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Doerner, William M. and Doerner, William G.

Florida State University, Department of Economics, Florida State University, College of Criminology Criminal Justice

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**COLLECTIVE BARGAINING AND JOB BENEFITS:
THE CASE OF FLORIDA DEPUTY SHERIFFS**

by

William M. Doerner
Department of Economics
Florida State University
Tallahassee, FL 32306-2180
wmdoerner@fsu.edu

and

William G. Doerner
College of Criminology & Criminal Justice
Florida State University
Tallahassee, FL 322306-1120
Phone: 850-644-7372
Fax: 850-644-9624
wdoerner@fsu.edu

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ABSTRACT

In 2003, the Florida Supreme Court lifted the ban that prohibited sheriff deputies from engaging in collective bargaining. Borrowing data from the *Criminal Justice Agency Profile*, an annual census of Florida law enforcement agencies, enables this study to entertain two questions. First, what impact did the decision have on job benefits? Second, would restricting analysis to agencies with 100+ sworn members alter the findings? Fixed-effects panel data analyses reveal a noteworthy effect on starting salaries. Focusing on just larger agencies leads to a dramatic underestimation of the decision's impact. The timeliness of this study is appraised in light of pending federal legislation.

Keywords: *collective bargaining; unionization; salary; job benefits; Florida*

Introduction

In January of 2003, the Florida Supreme Court ruled that sheriff deputies had the constitutional right to engage in collective bargaining (*Coastal Florida PBA*, 2003). Although Florida municipal police officers began unionizing in 1968 (Pynes & Corley, 2006), county sheriff deputies were exempted from similar coverage. Chapter 447 (*Florida Statutes*, 1975) specifically reserved the state legislature's right "to regulate the activities and affairs of labor unions" in sheriff offices (SOs). The *Coastal Florida PBA* ruling reversed this line of thinking.

This decision prompted an immediate flurry of activity. In the ensuing six months, labor organizations filed 30 petitions with the Florida Public Employees Relations Commission (PERC) to represent sheriff employees in 18 counties (Cohee, 2003). Over the next three years, elections to choose a bargaining agent were held in 13 sheriff agencies. Unions prevailed in ten contests and absorbed almost 7,000 potential members (Pynes & Corley, 2006). Twenty-eight agencies had a collective bargaining agreement (CBA) in place by the end of 2008. These organizations housed 15,581 sworn personnel or 76% of all sheriff deputies in the state (Florida Department of Law Enforcement, 2009).

This series of events provides an opportunity to investigate the impact of unionization upon salaries and job benefits in Florida sheriff departments. Before going further, it would be helpful to visit the decision in greater detail.

Florida Case Law

The issue of whether Florida deputy sheriffs could unionize arose in *Murphy v. Mack* (1978). The key question was whether deputies were "appointees" or "employees." Although the Florida Supreme Court held they were public employees, it noted that the legislature went to great

lengths to retain final authorization and to exclude sheriff deputies from collective bargaining (Dietzen, 2000).

The court reiterated its position when the same issue materialized the following year. As public employees, deputy sheriffs were barred legislatively from engaging in collective bargaining (*Ison v. Zimmerman*, 1979). The distinction between “appointee” and “employee” gradually spread to situations involving other state constitutional officers. Eventually, the Florida Supreme Court would retreat from its *Murphy* dogma.

Service Employees (2000) challenged *Murphy* by asking whether deputy court clerks were public employees under Florida law. The plaintiff, a staff member of the Clerk of the Circuit Court in Orange County, contended her union activism led to her job termination. The Florida PERC dismissed the allegation of an unfair labor practice under the *Murphy* logic. The Florida Supreme Court, though, ruled on behalf of the ousted employee, which paved the way to extending collective bargaining rights to sheriff deputies. The justices noted that Article I, Section 6 of the Florida Constitution guarantees private and public employees the right to collective bargaining. It reads:

The right of persons to work shall not be denied or abridged on account of membership or non-membership in any labor union or labor organization. The right of employees, by and through a labor organization, to bargain collectively shall not be denied or abridged.

In tracing legislative intent over the years, the court found ample provisions supporting this constitutional right. More significantly, the justices drew a bright line between “managerial” and “ordinary” employees. They held that managers were not public employees for collective

bargaining purposes and that only ordinary employees could unionize. Under common law, the position of deputy officer was a mere extension or appendage of the appointing constitutional officer. With the passage of time, however, the distinction between being “appointed” versus “employed” has become lost.

This change of direction did not go unnoticed. Less than a month after *Service Employees* was decided, a local law enforcement union filed a petition with PERC to represent Brevard County deputies (Ruby, 2003). Given the contradiction between *Murphy* and *Service Employees*, the Florida Supreme Court granted *certiorari*.

The first judicial act was to rephrase the question posed in *Coastal Florida PBA* (2003). Instead of determining whether deputy sheriffs were excluded statutorily from collective bargaining, the court investigated whether the state constitution banned such activity. The court emphasized two important points. First, sheriff deputies performed the same work as municipal police officers who possessed collective bargaining rights. Second, the legislature already allowed deputy sheriffs in some counties to unionize. In the end, the court ruled there was no compelling governmental interest to be gained or preserved by depriving some sheriff deputies of the constitutional right to engage in collective bargaining. As a result, the Florida Supreme Court ruling on January 30, 2003 extended the right to collectively bargain to all sheriff deputies.

This institutional change prompted the current study. While an additional question will be articulated later, one query surfaces at this point. That is, did the 2003 *Coastal* ruling impact entry-level salaries and other job benefits for sheriff deputies?

Literature Review

Critics characterize the existing law enforcement collective bargaining literature as terribly

outdated, silent on key issues, and constrained by methodological shortcomings. For example, Wilson and his colleagues (2006, p. 23) complained “one of the key limitations in this literature is the lack of recent analysis using data collected after the 1980s.” A decade earlier, Zhao and Lovrich (1997, p. 510) lamented “there is an absolute dearth of empirical assessment on the effect of collective bargaining on supplemental compensation levels,” a refrain Briggs et al. (2008, p. 229) echoed. Furthermore, studies rely upon cross-sectional data from a single point in time (Briggs et al., 2008, p. 232; Wilson et al., 2006, p. 21). In short, there is room for improvement.

Wilson et al. (2006) used the *Law Enforcement Management and Administrative Statistics* (LEMAS) data from 1990, 1993, 1997, and 2000 to explore the impact of unionization on starting salaries in 352 municipal police departments. While collective bargaining was responsible for higher entry-level salaries, the economic environment (city budget, per capita income, and unemployment) also contributed to pay differentials. Similarly, Zhao and Lovrich (1997), along with Briggs et al. (2008), concentrated on salary supplements (hazardous duty assignments, differential shift pay, education incentives, and merit pay). They, too, analyzed LEMAS data and found that unionization was a significant determinant of these job benefits in large municipal agencies. Although these three studies combined time-series with cross-sectional data, they simply measured the presence or absence of a collective bargaining agent without examining the impact of unions that have operated continuously for long periods.

The findings from these three studies (Briggs et al., 2008; Wilson et al., 2006; Zhao & Lovrich, 1997) are congruent with past literature (Delaney, Feuille, & Hendricks, 1984; Feuille & Delaney, 1986; Geley & Chandler, 1995; Hall & Vanderporten, 1977; Ichnioski, Freeman, & Lauer, 1989; O’Brien, 1992; O’Brien, 1994). Thus, it would appear that collective bargaining in

contemporary law enforcement is associated with salary gains and increased job benefits.

Methods

Data Source

In 1997, the Florida Department of Law Enforcement (FDLE) began conducting an annual census of all law enforcement agencies in the state called the *Criminal Justice Agency Profile* (CJAP). CJAP resembles the national LEMAS inquiry, administered periodically by the federal Department of Justice (Reaves & Hickman, 2004). Like LEMAS, CJAP procures information about entry-level personnel, employee benefits, pay standards, agency characteristics, officer demographics, and other organizational features.

Despite similarities, CJAP differs from LEMAS in at least three critical ways. First, CJAP is restricted to just Florida law enforcement agencies. Since the *Coastal* (2003) ruling extends only to Florida SOs, CJAP is a more appropriate database. Second, CJAP surveys all SOs, regardless of size. In contrast, LEMAS covers all agencies larger than 100 sworn full-time members and relies on stratified sampling to select smaller agencies.¹ Third, CJAP gathers data annually, whereas LEMAS appears every three or four years. This yearly collection enables CJAP to capture operational changes almost immediately after they occur. At the time of this study, the 2003 LEMAS was the most recent data set. Given that the *Coastal* decision was announced in early 2003, reliance upon LEMAS would preclude a full understanding of the case's impact.

Past research has restricted LEMAS analyses to large agencies. As a result, the present study is uniquely positioned to probe an issue the literature has yet to entertain. That is, do the original CJAP findings remain intact after eliminating smaller agencies?

Study Group

Each of the 67 Florida counties has a SO. Prior to *Coastal*, deputies could unionize only if the Florida legislature extended a special privilege. There were six unionized SOs in 2000 and three more counties joined the fold in 2001.² These nine agencies were eliminated because they were immune from the *Coastal* decision. The remaining 58 SOs span a six-year period (2003–2008) and yield a total of 348 possible observations in the panel analysis.³ Missing data will be mentioned where appropriate.

Dependent Variables

Five indicators (entry-level salary, annual leave, salary supplements, buy-back provisions, and a tuition reimbursement plan) are used to determine whether unionization impacted the job benefits package that Florida sheriff deputies receive.

The CJAP survey asks administrators to report the base pay for first-year deputies. These figures do not include bonuses, overtime pay, employer retirement contributions, medical/dental subsidies, take-home cars, disability insurance coverage, life insurance premiums, or the like.⁴

Unions also negotiate paid holidays, personal days, annual vacation time, and sick hours. Some of these items are arranged on different metrics. For example, holidays and personal leave are recorded in days, but sick and vacation time appear in hours. Converting hours into days is complicated by changes in the standard shift. In 2000, 34% of the SOs adhered to an eight-hour shift and 49% utilized a twelve-hour schedule. By 2008, only 13% retained an eight-hour shift and 78% operated on a twelve-hour day. In addition, there were 70 missing data points for vacation hours and 34 for sick hours. As a result, annual leave is simply the aggregate count of personal days and paid holidays.

The current study merges shift differential and longevity pay into a single binary indicator

to reflect salary supplements. Having shift differential pay means that deputies are paid more to work undesirable hours, such as the midnight shift or weekends. Longevity pay indicates whether employees receive increments or bumps in salary depending upon their accumulated experience or years of service.⁵

A buy-back program allows employees the flexibility to cash in unused leave or trade those hours for annual leave or personal holidays.⁶ The variable was coded as the number of reported whole hours of buy-back.

Finally, some agencies provide financial assistance to degree-seeking deputies enrolled in college courses.⁷ Sheriffs who help underwrite educational costs are enabling deputies to earn extra pay and retirement benefits derived from state, not local, funds. While the exact nature of these tuition reimbursement plans may vary from partial to full support and may depend upon the recipient's classroom performance, any employer support was coded as a value of one.⁸

Table 1 contains descriptive statistics for the five dependent variables. The average salary during the post-*Coastal* period was \$5,299 higher than the 2000–02 earnings. Annual leave increased by .4 of a day over the same time frames. SOs with salary supplements inched up from 32% to 38%, buy-back provisions moved from 44% of 46% of the agencies, and tuition reimbursement programs changed from 51% to 55%.

—TABLE 1 ABOUT HERE—

Independent Variables

Variables representing agency traits and county economic conditions were gathered to help gauge differences between unionized and non-unionized SOs. Agency characteristics included union status, how many years the union had represented sheriff deputies, sworn size, and fiscal

expenditures. The county median household income and unemployment rate reflected economic conditions. The next several paragraphs describe these variables in greater detail.

CJAP records whether sheriff deputies are governed by a CBA. These data displayed a minimal amount of missing points and omitted values were easily rectified through an independent source. Santa Rose County had a history of union representation, but its series was interrupted by a single year indicating the absence of a labor organization. Columbia County had a long record of non-union status marred by a report of active union status. These discrepancies were reconciled through PERC, the state agency tasked with monitoring bargaining units, certifying union election outcomes, and handling unfair labor practice disputes. A check of PERC files revealed that neither agency had a change in collective bargaining status. One other county surfaced with an anomaly. Polk County SO decertified its bargaining agent in 2008 by a majority vote of the members.

A more probing agency consideration was the creation of a duration variable that tallied the number of years a CBA has been in place. As Ichniowski et al. (1989) noted, the effects of collective bargaining on law enforcement compensation depend upon state bargaining laws. Since the current sample hails from just Florida, it avoids interstate comparative difficulties. However, the present study adds a new wrinkle by recognizing that not all labor union gains are immediate. Past studies have relied upon a simple dichotomous measure to reflect whether a CBA was in place. Such an approach overlooks union growth and experience. This study hypothesizes that newly formed unions tend to focus on salary issues initially. Once these concerns are remedied, attention turns to other job benefits. Thus, while unionization impacts salary levels very quickly, the effects on other job benefits are not immediate. One way to capture this influence is to go

beyond a presence-or-absence indicator and include the number of years a CBA has been in effect. This strategy will enable the current study to highlight a delayed relationship, if one exists.

Previous research, based upon large municipal police agencies that responded to the LEMAS inventory, has uncovered other variables connected to employment compensation. The number of sworn personnel sometimes emerges as a weak determinant of salary and benefits (Briggs et al., 2008; Wilson et al., 2006; Zhao & Lovrich, 1997). LEMAS consists of a two-prong data collection effort. The first part surveys all agencies containing at least 100 sworn personnel. The second portion relies upon a stratified sample of agencies with less than 100 sworn. In contrast, CJAP contains a complete enumeration of all Florida agencies, regardless of size.

The original plan was to include a measure of fiscal capacity, similar to previous studies (Briggs et al., 2008; Wilson et al., 2006; Zhao & Lovrich, 1997). Earlier projects examined agencies with 100+ sworn members and defined fiscal capacity as agency expenditures per capita. An effort was made to capture similar data for Florida SOs. The Legislative Committee on Intergovernmental Relations (LCIR), housed within the Florida Department of Financial Services, routinely compiles this information from annual county reports. However, inspection of the data revealed wild fluctuations for no apparent reason.⁹ These instances, along with other extreme valuations, rendered the data suspect. Discussions with LCIR personnel could not allay these worries. As a result, these flawed budgetary data were eliminated from further consideration.

Two economic controls were selected. Median household income was used to assess the general economic environment. The U.S. Census Bureau (2009) publishes yearly county estimates in its *Small Area Income and Poverty Estimates* series. Another economic indicator, the county unemployment rate, was obtained from the *Florida Statistical Abstract*, an annual

compilation by the Bureau of Economic and Business Research at the University of Florida.

Summary statistics for the independent variables appear in Table 2. The post-*Coastal* years saw 27% of the SOs unionize, with the typical agent being in place for almost a year (0.82). The SOs grew in size and the median income rose to \$39,479 per household. The unemployment rate dipped from 4.9 to 4.5 over the series.

—TABLE 2 ABOUT HERE—

The Model

The expectation is that unionized SOs will have, on average, higher salaries and increased fringe benefits. A panel or longitudinal model could conceptualize these claims as:

$$Benefit_{ct} = \alpha_{ct} + \beta_1 * Union_{ct} + \beta_2 * Controls_{ct} + C_c + T_t + \varepsilon_{ct}$$

where subscript c refers to the SO and the subscript t indexes the year. *Union* represents the presence or absence of collective bargaining representation. *Controls* is a vector of several variables that accounts for the number of certified deputies, median household income, and the unemployment rate. The fixed-effects term, C , controls for heterogeneity between counties. The variable T is a time trend that absorbs constant marginal change across years.

An event study should analyze dependent variables in terms of level and growth. Since LEMAS has three to four years elapsing between observations, the prior literature is limited to commenting on level or intercept shifts that stem from unionization. The ability to measure the growth rate or first derivative is lost with the multi-year gaps. Using a full panel, the present study overcomes that hurdle by incorporating changes over time and rewriting the equation as:

$$Benefit_{ct} = \alpha_{ct} + \beta_1 * Union_{ct} + \beta_2 * Union_{ct} * Years_t + \beta_3 * Controls_{ct} + C_c + T_t + \varepsilon_{ct}$$

The coefficient representing the interaction of *Union* with *Year* will indicate whether salary and

other job benefits change immediately or are delayed based on the number of years unionized.

Results

Table 3 displays the annual entry-level salary, according to union status, for the 58 SOs affected by the *Coastal* decision. While the 2003 salary difference between unionized and non-unionized sheriff offices is not statistically important, there is a significant salary gap for all the ensuing years. By 2008, unionized deputies are paid \$4,989 more a year than their non-unionized counterparts. To place these salary differentials into perspective, assume that a new recruit joins an average-paying SO in 2003 and earns the average starting salary every year thereafter. If this hypothetical deputy works in a unionized agency, then he or she would accumulate an extra \$24,274 over the six-year period. Of course, it remains to be seen whether variables other than union representation are impinging upon the income figures.

—TABLE 3 ABOUT HERE—

The right-hand portion of Table 3 enables one to compare starting salaries in unionized and non-unionized SOs that employ 100+ sworn deputies. None of those paycheck differentials attain significance. Even so, a hypothetical rookie working in the typical larger unionized SO would have earned \$8,117 more than his or her non-unionized counterpart during this period.

Table 4 summarizes the fixed-effects panel data analyses predicting starting salary and benefits for all 58 SOs affected by the 2003 *Coastal* decision. The first equation for each dependent variable contains the intercept shift driven by unionization, the exact same approach taken by previous studies. The second equation introduces an interaction term that tests whether the slope changes in the series according to how many years a union has been operating.

—TABLE 4 ABOUT HERE—

Three variables (sworn size, unemployment rate, and union status) are significant predictors of annual salary. Starting pay goes up as agencies become larger, as the unemployment rate drops, and for unionized SOs. When SO size is one standard deviation larger than the mean number of sworn personnel, a wage premium of \$2,169 is added to entry-level salary. Similarly, a 1% decrease in the unemployment rate produces a \$273 increment. Unionization causes salary levels to shift upward. The presence of a union hikes salaries by \$845 for entering personnel.

Two significant predictors emerge in Equation 2, the unemployment rate and the newly introduced interaction term that combines union presence with tenure. The significant interaction term suggests that the approach used in previous studies might be slightly misleading. Mature bargaining units secure raises faster than SOs with newly minted unions, as well as non-unionized agencies. Annual salaries increase by \$343 (1.2% of the mean in 2003) with each additional year of union status. The 74% variation explained is on par with the Wilson et al. (2006) findings.

Turning to post-estimation analysis, a Breusch and Pagan Lagrangian multiplier test rejects a random-effects model because of significant variation in the county random-error components. A random-effects approach imposes zero correlation between the independent variables and the counties they describe. In other words, any pertinent variable affecting a county must be well specified. A fixed-effects approach is useful for capturing unobserved variations in each county by relaxing that last assumption to allow correlation and eliminate deviations within the counties. A time plot (not shown here) warns that annual shocks are likely present because entry-level salaries rise each year for both unionized and non-unionized agencies. Without including the significant year trend factor, residual shocks would have driven up estimated wage coefficients across counties and prevented the error term from representing white noise.

The remaining panels in Table 4 examine job benefits. Union presence only alters the salary supplement and buy-back provisions. Interestingly, this finding would not have emerged had the present study relied upon the same strategy that previous studies have selected. While there is no impact derived from unionization, the slope shifts are statistically significant for salary supplements and buy-back provisions. The likelihood of securing salary supplements increases by 61.3% for each additional year a union bargains on behalf of deputies during the post-*Coastal* era. For buy-back provisions, the estimation results are reversed with the intercept now being positive and the interaction term having a negative coefficient. A negative slope occurs because bargaining units immediately obtain buy-back provisions and there are no further changes. If initial negotiations do not procure this job condition, future agreements have even less success.

The declining sample sizes indicate a lack of variation among agencies. For example, there are 348 data points when the focus is on annual leave, but only 42 observations for tuition reimbursement. If an agency does not provide educational assistance in 2003, it continues not to do so over the years. While the fixed-effects approach works well when analyzing salaries, the estimations for tuition reimbursement are inconclusive. In short, it appears that post-*Coastal* CBAs elevate entry-level salaries, but have a spotty influence on other job aspects.

Table 5 confines analysis of entry-level salary and job benefits to SOs with at least 100 members. While the unemployment rate is a significant predictor of annual pay, union status is not. This solution would make it seem as if collective bargaining is unsuccessful at eliciting better salaries in the larger SOs. However, the picture changes when an interaction term is introduced. The second equation shows that unionization does have a significant impact on salary levels. Unionized agencies enjoy estimated marginal salary gains around \$408, an annual bonus

of 1.3%, every year after the *Coastal* decision. If the estimation relies on dummy variables for the years unionized (not shown here) instead of interacting union status with tenure, then a CBA would not return a statistically significant influence on salary growth rates until the fifth or sixth year. A considerable passage of time might be needed before a CBA sways starting salaries.

—TABLE 5 ABOUT HERE—

None of the other job benefits, with the exception of buy-back provisions, fare any better with collective bargaining. Union presence shows a consistent lack of influence upon annual leave, salary supplements, and tuition repayment in the larger agencies because these job benefits exhibit no variation over time. This lack of impact is a bit surprising because larger *Coastal*-affected agencies are more likely to unionize (41% for 100+ SOs versus 27% for all *Coastal*-affected agencies). Nonetheless, the bigger SOs report a smaller premium for average salaries of unionized and non-unionized departments (a 6.2% gap compared to a 16.0% change by 2008). Perhaps, unions serve some other purpose beyond reforming the selected job benefits.

Discussion

Two focal concerns frame the current study. The first issue is whether the Florida Supreme Court *Coastal* decision impacted entry-level salaries and other job benefits for deputy sheriffs. Past studies have found that unionization increased salaries (Wilson et al., 2006) and improved supplementary packages (Briggs et al., 2008; Zhao & Lovrich, 1997). The current analysis reveals that unionization in Florida SOs raised starting salaries immediately and decisively, but did not alter other job benefits over the course of the study period.

The second matter this paper probes is whether restricting the study group to agencies that employ 100+ sworn members, a common cut-off point in the literature, yields comparable results.

Until recently, LEMAS was the sole purveyor of any systematic effort to describe local law enforcement agencies and their operations. Researchers had no alternative data sources at their immediate disposal. As a result, analysts had to be content with the fact that LEMAS completely enumerated larger agencies, but only sampled smaller ones. The advent of CJAP, however, allows an empirical determination as to whether this size restriction produces skewed results.

Initial salary comparisons show that unions are responsible for higher starting salaries when examining all CJAP entrants, but not when the focus is restricted to just the larger SOs. The salary contrast between all CJAP agencies and the bigger SOs is substantial. Using the entire CJAP database, the hypothetical deputy sheriff working under a CBA would accumulate \$24,274 more in earnings than his or her non-unionized counterpart. That difference drops to \$8,118 when examining just the larger SOs. While union salaries might not be significantly higher in bigger agencies on an annual basis, the cumulative effect of a higher earning power is noticeable.

The results drawn from the multivariate examination of salaries and job benefits depend upon the analytical strategy chosen. If this paper had followed the previous literature and focused on solely the larger SOs, collective bargaining would not have registered a significant impact on entry-level salaries. Furthermore, restricting union status to a simple dummy variable leaves the impression that a CBA exerts an instantaneous shift in benefit levels. The current inclusion of an interaction term highlights the need to measure a growing or fading marginal effect gained from union tenure. Without this interaction term, the 100+ group makes it look like unions have no material impact on any form of compensation. However, for both the CJAP and bigger SOs, collective bargaining registers a change in growth rates on entry-level salary and at least one other benefit. Although other benefits remain relatively unchanged over the 2000–08 interval, unions

swiftly secure better buy-back provisions. Overall, a study restricted to larger agencies would have risked overstating the limited results and would have missed out on the possibility that salary supplements improve with prolonged union presence. With these comments in mind, it might be prudent for future researchers to be mindful of this effect when analyzing just larger agencies.

Limitations

Several other aspects deserve attention before embracing these findings as definitive. First is the assumption that all unions are interchangeable. Experience indicates they are not. For instance, Polk County deputies grew disenchanted with their fledgling union, voted to oust the bargaining agent, and declined further union advocacy. According to PERC, the Fraternal Order of Police and the Police Benevolent Association are the most common labor representatives. Both groups have a network of state chapters and a national headquarters. Whether one entity is a better organizer or wields greater negotiating strength at the bargaining table remains unknown.

Second, this paper does not track union activities. Unions can endorse candidates who are running for local offices, make campaign contributions, and mobilize volunteers for numerous tasks. The influence of these activities is not assessed in this study.

Third, Florida is a “right-to-work” state. Even though *Coastal* authorizes collective bargaining, deputy sheriffs cannot be required to join the union as a condition of gaining or continuing employment. Because Florida unions negotiate on behalf of all in-unit members, they provide representation for both dues-paying members and “free riders.”¹⁰ Many labor groups regard their directories as proprietary information and do not disclose membership statistics. As a result, a simple dichotomous indicator regarding union status might not be sensitive to the degree of representation or the strength of a labor group in an agency.

Fourth, it is assumed that all CBAs are equivalent. Even though Carter and Sapp (1992) show that considerable variation exists in contract clauses, no effort was expended to compare and contrast CBA contents. For example, a multi-year wage agreement carved out prior to the current recession might contain much more lucrative terms than pay raises negotiated during a more austere climate, especially one characterized by high unemployment rates and declining budgets.

A fifth consideration is that a local cost-of-living adjustment is not an easy task to undertake. The Bureau of Economic and Business Research (2009) contains several inflationary measures of the Consumer Price Index. Many of these calibrations portray Florida county annual inflation rates as falling within the 0–1% range. In contrast, national figures from the U.S. Department of Labor (2009) suggest slightly higher rates in the neighborhood of a 2–3% climb. While the Bureau of Economic and Business Research (2009) constructs an index for essential public workers like Florida teachers, it does not calculate similar wage indices for county law enforcement personnel. An index to overcome these limitations would be helpful.

Sixth, the present study overlooks one of the most significant union protections, a standardized grievance procedure and the addition of *Weingarten* rights (*National Labor Relations Board v. Weingarten*, 1975).¹¹ It is true that the state “Law Enforcement Officers’ Bill of Rights” (*Florida Statutes 2009*, Chapter 112.532), along with the *Garrity Rule* (*Garrity et al. v. New Jersey*, 1966), already guarantee certain procedural safeguards to law enforcement officers under criminal investigation. However, CBAs can afford an additional layer of protection against overzealous and unfair management practices when it comes to disciplinary actions.¹²

Seventh, the supplemental job benefits analyzed in this paper are limited to annual leave amounts, salary supplements, buy-back provisions, and tuition reimbursement. Other perks (take-

home vehicles, clothing/cleaning allowances, wellness program subsidies, cell phones) have gone unnoticed, both in the present study and in the past literature.

Another consideration lies with the effort to integrate the CJAP and LEMAS instruments. While the two systems may appear similar, they differ in how they acquire responses about collective bargaining. LEMAS asks, “Is collective bargaining authorized for your agency’s employees?” The instructions are to check either a “yes” or a “no” box for sworn personnel and again for non-sworn personnel. This approach could invite distortion. For example, prior to *Coastal*, only the nine counties that had won legislative approval could respond affirmatively. After the *Coastal* decision, all 67 SOs could indicate “yes” because the Florida Supreme Court struck down pre-existing barriers and now authorized collective bargaining for all sheriff deputies. Being eligible to unionize does not necessarily imply that an agency actually traversed the steps necessary to install a union. In contrast, the CJAP is more direct with its query, “Does your agency belong to a collective bargaining unit (PBA, FOP, etc.) for sworn personnel?”

Eighth, another difference associated with the LEMAS approach stems from the interval between successive administrations of the survey. Past studies use LEMAS data covering the years 1990, 1993, 1997, and 2000. The gaps between the survey years could introduce imprecision. For example, members may have moved to disband their collective bargaining unit, observed the mandatory one-year grace period required by the Florida PERC, and then voted in a new union. Without continuously tracking union activity annually, LEMAS would overlook this internal turmoil and mischaracterize union presence as uninterrupted over this entangled period.

Ninth, the purpose of this study was to analyze the impact of the *Coastal* decision, not unionization *per se*. The Florida Legislature had granted permission to nine SOs to unionize prior

to *Coastal* and, as a result, they were eliminated from the study group. Had these counties been included, seven would have joined the 100+ sworn member group.¹³

Finally, there is a need to consider short-term versus long-term effects. The present study uncovered a delayed effect with the interaction term. There appears to be a five- or six-year lagged impact on starting salaries. Whether it takes longer for spin-off benefits to materialize remains undetermined. It may very well be that certain benefits emerge only after collective bargaining agent has accrued sufficient time to establish an effective stronghold.

Conclusion

This study addresses the issue of what happened to entry-level salaries and other job benefits once Florida deputy sheriffs won the right to unionize. This topic is extremely timely, especially when viewed against the backdrop of ongoing federal congressional activity. Sponsors have renewed efforts to make collective bargaining a national priority by introducing a bill titled “The Public Safety Employer-Employee Cooperation Act of 2009” for consideration in the 111th Congress. The House version (H.R. 413) debuted in January of 2009; the Senate companion (S. 1611) was unveiled in August of 2009. This legislative initiative grants the right to engage in collective bargaining to public safety employees, which includes law enforcement officers. An earlier version passed the House on July 17, 2007 by a vote of 314–97, only to languish in the Senate chambers. The National Sheriffs’ Association (Webre, 2008) and the International Association of Chiefs of Police (2008) have expressed their opposition to this legislation, while the National Association of Police Organizations (2009) and the Fraternal Order of Police (2009) have thrown their weight behind these proposals. Obviously, Florida does not stand alone in its historical opposition to collective bargaining. Should proponents rally and gain passage of the

pending federal legislation, the current study may offer insight as to what lies ahead.

Notes

1. The 2003 CJAP reveals that 57% ($n = 33$) of the SOs affected by *Coastal* contained fewer than 100 deputies. The stratified sampling strategy employed by the 2003 LEMAS enlisted 17% ($n = 7$) of the Florida SOs with less than 100 sworn personnel. Thus, if the current study relied upon LEMAS as the database, it would eliminate over three-fourths of the smaller SOs that the *Coastal* ruling could impact and the resulting sample size would be too small to conduct any sophisticated statistical analysis. Also, reliance upon LEMAS over several points in time would render the current study cross-sectional rather than longitudinal because LEMAS samples the smaller SOs.
2. The state legislature allowed Broward, Charlotte, Escambia, Jacksonville, Miami-Dade, and Volusia SOs to unionize in 2000. Flagler, Monroe, and Nassau SOs gained permission in 2001.
3. The total number of observations represents the 58 SOs multiplied by the number of years (6).
4. Ten of the 522 salary data points displayed missing values. Each SO with a missing value had recorded a starting salary for the previous and the subsequent years so the mid-point value was substituted to alleviate this problem. Five interpolated values occurred in 2003, the year of the *Coastal* ruling. However, it would appear that any interference caused by these missing values would be minimal in light of how long it typically takes management and labor to reach a CBA. The procedure in Florida for instituting union representation involves canvassing employees for their signatures to demonstrate a sufficient interest in organizing, presenting a petition for representation to PERC, PERC reaching a determination as to which employees are eligible for

inclusion in the unit, arranging for and holding an election, and certifying the election results. Negotiations between management and workers can begin only after this three-to-five-month process is completed. Once both sides have come to terms, a formal contract ratification vote must take place before the CBA becomes binding. If management and labor are unable to agree on a contract, an impasse is declared and the matter is referred to a mediator and, if necessary, to a special master for binding arbitration (Florida PERC, 2004). In light of all these steps and the time they consume, it is highly unlikely that efforts to engage in collective bargaining impinged upon the 2003 salary.

5. Thirteen SOs had missing data regarding shift differential pay. Nine displayed the same information for at least three years prior to and three years after the missing data point. Four SOs registered the same values for five years prior to the missing data point and for two years afterwards. Longevity pay exhibited almost the exact same pattern. Given this consistency, missing values were converted so as to represent the observed pattern in the ongoing series.

6. Sixteen SOs presented missing data points, with most showing a disruption in an ongoing series of consistent values. Hence, substitutions were introduced to allow the continuation of these patterns

7. Florida sponsors a salary incentive program whereby the state augments the monthly salary of sworn personnel who have earned a two-year and/or a four-year college degree (*Florida Statutes 2009*, Chapter 943.22).

8. Perusal of the data revealed 17 data points were missing. Nearly all these instances involved a sequence of consistent values in which one entry was missing. These values were changed to produce a uniform series of observations.

9. For example, the Alachua County figures showed the per capita expenditure to be \$1.03 in 2000, but then dropping to 34 cents in 2001 only to soar to almost \$105 in 2002. Similarly, Okaloosa County vacillated from an expenditure of \$0.46 in 2000 to over \$91 the following year.

10. “Free riders” are workers who enjoy CBA benefits even though they do not pay union dues.

11. *Weingarten* protections include the right to have a union representative present during internal affairs interviews.

12. For example, the Florida PERC recently rescinded a five-day suspension levied against a deputy, who also served as a union official, after the Sheriff deemed comments the deputy wrote in a union newsletter were derogatory (*Dickey v. Gee*, 2009). Instances like this illustrate how unions can operate as a check-and-balance against unfair labor practices.

13. The 2000 CJAP reported the following sworn sizes for the eliminated SOs: Broward (1,064), Charlotte (178), Dade (3,020), Duval (1,515), Escambia (344), Flagler (61), Monroe (168), Nassau (53), and Volusia (378).

TABLE 1

Descriptive Statistics, Dependent Variables (*n* = 58)

Year	Salary		# Annual Leave Days		Salary Supplement	Buy-Back	Tuition Reimbursement
	M	SD	M	SD	M	M	M
2000	\$23,956	\$3,486	10.3	4.59	0.34	0.43	0.47
2001	\$25,064	\$3,695	10.7	4.65	0.29	0.48	0.55
2002	\$26,262	\$4,139	10.0	3.21	0.31	4.41	0.52
2003	\$27,405	\$4,275	10.4	3.22	0.33	0.40	0.50
2004	\$28,251	\$4,670	11.0	4.27	0.34	0.45	0.53
2005	\$29,862	\$4,529	10.3	3.30	0.41	0.41	0.57
2006	\$31,491	\$4,946	10.5	3.24	0.41	0.45	0.57
2007	\$32,455	\$5,275	10.6	3.05	0.34	0.53	0.57
2008	\$32,895	\$5,234	11.1	4.36	0.41	0.53	0.55
2000–02	\$25,094	\$3,773	10.3	4.15	0.32	0.44	0.51
2003–08	\$30,393	\$5,226	10.7	3.60	0.38	0.46	0.55
2000–08	\$28,627	\$5,425	10.5	3.80	0.36	0.46	0.54

Note: Salary is not adjusted for inflation. A code of 1 for salary supplement, buy-back, and tuition reimbursement indicates the presence of that attribute, while a code of zero indicates absence.

TABLE 2

Descriptive Statistics, Independent Variables (*n* = 58)

Year	Union %	Years Unionized		Sworn Size		Median Household Income		Unemployment	
		M	SD	M	SD	M	SD	M	SD
2000	0.00	0.00	0.00	170	249	\$34,795	\$6,400	4.1	0.87
2001	0.00	0.00	0.00	174	254	\$33,954	\$6,507	4.9	0.93
2002	0.00	0.00	0.00	178	262	\$34,168	\$6,685	5.7	0.91
2003	0.16	0.16	0.37	182	265	\$35,066	\$6,788	5.1	0.91
2004	0.21	0.36	0.74	185	265	\$36,597	\$7,030	4.5	0.93
2005	0.24	0.60	1.14	188	270	\$38,663	\$7,080	3.7	0.74
2006	0.33	0.93	1.53	193	272	\$40,659	\$7,858	3.4	0.68
2007	0.33	1.26	1.96	202	286	\$42,857	\$7,925	4.1	0.87
2008	0.34	1.60	2.40	204	295	\$43,034	\$8,199	6.2	1.29
2000–02	0.00	0.00	0.00	174	254	\$34,306	\$6,504	4.9	1.11
2003–08	0.27	0.82	1.59	192	274	\$39,479	\$8,027	4.5	1.31
2000–08	0.18	0.55	1.36	186	267	\$37,755	\$7,932	4.6	1.26

Note: Median household income is not adjusted for inflation.

TABLE 3

Average Entry-Level Salary by Union Status

Year	<i>Coastal-Affected Agencies</i>					Agencies with 100+ Sworn				
	Union	<i>n</i>	No Union	<i>n</i>	<i>t</i> -test	Union	<i>n</i>	No Union	<i>n</i>	<i>t</i> -test
2003	\$29,444	9	\$27,030	49	1.577	\$30,302	7	\$30,911	20	-0.466
2004	\$30,810	12	\$27,584	46	2.201*	\$32,586	9	\$31,481	19	0.819
2005	\$32,195	14	\$29,120	44	2.294*	\$33,806	10	\$32,905	18	0.694
2006	\$34,994	19	\$29,785	39	4.305*	\$36,230	14	\$34,041	14	1.701
2007	\$36,060	19	\$30,699	39	4.108*	\$37,490	14	\$35,172	14	1.826
2008	\$36,164	20	\$31,175	38	3.845*	\$37,785	14	\$35,572	14	1.637

Note: Salary is not adjusted for inflation.

TABLE 4

Fixed-Effects Panel Data Analysis, *Coastal*-Affected Agencies ($n = 58$)

Variable	Salary		# Annual Leave Days		Salary Supplement		Buy-Back		Tuition	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Sworn Size	0.01*	0.01	0.00	0.00	-0.04	-0.04	-0.02*	-0.02	0.06	0.06
	(0.00)	(0.00)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.05)	(0.05)
Median HH Income	-0.03	-0.03	0.13	0.13	-0.06	-0.05	0.10	0.09	-0.10	-0.10
	(0.05)	(0.05)	(0.07)	(0.07)	(0.13)	(0.14)	(0.10)	(0.10)	(0.28)	(0.28)
Unemployment Rate	-0.27*	-0.31*	0.19	0.20	-0.13	-0.18	0.15	0.19	-0.85	-0.85
	(0.08)	(0.08)	(0.11)	(0.11)	(0.20)	(0.21)	(0.17)	(0.17)	(0.52)	(0.52)
Union (1 = yes)	0.85*	0.31	0.26	0.31	-0.86	-1.68	0.85	1.72	N/A	N/A
	(0.41)	(0.43)	(0.56)	(0.62)	(1.13)	(1.25)	(0.86)	(0.99)	N/A	N/A
Union * Years Unionized	—	0.34*	—	-0.03	—	0.61*	—	-0.51*	—	N/A
	—	(0.11)	—	(0.16)	—	(0.31)	—	(0.26)	—	N/A
Year Trend	1.20*	1.13*	-0.17	-0.16	0.43	0.27	0.10	0.18	0.42	0.42
	(0.10)	(0.10)	(0.14)	(0.14)	(0.26)	(0.27)	(0.19)	(0.20)	(0.59)	(0.59)
Number of Observations	348	348	348	348	144	144	210	210	42	42
Number of Agencies	58	58	58	58	24	24	35	35	7	7
R^2 within	0.73	0.74	0.02	0.02	—	—	—	—	—	—

<i>F</i> -statistic	153.0	133.1	1.36	1.14	—	—	—	—	—	—
χ^2	—	—	—	—	6.20	10.69	14.29	18.28	8.42	8.42
Log-likelihood	-573.9	-568.0	-689.3	-689.2	-46.89	-44.65	-73.21	-73.22	-11.37	-11.37

Note: Annual leave is a linear solution; the others are logistical solutions. Salary and household income are expressed in thousands of dollars and are not adjusted for inflation. Standard errors are in parentheses.

* denotes significance at the .05 level of analysis.

TABLE 5

Fixed-Effects Panel Data Analysis, Agencies with 100+ Sworn ($n = 28$)

Variable	Salary		# Annual Leave Days		Salary Supplement		Buy-Back		Tuition	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Sworn Size	0.01 (0.00)	0.01 (0.00)	-0.00 (0.01)	-0.00 (0.01)	-0.14* (0.06)	-0.13* (0.06)	-0.03* (0.01)	-0.03 (0.01)	0.05 (0.06)	0.05 (0.06)
Median HH Income	0.08 (0.07)	0.09 (0.07)	0.34* (0.16)	0.34* (0.16)	0.15 (0.37)	0.33 (0.42)	0.25 (0.26)	0.13 (0.40)	-0.07 (0.28)	-0.07 (0.28)
Unemployment Rate	-0.24* (0.10)	-0.27* (0.09)	0.41 (0.21)	0.40 (0.21)	-0.84 (0.54)	-0.73 (0.52)	0.18 (0.28)	0.25 (0.34)	-0.62 (0.60)	-0.62 (0.60)
Union (1 = yes)	-0.47 (0.46)	-0.94* (0.46)	0.62 (1.00)	0.49 (1.05)	-2.61 (3.07)	-3.16 (2.97)	0.98 (1.24)	3.74 (2.22)	N/A N/A	N/A N/A
Union * Years Unionized	— —	0.41* (0.12)	— —	0.11 (0.28)	— —	0.85 (0.97)	— —	-1.97* (0.64)	— —	N/A N/A
Year Trend	1.16* (0.16)	0.99* (0.16)	-0.67 (0.35)	-0.71 (0.37)	1.52 (0.88)	0.73 (1.16)	0.00 (0.49)	0.97 (0.78)	0.40 (0.69)	0.40 (0.69)
Number of Observations	167	167	167	167	65	65	78	78	23	23
Number of Agencies	28	28	28	28	11	11	13	13	4	4
R^2 within	0.81	0.82	0.05	0.05	—	—	—	—	—	—

<i>F</i> -statistic	112.4	102.9	1.30	1.10	—	—	—	—	—	—
χ^2	—	—	—	—	22.55	23.62	12.04	28.78	5.50	5.50
Log-likelihood	-252.2	-245.3	-383.3	-383.1	-11.06	-10.53	-23.98	-15.60	-6.76	-6.76

Note: Annual leave is a linear solution; the others are logistical solutions. Salary and household income are expressed in thousands of dollars and are not adjusted for inflation. Standard errors are in parentheses.

* denotes significance at the .05 level of analysis.

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