Collective Bargaining Rights and Police Misconduct: Evidence from Florida

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Abstract

Growing controversy surrounds the impact of labor unions on law enforcement behavior. We provide quasi-experimental 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 over 1996-2015, we implement a difference-in-difference approach in which police departments (which were unaffected by *Williams*) serve as a control group for sheriffs' offices. Our estimates imply that collective bargaining rights led to a substantial increase in violent incidents of misconduct among sheriffs' offices, relative to police departments. This result is robust to including only violent incidents involving officers hired before *Williams*, suggesting that it is due to a deterrence mechanism rather than to compositional effects on the types of officers hired post-*Williams*. While there is some evidence consistent with a "bargaining in the shadow of the law" effect among sheriffs' offices that did not unionize, unionization is associated with higher levels of violent misconduct in an event-study framework, and so appears to be a channel for the effect.

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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 its opportunity cost.[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 (e.g., Fryer 2019), while others focus on the role of collective bargaining (e.g. Huq and McAdams 2016; Rushin 2017). Of greatest relevance, many scholars draw upon case studies to argue that unions impede progressive policy reform (Walker 2008; Epp 2009; McCormick 2015; Fisk and Richardson 2016; Bies 2017) and one scholar (Rad 2018) observes a positive correlation between union-secured procedural protections of police and police abuses. No previous work, however, has offered

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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 (e.g. Feuille and Delaney 1986; Freeman and Valletta 1988; Trejo 1991; Wilson et al. 2006; Briggs et al. 2008; Doerner and Doerner 2010; Frandsen 2016).

[4] 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. 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.

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 unionize 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 non-unionized agencies bargaining "in the shadow of" collective bargaining rights. Our focus on collective bargaining rights is motivated in part by findings in labor economics that strong labor laws are associated with higher wages even for nonunionized workers (e.g., Freeman and Valletta 1988; Ichniowski, Freeman, and Lauer, 1989).

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 2, law enforcement officers at agencies in the treatment and control groups perform similar job functions and are drawn from similar pools of applicants.

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 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 before reaching the FDLE. 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. Public concern is strongest regarding these highly salient incidents. They are also most distinctively characteristic of law enforcement activity and arguably less susceptible to changes in reporting behavior that might accompany unionization.

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."[5] The processes generating the ATMS data are quite complex, however, as detailed in Section 3.[6] The filtering of (most) violent incidents through local agencies' disciplinary processes raises the concern that the conferral of collective bargaining rights (and unionization, where it occurred) may affect the likelihood that a complaint is sustained at the local agency level (and hence the probability that it is recorded in the ATMS database). The most natural assumption, given that collective bargaining rights increase officers' bargaining power, is that this likelihood declines after *Williams*. If we nonetheless observe an increase in (recorded) violent incidents for SOs, then it follows that the actual number of violent incidents increased, possibly by even more than the estimated effect.

To illustrate this point, suppose that prior to *Williams* there are 10 actual violent incidents and the probability of complaints about these being sustained is 0.5. In expectation, we would observe 5 violent incidents in our data. Assume that, following *Williams*, the number of actual violent incidents increases to 20, while the probability of complaints about these being sustained falls to 0.35. We would then expect to observe 7 violent incidents in our data (a post-*Williams*

[5] 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.

[6] 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.

increase of 2). Of course, the probability of complaints being sustained could, in principle, fall sufficiently that the number of observed violent incidents falls. Thus, not much could be inferred if we were to observe a decrease in (recorded) violent incidents for SOs, as this could be due to a decline either in the actual number of violent incidents or in the probability that complaints are sustained. If, however, we observe an increase in recorded violent incidents, we can infer that the actual number of violent incidents increased – possibly by more than the observed increase.[7]

We employ a fixed-effects Poisson model for count data, controlling for agency and year fixed effects and an extensive set of local and agency characteristics. To establish that the treatment and control groups experienced similar trends in violent misconduct prior to *Williams*, we implement a number of tests of the parallel trends assumption. In our baseline regression analysis, 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, note that the baseline rate of recorded violent incidents is low. The estimated effect implies an increase of 0.2 violent 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. Of course, these are merely the *recorded* incidents – those sustained by the local agency and reported to the state agency – and we are not saying that the actual number of violent incidents is so low. The estimated percentage increase could potentially apply to a much larger number of unrecorded violent incidents. The bottom line is a substantial divergence between SOs and PDs following *Williams*.

The basic result is robust to a wide variety of tests reported or summarized in Section 4.4. These fall into several distinct categories. First, we address the possibility of measurement error in our violent incidents variable by constructing alternative measures consisting only of violent incidents that have been more thoroughly verified by the FDLE's investigative process, and find very similar results. Second, we address a potential alternative explanation that unionization may lead to greater bureaucratization and an increased tendency to formalize complaints. We find no evidence of a substitution from separations (without a record of moral character violations) to

[7] To the extent that agencies that are unionized or have collective bargaining rights are less likely to sustain civilian or internal affairs complaints because of stronger procedural protections for officers, it is possible that fewer such complaints will be initiated. This effect would further reinforce the argument in the text.

formal proceedings (involving moral character violations) post-*Williams*. Third, we consider and analyze a variety of alternative samples and outcome variables.

The paper also explores potential mechanisms for our result. We use an event-study framework, focusing on the timing of unionization, to show that unionization is associated with subsequent increases in the number of violent incidents. While subject to some endogeneity concerns, this evidence is consistent with unionization serving as one channel through which collective bargaining rights affect violent misconduct. There is, however, also some evidence consistent with a 'bargaining in the shadow of the law' effect among SOs that did not unionize.

It is possible that *Williams* affected the types of officers hired at SOs, for instance, attracting a larger share of violence-prone officers. To address such compositional effects, we construct an alternative measure of violent incidents that is restricted to those involving officers hired pre-*Williams*. The estimated effect is virtually identical to the baseline result, suggesting that our finding is due to a deterrence mechanism rather than to compositional effects on the types of officers hired post-*Williams*. We also test more directly for a deterrence mechanism by constructing a measure of the probability that an officer involved in a violent incident is terminated. While this measure is quite noisy, it appears to decline after *Williams* at SOs (relative to PDs) and also to decline after an agency unionizes. This reinforces the idea that increased violent misconduct following the conferral of collective bargaining rights is due to a reduction in expected sanctions.

This paper presents the first quasi-experimental evidence on the impact of collective bargaining rights on police misconduct. In contemporaneous work, Goncalves (2020) uses a similar dataset from Florida (along with a national database) to analyze the impact of unionization on (violent and nonviolent) police misconduct. Using an empirical strategy comparing Florida agencies with successful and unsuccessful unionization elections, he finds statistically insignificant and relatively small effects of unionization on misconduct. In Section 4 below, we discuss the relationship between our paper and Goncalves (2020) in detail and seek to reconcile the apparently contrasting findings.

Our paper proceeds as follows. Section 2 describes the relevant legal developments relating to collective bargaining rights under Florida law, and how they provide the basis for our empirical strategy. Section 3 details our data sources. Section 4 presents our results. Section 5 concludes.

2) Empirical Strategy

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. 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 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.

Although the *Williams* decision has been noted in prior literature, it has not previously been used to construct a quasi-experimental framework. Pynes and Corley (2006, p. 299) highlight *Williams* as part of the "unusual history of collective bargaining" rights in Florida in an historical account of collective bargaining rights among Florida law enforcement agencies. 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.[8] We elaborate on our empirical design below.

2.2) **Williams** *as a Source of Exogenous Variation in Collective Bargaining Rights*

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 are either associational[9] or exploit potentially endogenous unionization decisions, creating challenges for causal inference.[10] For example, one source of potential endogeneity is that agencies in which officers anticipate an increased number of violent incidents in the future, or an increased probability of their detection, may be more inclined to unionize. At the same time, it may be the case 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. An event-study framework such as that used in some of the prior literature (and in Section 4.5 below) to analyze outcomes before and after unionization can address many of these concerns, but not all (for instance, the possibility of unobserved anticipation of future changes remains). Our primary approach avoids these challenges altogether

[8] 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.
[9] 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). Likewise, 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).
[10] See, for example, Anzia and Moe (2014). Other work (e.g. Ichniowski, Freeman, and Lauer 1989; Frandsen 2016) uses changes in state law with respect to public sector unions, which are exogenous with respect to individual unions but may be affected by changes in state-level factors that also affect the outcome variables. Our approach holds state-level factors constant by focusing on quasi-experimental variation across agencies in the same state.

by focusing on the (exogenous) conferral of collective bargaining *rights* rather than on (potentially endogenous) unionization.

Our strategy requires basic 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.[11] 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).

It is worth noting several factors that tend to dampen the estimate obtained using this approach. First, Florida is a right-to-work state, which generally limits the ability of employees to organize effectively.[12] 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,[13] and thus leaves less space for collective bargaining to do the same. 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. Fourth, a prior case – holding that appointed deputies of court clerks were "employees" under the statute granting employees

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

[12] In Florida, 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."

[13] 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 complements 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.).

collective bargaining rights – may have led to anticipation of the *Williams* outcome and perhaps to bargaining in the shadow of that outcome.[14] Finally, for roughly eight years following *Williams*, Florida sheriffs claimed the unilateral authority to resolve bargaining impasses with deputy unions, creating uncertainty about the extent of union power.[15]

This legal background makes it more difficult to detect any effects of collective bargaining rights on law enforcement behavior. In addition, 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, 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 factor dampening our estimate.

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

[14] See *Service Employees International Union Local 16, AFL-CIO v. Public Employees Relations Commission* (752 So.2d 569 [2000]). 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*.

[15] Parties to collective bargaining reach a bargaining impasse when they cannot agree on the terms of the employment contract. Florida law provides that a public entity's "legislative body" will resolve any impasse between the entity and one of its public employee unions (F.S.A. § 447.403). Once deputies began to organize in Florida, there was disagreement 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 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])). According to conventional thinking, deputy unions have more bargaining power when the county's commissioners resolve impasses than when the sheriff does.

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 that 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. Using a field indicating when the FDLE received case-related documentation, we are able to fill in missing year information for most of the complaints. 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 trigger the greatest public concern. They are also most distinctively characteristic of law enforcement activity and arguably less susceptible than other incidents to changes in reporting behavior that might accompany unionization. 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.[16] 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).[17]

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." 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 is 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.[18]

As discussed in Section 2 above, 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.[19] 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

[16] 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.
[17] 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.
[18] The results for nonviolent incidents are discussed in Section 4.4.3.
[19] The results when including corrections officers are discussed in Section 4.4.3.

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 their demographic characteristics. The employment database also records separations (i.e., the departure of officers from law enforcement agencies) and provides codes indicating the reason for each separation. We extract information about separations to construct a variable that reflects the number of separations at the agency-year level that are coded as being due to an "agency policy violation" (as described in more detail in Section 4.4.2). We also construct a variable that captures the number of involuntary separations that occur close in time to a violent incident involving that officer (as described in more detail in Section 4.5.3).

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 covers all law enforcement agencies in the state, including both SOs and PDs. The CJAP database reports extensive information about each agency at the agency-year level.[20] Most important for our purposes – and in particular for the event-study analysis of unionization in Section 4.5 – it records whether a collective bargaining unit (CBU) existed for sworn officers in each law enforcement agency in Florida in each year over the period

[20] 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 very limited.

2000-2015. A CBU, as used in CJAP, is an employee organization that represents the interests of officers vis-à-vis management, typically but not exclusively through collective bargaining. The existence of a CBU in a particular agency-year therefore indicates a high likelihood that the agency was operating under the terms of a CBA at that time.

Unfortunately, CBU status was not recorded in surveys prior to 2000. Moreover, only about half of agency-years over 2000-2015 have a clear "yes" or "no" entry.[21] To address the large number of missing values, we use alternative imputation procedures. Our baseline characterization of CBU status involves a presumption that the status quo persists until a different nonmissing value is encountered; for instance, if an agency has missing values for 2000 to 2004 and 2006-2007, "no" in 2005 and "yes" in 2008 and thereafter, then the imputed value of the CBU variable is missing in 2000-2004, 0 in 2005-2007, and 1 in 2008-2015.[22] We also use an alternative approach that presumes that missing values indicate the absence of a CBU. In the example above, this approach would impute values of the CBU variable of 0 in 2000-2007 and 1 in 2008-2015. This alternative approach leads to very similar results for the analysis using CBUs (as described in Section 4.5).

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. The sample, which consists of 6217 observations on 316 agencies, is the maximal sample available for analysis, and excludes agency-years recorded as having zero law enforcement and concurrently certified officers. It should be noted, however, that the fixed-effects Poisson regression that we use below

[21] For instance, all agencies have missing values in 2012. In 2008, the only nonmissing values are "yes" entries, with missing values for all other agencies. In addition, many agencies have further missing values for some years.
[22] We also impute CBU = 0 for SOs (other than those noted below as having obtained collective bargaining rights by special dispensation) prior to *Williams*.

omits certain observations, such as agencies for which the outcome variable is always zero. The estimation samples are therefore typically somewhat smaller.

The control group consists of the 258 PDs for which data is available. In defining SOs for purposes of this analysis, we account for the fact that nine of the 67 SOs in Florida had obtained county-specific legislation before 2003 allowing them to engage in collective bargaining.[23] 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.

A first step in the study is to verify that *Williams* did indeed impact unionization activity among SOs. Figure 2 plots the fraction of SOs and PDs with CBUs, 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, unionization activity begins among SOs within the same year. The fraction of SOs with CBUs keeps rising sharply for about three years, before stabilizing around 2006 (although there is a small decline in 2015). Overall, 26 of the 58 SOs in our treatment group unionize at some point over 2003-2015; these 26 SOs employ about 70% of sheriffs' deputies in our sample. Another important point to note from Figure 2 is that the fraction of PDs with CBUs remains quite stable, at a little over a half, throughout this period. This suggests that, while unionization may potentially affect outcomes for PDs, this impact is unlikely to have changed before and after *Williams*.

4) Results

4.1) Empirical Specification

In implementing the 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

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

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 in these circumstances to use a specification that better accommodates count data (e.g., Wooldridge 2002, p. 645).

In particular, we use a fixed-effects Poisson model:

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

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 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 nine 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.[24] $\mathbf{X}_{it}$ is a vector of control variables, described below. μ_i is an agency fixed effect and δ_t is a year fixed effect, while ϵ_{it} is a residual.

The specification in Equation (1) essentially models the conditional expectation – denoted $\ln(\mathrm{E}[Y_{it}|X])$ – of the number of violent incidents (given the right-hand-side variables) as being linear in the independent variables.[25] It is important to emphasize, however, that the coefficient estimates are generated using a Poisson framework. In particular, we compute robust standard errors that are clustered at the agency level, which implies that our results are obtained using Poisson quasi-maximum likelihood estimation (QMLE). Poisson QMLE is known to have attractive robustness properties; the coefficients can be estimated consistently under fairly general conditions even when the Poisson distributional assumptions for ϵ_{it} are violated (Wooldridge 2002, p. 649). For instance, the Poisson distribution assumes that the variance is equal to the mean, although in many applications of count data the variance exceeds the mean, a situation referred to as "over-dispersion." Over-dispersion may lead to standard errors that are too small, but cluster-robust standard errors correct for this (e.g., Cameron and Trivedi 1998, pp. 63-65). In general, the

[24] 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 the violent incident rate yields quite similar results, however (as reported in Section 4.4.3).

[25] We are grateful to an anonymous referee for suggesting this alternative interpretation.

fixed-effects Poisson estimator is consistent under standard assumptions regarding the conditional expectation, regardless of the distribution of the error term (Wooldridge 1999; Wooldridge 2002, p. 675).

The control variables in $\mathbf{X}_{it}$ include 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.[26]

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.

4.2) Tests for Parallel Trends

A crucial assumption of our difference-in-difference approach is that the post-2003 trends in the latent index (i.e., the unobserved propensity for violent incidents) for SOs and PDs would counterfactually (i.e., absent *Williams*) have been identical.[27] As this assumption cannot be directly

[26] 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.

[27] 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. However, the popularization of filming law enforcement with smartphones appears to have occurred too recently to explain our results (Ouss and Rappaport, 2019).

tested, the standard practice is to examine whether SOs and PDs experienced similar trends in violent incidents prior to *Williams*. Placebo tests (or false experiments) that test for "effects" for years such as 2000 and 2001 – over the pre-*Williams* period – yield insignificant coefficients that are close to zero, which suggests the absence of differential prior trends. To explore this question further and to 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 compute 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. This is a simplified version of Equation (1) that de-means the data and controls for common time shocks and changes in the size of agencies. 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.

To smooth out the noise apparent in the mean residual, Figure 3 also plots the lines of best fit for the residuals from the simple Poisson regression described above, separately for SOs and PDs over 1996-2002.[28] These trends are fairly closely parallel prior to *Williams*. For the post-*Williams* period, we use a more flexible local polynomial approach with a quadratic specification, as a larger number of years is available. 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.

In Figure 3, there is a noticeable spike in the residual of violent incidents at SOs in 2006.[29] To address possible concerns about this year, Figure 4 shows the same plots and trend lines as those in Figure 3, but with all data for 2006 omitted (and with linear trend lines for 2003-2005 and 1996-2002, and a local polynomial approach with a quadratic specification for 2007-2015). Although the increase is not as dramatic, it remains the case that the mean residual of violent

[28] Note that the resulting time trends use the residuals at the agency level for each year rather than the mean residuals averaged over all SOs and over all PDs.

[29] Given the unusual nature of this spike, it is important to determine whether it may be attributable to measurement error or 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 did not change dramatically in 2006 relative to prior years.

incidents rises substantially for SOs following *Williams*, especially in the immediate post-*Williams* years. Thus, the basic pattern in Figure 3 is robust to excluding data from 2006. Moreover, the regression results are robust to excluding all observations for 2006 (as reported in Table 2, Column 3 and discussed further below).

An alternative approach to test for parallel pre-trends (and to study the time pattern of the effect) is to estimate the difference between the number of violent incidents at SOs and PDs (controlling for agency and year fixed effects and the variables in $\mathbf{X}_{it}$) for each year of our sample period. We implement this approach by modifying Equation (1) as follows, using 2002 (the year immediately prior to *Williams*) as the omitted year:

$$Y_{it} = \exp\left(\sum_{\substack{j=1996,\\ j\neq 2002}}^{2015} \xi_j (SO_i * Yearj_t)_{it} + \zeta Officers_{it} + \eta \mathbf{X}_{it} + \mu_i + \delta_t\right)\epsilon_{it} \tag{2}$$

where $Yearj_t$ is an indicator variable = 1 if $t = j$ and zero otherwise.

Figure 5 shows the estimated coefficients of $(SO_i * Yearj_t)_{it}$ for a ten-year period around *Williams* (1998-2007), though the regression specification from which these estimates are obtained includes interactions between SO_i and year dummies for each year from 1996-2015, apart from the excluded year of 2002 for which the coefficient is normalized to zero. Prior to *Williams*, the estimated coefficients are mostly close to zero, indicating that there are no detectable pre-trends. After *Williams*, the estimated coefficients are all positive and the coefficient for 2006 is (individually) statistically significant. While coefficients for other years are not individually statistically significant, the regression results discussed below show that the coefficients for the post-*Williams* years are jointly statistically significant.

In Figure 3, 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 3 whether this results in a long-run level that is similar to or higher than the pre-*Williams* level. The estimates from Equation (2) for later years (beyond those shown in Figure 5) provide direct evidence on this question. For 2008-2015, the estimated coefficients are mostly positive and are of borderline statistical significance for some years, although they are generally smaller in magnitude than those for the immediate post-*Williams* years. This suggests that there is some degree of long-run persistence of the baseline effect, although at the same time it weakens over time. Note, though, that the latter may be due to changes in reporting behavior that may lag changes in actual misconduct. For instance, the probability of a complaint being sustained by a local agency may decline gradually over time, leading to an apparent weakening of the *Williams* effect in later years even if the prevalence of actual violent misconduct at SOs remains persistently higher.

4.3) Basic Regression Results

The results from the specification in Equation (1) are reported in Table 2. The maximal sample over 1996-2015 consists of 6217 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 4,681 observations on 238 agencies.[30] As discussed earlier, the reported standard errors are robust and clustered at the agency level. The variable of interest is the interaction of a post-*Williams* dummy with a dummy for SOs. This has a positive coefficient of about 0.34 that is statistically significant at the 5% level.

As the Poisson specification takes an exponential form, interpreting the magnitude of the estimated effect is not straightforward, as it potentially depends on the values of all the independent variables. The impact of *Williams* (i.e., of a one-unit increase in $(Post_t * SO_i)_{it}$) on violent incidents in percentage terms can be approximated by $100(e^{0.34} - 1) \approx 40\%$, holding all other independent variables fixed. That is, the estimated coefficient implies that collective bargaining rights lead to about a 40% increase in incidents of violent officer misconduct. While this may seem surprisingly large, the baseline frequency of recorded violent incidents is low. The mean number of recorded 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 recorded incidents per year for a typical SO, from 0.5 to 0.7. Of course, the total number of incidents, including those that never trigger investigations or are not reported to the FDLE, is necessarily higher. Thus a 40% increase could represent a much larger number of actual additional incidents.

It is possible to compute more formally the marginal effects corresponding to the estimated coefficient, taking account of the fact that the variable of interest is an interaction term. The average marginal effect (AME) averages the change in the number of violent incidents due to

[30] In addition, nine agencies that have some missing values for certain control variables. Note, however, that the results in Column 1 are virtually identical when omitting these agencies and using a fully balanced panel.

Williams over all values of the other independent variables, while the marginal effect at the mean (MEM) evaluates the change in the number of violent incidents due to *Williams* at the mean values of the other independent variables. Both of these approaches yield a marginal effect of about 0.2 – i.e., an increase of about 0.2 in the number of violent incidents at SOs (relative to PDs) following *Williams*. This estimated marginal effect 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*.

The baseline analysis in Column 1 uses the full sample period 1996-2015. This is fairly long, especially for the post-*Williams* period. In Column 2, 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 is apparent in Figure 3, there is a noticeable spike in violent incidents at SOs in 2006. Reassuringly, the regression results are robust to excluding all observations for 2006 (as reported in Column 3 of Table 2). The estimated effect of *Williams* on violent incidents is positive and statistically significant, though somewhat smaller in magnitude. Thus, it does not appear that any unusual factors specific to 2006 are driving the baseline result.

Our baseline specification (Equation (1)) models the number (or count) of violent incidents (while controlling for the number of officers). An alternative approach is to model instead the rate of violent incidents (i.e., the number of violent incidents scaled by the number of officers). In a Poisson framework, the rate is modeled by modifying Equation (1) to include the natural logarithm of the scaling variable – termed the "exposure" variable, in this case the number of officers – while constraining its coefficient to equal one (e.g., Cameron and Trivedi 1998). Column 4 of Table 2 shows the results from this specification. The basic result is robust to modeling the rate (rather than number) of violent incidents.

4.4) Robustness Checks and Extensions

Our baseline result withstands a variety of robustness checks, some of which have been noted earlier in the paper. The key tests are reported in Table 3 but several others are briefly summarized and not reported in the interests of brevity.

4.4.1) Potential Measurement Error in Measuring Violent Incidents

As noted in Section 3 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 1 of Table 3 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 2 of Table 3, is again statistically significant and similar in magnitude to the baseline effect.[31]

4.4.2) Potential Changes in Reporting or Enforcement Practices

As noted previously, unionization – or changes in bargaining power in the shadow of collective bargaining rights – may decrease the likelihood that complaints are sustained at the local agency level. This would tend to dampen the estimated impact of *Williams* on our measure of violent incidents, which consists primarily of complaints sustained by local agencies.

[31] 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 violence-related misconduct occurred but where 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.

On the other hand, a potential alternative explanation for a post-*Williams* increase in reported violent incidents within SOs is that unionization (or its possibility) may result in greater bureaucratization of the investigation process. Even with no increase in actual misconduct, incidents may begin to be recorded, investigated, and reported to the FDLE when previously they would not have entered the dataset. As our analysis relies on incidents reported to the FDLE, it is not possible to address fully concerns of this nature. It is possible, however, to test one potentially important variant of this hypothesis: that conduct the sheriff may have sanctioned through a separation pre-*Williams* is (correctly) reported to the FDLE post-*Williams* as a moral character violation. All separations, including those for moral character violations, are recorded in the ATMS database. But a separation involving a moral character violation could be coded by the sheriff as something less serious – for instance, as an agency policy violation – to avoid conflict or to enable the officer to move to a different agency.

To address this possibility, we use the ATMS employment database to construct a measure of separations coded as agency policy violations but *not* as moral character violations. The alternative hypothesis would imply that the number of these separations would decline after *Williams*, as the underlying conduct begins to be reported correctly as moral character violations. As shown in Column 3 of Table 3, the estimated coefficient for these separations is instead positive and statistically insignificant. The 95% confidence interval is approximately [-0.09, 0.51]. Thus, it is possible to rule out at the 95% level a decline in this type of separations of more than 0.09, which represents only about a quarter of the estimated baseline effect on violent incidents (the coefficient of 0.34 in Table 2, Column 1). In other words, any post-*Williams* substitution from recording incidents as agency policy violations to recording them as moral character violations can at most explain only a relatively small fraction of the baseline effect.

4.4.3) Alternative Samples and Outcomes

Here we consider three alternative samples. First, as previously discussed, the baseline analysis excludes corrections officers, who are mostly found in SOs. It uses data on violent incidents involving only officers certified solely in law enforcement or concurrently certified in corrections. Omitting concurrently certified officers – and using violent incidents involving law enforcement officers only – leads to essentially identical results. Adding violent incidents involving corrections officers to our measure – so that it encompasses violent incidents associated with law enforcement, corrections, and concurrently certified officers – leads to a post-*Williams* time pattern that is quite similar to that in Figure 5, including a statistically significant coefficient in 2006. It is more difficult to rule out pre-existing trends in violent incidents, however, than is the case in Figure 5. In particular, the estimates are mostly positive before as well as after *Williams* and are larger for some prior years than for some post-*Williams* years. Overall, this reinforces our earlier argument that including corrections officers' violent incidents tends to make SOs and PDs less comparable and so undermines the premises of the difference-in-difference framework.

Second, a significant fraction (about 20%) of violent incidents in our data involve domestic violence. There is a strong conceptual case for including these incidents, which are likely to involve civilian victims. In any event, the results are quite similar when violent incidents involving domestic violence are omitted from our dataset.

Third, as discussed in Section 3, we separate out a category of misconduct that involves express or implied violence. For the residual category of nonviolent incidents of misconduct, there is no discernible differential impact of *Williams* for SOs relative to PDs. This may seem puzzling, as the deterrence mechanism we hypothesize – and for which we present support below – should in principle apply to nonviolent as well as violent misconduct. But as we observed in the introduction, if collective bargaining rights decrease the probability of detecting police misconduct, and misconduct therefore rises, *observed* misconduct could rise, fall, or remain constant. There is no reason the net effect of lower detection rates and higher misconduct rates will be the same for all categories of misconduct. If collective bargaining rights make detection of misconduct more difficult, it is possible, for example, that sheriffs will prioritize the detection of their deputies' violent misconduct over the detection of nonviolent misconduct. Thus, complaints sustained at the local agency level could fall more sharply after *Williams* for nonviolent incidents than for violent ones. This would make it more difficult to detect an effect for nonviolent than for violent incidents, even if actual nonviolent misconduct were to increase.

The absence of an effect for nonviolent incidents may help reconcile our results with the contemporaneous work of Goncalves (2020). In pertinent part, Goncalves (2020) analyzes the impact of unionization on police misconduct using FDLE data from Florida. While he finds no statistically significant effect of unionization on misconduct – and rules out a positive effect greater than about 10% to 20% – his study differs from ours along several important dimensions. First, his misconduct measure is not restricted to violent incidents. Indeed, his result is fairly consistent with what we find when using nonviolent incidents – or all incidents, both violent and nonviolent

– as our outcome variable. We nonetheless maintain that violent misconduct is of particular public and policy concern and warrants separate analysis. Second, Goncalves' research question concerns unionization rather than collective bargaining rights. The empirical strategy – using hand-collected data on unionization elections – involves comparing Florida agencies in which unionization elections are successful to those in which they are not. It does not use the variation in collective bargaining rights across SOs and PDs created by *Williams*. As we have argued, the *Williams* decision provides a source of exogenous variation that reveals the causal impact of collective bargaining rights in a manner that cannot be readily replicated in other ways.

4.5) Exploring Potential Mechanisms

What mechanisms might explain our results? There are several paths of inquiry here. First, to what extent are the results driven by actual unionization rather than the simple conferral of collective bargaining rights? Second, whether due to actual or potential unionization, is the causal mechanism a reduction in deterrence of law enforcement misconduct or a compositional effect on the types of officers who join particular agencies? The next three subsections address these questions.

4.5.1) The Role of Unionization

Testing whether unionization leads to more violent misconduct is fraught with the potential endogeneity issues raised in Section 2. While acknowledging these concerns, we seek to mitigate them to the extent possible by focusing on the precise timing of the formation of CBUs within an event-study framework. We use the CBU variable described in Section 3 to identify the first year (denoted t_i^*) in which agency i is recorded as having a CBU. A substantial number of agencies never have CBUs; for these agencies, the typical pattern is for a CBU to be organized at some point within our sample period and for the agency to remain unionized for the remainder of the sample period. A few agencies that previously unionized revert to non-union status at the very end of our period (in 2015); these agency-years are omitted from our analysis below.[32]

We use t_i^* to construct a series of event-time dummies that we denote by b_{it}^k, where $b_{it}^k = 1$ when agency i is k years before or after t_i^* in year t (and zero otherwise). Note that these event-

[32] There are 28 agencies out of 316 that revert from being unionized to being non-unionized in 2015. Because there is typically at most one unionization event per agency, we do not use what is termed a "stacked" event-study approach in which each unionization event is treated as a separate observation. The gain from doing so would be minimal even for the small number of agencies that cease to be unionized because there are no post-event observations following de-unionization in 2015.

time dummies differ across agencies in a given year: for instance, the event-time dummy for being 2 years after a CBU is first organized may take the value 1 in 2006 for agency A (which unionized in 2004), while the same event-time dummy takes the value 1 for agency B (which unionized in 2006) in 2008. In the reported results, k takes on values from -5 to 5. The data are binned at the endpoints such that k = -5 includes all observations that are 5 or more years before unionization and k = 5 includes all observations that are 5 or more years after unionization. The event-year immediately prior to unionization (k = -1) is excluded; the coefficient is normalized to zero and used as the baseline.

Some agencies had a CBU in the first year for which data is available, which is typically 2000. For these agencies, the event-time dummies cannot be defined, as we have no information on when unionization occurred. Thus, these agencies are omitted from the analysis. For agencies that never unionized, all event-time dummies are equal to zero. The reported results include these agencies, as their inclusion increases the precision of the estimates of the control variables without affecting the estimation of the coefficients of the event-time dummies. The results are very similar when agencies that never unionized are omitted, however.

The event-study specification that we use can be expressed as:

$$Y_{it} = \exp\left(\sum_{\substack{k=-5,\\ k\neq -1}}^{5} \varphi_k b_{it}^k + \nu Officers_{it} + \omega \mathbf{X}_{it} + \mu_i + \delta_t\right)\epsilon_{it} \tag{3}$$

where, noting that $1\{.\}$ is the indicator function:

$$b_{it}^k = \begin{cases} 1\{t - t_i^* \leq -5\} \text{ if } k = -5 \\ 1\{t - t_i^* = k\} \text{ if } k \in [-4, 4] \\ 1\{t - t_i^* \geq 5\} \text{ if } k = 5 \end{cases} \tag{4}$$

The estimated coefficients of the event-time dummies from Equation (3) are shown in Figure 6. This regression uses data for all agencies (PDs as well as SOs), but the patterns are very similar when the sample is restricted to SOs.

Two points are noteworthy. First, there appears to be a decreasing trend in violent incidents before unionization. This may indicate "positive" selection into unionization (i.e., that agencies whose officers choose to unionize are those with declining levels of violent misconduct). This implies that a naïve model of violent incidents on CBU status would tend to underestimate the causal impact of unionization on violent incidents because the counterfactual is likely to be that incidents would have continued to decline but for unionization. Second, and most important for our purposes, there is an increase in violent incidents following unionization that is statistically significant in some years, notwithstanding the declining trend before unionization. While it remains possible that officers choose to unionize because they anticipate a future increase in violent incidents – even though their level of violent incidents has been declining in the recent past – Figure 6 suggests that actual unionization is likely to be a mechanism through which the effect of collective bargaining rights operates.

Nonetheless, actual unionization does not appear to be the entire explanation. If we restrict the treatment group in Equation (1) to SOs whose officers never unionized, there is still what appears to be a substantial effect of *Williams*. Although it is only of borderline statistical significance – and the choice not to unionize is potentially endogenous – this seems broadly consistent with the idea of bargaining in the shadow of the law. The right to unionize gives non-unionized deputies more bargaining power, as sheriffs may be 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.

4.5.2) Testing for Compositional Effects

Determining that unionization appears to be largely, though not entirely, responsible for our results still does not tell us *how*, exactly, the causal mechanism works. One possibility is that the conferral of collective bargaining rights led to compositional effects on the types of officers who are attracted to joining particular agencies, especially those that are unionized versus those that are not. This seems plausible because the number of new hires and the number of voluntary separations at SOs relative to PDs increased around the time of *Williams* (though this appears to be a continuation of a pre-existing trend, rather than a causal effect of *Williams*). Increased turnover may have involved an influx of new officers with potentially different characteristics, such as being more prone to violent incidents or aggressive policing, into SOs post-*Williams*.[33]

We emphasize that changes in the composition of officers *per se* are not a challenge to the basic result. They are instead better viewed as a mechanism through which the treatment effect of

[33] A related, but somewhat distinct, possibility is that the number of new officers *per se* increases violent incidents due to inexperience. Adding the number of new hires to the basic specification in Equation (1), however, leads to a coefficient virtually identical to the baseline result.

collective bargaining rights or unionization may operate on the treatment group. A potential concern, however, is that the treatment may in some circumstances affect the control group. In particular, suppose that officers with a high risk of involvement in violent incidents tend to choose unionized agencies. Unionization among SOs may then have induced violence-prone PD officers to move from unionized PDs to unionized SOs, as *Williams* eliminated a particular disincentive to join SOs. In this scenario, the treatment may have affected the control group through a compositional effect that reduced the number of violence-prone officers.[34]

We test for compositional effects by constructing an alternative measure of violent incidents that includes only violent incidents associated with officers who were hired prior to *Williams*. The ATMS employment database provides the exact hiring date, so we can omit all violent incidents involving officers hired from January 2003 onwards. This eliminates the effects of post-*Williams* sorting of officers across agencies and excludes compositional effects more generally. As reported in Column 4 of Table 3, the estimate using this alternative measure of violent incidents is virtually identical to the coefficient in the baseline specification in Table 2, Column 1. This implies that our basic result is not driven by compositional effects and suggests that a deterrence mechanism may be at work instead. The next subsection explores direct evidence for the deterrence hypothesis.

4.5.3) Testing for Deterrence Effects

We have previously suggested that unionization may provide procedural protections that undermine detection and sanctioning of misbehaving officers (and that nonunionized deputies bargain in the shadow of these rights). The most direct path for this mechanism is the CBA, which may contain provisions – beyond those in Florida's LEOBOR – that make internal disciplinary investigations more difficult. To explore this, we hand-collected CBAs from Florida law enforcement agencies and identified several such provisions. 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,[35] preventing the agency from making

[34] 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.

[35] 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.

independent disciplinary decisions. Other rights include a tightened time limit on internal affairs investigations and expungement of old records, even when the officer is found to have engaged in misconduct.[36] These procedural rights raise the cost of terminating misbehaving officers and thereby lower deterrence. Our results are also consistent with the possibility that the political influence of unions leads to local legislation that embodies similar procedural protections. In addition, 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.

To test for deterrence effects, ideally we would measure whether the expected sanction attached to a given type of misconduct differed before and after *Williams*. As stated in the introduction, unions seek and often obtain procedural protections, so the most natural assumption is that the probability of a sanction falls after *Williams*; presumably, sheriffs' deputies would be reluctant to vote for a union if the probability would rise. But expected sanctions depend also on their severity, which is difficult to assess. While the ATMS database reports the sanctions (if any) imposed on officers by the FDLE, it does not report the sanctions imposed by local agencies. Also, the FDLE sometimes imposes no sanction even for serious misconduct because it views the local agency's sanctions as having been sufficient. Coding this as "no sanction" would be highly misleading. It is therefore impossible, with the data available, to measure the overall, combined sanctions imposed on officers for misconduct before and after *Williams*. We instead focus on a single, particularly salient employment-related sanction – whether officers are terminated for violent incidents. In particular, we test whether *Williams* affected the frequency with which officers at SOs are terminated for violent incidents relative to officers at PDs.

The ATMS employment database reports the date of separation and a code for the reason the separation occurred. From this code, we can infer whether a separation was involuntary. We construct a measure of the number of involuntary separations in an agency-year for which the

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.").

[36] 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.").

terminated officer was involved in a violent incident around the same time. In particular, we use a [-1, +3] interval, where a separation is up to one year before, or up to three years after, a complaint is opened regarding a violent incident in which the officer was involved. We use this measure of terminations associated with violent incidents at the agency-year level as the dependent variable in a specification similar to that in Equation (2). We include the number of violent incidents at the agency-year level on the right-hand side to scale the number of terminations. The estimated coefficients are shown in Figure 7. While the estimates are quite noisy, terminations seem to decline at SOs (relative to PDs) after *Williams*; this difference becomes statistically significant in 2009. While far from being conclusive, Figure 7 is broadly consistent with a decline in the likelihood of termination of officers involved in violent incidents after the conferral of collective bargaining rights.

We also use the measure of terminations associated with violent incidents as the dependent variable in an event-study specification similar to that in Equation (3), apart from the inclusion of the number of violent incidents on the right-hand side to scale the number of terminations. The estimated coefficients are shown in Figure 8. Again, the estimates are quite noisy. They tend, however, to be negative following unionization, indicating a decrease in the probability of termination conditional on a violent incident, and are of borderline statistical significance in some years. Overall, the evidence in Figures 7 and 8 is consistent with a decline in the likelihood of termination, and hence with a decrease in sanctions serving as a deterrence-based mechanism underlying our basic result.[37]

5) Conclusion

The determinants of law enforcement misconduct have become a question of wide interest to scholars, policymakers, and the public. We provide the first quasi-experimental evidence on the impact of collective bargaining rights on misconduct by law enforcement 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 serve as a control group for sheriffs' offices. Our estimates imply that collective bargaining rights

[37] Note that to the extent that unionization or collective bargaining rights make it less likely that a complaint is sustained for a given set of facts, the approach we use here would understate the decline in the likelihood of termination conditional on a violent incident: the denominator would decrease, mechanically increasing the probability of termination.

led to about a 40% increase in violent incidents of misconduct among sheriffs' offices. This result survives a wide variety of robustness checks and tests for alternative explanations.

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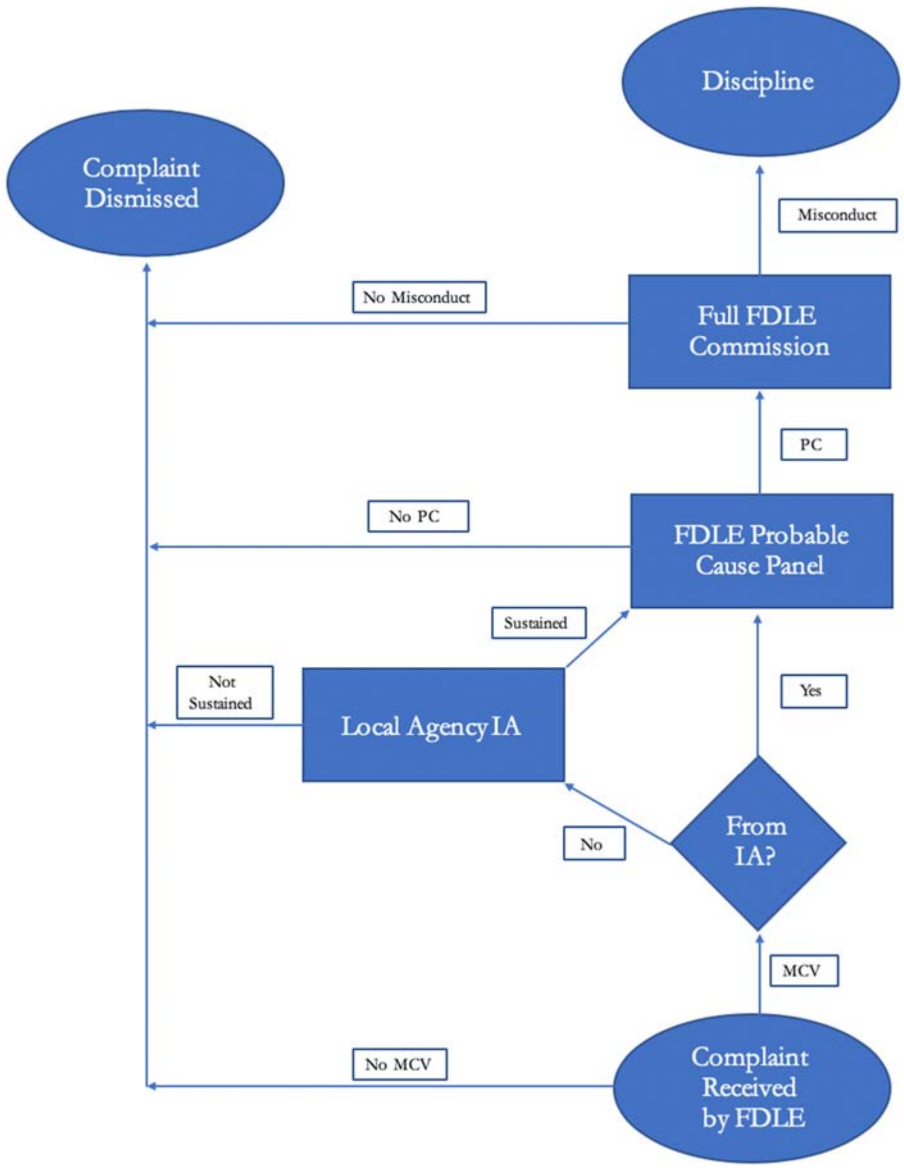
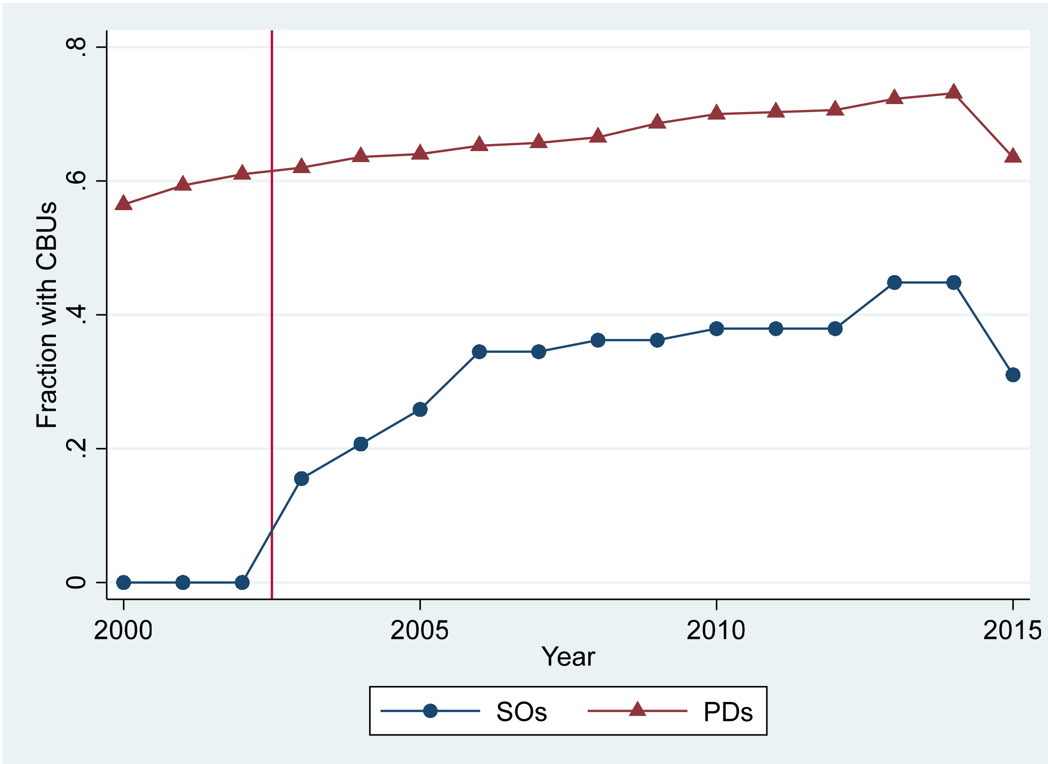
Figure 1: Flowchart of the FDLE Complaint Process



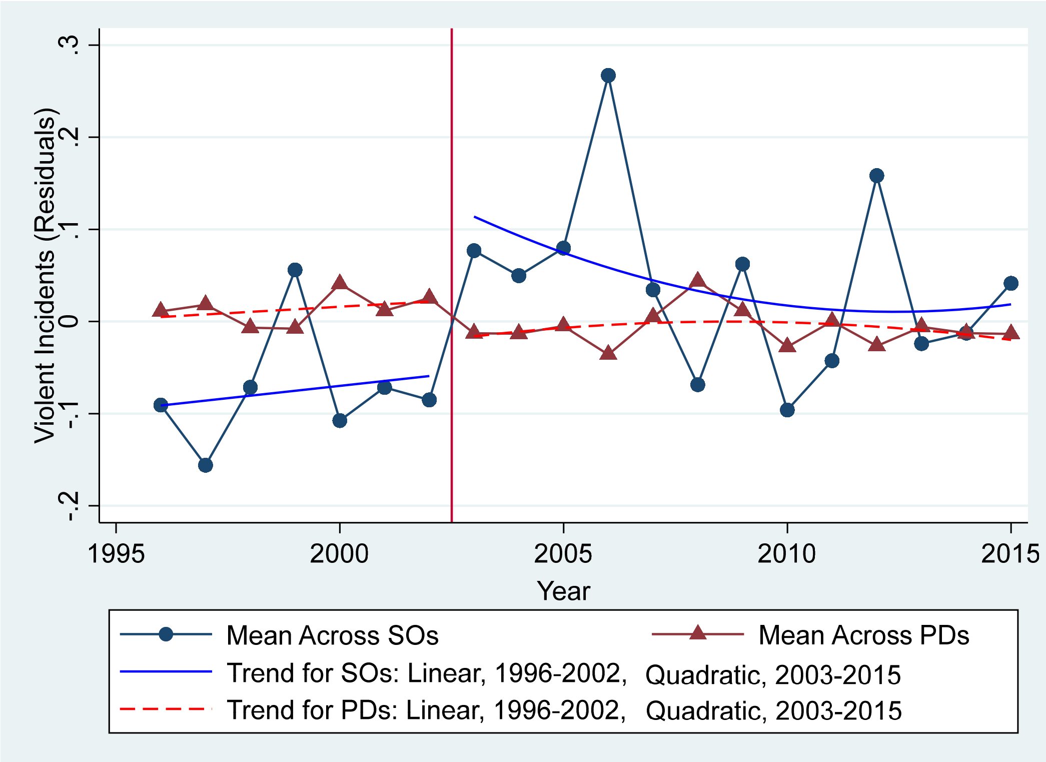
Note: This flowchart depicts a slightly simplified representation of the process by which the Florida Department of Law Enforcement (FDLE) handles misconduct complaints. The process begins at the bottom of the chart, with the receipt of a complaint. FDLE staff first screen out complaints that do not, 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: Collective Bargaining Units - Florida Law Enforcement Agencies, 2000-2015



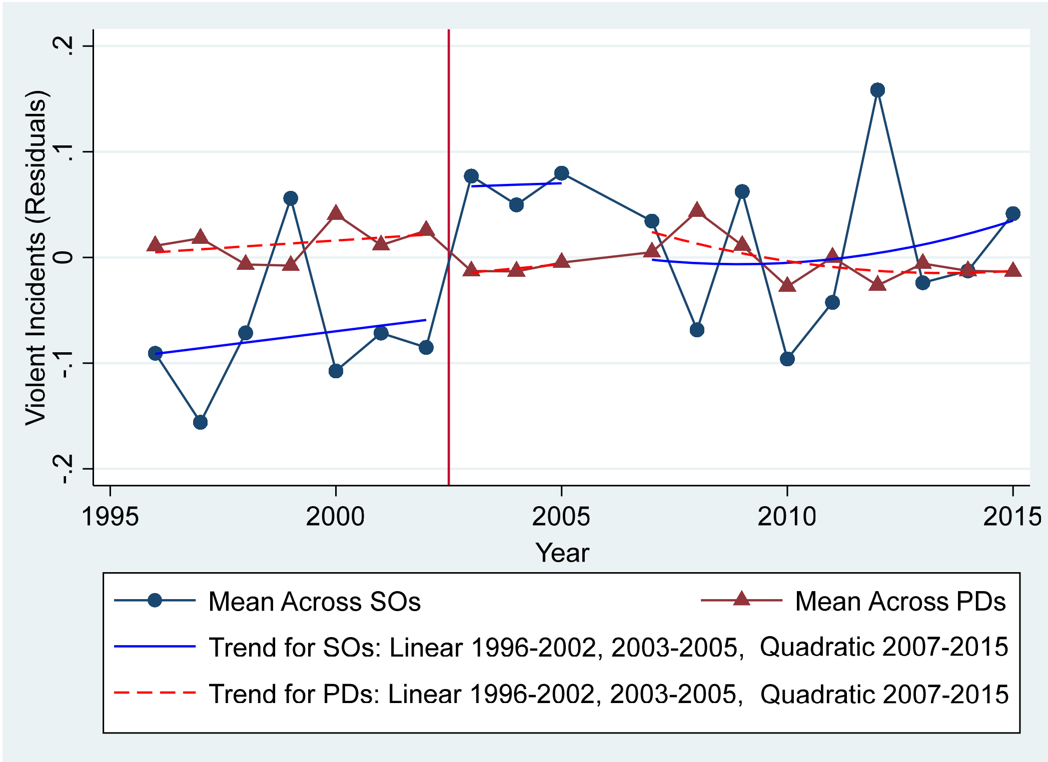
Note: This graph depicts the fraction of Florida law enforcement agencies for which the Criminal Justice Agency Profile (CJAP) data reports the existence of a collective bargaining unit (CBU). This fraction is reported separately for the treatment group of sheriffs' offices (SOs; excluding the nine SOs whose deputies 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: Trends in Violent Incidents, Florida Law Enforcement Agencies, 1996-2015



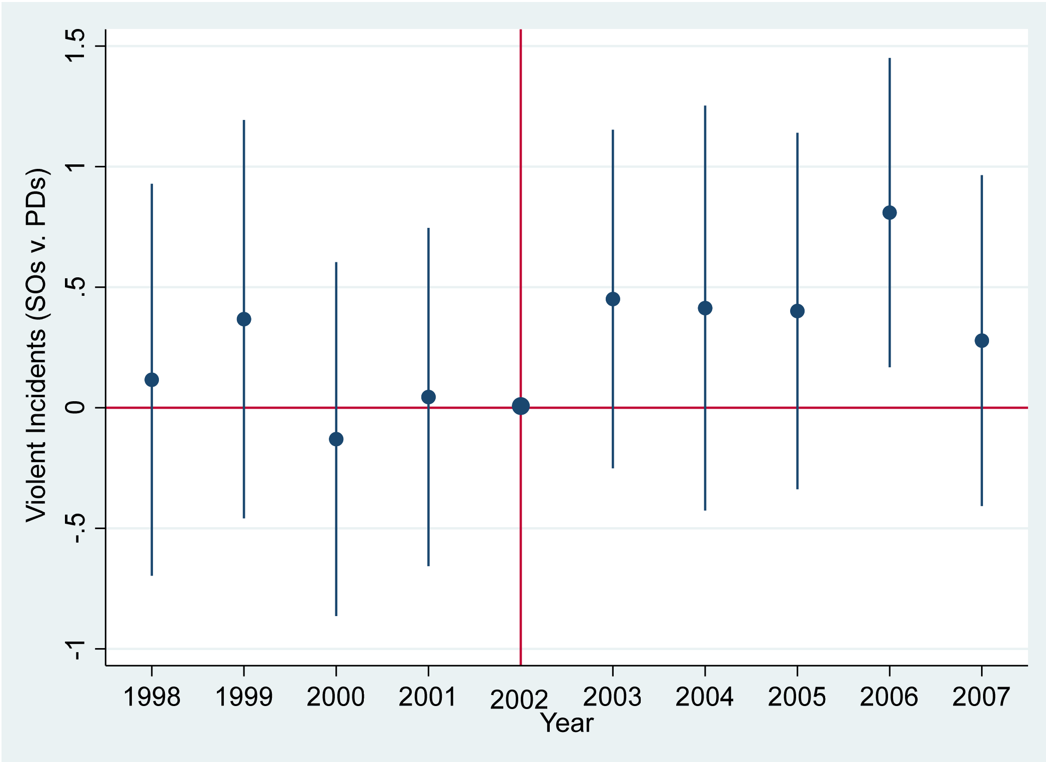
Note: This graph represents the time pattern of violent incidents in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database, separately for the treatment group of sheriffs' offices (SOs; excluding the nine SOs whose deputies obtained collective bargaining rights prior to 2003) and the control group of police departments (PDs). The mean across SOs and the mean across PDs are 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 smoothed trend lines are obtained by regressing these residuals 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).

Figure 4: Trends in Violent Incidents, Florida Law Enforcement Agencies, Excluding 2006



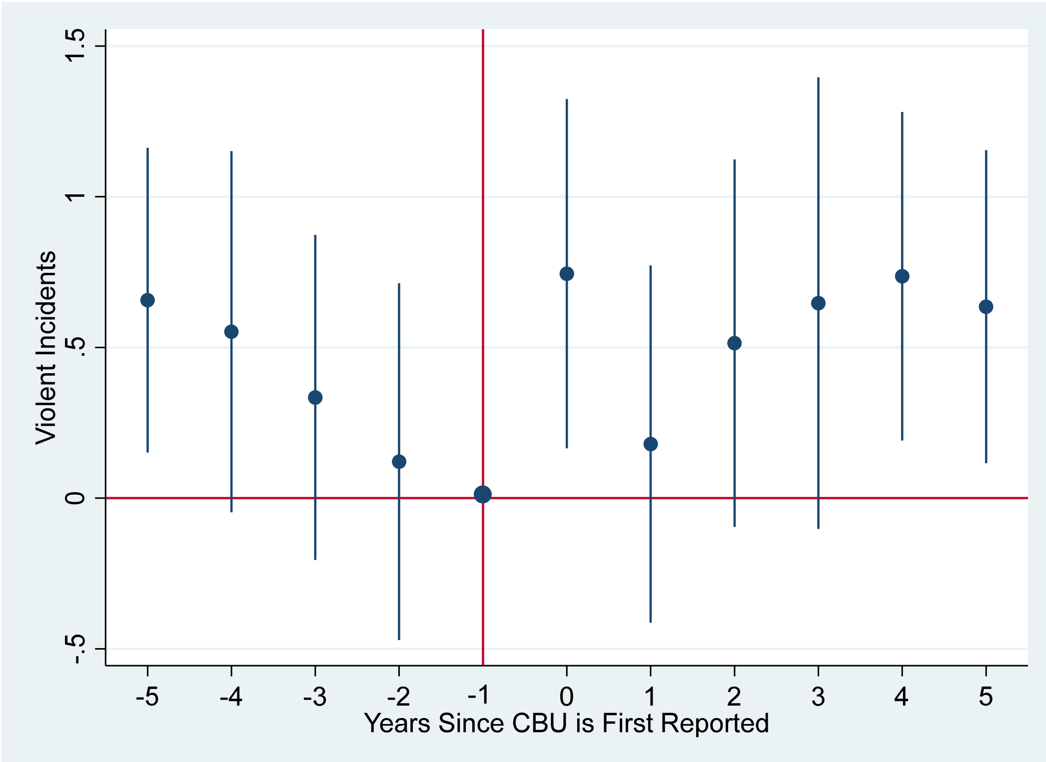
Note: This graph represents the time pattern of violent incidents in the Florida Department of Law Enforcement (FDLE) Automated Training Management System (ATMS) database, separately for the treatment group of sheriffs' offices (SOs; excluding the nine SOs whose deputies obtained collective bargaining rights prior to 2003) and the control group of police departments (PDs), excluding data for the year 2006. The mean across SOs and the mean across PDs are 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 smoothed trend lines are obtained by regressing these residuals on the year (separately for SOs and PDs), using linear specifications for the pre-*Williams* period (1996-2002) and for 2003-2005 and a local polynomial approach with a quadratic specification for 2007-2015. The vertical red line represents the year of the *Williams* decision (2003).

Figure 5: Violent Incidents at SOs Relative to PDs by Year, 1998-2007



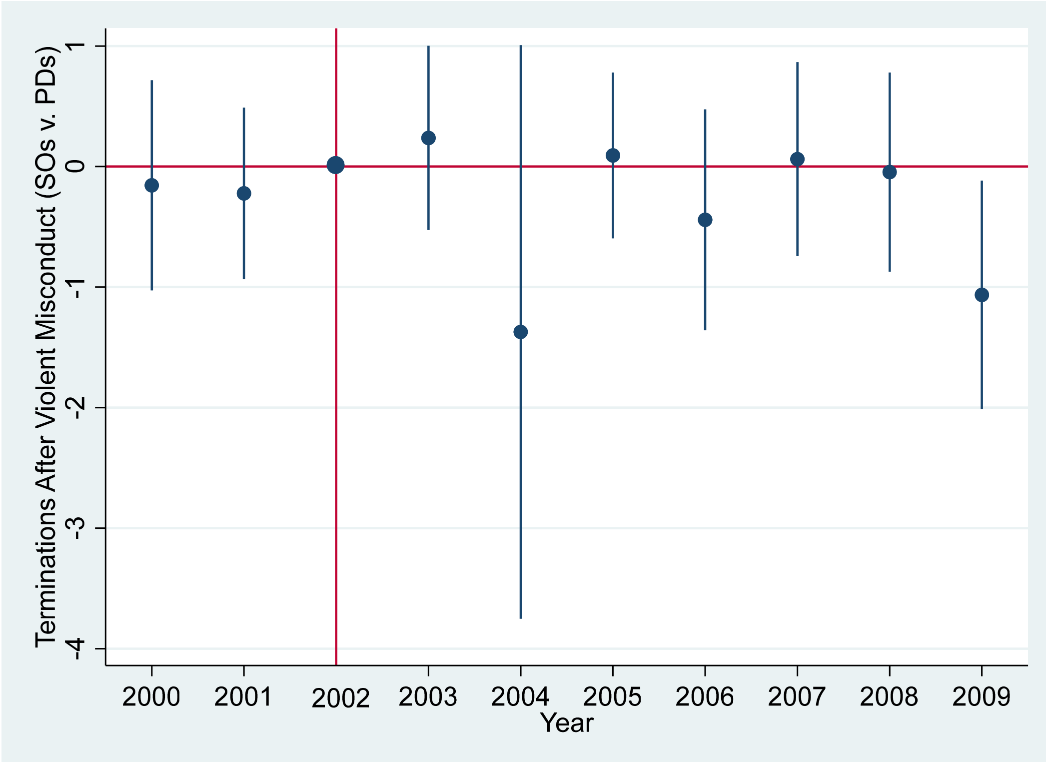
Note: This graph depicts the coefficients on interaction terms between an indicator for the treatment group of sheriffs' offices (SOs; excluding the nine SOs whose deputies obtained collective bargaining rights prior to 2003) and a series of year dummies. The excluded (baseline) year is 2002. The coefficients are obtained from the specification in Equation (2) – i.e. a Poisson regression of the number of violent incidents on agency fixed effects, year fixed effects, the number of law enforcement and concurrently certified officers associated with each agency-year, and the control variables specified in Equation (1). Although the graph shows only the ten years around *Williams*, the regression includes interactions between the SO indicator and year dummies for each year from 1996-2015 (apart from the excluded year of 2002). The vertical red line represents the baseline year (2002), immediately before the *Williams* decision.

Figure 6: Violent Incidents Before and After Unionization



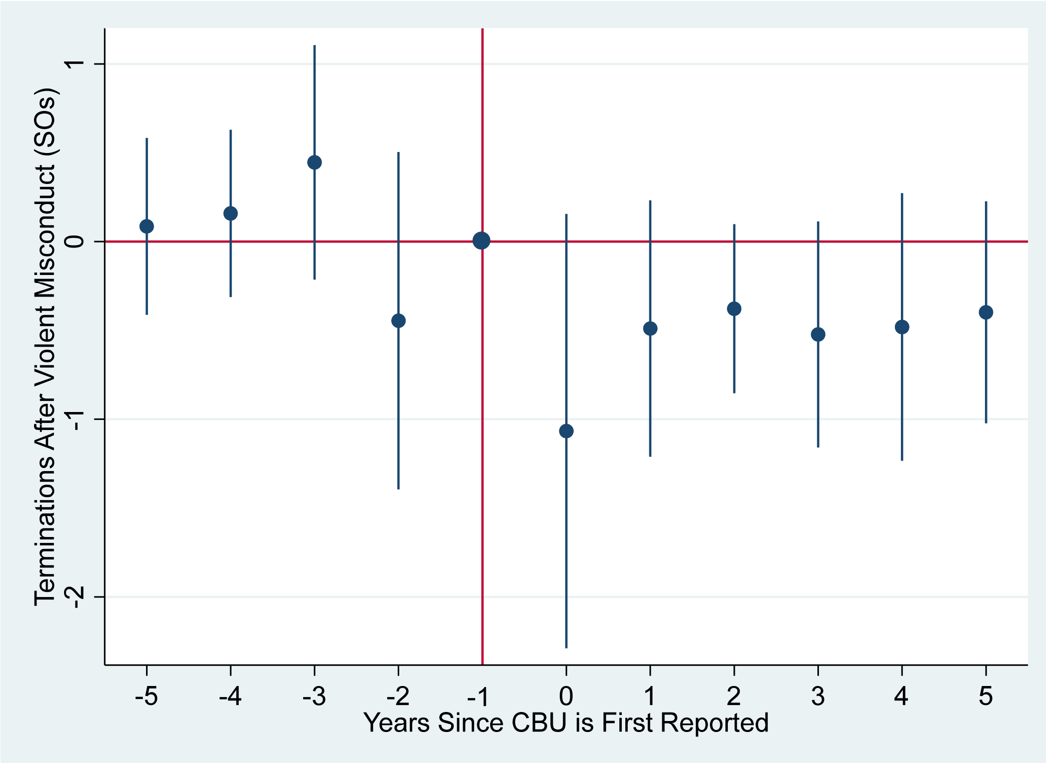
Note: This is an event-study graph showing the relationship between unionization and the number of violent incidents (using data for all agencies, i.e., both SOs and PDs). Unionization is defined as occurring in the first year for which the Criminal Justice Agency Profile (CJAP) data reports the existence of a collective bargaining unit (CBU). The event-study coefficients are based on the specification in Equation (3). The excluded (baseline) event-time indicator is -1 (i.e., the year immediately before a CBU is first reported). The event-time indicator -5 includes all years that are 5 or more years before a CBU is first reported, while event-time indicator 5 includes all years 5 years and more after that year. The estimation excludes all agencies for which the first nonmissing CBU entry is “yes” and includes never-treated agencies (i.e., those for which a CBU was never reported). The vertical red line represents the baseline event-time indicator -1 (i.e., the year immediately before a CBU is first reported).

Figure 7: The Impact of *Williams* on Terminations Associated with Violent Incidents



Note: This graph depicts the coefficients on interaction terms between an indicator for the treatment group of sheriffs' offices (SOs; excluding the nine SOs whose deputies obtained collective bargaining rights prior to 2003) and a series of year dummies. The excluded (baseline) year is 2002. The coefficients are obtained from a Poisson regression of the number of terminations associated with violent incidents on agency fixed effects, year fixed effects, the number of violent incidents, the number of law enforcement and concurrently certified officers associated with each agency-year, and the control variables specified in Equation (1). Although the graph shows only the years 2000-2009, the regression includes interactions between the SO indicator and year dummies for each year from 1996-2015 (apart from the excluded year of 2002). The vertical red line represents the baseline year (2002), immediately before the *Williams* decision.

Figure 8: Terminations Associated with Violent Incidents Before and After Unionization



Note: This is an event-study graph showing the relationship between unionization and the number of terminations associated with violent incidents for SOs (excluding the nine SOs whose deputies obtained collective bargaining rights prior to 2003). Unionization is defined as occurring in the first year for which the Criminal Justice Agency Profile (CJAP) data reports the existence of a collective bargaining unit (CBU). The event-study coefficients are based on the specification in Equation (3), using the number of terminations associated with violent incidents as the dependent variable. The excluded (baseline) event-time indicator is -1 (i.e., the year immediately before a CBU is first reported). The event-time indicator -5 includes all years that are 5 or more years before a CBU is first reported, while event-time indicator 5 includes all years 5 years and more after that year. The estimation excludes all agencies for which the first nonmissing CBU entry is "yes" and includes never-treated agencies (i.e., those for which a CBU was never reported). The vertical red line represents the baseline event-time indicator -1 (i.e., the year immediately before a CBU is first reported).

Table 1: Summary Statistics

Variable	Number of Observations	Mean	Standard Deviation
Violent Incidents	6,217	0.286	0.785
Violent Incidents, Excluding Potentially Unverified External Complaints	6,217	0.217	0.651
Violent Incidents, Excluding All Potentially Unverified Complaints	6,217	0.173	0.568
Violent Incidents Involving Officers Hired Pre-*Williams*	6,217	0.194	0.635
Separations Due to Agency Policy Violations	6,217	0.431	0.961
Terminations Associated with Violent Incidents	6,217	0.140	0.456
Number of Officers (Law Enforcement and Concurrently Certified)	6,217	98.445	190.055
Indicator for Sheriffs' Offices (SOs)	6,217	0.187	0.390
Indicator for Post-*Williams* years	6,217	0.647	0.478
Resident Population (thousands)	6,217	635.581	731.728
Fraction of Resident Population Aged 18-24	6,217	0.062	0.022
Hispanic Fraction of Resident Population	6,217	0.156	0.166
African American Fraction of Resident Population	6,217	0.152	0.091
Unemployment Rate (%)	6,217	6.084	2.561
Arrests (thousands)	6,205	0.716	1.592
Indicator for Collective Bargaining Units	4,760	0.583	0.493

Note: This table reports summary statistics for the variables used in our analysis. Florida has 258 PDs and 67 SOs (one per county), but we exclude from the analysis nine SOs whose deputies obtained collective bargaining rights through county-specific legislation prior to 2003. We also restrict the analysis to agency-years where the number of law enforcement and concurrent officers is greater than zero. The sample period is 1996-2015. "Violent Incidents" is the number of incidents (relating to law enforcement and concurrent officers) reported in the ATMS database that we classify as involving violence (express or implied). These fall within one of the categories listed in the Appendix. "Separations Due to Agency Policy Violations" refers to the number of officers who leave an agency following an agency policy violation. "Terminations Associated with Violent Incidents" refers to the number of officers who leave an agency in a [-1,3] year period around the opening of a complaint concerning a violent incident in which they were involved. The number of officers is the total number of law enforcement and concurrent officers employed at the agency, as reported in the ATMS database. The resident population, the fraction of the resident population aged 18-24, and the Hispanic and African American fractions of the resident population are from Census Bureau estimates and are at the county-year level. The unemployment rate is from the Bureau of Labor Statistics and is at the county-year level. The number of arrests is from the UCR dataset and is at the agency-year level (i.e., pertaining to the area under the jurisdiction of a given agency). The indicator for collective bargaining units = 1 if the agency is recorded as having a collective bargaining unit in the CJAP dataset.

Table 2: The Impact of Collective Bargaining Rights on Violent Incidents

	(1)	(2)	(3)	(4)
	Dependent Variable: Violent Incidents			Dependent Variable: Violent Incident Rate
Post-*Williams**SO	**0.33578*****	**0.50970****	**0.26477****	**0.26392****
	(0.126)	**(0.204)**	**(0.131)**	**(0.121)**
Number of Officers	0.00056	0.00011	0.00079	
	(0.001)	(0.002)	(0.001)	
Control Variables, Agency and				
Year Fixed Effects?	Yes	Yes	Yes	Yes
Sample Period	1996-2015	1999-2006	1996-2005 2007-2015	1996-2015
Observations	4,681	1,400	4,428	4,681
Number of Agencies	238	176	237	238

Note: This table reports Poisson 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). In Column 4, the number of (law enforcement and concurrently certified) officers is used as the exposure variable: i.e., the natural logarithm of the number of officers is included in the regression and its coefficient is constrained to be 1. This implies that the specification in Column 4 models the violent incident rate rather than the number of violent incidents. Control variables are 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: Robustness Checks and Extensions

	(1) Dependent Variable: Violent Incidents, Excluding Potentially Unverified External Complaints	(2) Dependent Variable: Violent Incidents, Excluding All Potentially Unverified Complaints	(3) Dependent Variable: Separations Due to Agency Policy Violations	(4) Dependent Variable: Violent Incidents Involving Officers Hired Pre-*Williams*
Post-*Williams**SO	**0.34361****	**0.35516****	0.20944	**0.34600****
	(0.142)	**(0.157)**	(0.152)	**(0.171)**
Number of Officers	-0.00009	0.00038	0.00059	0.00060
	(0.001)	(0.001)	(0.001)	(0.001)
Control Variables, Agency and				
Year Fixed Effects?	Yes	Yes	Yes	Yes
Sample Period	1996-2015	1996-2015	1996-2015	1996-2015
Observations	4,213	4,016	5,540	4,026
Number of Agencies	214	203	281	204

Note: Columns 1, 2 and 4 report Poisson regression results for the number of violent incidents (defined in several alternative ways) at the agency-year level. In Column 1, the definition of violent incidents excludes potentially unverified external complaints. In Column 2, the definition of violent incidents excludes potentially unverified complaints from all sources. In Column 4, the dependent variable includes only violent incidents involving officers hired in the pre-*Williams* period (up to January, 2003). In Column 3, the dependent variable is the number of officers who experience separations due to agency policy violations. 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). Control 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