar in magnitude and significance when adding the extensive set of demographic, economic, and arrest controls described previously (Column 2).

As the Poisson specification takes an exponential form, the percentage impact of *Williams* on violent incidents is given by $100(e^{0.34} - 1)$, holding all other independent variables fixed.

Thus, the estimated coefficient implies that collective bargaining rights lead to a quite substantial 40% increase in complaints of violent officer misconduct. While this may seem implausibly large, the baseline frequency of violent incidents is low. The mean number of violent incidents per year for SOs prior to *Williams* is about 0.5, while the mean number of officers at SOs in the pre-*Williams* period is 210. The estimated effect thus implies an increase of about 0.2 complaints per year for a typical SO, from 0.5 to 0.7. That corresponds to one officer (out of 210) being involved in an additional violent incident over a five-year period. Viewed this way, the estimated effect does not seem overly large, while still suggesting a substantial divergence in violent incidents between SOs and PDs following *Williams*. Moreover, Goncalves (2019) characterizes effects of unionization on misconduct of up to about 20% as being "small" and indicative of a "precise zero" result. In comparison, our estimate of the effect of collective bargaining rights is larger, but not dramatically so. This underscores that our estimate does not seem implausibly large when viewed in context.

The baseline analysis in Columns 1 and 2 uses the full sample period 1996-2015. This is fairly long, especially for the post-*Williams* period. In Column 3, we focus on a narrower window immediately around 2003: the period 1999-2006. The estimated effect over this shorter period remains statistically significant despite the substantially smaller sample size, and is somewhat larger in magnitude.

As noted previously, it is not readily apparent from Figures 3 and 4 whether the *Williams* effect persists over time or reverses – i.e., whether SOs experience a permanently higher level of violent incidents post-*Williams* or whether violent incidents spike for only a few years and then converge to the counterfactual level. To test this formally, we adopt the following approach. We exclude from our sample the immediate post-*Williams* years (2003-2008) and compare violent incidents over 2009-2016 to those over the pre-*Williams* period (1996-2002). If the level of violent incidents becomes permanently higher for SOs, we should detect this effect in the later time period. Column 4 of Table 2 reports this estimate: it is very similar in magnitude to the baseline estimate in Column 2 and is statistically significant. This result suggests that the conferral of collective bargaining rights led to a sustained increase in the level of violent incidents at SOs.

4.3) The Role of Collective Bargaining Agreements

While *Williams* conferred collective bargaining rights on all SOs, only a subset of SOs ultimately adopted CBAs. By 2006, 18 SOs (representing 56% of sheriffs' deputies in our principal dataset) had CBAs while 38 did not; the CBA variable in the CJAP data is missing for the other 2

SOs. This situation remained virtually unchanged for the rest of our sample period. It is thus possible to examine separately the impact of *Williams* on the subset of SOs that subsequently adopted CBAs and the subset of SOs that did not. In doing so, we seek to distinguish between the impact of CBAs *per se* and the impact of bargaining in the shadow of collective bargaining rights. The former would apply only to the subset of SOs that adopted CBAs, while the latter would apply to all SOs.

This test is important because a potential alternative explanation for a post-*Williams* increase in reported violent incidents within SOs is that unionization may result in greater bureaucratization of the investigation process. Even with no increase in actual misconduct, sheriffs forced to bargain collectively with a deputies' union might begin to formally investigate, record, and report violent incidents to the FDLE when previously they would have handled the matter informally and the incident would not have entered the dataset. Both subsets of SOs (with and without CBAs) are "treated" in our design, as the treatment is obtaining collective bargaining rights rather than executing a CBA. Only those SOs that adopted CBAs ought to have undergone the bureaucratization process associated with unionization, however, and so any evidence of a "shadow" effect among non-CBA SOs casts doubt on this alternative explanation.

In addition, the exercise of separating SOs with and without CBAs helps to pinpoint whether the mechanism underlying our basic result involves collective bargaining. It can also be viewed as a type of falsification test – if the baseline effect appears only or even primarily among those SOs *without* CBAs, that may cast doubt on whether the post-2003 divergence in violent incidents at SOs and PDs is attributable to *Williams*. It should be stressed, however, that the existence of some effect among non-CBA SOs is not in itself problematic, as this would simply reflect bargaining in the shadow of collective bargaining rights. As previously discussed, there is considerable evidence in the labor economics literature of such "shadow" effects of collective bargaining.

Column 1 of Table 3 reports the results of estimating Equation (1) using as the treatment group only those SOs that subsequently adopted CBAs. We define this treatment group as consisting of all SOs that had a CBA in any (post-*Williams*) year of our sample period; however, in virtually all cases, CBAs were adopted by 2006. The control group (as in the baseline analysis) consists of all PDs. As shown in Column 1, the estimated effect for this subset of SOs is positive, statistically significant, and somewhat larger than the baseline effect for all SOs (Column 2 of

Table 2). Column 2 of Table 3 reports the results of estimating Equation (1) using as the treatment group only those SOs that never adopted CBAs. The estimated effect for this subset of SOs is of borderline statistical significance and somewhat smaller than the baseline effect for all SOs.

These results suggest a stronger effect among SOs that subsequently adopted CBAs and a weaker effect among those that did not. The direction of this difference is consistent with our expectations – that is, with a substantial effect of bargaining in the shadow of collective bargaining rights and a further incremental impact of adopting a CBA. The existence of some (albeit weaker) effect for SOs without CBAs provides some evidence of bargaining in the shadow of collective bargaining rights. It also suggests that bureaucratization cannot fully account for our results, as even SOs that did not unionize appear to have experienced an increase in violent incidents relative to PDs.

Nevertheless, the confidence intervals for the estimates in Columns 1 and 2 of Table 3 overlap. It is thus not possible to distinguish these effects statistically. This may be because SOs adopted CBAs shortly after *Williams*, mostly between 2003 and 2006, including nine SOs in 2003 itself. This timing makes it difficult to disentangle the "shadow" effect from the CBA effect, as does the relatively small number of SOs in each subset.

A slightly different way to make this point is to augment Equation (1) with a triple interaction term, interacting the post-*Williams* period with the SO indicator and with an indicator for the existence of a CBA:

$$Y_{it} = \exp(\beta_1(Post_t * SO_i)_{it} + \beta_2(Post_t * SO_i * CBA_{it})_{it} + \beta_3 Officers_{it} + \gamma \mathbf{X}_{it} + \mu_i + \delta_t)\epsilon_{it} \quad (2)$$

Here, CBA_{it} is an indicator variable equal to one if a CBA is reported as existing for agency i in year t. The baseline $(Post_t * SO_i)_{it}$ term now captures the impact of *Williams* on violent incidents, which can be interpreted primarily as a "shadow" effect. The triple interaction term $(Post_t * SO_i * CBA_{it})_{it}$ captures the incremental increase in violent incidents at SOs that have a CBA in year t, with the implicit control group being SOs without a CBA in that year.

Column 3 of Table 3 reports the results from the specification in Equation (2). Note that, because CBA_{it} is available only from 2000, the sample period here is 2000-2015. The baseline *Williams* effect is positive and statistically significant, indicating a substantial "shadow" effect. The coefficient on the triple interaction term is positive and fairly sizable, consistent with an additional increase in violent incidents following the adoption of a CBA. It falls short of statistical

significance, however. This may be due to the relatively small number of SOs, recalling that the triple interaction term compares SOs that have a CBA in a particular year to SOs that do not have one in that year.

Yet another approach is to focus more directly on the estimated coefficient of CBA_{it} for SOs, using as the implicit control group all PDs as well as SOs that do not have a CBA in year *t*:

$$Y_{it} = \exp(\beta_1(CBA_{it} * SO_i)_{it} + \beta_2 Officers_{it} + \gamma \mathbf{X}_{it} + \mu_i + \delta_t)\epsilon_{it} \quad (3)$$

Note that, in principle, it is possible to instrument for $(CBA_{it} * SO_i)$ using the interaction between the post-*Williams* years and SO_i in an instrumental variables (IV) framework. In a linear IV model (with agency and year fixed effects), however, the first-stage F-statistic is not sufficiently large to overcome concerns about the instrument's strength. This is possibly because the number of SOs is relatively small in relation to PDs, so that the exogenous variation created by *Williams* thus applies only to a relatively small fraction of all agencies.

Column 4 of Table 3 reports the results from the specification in Equation (3). It shows that CBAs among SOs are associated with a statistically significant and sizable increase in violent incidents. This supports the idea that the mechanism underlying our baseline *Williams* effect is related to unions and collective bargaining, as opposed to some extraneous factor (unrelated to *Williams*) that affected SOs differentially after 2003. Thus, examining the adoption of CBAs by SOs generally supports our basic claims, even though the decomposition of our baseline estimate into a shadow effect and a CBA effect is elusive given the available variation in our data.

It should be noted, however, that if we instead use all of the variation in CBA_{it} (for PDs as well as SOs) in a "naïve" panel specification that ignores endogeneity concerns, we do not detect a statistically significant association between CBAs and violent incidents. Indeed, the coefficient is close to zero when this specification is run on only the PD subsample. Of course, an agency's adoption of a CBA is potentially endogenous, as discussed above, and so the absence of an association may reflect a downward bias in the estimate due to self-selection. For SOs alone (as in Column 4 of Table 3), self-selection is less of a concern because CBAs were adopted over a relatively short period (primarily 2003-2006) soon after the *Williams* decision. SOs' CBAs are thus less likely to have been pursued endogenously in a manner that may bias estimates of their impact on violent incidents. SOs were exogenously unable to unionize prior to *Williams*, in other words, and so at least the *timing* of their CBA adoption is partially exogenous.

The absence of an association between CBAs and violent incidents in the full sample may also help reconcile our results with the apparently contrasting findings in the contemporaneous work of Goncalves (2019). While also using a national database of fatal incidents, Goncalves (2019) analyzes the impact of unionization on police misconduct using FDLE data from Florida. His study differs from ours along a number of important dimensions. His research question concerns unionization rather than collective bargaining rights. The empirical strategy – using hand-collected data on unionization elections – involves comparing Florida agencies (predominantly PDs) in which unionization elections are successful to those in which they are not. Goncalves (2019) does not use the variation in collective bargaining rights across SOs and PDs created by *Williams* and cannot detect any "shadow" effects of collective bargaining rights. Finally, Goncalves' (2019) misconduct measure is not restricted to violent incidents.

Despite these differences, the papers do contain some overlapping elements, and so it is worth discussing the relationship between them, especially in view of the apparently differing results: Goncalves (2019) finds no statistically significant effect of unionization on misconduct and his estimates rule out a positive effect greater than about 10% to 20%. The differing results using similar Florida data may seem puzzling, especially if one views the conferral of collective bargaining rights as an "intent-to-treat" and unionization as the actual treatment (although, as discussed earlier, this is not our preferred interpretation). Note, however, that the two results are not necessarily inconsistent. It could be the case, in principle, that bargaining in the shadow of collective bargaining rights is sufficiently effective with respect to (formal or *de facto*) procedural protections that actual unionization does not have any detectable incremental effect. This would raise the question of why officers would ever choose to unionize in such a scenario, but it may be that bargaining in the shadow of collective bargaining rights is less effective with respect to wages and benefits than procedural protections.

While we do not have data on unionization elections, the Goncalves (2019) result is consistent with what we find when regressing violent incidents on unionization for the full sample (i.e., both PDs and SOs) or for PDs alone. In that sense, the results of the two papers are fairly consistent in the sphere in which they overlap. Our argument in this paper, however, is that the *Williams* decision provides a source of exogenous variation that reveals the causal impact of collective bargaining on misconduct in a way that cannot otherwise be replicated (for instance, by studying the effect of unionization). The impact of *Williams* is manifested both directly, as the

difference-in-difference *Williams* effect (our baseline result in Table 2, Column 2), and indirectly, by generating exogenous variation in the timing of SOs' adoption of CBAs (as in Table 3, Column 4). We thus contend that the evidence from *Williams* is crucial in understanding the causal effect of collective bargaining on law enforcement behavior.

4.4) Robustness Checks and Extensions

Table 4 reports a number of additional robustness checks and extensions. As noted earlier, the number of violent incidents at SOs spikes in 2006. Column 1 of Table 4 reports the results when all observations for 2006 are omitted. The estimated effect of *Williams* on violent incidents is positive and statistically significant, though somewhat smaller in magnitude. This reassures us that any unusual factors specific to 2006 are not driving the baseline result.

As also noted above, in some instances the FDLE does not sustain a complaint. This could be because the incident, though "misconduct" in a colloquial sense, did not satisfy the legal definition for a "moral character" violation. It could also be because evidence of the incident was insufficient to satisfy the FDLE's "clear and convincing" evidence requirement. Unfortunately, we cannot distinguish between these two possibilities. To ensure that our results do not depend on complaints that potentially lack a factual basis, we construct two alternative measures of "verified" violent incidents. We communicated with FDLE staff to identify five terminal "complaint status" codes that indicate that a complaint may have lacked an adequate factual basis – the employing agency may not have sustained the complaint (*potentially* for factual insufficiency) or, in a small number of cases, the employing agency *did* sustain the complaint but FDLE staff nevertheless concluded that the evidence would be insufficient to satisfy the FDLE's higher evidentiary standard. Complaints that terminate with these codes do not reach the FDLE's probable cause panel (see Figure 1).

Our first measure of "verified" violent incidents excludes all complaints that terminated with one of the five codes just mentioned *unless* the complaint originated in the employing agency's internal affairs process, in which case we can be sure that the employing agency sustained the allegation, because the complaint was forwarded to the FDLE. Column 2 of Table 4 reports results using this variable. The estimated effect is statistically significant and similar in magnitude to our baseline result. The second measure of "verified" violent incidents excludes *all* complaints that terminated with one of the five codes, even if they originated in the employing agency's internal affairs process. The estimated effect using this variable, reported in Column 3 of Table 4,

is again statistically significant and similar in magnitude to the baseline effect. It should be emphasized that both of these measures are conservative estimates of "verified" complaints because they exclude some complaints that were screened out for "legal" rather than evidentiary reasons – that is, they exclude complaints for which it was clear that some misconduct occurred but the misconduct did not meet Florida's legal definition of a "moral character" violation. One example might be the use of force that violates a local agency's relatively restrictive use-of-force policy but is not considered "excessive force" sufficient to establish a "moral character" violation on the FDLE's view.

We described in Section 2.2 the legal developments with respect to the "legislative body" question that lingered after *Williams*. To the extent the definition of the legislative body, which resolves bargaining impasses, matters for misconduct, resolution of legal uncertainty about this issue – which occurred around 2010 or 2011 in the deputies' favor – may generate additional effects. The similarity of the estimate in Column 4 of Table 2 to the baseline estimate, however, seems to suggest that resolution of the "legislative body" question in fact had little independent impact on violent incidents.

We explicitly test for the effect of these later legal developments by running a difference-in-difference model analogous to Equation (1), with the variable of interest defined as an interaction between an indicator for post-2010 years and an indicator for SOs. This is run over the sample period 2003-2016, to focus only on the post-*Williams* period. As shown in Column 4 of Table 4, the estimated coefficient is close to zero and statistically insignificant. The test thus does not reveal any detectable effect from resolution of the "legislative body" question on the number of violent incidents, and suggests that the impact of *Williams* has largely been captured by our earlier results (in Table 2). It is, however, fairly imprecisely estimated, so that an effect of similar magnitude to the baseline effect in Table 2, Column 2 cannot be ruled out at the 95% level.

Our baseline results are also robust to a number of other tests that are not reported for reasons of space. Several of these relate to the key assumption of our difference-in-difference approach, namely that the treatment and control groups experienced parallel trends in the period prior to the treatment. As discussed earlier, visual inspection of the graphs in Figures 3 and 4 does not indicate that SOs and PDs experienced different trends in violent incidents prior to *Williams*. It is possible in principle to add linear agency-specific trends to Equation (1). This involves hundreds of additional variables, however, and the Poisson maximum-likelihood procedure fails

to converge. We can, however, add two linear trends, one for SOs and the other for PDs, which allows the average trend for the agency types (though not each individual agency) to differ. The results are quite similar. Adding leads of the interaction term of interest to the specification in Equation (1) (e.g., Autor 2003) also suggests that there were no differential effects for SOs and PDs in the years prior to *Williams*.

A further important assumption of the difference-in-difference approach is that no other factor changed differentially for SOs and PDs after 2003. One such possibility is that the increasing use of smartphones to film law enforcement officers may explain the results. For example, if smartphone penetration (or use) increased faster after *Williams* in areas patrolled by SOs than by PDs, the apparent rise in violent incidents in SOs relative to PDs could reflect instead a (relative) improvement in reporting and documentation of incidents in those areas. Fortunately for our purposes, the popularization of filming law enforcement with smartphones appears to have occurred too recently to explain our results (Ouss and Rappaport 2019).

Another potential explanation might be that unionization among SOs induced PD officers with a high risk of involvement in violent incidents to move to unionized SOs – under the assumption that these officers previously would have been concentrated in unionized PDs. In this scenario, the control group may be affected by the treatment through a compositional effect that reduces the number of violence-prone officers. Of course, the change-in-composition hypothesis assumes that violence-prone officers anticipate more lenient treatment in unionized forces, which itself implies that the probability of detection and termination is lower in such agencies. In any event, one way to address this possibility is to drop all agencies with CBAs, so the analysis is unaffected by officers' sorting into unionized agencies. This results in a sample size of less than half our baseline sample, primarily because the majority of PDs are unionized and some of those that are not are small and never have positive numbers of violent incidents. Nonetheless, the result is quite similar to our baseline finding, suggesting that our baseline is robust to the possibility that violence-inclined officers are sorting into unionized agencies.

A variety of other robustness checks have been noted throughout the paper. Adding to the control group the nine SOs that adopted CBAs through special arrangements prior to *Williams* – rather than excluding them altogether from the analysis, as in the baseline results – leads to very similar results. Adding crime rates as controls – either instead of or in addition to our measure of arrests – also leads to virtually identical results. Adding measures of the racial and gender

composition of officers, too, does not affect the results. As noted earlier, the baseline analysis uses data on violent incidents involving officers certified solely in law enforcement as well as those concurrently certified in corrections. Omitting the latter – and using law enforcement officers only – leads to essentially identical results. Finally, a significant fraction of violent incidents in our data involve domestic violence. There is a strong conceptual case for including these incidents, which likely involve civilian victims. In any event, the results are quite similar when they are omitted.

A potentially interesting question is the extent to which agency characteristics may mediate the estimated effect of collective bargaining rights on violent incidents. For example, we examine whether the racial diversity of agencies influences the size of the effect by adding an interaction between $(Post_t * SO_i)_{it}$ and the fraction of African American officers in an agency-year (with the latter variable also added separately to the model). The coefficient on this interaction term is negative – consistent with greater racial diversity mitigating the effect of collective bargaining rights on violent incidents – however, it is not statistically significant. We also test for an intermediating effect of the gender of officers by adding an interaction between $(Post_t * SO_i)_{it}$ and the fraction of male officers in an agency-year (with the latter variable also added separately to the model). The coefficient on this interaction term is positive – consistent with a greater fraction of male officers magnifying the effect of collective bargaining rights on violent incidents – however, again it is not statistically significant.

5) Discussion

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 on misconduct by law enforcement officers. Using a comprehensive state administrative database of "moral character" violations reported by local agencies in Florida and an empirical strategy based on 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 violent incidents, relative to a control group of PDs that were unaffected by *Williams*. These results are robust to a wide variety of robustness checks and tests for alternative explanations.

While some of these points have been raised earlier, it is worth reiterating that there are several sources of potential bias against these findings. First, Florida is a right-to-work state, which

generally limits the ability of employees to organize effectively. Second, Florida has a statutory LEOBOR that applies to all law enforcement officers, leaving less space for collective bargaining to secure procedural protections from disciplinary investigations. Third, the existence of a state-level FDLE disciplinary mechanism also limits the effect of collective bargaining for law enforcement officers in Florida compared to states that lack this sanction. Whereas individual agencies can at most terminate an officer, the FDLE has the power to "decertify" officers so they cannot be hired by any other law enforcement agency in the state; this power is not easily cabined by collective bargaining. CBAs can lower the probability of detecting misconduct, but the effect of a given decrease in the probability of detection will be less damaging to deterrence in states with the decertification sanction than in states with only a local sanction.

Fourth, our measure of violent incidents consists mostly of misconduct claims that have been sustained by a local agency. To the extent that unionized agencies are less likely to sustain civilian or internal affairs complaints because of the strong procedural protections CBAs afford, it is possible that fewer such complaints will be initiated. And even if the same number of complaints were initiated, the number of sustained complaints reported to the FDLE would fall. Fifth, the *Service Employees* case discussed in section 2 – holding that appointed deputies of court clerks were "employees" under the statute granting employees collective bargaining rights – may have led to anticipation of the *Williams* outcome and perhaps to bargaining in the shadow of that outcome. This would dampen the observed impact of the *Williams* decision.

Finally, we noted earlier that the duties of sheriff deputies and police officers are 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 deputies' outside option. Their ability to seamlessly switch employers to a unionized PD would have resulted in their receiving these benefits even prior to *Williams*, depriving *Williams* of 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 bias against our result.

What mechanisms might explain our results? We have previously suggested that unionization may provide procedural protections that undermine detection and sanctioning of

misbehaving officers. The most direct path for this mechanism is the SO CBA, which may contain provisions – beyond those in Florida's LEOBOR – that make internal disciplinary investigations more difficult. For example, some Florida CBAs authorize law enforcement officers to challenge any discipline the local agency seeks to impose through arbitration or other administrative review,[16] preventing the agency from making independent disciplinary decisions. Other rights include a tightened time limitation on internal affairs investigations and expungement of old records, even when the officer is found to have engaged in misconduct.[17] These additional procedural rights raise the cost of terminating misbehaving officers and thereby lower deterrence. (In ongoing efforts, we are collecting CBAs from Florida law enforcement agencies with the aim of identifying specific provisions that provide procedural protections beyond those in Florida's LEOBOR.) Second, our results are consistent with the idea that the political influence of unions may lead to local legislation that embodies similar procedural protections. As an alternative to procedural protections, and as a third possible mechanism, the processes of successful unionization drives, collective bargaining meetings, and union officer elections may increase solidarity among officers and thereby strengthen a code of silence that impedes the detection of misconduct.

Although these unionization mechanisms explain our overall results, they would not explain the effects we found for SOs without CBAs. The standard explanation here is bargaining in the shadow of law. The right to unionize gives non-unionized deputies more bargaining power because sheriffs are more reluctant to alienate deputies who can credibly threaten to unionize. As one specific possibility, sheriffs before *Williams* may have disregarded some procedural rights that deputies possess under Florida's LEOBOR, yet honored those rights even for non-unionized deputies once *Williams* created the unionization threat. Future research should aim to narrow down the responsible causal mechanisms.

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[16] 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.").

[17] 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.").

6) Conclusion

This paper provides quasi-experimental evidence on the impact of collective bargaining rights on law enforcement misconduct, an issue of pressing public importance. Our empirical strategy exploits the 2003 Florida Supreme Court decision (*Williams*) that conferred collective bargaining rights on sheriffs' deputies, while leaving police departments unaffected. Using a Florida state administrative database of "moral character" violations reported by local agencies between 1996 and 2015, we implement a difference-in-difference approach in which police departments serve as a control group for sheriffs' offices. Our estimates imply that collective bargaining rights led to about a 40% increase in violent incidents of misconduct among sheriffs' offices, corresponding to one incremental incident over a five-year period for the typical sheriff's office. This effect is concentrated among sheriffs' offices that unionized. Together, these results provide strong evidence for a "shadow effect" from collective bargaining rights and some evidence of a "CBA effect" among SOs. Although it is difficult, with the variation that exists in our dataset, to disentangle these two effects, collective bargaining – rather than some extraneous factor – best explains these findings overall.

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Figure 1: Flowchart of the FDLE Complaint Process

Note: This flowchart depicts a slightly simplified representation of the process by which the Florida Department of Law Enforcement (FDLE) handles misconduct complaints. The process begins at the bottom of the chart, with the receipt of a complaint. FDLE staff first screen out complaints that do not, on their face, allege a "moral character" violation (MCV). To proceed further, all complaints must be sustained by the employing agency's internal affairs (IA) division; those that were not sustained before reaching the FDLE are sent to the local agency for IA review. Complaints that both allege an MCV and are sustained in IA are forwarded to an FDLE probable cause (PC) panel. If PC is found, the complaint proceeds to full commission review. If the commission finds misconduct by "clear and convincing" evidence, it may discipline the officer.

Figure 2: CBAs Among Florida Law Enforcement Agencies, 2000-2016

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). The vertical red line represents the year of the *Williams* decision (2003).

Figure 3: Violent Incidents, Florida Law Enforcement Agencies, 1996-2015

Note: This graph represents the time pattern of violent incidents in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database, 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). The graph is based on the residuals from a Poisson regression of the number of violent incidents on agency fixed effects, year fixed effects, and the number of law enforcement and concurrently certified officers associated with each agency-year. These residuals are averaged across SOs and PDs for each year. The vertical red line represents the year of the *Williams* decision (2003).

Figure 4: Trends in Violent Incidents, Florida Law Enforcement Agencies, 1996-2015

Note: This graph represents the trends in the time pattern of violent incidents in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database, 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). The graph is based on the residuals from a Poisson regression of the number of violent incidents on agency fixed effects, year fixed effects, and the number of law enforcement and concurrently certified officers associated with each agency-year. These residuals are then regressed on the year (separately for SOs and PDs), using a linear specification for the pre-*Williams* period (1996-2002) and a local polynomial approach with a quadratic specification for the post-*Williams* period (2003-2015). The vertical red line represents the year of the *Williams* decision (2003).

Table 1: Summary Statistics

Panel A: Sheriffs' Offices (SOs)

Variable	1996-2002			2003-2015		
	Number	Mean	Standard Deviation	Number	Mean	Standard Deviation
CBA = 1	160	0	0	721	0.2981969	0.457784
Violent Incidents	406	0.5147783	1.012772	754	0.6220159	1.169144
Violent Incidents, Excluding Potentially Unverified External Complaints	406	0.3866995	.8319072	754	0.4708223	0.9626891
Violent Incidents, Excluding All Potentially Unverified Complaints	406	0.2807882	0.6629343	754	0.4005305	0.8677793
Number of Officers (Law Enforcement and Concurrent)	406	210.2685	276.1115	754	266.9509	340.7105
Resident Population (thousands)	406	174.3065	250.9221	754	209.805	295.1946
Fraction of Resident Population Aged 18-24	406	.0601138	.023217	754	0.0666087	0.0267435
Hispanic Fraction of Resident Population	406	.0739729	.0786061	754	0.1108455	0.1022866
African American Fraction of Resident Population	406	.1424147	.1009931	754	0.1441849	0.0952054
Unemployment Rate (%)	406	5.181034	2.096454	754	6.748939	2.759007
Arrests (thousands)	406	1.74397	2.788198	754	1.790751	2.726963

Note: Panel A reports summary statistics for the 58 SOs in the treatment group, separately for the pre-*Williams* period (1996-2002) and the post-*Williams* period (2003-2015). 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. "Violent Incidents" is the number of incidents (involving law enforcement and concurrent officers) reported in the ATMS database that we classify as "violent" using the categories in the Appendix. 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. Arrests 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	**1996-2002**			**2003-2015**		
	Number	Mean	Standard Deviation	Number	Mean	Standard Deviation
CBA = 1	697	0.5638451	0.4962632	3,112	0.5851542	0.4927746
Violent Incidents	1,806	0.2414175	0.736369	3,354	0.1979726	0.6189536
Violent Incidents, Excluding Potentially Unverified External Complaints	1,806	0.1749723	0.5759293	3,354	0.1550388	0.5432062
Violent Incidents, Excluding All Potentially Unverified Complaints	1,806	0.1306755	0.4921323	3,354	0.1258199	0.4807366
Number of Officers (Law Enforcement and Concurrent)	1,806	59.0454	116.4807	3,354	65.22004	121.8168
Resident Population (thousands)	1,806	673.8522	713.3858	3,354	768.0201	792.6498
Fraction of Resident Population Aged 18-24	1,806	0.0574986	0.0199476	3,354	0.0638977	0.0227344
Hispanic Fraction of Resident Population	1,806	0.1397922	0.1659797	3,354	0.1866478	0.1789958
African American Fraction of Resident Population	1,806	0.1510611	0.0930864	3,354	0.1566774	0.089946
Unemployment Rate (%)	1,806	4.888213	1.593196	3,354	6.709763	2.715023
Arrests (thousands)	1,780	0.4468635	1.11458	3,353	0.4773337	0.9763213

Note: Panel B reports summary statistics for the control group (PDs), separately for the pre-*Williams* period (1996-2002) and the post-*Williams* period (2003-2015). There are up to 258 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: The Impact of Collective Bargaining Rights on Violent Incidents

	(1)	(2)	(3)	(4)
	Dependent Variable: Violent Incidents			
Post-*Williams**SO	**0.33594*****	**0.33869*****	**0.50854****	**0.32888****
	(0.129)	**(0.125)**	**(0.204)**	**(0.153)**
Number of Officers	0.00044	0.00060	0.00024	0.00047
	(0.000)	(0.001)	(0.002)	(0.001)
Resident Population		-0.00021	-0.00184	0.00001
		(0.001)	(0.002)	(0.001)
Fraction of Resident Population		7.97497	-0.52452	-1.88661
Aged 18-24		(8.592)	(9.606)	(10.940)
Hispanic Fraction of Resident		3.26136	7.63641	2.87675
Population		(2.534)	(9.155)	(2.658)
African American Fraction of		-7.47212*	-15.48351	-6.05630
Resident Population		(4.041)	(10.905)	(4.296)
Unemployment Rate (%)		-0.04922	-0.14580	-0.03843
		(0.037)	(0.121)	(0.041)
Arrests		0.00071	0.02754	-0.00416
		(0.016)	(0.084)	(0.017)
Agency and Year Fixed Effects?	Yes	Yes	Yes	Yes
Sample Period	1996-2015	1996-2015	1999-2006	1996-2002, 2009-2015
Observations	4,760	4,740	1,408	2,959
Number of Agencies	238	238	176	212

Note: This table reports regression results for the number of violent incidents at the agency-year level. The primary variable of interest is the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for sheriffs' offices (SOs). 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: The Role of Collective Bargaining Agreements

	(1) Treatment Group: SOs with a CBA by 2015	(2) Treatment Group: SOs without a CBA by 2015	(3) All Agencies	(4) All Agencies
	Dependent Variable: Violent Incidents			
Post-*Williams**SO	**0.37135****	**0.32252***	**0.38159****	
	(0.150)	**(0.185)**	**(0.179)**	
Post-*Williams**SO*CBA			**0.18533**	
			(0.151)	
SO*CBA				**0.31124****
				(0.152)
Number of Officers	0.00078	-0.00076	0.00050	0.00109
	(0.001)	(0.001)	(0.001)	(0.001)
Resident Population	-0.00051	-0.00031	-0.00021	-0.00047
	(0.001)	(0.001)	(0.001)	(0.001)
Fraction of Resident Population	3.70472	15.12869**	9.40647	9.54942
Aged 18-24	(9.010)	(7.161)	(8.685)	(8.907)
Hispanic Fraction of Resident	4.36952	5.69048**	8.55421**	8.33416**
Population	(3.539)	(2.888)	(4.162)	(4.004)
African American Fraction of	-8.42355*	-8.89584**	-12.65003**	-13.65734***
Resident Population	(4.299)	(4.275)	(5.250)	(5.242)
Unemployment Rate (%)	-0.04728	-0.05566	-0.12740**	-0.12607**
	(0.042)	(0.044)	(0.059)	(0.059)
Arrests	-0.00151	-0.01813	0.00345	-0.00604
	(0.017)	(0.020)	(0.022)	(0.019)
Agency and Year Fixed Effects?	Yes	Yes	Yes	Yes
Sample Period	1996-2015	1996-2015	2000-2015	2000-2015
Observations	4,180	4,240	3,285	3,285
Number of Agencies	210	213	213	213

Note: This table reports regression results for the number of violent incidents at the agency-year level. The primary variable of interest in Columns 1-3 is the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for sheriffs' offices (SOs). In Column 3, an interaction term between this variable and an indicator for CBA=1 is also introduced. In Column 4, the variable of interest is an interaction between an indicator for sheriffs' offices (SOs) and an indicator for CBA=1. 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: Robustness Checks and Extensions

	(1) Dependent Variable: Violent Incidents	(2) Dependent Variable: Violent Incidents, Excluding Potentially Unverified External Complaints	(3) Dependent Variable: Violent Incidents, Excluding All Potentially Unverified Complaints	(4) Dependent Variable: Violent Incidents
Post-*Williams**SO	**0.26839****	**0.34694****	**0.36398****	
	(0.130)	**(0.141)**	**(0.156)**	
Post-2010*SO				**0.02445**
				(0.170)
Number of Officers	0.00083	-0.00005	0.00037	0.00033
	(0.001)	(0.001)	(0.001)	(0.001)
Resident Population	-0.00013	0.00002	-0.00026	-0.00188
	(0.001)	(0.001)	(0.001)	(0.002)
Fraction of Resident Population	7.95029	7.07081	12.01542	13.47222
Aged 18-24	(9.569)	(6.741)	(8.239)	(10.393)
Hispanic Fraction of Resident	2.75790	2.55205	2.97863	8.53406
Population	(2.576)	(2.910)	(2.671)	(6.847)
African American Fraction of	-7.10613*	-5.20808	-6.00711	-8.54145
Resident Population	(3.997)	(4.315)	(4.688)	(6.986)
Unemployment Rate (%)	-0.04400	-0.03164	-0.04711	-0.09265*
	(0.037)	(0.042)	(0.047)	(0.054)
Arrests	0.00238	0.00824	0.01853	0.00502
	(0.017)	(0.020)	(0.022)	(0.026)
Agency and Year Fixed Effects?	Yes	Yes	Yes	Yes
Sample Period	1996-2005 2007-2015	1996-2015	1996-2015	2003-2015
Observations	4,483	4,264	4,072	2,625
Number of Agencies	237	214	204	202

Note: This table reports regression results for the number of violent incidents (defined in several alternative ways) at the agency-year level. The primary variable of interest in Columns 1-3 is the interaction between a post-*Williams* indicator (for years beginning in 2003) and an indicator for sheriffs' offices (SOs). The primary variable of interest in Column 4 is the interaction between an indicator for post-2010 years and an indicator for sheriffs' offices (SOs). 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.

Appendix: Types of Offenses Committed by Law Enforcement or Concurrent Officers in the FDLE ATMS Database That Are Included in "Violent Incidents"

Offense Type	Number of Incidents (Baseline Measure)	Number of Incidents Excluding Potentially Unverified External Complaints	Number of Incidents Excluding All Unverified External Complaints
Aggrav Asslt Pol Off	10	8	7
Aggravated Assault	92	65	51
Aggravated Stalking	1	1	1
Arson	15	11	9
Assault	403	304	256
Battery	9	7	5
Battery - Domestic Violence	428	300	246
Battery-Domestic Violence-Strang.	1	0	0
Child Abuse	6	6	6
Cruelty to Animals	2	2	1
Cruelty Toward Child	34	25	21
Culpable Negligence	27	20	16
Cyberstalking	1	1	0
Disorderly Conduct	11	11	10
Excess Force by Corr	36	35	24
Excess Force by LEO	410	267	181
False Imprisonment	1	1	1
Harassing Communica.	5	4	4
Hit And Run	10	9	7
Homicide	13	12	11
Improper Exhibition of Dangerous Weapon	4	4	4
Indecent Exposure	31	30	22
Intimidation	5	4	3
Kidnapping	58	42	39
Lewd & Lascivious Exhibition	1	1	0
Lewd & Lascivious Molestation	4	4	2
Loitering and Prowling	5	3	2
Manslaughter	4	1	1
Manslaughter-Vehicle	6	4	3
Neglect Child	10	8	8
Neglect Family	1	0	0
Peeping Tom	3	1	1
Public Order	1	1	1
Riot-Engaging In	2	2	2
Robbery	20	17	15

Sex Assault	104	84	66
Sex Asslt-Carnal Abu	4	3	2
Sex Off Agst.Child	93	88	79
Sex Offense	125	111	91
Sexual Battery	3	3	1
Simple Assault	46	39	33
Stalking	80	64	53
Stat Rape-No Force	2	2	1
Unlawful Sexual Activity with a Minor	1	1	1
Video Voyeurism	1	1	1
Viol Repeat Violence Injunct	28	26	23
Violation for Injunct. for Protect. Against Domestic Violence	1	1	1