Pay, Reference Points, and Police Performance[*]

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Abstract

Final offer arbitration provides a unique opportunity to study the relationship between pay, reference pay and on-the-job performance. I consider compensation disputes between New Jersey police officers and their city employers that were resolved in arbitration, and offer evidence that arbitrator rulings influenced future police performance. Before arbitration there is not a significant difference in the number of arrests between cities where the arbitrator ruled in favor of the police and cities where the arbitrator ruled against them. However, in the months following arbitration a 10% difference appears, with winning police forces yielding more arrests. Police arbitration losses are additionally associated with a 3% increase in per capita crime and a relative decrease in the probability that an arrestee is a serious offender. I calculate the gap between the expected and implemented award. The larger this gap, the larger the effect of a police arbitration loss on performance. However, there is no discernable relationship between the size of this gap and performance following a win. The findings suggest that: (i) police effort is a function of changes in wages in relation to a reference point; (ii) large losses resonate more strongly than large wins; (iii) winning matters, even for small stakes; and (iv) the reference point is closely related to a rationally formed expectation of eventual payoffs.

While much of our understanding of labor markets derives from the idea that workers respond to incentives, an emerging body of experimental work provides evidence that psychological and non-market factors are important determinants of employee performance and well-being [Fehr and Gachter (2000)]. Surveys of employers suggest that deviations from reference wages affect worker morale and that managers are reluctant to lower wages [Bewley (1999); Blinder and Choi (1990); Kaufman (1984); Agell and Lunborg (1999)]. For example, Blinder and Choi (1990) report that 95% of the managers they surveyed responded that employee effort would fall if "wage policy is generally considered to be unfair".[1] Brown, Oswald, and Qian (2005) present survey and experimental evidence that relative pay matters for employee happiness. While there is a growing acceptance that deviations from reference wages affect employee well-being, little is known about the effect of these deviations on productivity given the scarcity of studies in this area that use market data. The goal of this paper is to empirically assess the relationship between pay, reference pay and employee performance in a real labor market.

Identifying the causal reduced form effect of pay on performance is challenging since more productive workers tend to be better compensated. Moreover, while the theoretical literature on gift-exchange has emphasized the role of social norms as determinants of worker performance [Akerlof and Yellen (1990)], testing whether performance is affected by deviation of wages from reference points is complicated by the unobservability of the reference wage and the lack of guidance from the theories on its characteristics [Koszegi and Rabin (2004)]. I argue in this paper that arbitration systems offer an attractive real-world laboratory to investigate how on-the-job performance of labor market participants responds to changes in compensation and to deviations from a reference wage.

The use of arbitration as a dispute resolution procedure is prevalent in the public sector and is becoming increasingly important in the private sector following the U.S. Supreme Court's ruling in 2001 that employees can be required to submit all employment disputes to binding arbitration. A commonly used arbitration procedure to resolve contract disputes is final offer arbitration (FOA), in which disputing parties submit offers to an arbitrator who is constrained to choose one of the

[1] Malcomson (1999) reviews the literature on wage rigidities and managerial surveys on wage policy.

disputant's offers in a binding settlement. I employ a dataset containing information on final offer arbitration cases involving compensation disputes between New Jersey Police Bargaining Units and municipalities in the years between 1978 and 1996. After matching the arbitration data to monthly measures of police effectiveness by jurisdiction, I test whether police performance depends on the arbitration outcome and on the characteristics of the final offers. A key advantage of studying police officers, instead of workers in other occupations, is that it is fairly straightforward to quantify their performance.

While the decision to engage in arbitration may be correlated with a variety of municipal finance and labor market conditions, economic theory predicts that the arbitrator's ruling has very little relationship to the facts of the case. This is because the offers the disputing parties submit to the arbitrator serve as sufficient statistics for information relevant to police compensation. Taking the theory at face value, the arbitrator will randomly assign the award. Consistent with this prediction, I cannot reject that cities in which the arbitrator ruled against the police bargaining unit have the same municipal level characteristics as cities where the arbitrator ruled in its favor. The empirical strategy employed in this paper, however, is robust to violations of this condition as it allows for differences in pre-arbitration levels of police effectiveness and for permanent unobserved heterogeneity across bargaining unit and city employer pairs. An additional advantage to using arbitration cases to study the relationship between pay, relative pay and performance is that information drawn from arbitration cases includes both the pay raise that is enacted as well as the counter-offer, allowing for a unique method by which to assess the influence of reference points on employee performance outcomes.

This paper provides evidence that there are strong statistical relationships between arbitration outcomes and police performance measures. Specifically, I estimate that the per capita number of crimes cleared (solved) by arrest was 10% higher in the months following arbitration when arbitrators ruled in favor of unions, relative to when arbitrators ruled for the municipal employer. The "quality" of arrests also depends on the arbitration ruling; felony arrests in cities where the arbitrator ruled in favor of the police are associated with higher incarceration probabilities and longer

jail sentences. Additionally, I show that union losses are associated with a 3% increase in per capita crime rates in the months following the arbitration decision. These results are robust to a variety of controls and fixed-effect specifications designed to allow for permanent differences in city characteristics in a narrow window around the arbitration date. While there is no evidence that the disparities in performance are the result of changing numbers of police employed following arbitration, changes in police effectiveness may not be due to a behavioral response to the arbitration outcome, but rather, the selective turnover of officers. I address this explanation and argue that the evidence is generally not consistent with changing officer composition.

An attractive feature of the dataset is that information is available on the final offers of the parties in dispute. Using this information, it is possible to examine the determinants of performance changes due to differing arbitration rulings. The degree to which an arbitration decision is considered a win or a loss may depend on employee expectations upon entering arbitration. A growing number of laboratory experiments provide evidence that subjects behave as if they have reference-dependent preferences [Bateman *et al.* (1997); Mellers, Schwartz and Ritov (1999); Thaler (1980); Tversky and Kahneman (1991)]. Consider, for example, the following lottery: a person can win $32 with probability .8 or lose $8 with probability 0.2. Mellers, Schwartz and Ritov (1999) find that losing $8 in this lottery is more painful to participants than losing $8 in a lottery where the alternative is a loss of $32. These results suggest that utility depends not only on actual outcomes, but also what could have occurred in a different state of the world. This idea has been posited by numerous papers in the theoretical literature [Gul (1991), Kahneman and Tversky (1979), Koszegi and Rabin (2004), Sugden (2003), Tversky and Kahneman (1991)]. Experiments analyzing the effort/wage gradient find that when experimental subjects are offered higher wages by an employer, they reward the employer with higher effort [Fehr and Gachter (2000)]. However, the literature has found that this reciprocal behavior is still present when the wage is assigned by a random draw from a bingo machine, suggesting that disappointment may also be an important feature of the workplace [see eg. Charness (2002)].

While the experimental literature offers important suggestive evidence that effort depends on a worker's pay relative to a reference point, there are still few studies addressing this question using actual market data. Two studies on this question are Cappelli and Chauvin (1991) and Verhoogen, Burks and Carpenter (2003). Both studies use variation in local labor market conditions in the location of different plants within firms to test whether plants located in areas with better economic conditions experience lower dismissal rates. Both studies find a moderate relationship between local labor market conditions and the extent of dismissals and absenteeism occurring at the firms. Zeckhauser and Rizzo (2003) study the behavior of physicians, and specifically how future behavior is correlated to response on a survey question eliciting their target income. They find that target incomes are a good predictor of future hourly earnings, but not hours worked.

Allocative mechanisms, like FOA, are useful to study the behavioral responses of participants to counterfactual outcomes, in particular because we know exactly what parties demanded in negotiations. I find that the post-arbitration productivity differential between police in cities where the arbitrator ruled in favor of the union and in cities where he or she ruled against the union does not vary with the size of the award, suggesting that a simple effort/wage gradient cannot account for the post-arbitration performance changes observed in the data. Conditional on the arbitrator award, however, the post-arbitration productivity differentials vary in the difference between the union and employer offers, meaning that the change in performance of police officers following a pay raise depends not only on the amount of the pay raise, but on the counter-offer that was demanded but never implemented as well. Moreover, the decline in crimes cleared by arrest following a police arbitration loss is increasing in the size of the gap between the expected award—calculated using the final offers and estimated probabilities of employer arbitration wins—and the arbitrator award. Therefore, post-arbitration changes in police effectiveness are most serious when the award is far away from the counter-offer and considered to be a relative surprise. This finding suggests that rationally formed expectations about payoffs are a reasonable approximation to reference payoffs for police officers in this sample.

1. Conceptual Framework of Arbitration

Before proceeding, it is helpful to review the context surrounding the arbitration cases under analysis, as well as outlining a conceptual framework of final offer arbitration which will facilitate interpreting the models being estimated. Between 1978 and 1996 the default procedure for dispute resolution between police bargaining units and their employers in New Jersey was final offer arbitration.[2] Beginning in 1968, public sector employees in New Jersey were allowed to engage in collective bargaining but were not allowed to strike in cases where negotiations failed. As a result, many negotiations were drawn out, often resolved well-after the date of the contract. To remedy this problem, arbitration was legislated in 1977 specifying the procedure by which such impasses would be resolved. The New Jersey Fire and Police Arbitration Act mandated that collective bargaining must be initiated 120 days prior to contract expiration and if an agreement was not reached 60 days before the this day parties must begin arbitration proceedings.

Salient questions in the theoretical analysis of FOA are whether parties in dispute can reach an agreement before arbitration, what the determinants of the final offers are if the parties cannot reach an agreement, and how the arbitrator rules given the final offers. In traditional models of FOA the arbitrator rules in favor of the party whose offer is nearest to his or her preferred award. Farber's (1980) insight is that from the point of view of the disputants, the arbitrator's preferred award is stochastic. Therefore, the parties in dispute will make their decision under uncertainty and choose offers that maximize their expected utility. To make the model concrete, I present the basic setup of Farber's (1980) model of final offer arbitration.

Denote r_a as the arbitrator's preferred pay raise, r_e as the employer's proposed pay raise, r_u as the union's proposed pay raise, and w as the wage from previous contract. A simple decision rule for the arbitrator is to select the employer's offer if $| r_a - r_e | \leq | r_a - r_u |$. While disputing parties do not observe r_a, they do know its distribution, which by the arbitrator exchangeability condition has a

[2] Ashenfelter and Dahl (2003) and Lester (1984) review public sector dispute resolution procedures in New Jersey.

common distribution for all arbitrators.[3] The "facts of the case" enter into the model through the mean, μ, of r_a. If the police are productive, and deserve a sizable pay raise, then μ will be large, but how large depends on how the population of arbitrators value police performance. Under the assumption that the arbitrator rules in favor of the party whose offer is closest to his or her preferred award, disputing parties select offers that will maximize their expected utility given the offer of the opposing party by trading off the probability of winning in arbitration and the resulting payoff. Denoting *P* as the probability that the arbitrator rules in favor of the employer, the expected utility for the employer and union respectively is:

(1) $\mathrm{EU}(r_e, r_u) = P^*\mathrm{U}((1+r_e)*w) + (1\text{-}\mathrm{P})^*\mathrm{U}((1+r_u)*w)$,

(2) $\mathrm{EV}(r_e, r_u) = \mathrm{P}^*\mathrm{V}((1+r_e)*w) + (1\text{-}\mathrm{P})^*\mathrm{V}((1+r_u)*w)$.

The solution concept for this model is Nash equilibrium, both parties choose offers such that neither party can achieve higher expected utility by changing it. The key predictions of the model are that:

(i) *If disputing parties are equally risk-averse, the winner in arbitration is determined by a coin toss.* Assuming that both parties have the same constant absolute or relative risk-aversion, equilibrium offers will depend on the mean, μ, and standard deviation, σ, of the distribution of r_a and are made such that the arbitrator is on average indifferent between each of the offers. In this case, all of the information from the case that the arbitrator may consider to make a decision is embodied in the disputants' final offers and, therefore, each party's offer has an equal chance of being selected by the arbitrator. This prediction is compelling since it suggests that, given the final offers of the parties, the arbitrator decision is uncorrelated with such things as the characteristics of the cities, unions, and future outcomes, for example, of the expected future performance of police officers.

[3] Ashenfelter (1987) notes that since, generally, parties must each agree on the arbitrator in FOA, if arbitrators value work they will ensure that their decisions are unpredictable but drawn from the same distribution as other arbitrators. This feature of the theory of FOA is called arbitrator exchangeability.

(ii) *If parties exhibit constant absolute or relative risk aversion, differences in the degree of risk aversion between the parties result in offers leading the arbitrator to rule more often for the more risk-averse party. However, the probability of an employer win is fixed, and therefore is invariant to the facts of the case.* If the union is more risk-averse than the employer, then the parties will submit offers to the arbitrator which will result in greater than a ½ probability that the arbitrator rules in favor of the union. But in equilibrium, the probability of an employer win does not depend on μ, and therefore does not depend on the facts of the case. While in the data we may not observe an equal proportion of wins for police and employers, cities where police won or lost in arbitration should not be systematically different.[4]

(iii) *The offer spread ($r_u - r_e$) is a function of σ, the uncertainty about the arbitrator's preferred award.* The more unpredictable the arbitrator's preferred award, the larger is the spread in offers.

The model is silent as to why arbitration occurs in the first place. If parties know the distribution of the arbitrator's preferred award then they should settle at the mean of the award distribution. However, if disputants have divergent beliefs regarding the population distribution of arbitrator awards, the arbitrator will have to take into account information relevant to the case and rule in favor of the party whose belief about the mean of the award distribution lies closest to the truth. This explanation poses a problem for the empirical analysis only if the arbitrator has better information about future police performance than either of the disputing parties. Since the offers presented to the arbitrator will not be formed taking that information into account, one may observe post-arbitration performance differentials that are correlated to the arbitrator's ruling.

After discussing the data and the empirical strategy, I offer evidence in Section 3 that supports the prediction that arbitrator rulings are in fact orthogonal to the facts of the case. Concretely, I cannot reject that cities are similar in the pre-arbitration period depending on whether the police won or lost in arbitration in a number of observable characteristics. While there is no

[4] If the utility functions U( ) and V( ) exhibit declining relative risk aversion, then for small values of μ the union may be more inclined to make risky offer than for large values of μ. In this case, the probability of an employer

evidence of pre-arbitration differences in police performance as a function of future arbitration outcomes, the results summarized in Sections 4-7 show that differential arbitration rulings are correlated with post-arbitration measures of performance. The empirical analysis will show that post-arbitration performance differentials between winning and losing police forces arise after the arbitration date and are not due to differential pre-arbitration trends in performance. Nor are the performance differentials arising prior to arbitration, indicating that if the arbitrator is better informed about the future performance of police than either party, the arbitrator does *not* appear to be better informed about past performance. Lastly, I focus on the prediction (iii) of the theoretical model. In Section 9, I offer evidence that the greater the uncertainty upon entering into arbitration, the greater the productivity declines are if police lose, suggesting that a role of reference points in the quantity of effort supplied by workers. I argue in Section 10 that reference points are correlated to rationally formed expectations of payoffs.

2. Data Sources

Ideally, performance by police would be proxied by variables that are thought to unambiguously impact public welfare, for example, response times or complaints filed against police officers. Unfortunately, these measures are not systematically available for police departments from the period analyzed in this paper. Instead, main measure of police performance used in this paper is the number of crimes cleared by arrest per 100,000 residents in a municipality. Clearances refer to the number of crimes that have been "solved" by the arrest of one or more individuals.[5] In general, I will use the term "clearance rate" to denote the number of crimes cleared by arrest in a month per 100,000 capita. Police officers have discretion over the number of arrests they make through a number of mechanisms including overtime work, absenteeism (the "Blue Flu"), or simply the share of the working day spent actively policing.

Arrests represent costly effort for the police officers involved, due both to the energy expended in the act of arrest and also in the subsequent paperwork. However, under some

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win will depend on μ, and therefore on the facts of the case.

circumstances, arrests could be welfare reducing, for example, if police arrest residents randomly or, perhaps, target minorities in a discriminatory fashion. Nevertheless, a greater number of arrests may signal a more active police presence in communities and, in fact, police departments often base their own internal evaluations using this measure. I will also consider measures of performance that are more closely aligned to public welfare, in particular, crime rates and the sentencing outcomes of arrestees. These measures will not be at the core of analysis because of sporadic data availability, in the case of sentencing, and the extent of noise, in the case of the crime rate.

Three sources of data are used in this paper. Information about arbitration cases and rulings comes from New Jersey Public Employment Relations Commission (PERC) documents at the New Jersey Department of Labor and used in Ashenfelter and Dahl (2005). The data characterize FOA cases between cities and police unions in New Jersey between 1978 and 1995, and include information on the offers submitted to the arbitrator, which are expressed as percent changes on the previous contract's wage, and whether the arbitrator ruled in favor of the municipal employer or police bargaining unit. I match arbitration cases to monthly clearance and crime data from the FBI Uniform Crime Reporting System (UCR) data files for 1975 through 1996. The resulting data set contains 434 arbitration cases from 255 different cities over salary disputes.[6] As will be discussed in the next section, and in the Data Appendix, some of these cases will be dropped from the analysis because they lie too close to one another, resulting in overlapping event-study windows.

I also use data from the Offender Based Transaction Statistics (OBTS). These data track individuals arrested for felony crimes through the courts and, if convicted, the sentence. The data allow me to test whether arrestees have differential probabilities of conviction depending on the outcome of arbitration and allow me to examine whether the arbitration outcome affects the propensity for police to arrest more serious criminals, as measured by final sentencing and incarceration probabilities. A disadvantage to the OBTS data is that they are available for only a limited number of years.

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[5] Clearances will differ from the number of arrests if an individual is arrested for multiple crimes or if multiple arrests clear one crime, although these two measures are highly correlated.

[6] See Data Appendix for sample selection criteria.

3. Empirical Strategy

The models considered in this paper are identified off of the staggered timing of the arbitration rulings. Arbitration cases are staggered by year and month allowing one to estimate the effect of arbitration outcomes after controlling for year, season as well as arbitration case-specific heterogeneity. For each arbitration case I construct an arbitration window of length (N_1 ,N_2), which consists of the arbitration month, the N_1 months preceding arbitration and the N_2 months following arbitration. The analysis only includes cities that experienced arbitration and, for these cities, only months that are contained in the arbitration window. Because there are cities with multiple arbitration cases I drop a number of cases or months within cases when the arbitration windows overlap. The Data Appendix describes the rules used to determine inclusion into the study. In general, there is a tradeoff between the length of the arbitration window and the number of arbitration cases that are used.[7] The findings are robust to the use of different lengths of the arbitration window.

Cities in which the arbitrator ruled in favor of the police bargaining unit throughout this paper will be denoted as PBUW municipalities. Likewise, cities in which the arbitrator ruled in favor of the municipal employer will be denoted as PBUL municipalities. In the simplest estimator, I compare the average difference in clearances in PBUW and PBUL cities prior to arbitration and then the average difference in clearances after arbitration. The difference-in-difference is the estimated impact of arbitration rulings on clearances in PBUW cities relative to PBUL cities. In more sophisticated models I control for time and arbitration window dummies. By employing arbitration window fixed-effects the estimated effect of arbitration outcomes on clearances is calculated by comparing the mean difference in the per capita number of crimes cleared by arrest within arbitration windows for PBUW and PBUL communities.

4. Basic Evidence

Table 1 reports means of the cell-level dataset that is used in this analysis. The first column presents summary statistics for the full sample, the second column summarizes cities in which the

[7] If N_1 and N_2 are both equal to 23 months, then these rules results in the exclusion of 110 cases. If N_1 and N_2 are both equal to 17 months, then 86 cases are excluded.

police bargaining unit won in arbitration (PBUW municipalities) in the pre-arbitration period, and the third column provides pre-arbitration information on cities in which the police bargaining unit lost in arbitration (PBUL municipalities). Because of the small city sizes, there are also relatively few monthly crimes and clearances.[8] Cities experience an average of 62.93 violent crimes per 100,000 residents per month, amounting to about 14 violent crimes per month. As one examines narrower categories of crime, many cities do not have any clearance reports in a given month. Because of the presence of zeros in the data, I choose to analyze reports in per capita levels, rather than percentage changes or logs.

Column (1) shows that the employers only won 35% of their cases. Since this proportion is not close to 0.5, I conclude that arbitrators are not generally indifferent between the offers of the two parties. However, it is possible that union's are more risk-averse and submit conservative offers. A test of whether prediction (ii) of the theoretical model is borne out is whether observable characteristics of the cities and historical measures of police performance explain the arbitrator ruling. Column (4) presents the difference in means between PBUW and PBUL cities in the pre-arbitration period. Consistent with the theory of FOA presented above, the means do not reveal much of a difference in crime rate, per capita clearances, or other characteristics of PBUW and PBUL municipalities in the pre-arbitration period suggesting no obvious correlations between the arbitrator decision and the information set available to the arbitrator at the time of arbitration.

How do arbitration outcomes affect the number of crimes cleared by arrest? One may begin to answer this question by comparing the average number of clearances in the months prior to arbitration to the number of clearances in the months after arbitration for PBUW and PBUL cities. I present these averages in Figure 1 for the grand total of clearances using a relatively long (23,23) month bargaining window, which has the disadvantage of excluding many arbitration rulings, but allows one to examine both the persistence of effects and pre-arbitration trends over a relatively long time span.[9] Figure 2 plots these averages after applying a triangle smoother. The plots suggests that

[8] The cities under analysis have an average population of 21,987 (median of 11,934). On average, union's consist of 49 police officers, or about 87% of the police force in a given municipality.
[9] The sample means, as in most estimates in this paper, are weighted by population size of the municipality in 1970.

prior to arbitration PBUW and PBUL cities had similar monthly clearance rates, but that after arbitration the clearance rates in these two types of cities diverged, with police forces in PBUW cities clearing more crimes by arrest. This clearance rate differential appears to emerge around three months after arbitration, peaking at eight months, and persisting for at least 22 months. Visual inspections of Figures 1 and 2 reveal that PBUW and PBUL cities do not appear to have differential trends in per capita clearances prior to arbitration, something one would expect to see if the arbitrator incorporated trends in clearance rates as part of his decision rule even after conditioning on the final offers of the disputing parties.[10]

The estimated means broken down by event-time in Figure 1 suggest that police altered their policing intensity after arbitration. But since these estimates are calculated without the use of controls, it remains possible that the post-arbitration divergence is due to some omitted variable or time trend, although there are no obvious candidates. By adding season, year and arbitration window fixed-effects I hold constant many factors that could be driving these divergent post-arbitration clearance rates. In particular, I allow for unobserved heterogeneity across arbitration windows to hold constant permanent differences in clearance rates in cities around the time of arbitration.

Figure 3 is the regression-adjusted version of Figure 1. Specifically, I estimate the model:

$$(3) \qquad y_{mt\tau b} = \alpha + \eta_m + \mu_t + \gamma_b + \delta_\tau * \mathrm{PBUW_b} + \beta_\tau * \mathrm{PBUL_b} + \varepsilon_{mt\tau b},$$

where γ_b denotes an arbitration window effect, η_m is a seasonal effect, and μ_t is a time effect, and τ denotes months since arbitration. Because I will be estimating the model using a (23,23) bargaining window, τ takes on the values of –23 to 23. The estimated coefficients $\hat{\beta}_{-23},...,\hat{\beta}_{23}$ and $\hat{\delta}_{-23},...,\hat{\delta}_{23}$ are plotted against event-time, or months since the arbitration date. The estimates are normalized by restricting $\hat{\beta}_0$ and $\hat{\delta}_0$ to equal zero, so that they represent clearances relative to the arbitration date, and are plotted in Figure 3. Figures 3 confirms that the pattern observed in Figure 1

[10] Formally, I cannot reject that PBUW and PBUL cities have the same pre-arbitration trends in clearances at conventional levels of significance.

holds up after adjusting for arbitration window fixed-effects year and season dummies.[11] As with the raw means, there does not appear to be a difference in the trend of monthly clearance rates in the pre-arbitration period, but there is a marked divergence between the PBUW and PBUL municipalities in clearances in the months following arbitration.

I conduct inference by estimating the cumulative effect of arbitration rulings on clearance rates over each of the post-arbitration months. Intuitively, I calculate the difference in monthly clearance rates between PBUL and PBUW cities at each date after arbitration, and then subtract from these differences the difference in the average monthly clearance rate between PBUL and PBUW cities for the entire pre-arbitration period. I then sequentially cumulate and plot these difference-in-difference estimates across all post-arbitration months. Concretely, I estimate the following model:

$$(4) \qquad y_{mtcb} = \alpha + \eta_m + \mu_t + \gamma_b + \xi_\tau + \theta_\tau * \text{PBUL}_\text{b} + \varepsilon_{mtcb}, \tau = 1,...23,$$

where τ indexes the post-arbitration months, 1 through 23 in this case. The model is estimated using a shortened pre-arbitration window of 17 months in order to allow for more arbitration cases in the analysis.[12] The estimate $\hat{\theta}_k$ is the difference in the gap in per capita clearances between PBUL and PBUW cities in month *k* after arbitration, relative to the gap in clearances between PBUL and PBUW cities during the entire pre-arbitration period. A negative value of $\hat{\theta}_k$ means that the gap in clearances between PBUL and PBUW cities in the *k*th month after arbitration is wider than the average gap in clearances between these two groups during the entire pre-arbitration period, holding other things constant. For each post-arbitration date I cumulatively add the difference-in-difference estimates $\hat{\theta}_k$ to obtain the total unexplained gap in the number of clearances between PBUL and PBUW cities *j* months after arbitration.:

$$\hat{\omega}_j = \sum_{k=1}^{j} \hat{\theta}_k \ , \ j = 1,\ldots,23.$$

[11] During the pre-arbitration period, PBUW municipalities have somewhat higher clearance rates than PBUL municipalities relative to the arbitration date, implying that in the arbitration month PBUW cities experienced a decline in clearances relative to PBUL cities, since the normalization is with respect to arbitration date.
[12] Estimates are robust to the use of alternative arbitration windows including the (23,23) bargaining window.

The estimate $\hat{\omega}_j$ is the cumulative difference-in-difference estimate of the effect of winning versus losing arbitration rulings on clearances *j* months after arbitration.

Plots of $\hat{\omega}_j$ along with a 95% confidence interval are presented in Figures 4-6. Because there is autocorrelation in monthly clearances within municipalities, standard errors are clustered within the arbitration windows.[13] In Figure 4 the PBUL/PBUW clearance rate gap is significantly larger following arbitration than in the months before arbitration. For all three outcomes $\hat{\omega}_j$ is negative and downward sloping. The decline in clearances in PBUL municipalities relative to PBUW municipalities begins after the second month, although I cannot reject that $\hat{\omega}_j$ is significantly different than zero at the 5% level until after five months.[14] The post-arbitration difference in clearances between PBUL and PBUW communities appears to persist for approximately one year and the cumulative difference totals more than 200 crimes cleared by arrest per 100,000 capita.

5. Parameter Estimates

Table 2 reports parametric regression estimates corresponding to (12,12) and (17,17) bargaining windows.[15] Estimates in column (1), column (4) and column (7) are obtained by fitting the following model to the data:

$$(5) \qquad y_{mt\tau b} = \alpha + \varpi \mathrm{PBUL}_b + \delta(\mathrm{PBUL}_b * \text{post arbitration}_\tau) + \beta(\mathrm{PBUW}_b * \text{post arbitration}_\tau) + \varepsilon_{mt\tau b},$$

where, depending on the column, $y_{mt\tau b}$ denotes either the grand total of per capita clearances, violent crime clearances or property crime clearances in month *m*, year *t,* months since arbitration τ, and arbitration window *b*. Note that since (5) is estimated without the use of any regression adjustments, the estimates can be interpreted as simple differences in population-weighted means. The coefficient estimate $\hat{\beta}$ is the difference in average monthly clearances per 100,000 capita between the post-

[13] Clustered standard errors are about twice as large as those that are not clustered.
[14] One reason that it takes approximately three months for the PBUL/PBUW clearance rate differential to emerge may be that it takes time to develop cases leading to arrest.

arbitration period and the pre-arbitration periods in PBUW cities. The coefficient estimate $\hat{\delta}$ has an analogous interpretation for PBUL cities. The coefficient ϖ captures the average difference in per capita clearances between PBUL and PBUW cities prior to arbitration.

In column (1) of Panel A, the parameter β is estimated as 9.28 with a standard error of 3.14. The estimate implies that when the arbitrator rules in favor of the union, police forces obtained on average 9.28 more monthly clearances per 100,000 capita after arbitration than before arbitration. A union loss is not associated with a statistically significant reduction in post-arbitration clearances in this baseline specification. The coefficient δ is estimated as –3.96 with a standard error of 3.78. Note that it is not possible to separately identify the effect of winning in arbitration, losing in arbitration and simply finishing arbitration, irrespective of the outcome, on clearances since there are no arbitration cases that lead to no decision. But while there may be a post-arbitration effect on clearances irrespective of the arbitration outcome, it is still possible to identify the effect of a police win relative to a police loss. The fourth row of Table 2 corresponds to $\hat{\theta} = \hat{\beta} - \hat{\delta}$ which represents the change in the PBUW/PBUL per capita clearance rate differential between the post-arbitration and pre-arbitration periods. In column (1) of Panel A, I estimate this change as 13.25 clearances per 100,000 capita with a t-ratio of 2.70. This estimate implies that the difference in the number of monthly clearances per 100,000 capita between PBUW and PBUL cities widened by 9%, in the 12 months after arbitration.

Column (1) suggests that pre-arbitration differences between PBUW and PBUL in total per capita clearances are negligible. The police loss main coefficient is small, estimated as 0.40 in column (1) with large standard error of 16.20, implying that prior to arbitration, PBUW and PBUL municipalities had statistically indistinguishable clearance rates.

Column (3) presents estimates from a model that includes month and year controls as well as 348 arbitration window fixed-effects:

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[15] Because there is a trade-off in bargaining-size window and arbitration cases, in the (12,12) sample I just use arbitration cases that appear in the (17,17) sample to facilitate comparisons. Estimates from models that use all of the non-overlapping cases in (12,12) bargaining windows are qualitatively similar to the ones presented.

$$(6) \qquad y_{mt\tau b} = \alpha + \mu_m + \eta_t + \gamma_b + \delta(\text{PBUL}_\text{b} * \text{post arbitration}_\tau) + \beta(\text{PBUW}_\text{b} * \text{post arbitration}_\tau) + \varepsilon_{mt\tau b},$$

where μ_m and η_t represent month and year effects respectively, and γ_b are arbitration window effects. Note that the police loss main-effect is absorbed by the arbitration window dummy and is therefore omitted from the model. In this specification, when the arbitrator ruled in favor of the union, I estimate that there were 6.39 additional monthly clearances per 100,000 capita in the 12 months following arbitration than in the 12 months preceding the arbitration ruling. By contrast, when the arbitrator ruled in against the union, I estimate 7.81 fewer clearances per 100,000 capita in the post-arbitration period relative to the pre-arbitration period. The difference between these two estimated differences is 14.20, with a t-ratio of 3.35, corresponding to about a 10% decline in clearances in PBUL cities from the pre- to the post-arbitration period, relative to PBUW cities. Columns 4-9 show that the post-arbitration PBUW/PBUL differentials in clearances are present and roughly of the same magnitude for both violent and property crimes. Panel B presents estimates from statistical models (5) and (6) using a longer (17,17) bargaining window. In this panel, estimates of the post-arbitration change in the PBUW/PBUL clearance rate differential are attenuated as compared to Panel A, which can be expected since plots of PBUW and PBUL clearances by event-time indicate that the post-arbitration disparities in clearance rates between PBUW and PBUL cities dissipated somewhat after 12 months.

To investigate these patterns in more detail, Table 3 presents estimates of post-arbitration clearance rate differentials by specific crime type. To the extent that officers may exercise discretion on whom to arrest, following arbitration rulings they do not appear alter enforcement of murder and rape. However, the effect of arbitration rulings on clearances of assault and robbery crimes by arrest is large. Arbitration rulings also have a particularly strong effect on clearances for motor vehicle theft, which is interesting as making these arrests are clearly a function of effort, namely how many license plates police officers key into stolen vehicle databases.

6. Arbitration and Crime Rates

Clearances by arrest are of course highly correlated with reported crime. Are the results presented in Table 2 a byproduct of differential crime rates, rather than clearance rates, following arbitration? Such a pattern would be unusual, but if it were the case, then we would expect to see lower crime rates when the arbitrator ruled against the police and higher crime rates when the arbitrator ruled in favor of the police. In fact, clearance and crime rates move in the opposite direction after arbitration. Estimates of (6) with crime rates as the dependent variable are presented in Table 4. There appears to be no effect of arbitration outcome on the violent crime rate. However, arbitration rulings, in particular union losses, appear to be associated with an *increase* in the reported property crime rate and, as a result, an increase in total reported crime. Column (6) of Panel A shows that post-arbitration months in PBUL cities are associated with 18.55 additional monthly property crimes per 100,000 capita (t-ratio = 2.54), whereas union arbitration wins are not associated with any change in the property crime rate following arbitration. Some caution is warranted in interpreting the estimates for the reported crime outcome since they are measured somewhat imprecisely. While the difference-in-difference estimate of the effect of a union arbitration win relative to a union arbitration loss is fairly large, estimated as -19.10 in column (6) of Panel A, it is imprecise, having a standard error of 13.54. Nevertheless, these point estimates suggest the effect of an arbitration loss on the number of crimes cleared by arrest will in fact be larger in magnitude than the estimate reported in Table 2 since, generally, police make more clearances when crime rates are higher.

The estimates in Table 4 are additionally interesting from the perspective of the economics and crime literature. A longstanding question is the effect is of increased police presence on crime. In the context of this paper one can test whether criminal activity increases following reductions of police presence in the months following arbitration in PBUL cities. While the point estimates are somewhat imprecise, they suggest that the elasticity of per capita reported crime with respect to per capita clearances is -0.3, assuming that arbitration rulings affect crime only through changes in police presence. This elasticity is in line with the OLS estimates on the elasticity of crime with respect to police in Levitt (1997) and McCrary (2002). The increase in crime observed after police arbitration

losses may occur either because criminals are responsive to the reduced presence of police or through a containment channel, more clearances result in fewer free potential criminals.

To better assess the relationship between arbitration outcome and the per capita crime rate in the months after arbitration, I construct figures plotting the cumulative effect of arbitration rulings on reported crime over event time. Cumulative plots of post-arbitration crime rates for PBUL cities relative to PBUW cities, analogous to those presented in Figures 4-6, show that PBUL cities experienced elevated crime rates in the post-arbitration months relative to PBUW cities. However, the confidence intervals are very wide and the cumulative estimates are never significantly different than zero.[16] Recalling Table 4, however, there is a significant change in the crime rate in PBUL cities from the pre- to the post-arbitration period.[17] Therefore, it is instructive to make fewer demands on the data and simply plot the cumulative difference in PBUL crime rates at each post-arbitration month relative to the average crime rate in PBUL cities during the entire pre-arbitration period. Specifically, I estimate a model similar to (4):

$$(7) \qquad y_{mtcb} = \alpha + \eta_m + \mu_t + \gamma_b + \xi_\tau * PBUW + \theta_\tau * \mathrm{PBUL_b} + \varepsilon_{mtcb}, \tau = 1,...23,$$

and for each post-arbitration date I calculate the cumulative number of crimes reported up to that date in PBUL municipalities relative to the average crime rate in the entire pre-arbitration period:

$$\hat{\omega}_j = \sum_{k=1}^{j} \hat{\theta}_k \ , \ j = 1,\ldots,23.$$

Estimates of $\hat{\omega}_j$ are plotted in Figure 5 for total crime reports. Inspection of Figure 5 shows that there were approximately 245 excess crime reports per 100,000 capita in PBUL cities in the 23 months after arbitration. Crime reports appear to rise about 5 months after arbitration and are statistically distinguishable from 0 at the 5% level during months 8 through 10. Appendix Figures 3 and 3 break down the cumulative change in post-arbitration PBUL crime rates by severity of crime.

[16] Figure is available from the author upon request.
[17] This change is not statistically distinguishable from the change in the crime rate occurring from the pre- to the post-arbitration period in PBUW cities, which appears to be negligible, due to large standard errors on the difference-in-difference estimate.

The increases in reported crime in PBUL cities following arbitration are mostly due to rising property crime reports.

While there were statistically significant increases in reported crime in PBUL cities after arbitration, as stressed earlier, these change are not statistically distinguishable from the change in the crime rate occurring from the pre- to the post-arbitration period in PBUW cities, which is negligible but estimated with large standard errors. The noisiness that is inherent to the crime outcome complicates inference and leads to estimates that are measured imprecisely in some cases. Therefore, I will focus primarily on clearances as the outcome of interest in the subsequent analysis.

7. Arbitration and Sentencing Outcomes

In this section I focus on how arbitration affects the "quality" of arrests, focusing in particular on the probability of conviction and incarceration, and sentencing of arrestees. Unfortunately, the UCR data does not contain information on the final disposition of the arrestee, specifically, whether the arrest resulted in a conviction and, if so, the sentence. Instead I use information from administrative data on information of arrestees from the point of arrest through final disposition drawn from the Offender Based Transaction Statistics (OBTS). This series was produced by the Bureau of Justice Statistics with the intention of tracking individuals from the point of entry into the criminal justice through final disposition. By matching arrested individuals in the OBTS to agencies in the arbitration data files, I can test whether conviction rates, incarceration rates, and sentencing depends on the arbitration outcome at the time of arrest.

The OBTS files include New Jersey for the period 1987-1990, although identifiers for arresting agency and month are only available for 1989 and 1990. Cases in the OBTS are reported by date of final diposition. Data in the 1989 and 1990 files contain individuals who reached their final disposition in these two years. Therefore, for this section I limit the arbitration cases under analysis to those for which the first and last month of a (12,12) arbitration window occur between 1987 and 1990. This exclusion results in the use of 40 arbitration cases in the analysis. I match each individual to the municipality where he or she was arrested and retain individuals who were arrested

in the 12 months before or in the 12 months after an arbitration ruling.[18] Ultimately, I compare sentencing outcomes of individuals arrested for felonies in PBUW and PBUL jurisdictions between 1987 and 1990, obtaining final disposition between 1989 and 1990, and in municipalities that experienced arbitration between 1988 and 1989. Because the data files are organized by date of disposition, I am necessarily missing individuals who are involved in prolonged trials and whose cases may be relatively serious. This may present a problem when comparing the pre- to post-arbitration periods since individuals who appear in the dataset and were arrested post-arbitration will have had their cases disposed relatively quickly as compared to individuals arrested in the pre-arbitration period. However, it is still possible to compare post-arbitration outcomes in PBUW and PBUL municipalities.

The OBTS analysis, while limited by the relatively small number of arbitration cases that can be used, suggests that the differences in policing activity observed in the post-arbitration period are substantive. Panel A of Table 5 displays estimates from linear probability models for the probability of conviction (columns 1 and 2) and incarceration (columns 3 and 4). All models in the table include controls for demographic information of the arrestees, year and season effects, year of final disposition dummies and arbitration window dummies. The first interesting feature to note from Panel A is that the probability of conviction for individuals arrested in PBUW cities relative to the probability of conviction for individuals arrested in PBUL cities did not change from the pre- to the post-arbitration period, as seen from the difference-in-difference estimates on conviction probabilities in columns (1) and (2). These estimates suggest that the rising number of arrests in PBUW municipalities following arbitration does not appear to be the result of police trawling in the innocent. Individuals who are arrested after an arbitration decision were no less likely to be convicted if the arbitrator ruled in favor of the police relative to the case when the arbitrator ruled against the police.

The estimates in column (3) of Panel A imply that individuals arrested following union wins were more likely to be serious criminals as compared to individuals arrested after union losses. In

[18] I exclude arrestees for whom the charge was not reported. This exclusion has no qualitative effect in the resulting

column (3), the probability that an arrestee was incarcerated increased by 0.07 in PBUW cities relative to PBUL cities in 12 months after arbitration relative to the 12 months before arbitration. This estimate has a corresponding t-ratio of 3.18 and corresponds to approximately a 22% increase in the incarceration probability in PBUW cities from the pre- to the post-arbitration period relative to the change in the incarceration probability in PBUL cities. When conditioning on the charged offense code in column (4), the difference-in-difference estimate of a union win versus a union loss declines by only 0.022 points, suggesting that police in PBUW cities were arresting more serious offenders even within crime categories.

As seen in Panel B, conditional on conviction, the probability that an arrestee is incarcerated is 25% higher in PBUW cities than in PBUL cities in the 12 months after arbitration than in the 12 prior months. Individuals arrested in PBUW cities after arbitration and convicted could expect to serve around 9 additional months in prison than arrestees in PBUL jurisdictions. These effects of arbitration on sentencing outcomes are large and suggest that there were important differences in policing strategies depending on the arbitration outcome in the cities in this sample.

8. Arbitration and Employment in Police Departments

Changes in the number of police can be seen as a mechanism driving the observed changes in clearance and crime rates after arbitration. For example, employment in police departments may decline if, following a union arbitration loss, officers depart because they have improved outside options. This mechanism may be relevant since studies of job mobility, such as Topel and Ward (1992), find that wage changes are an important determinant of job changes. Conversely, employment will rise if, following a union win, police departments are better able to fill vacancies. It is possible to test whether employment changed in PBUW and PBUL cities after arbitration by using employment data of police officers compiled from Uniform Crime Report Law Enforcement Officer Killed and Assaulted (UCR LEOKA) data files. UCR LEOKA files contain reports by law enforcement agencies on the number of police and civilian personnel. Unlike the crime and

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estimates.

clearance reports from the UCR, LEOKA reports are made yearly, so the unit of analysis is far coarser than in the previous specifications.

Table 6 shows that employment at PBUW cities did not change significantly relative to employment in PBUL cities in the year after arbitration relative to the year before. In this table I make pre- and post-arbitration comparisons of the employment of police officers and civilian personnel in cities that experienced FOA. The difference in PBUW and PBUL employment is virtually unchanged in the year after arbitration relative to the year before arbitration. The difference-in-difference is estimated as 0.07 with a standard error of 3.56 in column (2). Interestingly, column (5) suggests that there were 5.5 additional civilian personnel per 100,000 capita after arbitration in PBUL departments, with a corresponding t-ratio of 1.90, although the difference-in-difference, -5.66, is estimated quite imprecisely with a standard error of 4.19. The interaction of the arbitration outcomes with an indicator that takes on the value of one if the population of the city at the time of arbitration is at or above the 75th percentile of the population distribution in the sample reveal no differences in post-arbitration employment of police officers or of civilian personnel even in large cities.

The results from Table 6 suggest that there were not systematic changes in employment levels as a result of arbitration outcomes. As a result, changes in the number of crimes cleared by arrest following arbitration can be interpreted as changes in the productivity of police officers. However, it remains possible that, in response arbitration rulings, police officers change their labor supply decision at the intensive margin, for example through changing overtime hours. In fact, this may be a mechanism through which the observed changes in performance measures from the pre- to post-arbitration period may occur. However, basic economic theory would not predict that these changes in overtime hours supplied by officers are the rational response to a change in prices. For example, all of the arbitration cases involved nominal pay raises, and most involved real pay raises. If the patterns seen in the data were the result of change in the labor supply of police officers at the intensive margin in response to the changes in the wage, then it would have to be the case that, on average, the substitution effect dominates the income effect when police win in arbitration, but the

income effect dominates when police lose. Such behavior would be unusual. Moreover, the demand for overtime hours may change in response to a new wage. If the labor demand curve is downward sloping, then presumably the increase in wages associated with a union win would lead the municipal employer to cut overtime hours, resulting in fewer arrests made.

Two additional mechanisms that may account for post-arbitration changes in productivity are turnover and adverse selection. The higher pay that is associated with an arbitration win may lead to reduced turnover, as in Salop (1979), and as a result, higher productivity. In the adverse selection case, arbitration losses may result in a situation where better members of police departments leave and are replaced by less skilled officers.[19] As the average skill-level of officers declines following arbitration, so then may measures of police effectiveness.[20] One reason that these mechanisms may have credence is that the largest gaps in performance between PBUW and PBUL cities occur several months after the arbitration ruling, perhaps corresponding to the time it takes for officers to quit the force. However, there are several reasons why these mechanisms fail to explain the patterns in clearance rates seen in the data. First, numerous studies suggest that the turnover rate of police officers is typically low, as compared to other occupations; typical officers have 10 years of seniority [Aamodt (2004)]. Second, all awards involved pay raises, so one would not expect turnover in these towns to rise, even if the police lost in arbitration. Third, Table 2 shows that union wins are associated with more clearances and union losses are associated with fewer. However, union contracts would not allow a city to replace a lower skilled officer with a higher skilled one. Therefore, any additional skilled recruits would fill existing vacancies, yet there is no evidence that the number of officers increased following union wins. Fourth, one would expect that if the patterns observed in Figures 1-6 were the result of changing officer composition or turnover, departments where police receive smaller raises should have experienced higher turnover and better quitting, thus resulting in productivity losses. However, I will show evidence that there is no relationship between

[19] See Weiss (1980) for an example of an adverse selection model where the quality of workers is affected by the wage paid.

[20] However, McCrary (2003) finds no evidence that the introduction affirmative action quotas in police departments led to increases in crime, even though the quotas meant hiring candidates with test scores below what would have been acceptable prior to their introduction.

the size of the award and the magnitude of the post-arbitration change in productivity in PBUL or PBUW municipalities. Specifically, in PBUL cities, the changes in clearance rates from the pre- to the post-arbitration period are almost entirely driven by the spread in the offers, but not the actual award implemented.[21]

9. Pay, Reference Pay and the Vince Lombardi Effect

I have provided evidence that in the case of New Jersey police officers, a tangible outcome of arbitration rulings can be garnered: there are significant changes in policing intensity in the months following arbitration in PBUW relative to PBUL municipalities. One can decompose the post-arbitration productivity differentials of PBUW and PBUL jurisdictions by the characteristics of the specific offers to explore the determinants of this relationship. Three explanations for the observed patterns in clearances are:

H1: *The Vince Lombardi effect:* the police response to the arbitration outcome is invariant to the spread or size of the final offers; it only matters whether police won or lost in arbitration.[22]

H2: *The effort/wage gradient:* Arbitration rulings are mechanically associated with wage growth, and therefore the productivity changes reflect changes in pay.

H3: *Reference pay and effort:* Productivity changes are associated with gains and losses relative to a reference point.

To preview the results, the evidence is strong on H3, the link between reference pay and the marginal disutility of effort. There is also suggestive evidence of a "Vince Lombardi" effect, meaning that when the spread in offers is infinitesimal, there is still a jump in productivity following a union win. However, there is no evidence to support an effort/wage gradient.

I begin with a description of the relationship between the deviation of the arbitrator award from the average of the two final offers and the clearance rate. Specifically, I model the per capita

[21] In PBUW cities there is no relationship between the change in arrest rates from the pre- to the post-arbitration period and the size of the award or the spread in the offers.

[22] Professional football coach Vince Lombardi is attributed to have said that "winning isn't everything, it's the only thing".

clearance rate as a function of whether the union won or lose in arbitration and a high-order polynomial in the deviation of the award from the average of the offers:

$$(8) \qquad y_{mt\tau b} = \alpha + \mu_{t,m} + \gamma_b + p(post \times \text{DEVAV}), \\ + \beta \text{post} \times \text{PBUW} + \eta \text{post} + \varepsilon_{mt\tau b}.$$

The function p( ) is a sixth order polynomial and DEVAV denotes the difference in the arbitrator award from the average of the offers. The dependent variable $y_{mt\tau b}$ is clearances per 100,000 capita in month *m*, year *t*, event-time τ , and arbitration window *b*. Note that the threshold between a union win and a loss is the point where DEVAV=0, or the point where the union and employer offers are equal. Figure 6 plots the estimated function $\hat{p}(\text{DEVAV}) + \hat{\beta}\text{PBUW} + \hat{\eta}$ over the support of DEVAV, through the sixth post-arbitration month (solid line) and the 12th post-arbitration month (dotted line).

Figure 6 shows that when the union has lost arbitration, in the region where DEVAV is negative, post-arbitration clearance rates increase monotonically in DEVAV. Therefore, the effect of an arbitration loss on performance becomes more pronounced as the award becomes small relative to the average of the offers, or as DEVAV becomes more negative. When DEVAV is just above zero, the union has won arbitration. At this point, in the sixth post-arbitration month, there appears to be a jump in productivity, although this jump attenuates after 12 months. This jump is suggestive of a "Lombardi" effect: wining matters for productivity, even when the stakes are small, and suggests that the productivity of police officers may depend on category-based counterfactuals. When the arbitrator rules for the police, the marginal disutility of effort is decreasing slightly in DEVAV, and certainly not increasing. In other words, the slope of the DEVAV/performance relationship changes at the threshold of a union win. Theories of reference-dependent preferences stress that losses resonate more than gains [Koszegi and Rabin (2003)]. Figure 6 shows that the negative productivity effects associated with large losses are far more prominent than the positive productivity effects associated with large gains.

To formally distinguish between the three non-mutually exclusive hypotheses—H1, H2 and H3—I estimate a model that nests these three alternatives by interacting the arbitration outcome with

the spread in offers and the arbitrator award. These interactions allow me to test whether there are heterogeneous effects from differential arbitration rulings as a result of differences in the award and the spread in offers. The model I estimate is:

$$
\begin{aligned}
(9) \qquad y_{mtrb} = {} & \alpha + \mu_m + \eta_t + \gamma_b \\
& + \delta_1(\text{PBUL}_\text{b} * \text{post arb}_\tau) + \delta_2(\text{PBUL}_\text{b} * \text{post arb}_\tau * \text{AWARD}_\text{b}) \\
& + \delta_3(\text{PBUL}_\text{b} * \text{post arb}_\tau * \text{SPREAD}_\text{b}) \\
& + \beta_1(\text{PBUW}_\text{b} * \text{post arb}_\tau) + B_2(\text{PBUW}_\text{b} * \text{post arb}_\tau * \text{AWARD}_\text{b}) \\
& + \beta_3(\text{PBUW}_\text{b} * \text{post arb}_\tau * \text{SPREAD}_\text{b}) + \varepsilon_{mtrb},
\end{aligned}
$$

where SPREAD is the union offer minus the employer offer, and award is the arbitrator award, which is the employer offer when the arbitrator rules against the union and the union's offer otherwise. A one percentage point increase in the spread between the union and employer offers results in β_3 and δ_3 additional clearances per 100,000 capita in post-arbitration months in PBUW and PBUL municipalities respectively. If either of these coefficients are sizable and, in particular, if β_3 is positive and δ_3 is negative, then there is evidence in support of H3; the post-arbitration performance differentials are at least partially determined by the distance of the award to the counter-offer. The estimates presented in Table 7 are derived by fitting model (9) to the data. I will begin discussing the clearance rate outcome and initially focus on the case where the arbitrator rules against the police bargaining unit. Then I will discuss the case where the arbitrator rules in favor of the union.

Table 7 shows that, for PBUL cities, the third hypothesis is largely supported whereas the first two are not.[23] The estimated coefficient on the post arbitration × PBUL interaction, in columns (1) and (2), is small in magnitude and is not statistically significant, suggesting no change in

[23] The assumption implicit in this interpretation is that omitted variables that are correlated to the spread in offers do not directly affect the magnitude of the post-arbitration change in productivity. Farber's (1980) model suggests that spreads in offers is a function of uncertainty over the arbitrators preferred awards. The size of the offer spread may also be correlated to failures of negotiator perspective taking or mental rigidity. The interpretation assumes that neither uncertainty nor these failures lead police to reduce effort after arbitration losses. A related point is that, in New Jersey, the spread in offers has declined over time and, consequently, the results can also be interpreted as capturing a downward trend in the magnitude of post-arbitration productivity declines in PBUL cities across arbitration years. While it is not possible to completely discount this possibility, I have experimented with the inclusion of interacting post arbitration × PBUL and post arbitration × PBUW with year trends, which do not qualitatively change the results of Table 7.

clearances from the pre- to the post-arbitration period in PBUL cities when the spread between the offers and the size of the award were small. The arbitrator award also has no discernable effect on the magnitude of the PBUW/PBUL clearance rate differential in the post-arbitration period, implying that the mechanism underlying the post-arbitration changes in clearance rates is more complicated than a straight effort/wage relationship.[24]

While the award does not appear to be an important determinant of the magnitude of the post-arbitration drop-off in clearances in PBUL cities, the spread on the offers is. More sizable differences in offers exacerbate the decline in clearances in PBUL communities following arbitration.[25] The coefficient on three way interaction of post-arbitration, PBUL, and SPREAD in column (1) is estimated as -11.82 clearances per 100,000 capita with a t-ratio of 4.44. This estimate means that a one percentage point increase in the difference between the union and the employer offer is associated with approximately 8% monthly clearances when the union loses. Appendix Figure 5 plots cumulative event-study estimates of the post-arbitration PBUW/PBUL clearance rate differentials for cities with 25% largest values of SPREAD and the 25% smallest values of the spread in offers. In this figure is it visually apparent that large gaps between union and employer offers result in large post-arbitration clearance rate differentials between PBUL and PBUW cities.

By contrast to the case of union losses, the results in Table 7 lend some support to H1 in the case of union arbitration wins. The post-arbitration increase in clearances observed in Table 2 and Figures 1-4 do not appear to be increasing in the arbitrator award nor in the spread of the offers as seen in column (1). Rather, the increase in clearances is driven almost entirely from the post-

[24] Shirking-style efficiency wage models predict a causal effect of the wage relative to an alternative market wage on effort. It is possible that the offer spread is acting as a proxy for the outside wage. To address this, I matched a subset of the cities in the arbitration sample to information on the number of employees and total payroll of police agencies from the Census of Municipalities and calculated the difference in the post-arbitration salary and the average salary of police officers in the same county. Using this measure, instead of the arbitration award, has no qualitative effect on the estimates. The main analysis does not use award levels because the Census of Municipalities is available only after 1982 and only includes only a subset of the cities in arbitration. Table and description of the analysis are available upon request.

[25] The gift-exchange class of efficiency wage models make predictions based on wage levels, whereas I have focused on wage changes. To determine whether the results are sensitive to whether spreads and awards are expressed as levels or percent changes, I reestimated model (9) with offer spreads and awards expressed as wage levels for a subset of cases matched to data from the Census of Municipalities. These estimates show no qualitative differences in the findings presented in Table 6. Table and description of the analysis are available upon request.

arbitration × PBUW second-order interaction, which is estimated as 22.52 with a standard error of 13.94.

An equivalent way to interpret the estimates in Table 7 is to recognize that when arbitrators rule against union's, the resulting decline in clearances decreases in relation to the size of the employer's offer and increases in relation to the size of the union's unrealized demand. In fact, since the coefficient on the post-arbitration × PBUL × AWARD interaction is essentially zero, the difference in the pre-arbitration and post-arbitration clearance rate is increasing in the employer's offer (the arbitrator award) by the same degree to which it is decreasing in the union's demand. This result is consistent with H3 for PBUL cities, the third explanation for the post-arbitration productivity differentials. Disappointment appears to play a role in the post-arbitration fall in clearances when arbitrators rule against union's, since for losing union's, the post-arbitration reductions in clearances are increasing in the counter-offer that was never implemented. This result can also be couched as the response of police officers with reference-dependent preferences to unfavorable arbitrator decisions by noting that the spread in final offers can be rewritten as the distance of the arbitrator award from a reference-point, which I assume to be a convex combination of the two offers. Specifically, the reference-point can be written as:

$$\text{(10)} \qquad \text{REFERENCE}_b = \alpha \text{FOEMP}_b + (1-\alpha)\text{FOUNION}_b ,$$

where the parameter $\alpha \in [0,1]$. When the arbitrator rules in favor of the police union, the difference in the offer obtained and the reference-point is:

$$\text{(11)} \qquad \begin{aligned} \text{FOUNION}_b - \text{REFERENCE}_b &= \alpha(\text{FOUNION}_b - \text{FOEMP}_b) \\ &= \alpha \text{SPREAD}_b . \end{aligned}$$

Similarly, if the arbitrator rules in favor of the municipality, the distance in the offer obtained from the reference point is:

$$\text{(12)} \qquad \begin{aligned} \text{FOEMP}_b - \text{REFERENCE}_b &= (1-\alpha)(\text{FOEMP}_b - \text{FOUNION}_b) . \\ &= -(1-\alpha)\text{SPREAD}_b . \end{aligned}$$

Therefore, the coefficient β_3 from model (9) can be expressed as $\beta_3 = \alpha\theta$, where θ represents the reduced-form response in performance resulting from deviations of the arbitrator award from the reference point, and α is a parameter that determines the location of the reference point between the two offers. One way to interpret α is as the probability of the employer win. This interpretation is on line with Koszegi and Rabin (2004) who argue that the reference point should be viewed as a rationally formed expectation. Taking this line of thought seriously, in the next section I exploit variation in the probability of employer wins across arbitration cases to test whether post-arbitration clearances depend on the deviation from a rationally formed reference point as posited in equations (11) and (12). Specifically, I test whether deviations in the arbitrator award from the expected award can account for differences in post-arbitration relative to pre-arbitration clearance rate, even after controlling for the spread in the offers.

10. Expectation-based counterfactuals

The psychology literature has found robust evidence that the pleasure of an outcome following a choice depends on both the utility of the outcome and comparisons of actual and counterfactual outcomes. For example, McGraw *et al.* (forthcoming) provide evidence that Olympic athletes make counterfactual comparisons based on their prior expectations leading silver medalists to be more disappointed than bronze medalists because, on average, the expectations of silver medalists are higher. From this perspective, the prior expectation is the relevant reference point for the utility derived from uncertain outcomes. In this section, I investigate whether the performance of police officers following arbitration depends on an estimate of the counter-factual expectation.

The starting point for this analysis is to estimate the predicted probability of arbitration employer wins using a probit model:

(13) $\hat{p}_b = F(\mathbf{X}_b\hat{\mathbf{b}})$,

where $F(\cdot)$ is the cumulative normal distribution, $\mathbf{x}_b$ is a vector of explanatory variables used to predict the probability of an employer win and $\hat{\mathbf{b}}$ is a vector of coefficients obtained from a probit regression. The vector $\mathbf{x}_b$ contains arbitration year dummies, the average of the final offers, and the length of the contract. Using these predicted probabilities it is possible to construct the expected arbitration award:

$$\text{(14)} \quad \text{EXPECTATION}_b = \hat{p}_b \text{FINALEMP}_b + (1 - \hat{p}_b)\text{FINALUNION}_b.$$

Finally, for each arbitration case I construct the difference between the expected award and the arbitrator award:

$$\text{(15)} \quad \text{DEVIATION}_b = \text{EXPECTATION}_b - \text{AWARD}_b = \begin{cases} -\hat{p}_b \text{SPREAD}_b \text{ if PBUW} = 1 \\ (1 - \hat{p}_b)\text{SPREAD}_b \text{ if PBUL} = 1 \end{cases}.$$

Note that DEVIATION depends both on the spread of the offers and the predicted probability of an employer win. Because of the variation in $\hat{p}_b$ across arbitration cases, it is still possible to separately identify the effect of larger spreads and deviations from expected awards on the size of the change in clearances following arbitration.

In Table 8 I include interactions of DEVIATION with post arbitration, PBUL, and PBUW dummies for the clearance rate outcome. Columns (1) and (3) show that there is a very strong statistical relationship between DEVIATION and the size of the post-arbitration change in clearances in PBUL cities. In column (1), the post-arbitration × PBUL × DEVIATION dummy has a coefficient of -21.38 with a t-ratio of 5.08. To interpret this coefficient, note that when the arbitrator rules against the police the actual award is smaller than the expected award. Therefore, in the PBUL case DEVIATION is positive, and a larger value of DEVIATION means that the expected award is further away from the actual award, thus resulting in lower performance.

In columns (2) and (4) I reestimate the models in Table 7, but now I also include interactions of DEVIATION with post arbitration, PBUL, and PBUW dummies. The main finding is that once I

allow the deviation of awards from the expected awards to compete with the spread in the offers, the coefficient on SPREAD × PBUL × post arbitration is substantially attenuated, estimated as -1.56 (t-ratio = 0.36) in the (12,12) sample. However, for PBUL cities, the interaction of post-arbitration and the deviation from the expected award (DEVIATION) is large and statistically significant, estimated as -19.14 (t-ratio=2.29) when using the (12,12) arbitration window. Therefore, in the case of a police arbitration loss, awards that lie closer to the expected awards correspond to smaller drop-offs in clearances by arrest following arbitrator decisions.

The reason that the coefficient on SPREAD is large in Table 7 but is virtually zero in Table 8 once DEVIATION is included in the model, is that SPREAD and DEVIATION are highly correlated, as is evident from (15). DEVIATION captures both the notion that some awards are more anticipated than others and the magnitude of the spread. For PBUL cities, the deviation from expected award dominates the spread of the offers when included in the same model, and this finding suggests that the effect of a union arbitration loss on clearances is greater both when the union offer is far from the employer offer and when there is greater anticipation of a union win. Therefore, the results in Tables 8 provide fairly strong evidence in support not only of the proposition that the performance of police officers depends on the deviation between the award and a reference point, but that the reference point is correlated with a rationally formed expectation of the arbitrator award.

11. Conclusion

Arbitration systems offer a rich setting to study how workers respond to relative changes in compensation. The advantage of the approach taken in this paper is multifold. First, information on both the enacted offers and the counter-offers allow me to explore how deviations from reference payoffs affect productivity. Second, theoretical models of FOA suggest that arbitration rulings are orthogonal to the facts of the case since the information that is relevant to compensation is already incorporated into the final offers. While I cannot reject that cities with different arbitration rulings are similar in a variety of characteristics before arbitration, the statistical models are flexible and the parameters of interest are identified even if the arbitrator draws from the information set available to

him or her at the time of arbitration. Lastly, high frequency data on clearances allows me to capture dynamics in performance that may not be detected using more coarse time intervals.

It is well known that FOA awards are low quality because they lie outside the range of negotiated settlements. This study shows that FOA can have additional inefficiencies arising through the behavioral response of participants to unfavorable outcomes. Ichniowski (1982) finds that arbitration helps reduce the propensity for police to strike, which suggests that police departments in contract disputes not subject to compulsory arbitration could, in principle, experience productivity losses that are even more sizable than the ones presented in this paper.

The findings in this paper suggest several avenues for future work. First, the literature on group polarization suggests that members of group discussions advocate more extreme positions than individuals who do not participate in group discussion. Additional work needs to be done to determine whether productivity responses to arbitration are exacerbated by the fact that the arbitration rulings in this paper represent group level outcomes. Second, models of final offer arbitration can be written to take into account the effect of differential rulings on productivity. For example, a question that arises is whether employers manage expectations of workers to minimize the behavioral costs arising from unfavorable payoffs. In some cases, it may be optimal for employers engaged in FOA to make offers that are unlikely to be selected in order to benefit from higher productivity when employees win. Such behavior could be one reason why employer win rates are fairly low in this study. Lastly, future studies should consider whether the behavioral responses associated with differential arbitration outcomes, as outlined in this study, represent a general phenomenon relating to allocative mechanisms that clearly demarcate winners from losers.

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Data Appendix:

NJ PERC:
The data provide information on FOA cases that took place in New Jersey between 1978 and 1995, and include information on the offers submitted to the arbitrator, which are expressed as percent changes on the previous contract's wage, and whether the arbitrator ruled in favor of the municipal employer or police bargaining unit. I exclude state, county and university or college law enforcement agencies and 4 arbitration cases for which the month of arbitration was unavailable. Six arbitration cases are excluded because there is no information on population for the corresponding towns. Including these towns with imputed values for population makes no substantive difference for the estimates.

FBI UCR:
I match arbitration to monthly clearance and crime data from the FBI Uniform Crime Reporting System data files obtained from the Inter-University Consortium for Political and Social Research (ICPSR) for 1975 through 1996. The data files include reports by police departments on felony crimes and clearances. Datasets were matched on the name of the municipality. This merge was complicated by non-uniform reporting of municipal names and because there are multiple cities in New Jersey with the same name. For example, New Jersey has five Washington Townships and a Washington Borough. In such cases I used additional information to match the cities, like county and census population size. I excluded 21 additional arbitration cases because 15 municipalities did not report to the UCR system during the year of arbitration. There are an additional 104 arbitration cases on non-compensation disputes that are not used in the main analysis. The resulting data set contains 434 arbitration cases from 255 different cities over compensation disputes. Some of these cases will be dropped from the analysis because they lie too close to one another, resulting in overlapping event-study windows. The rules used to determine eligibility for inclusion in cases with overlapping windows is described below.

Selection of cases with overlapping windows:
I employ the following selection rules when arbitration windows overlap:

(i) Exclude an arbitration window if the arbitration date falls on the post-arbitration period of another case (in the same city).
(ii) If the arbitration date in case A falls on the pre-arbitration window of case B, then only exclude months in A's window that overlap with the post-arbitration months of case B. By the previous rule, case B is excluded.
(iii) If the post-arbitration months in A overlap with the pre-arbitration months in B, then keep A and exclude B.

One may be concerned that in dropping only part of a bargaining window, I allow for the possibility of composition bias since some arbitration windows will be truncated. However, dropping incomplete bargaining windows does not qualitatively change the estimates, nor do specifications that control for arbitration window fixed-effects, which control for permanent differences in cities around the time of arbitration.

OBTS datafile:

File consists of individuals arrested for felonies between 1987 and 1990, obtained final disposition between 1989 and 1990, in municipalities that experienced arbitration between 1988 and 1989. There are 40 arbitration cases used in this analysis. For the sentence outcome, 13 offenders who received the death penalty were dropped from the sample. If the same offender appears more than once in the data, only the first offense is used. Offenders with a missing offense code or conviction code are dropped from the sample. Sentence is the maximum length of the jail sentence imposed for an offense expressed in fraction of a year. OBTS are available from ICPSR.

Figure 1: Month-by-month comparison of PBUW and PBUL average clearance rates

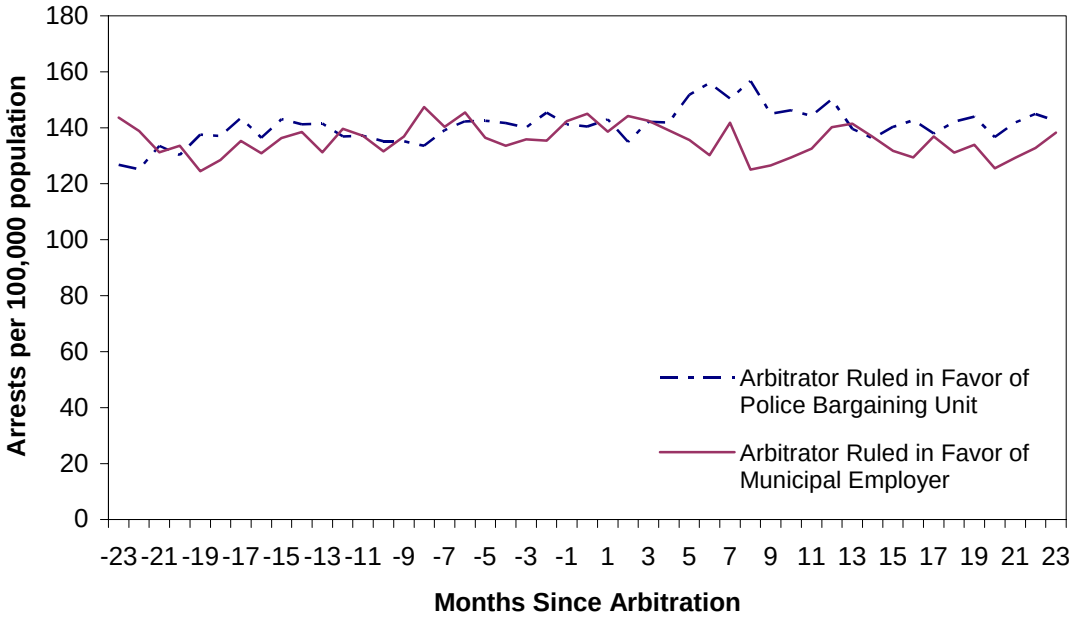
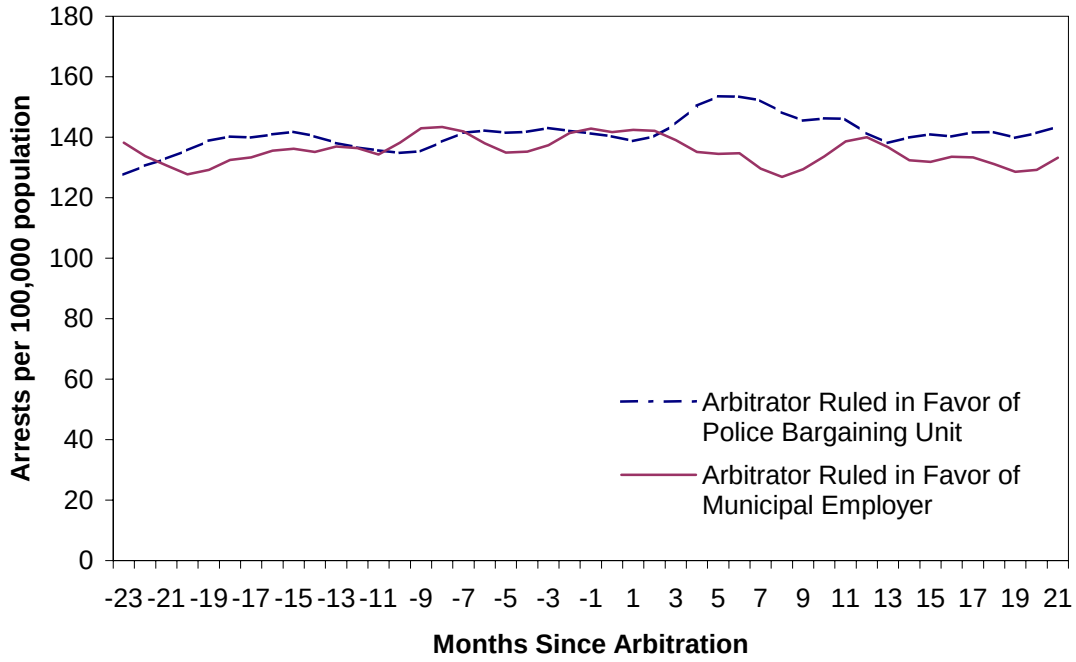
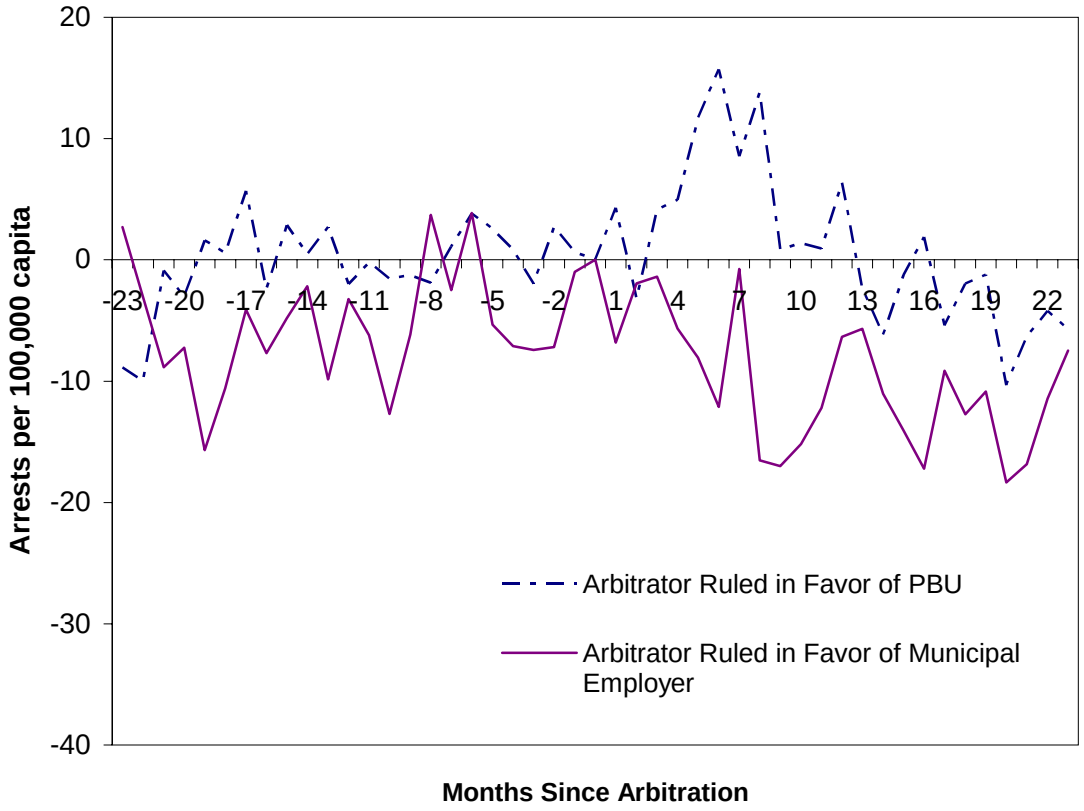


Figure 2: Smoothed month-by-month comparison of PBUW and PBUL clearance rates



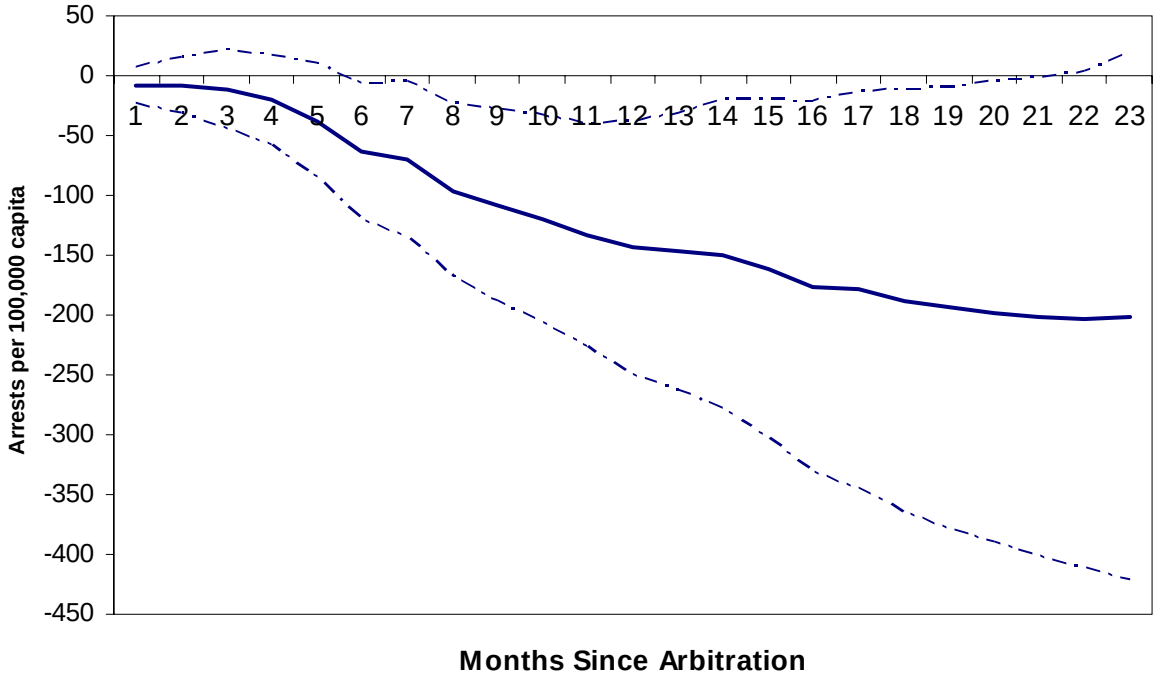
Notes: Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996. Smoothed means S_t, are $S_t = 0.25Y_{t-1} + 0.5^* Y_t + 0.25Y_{t+1}$, where Y_t denotes the raw mean in period t.

Figure 3: Event study estimates of the effect of arbitration rulings on per capita clearances; year, season and arbitration window controls



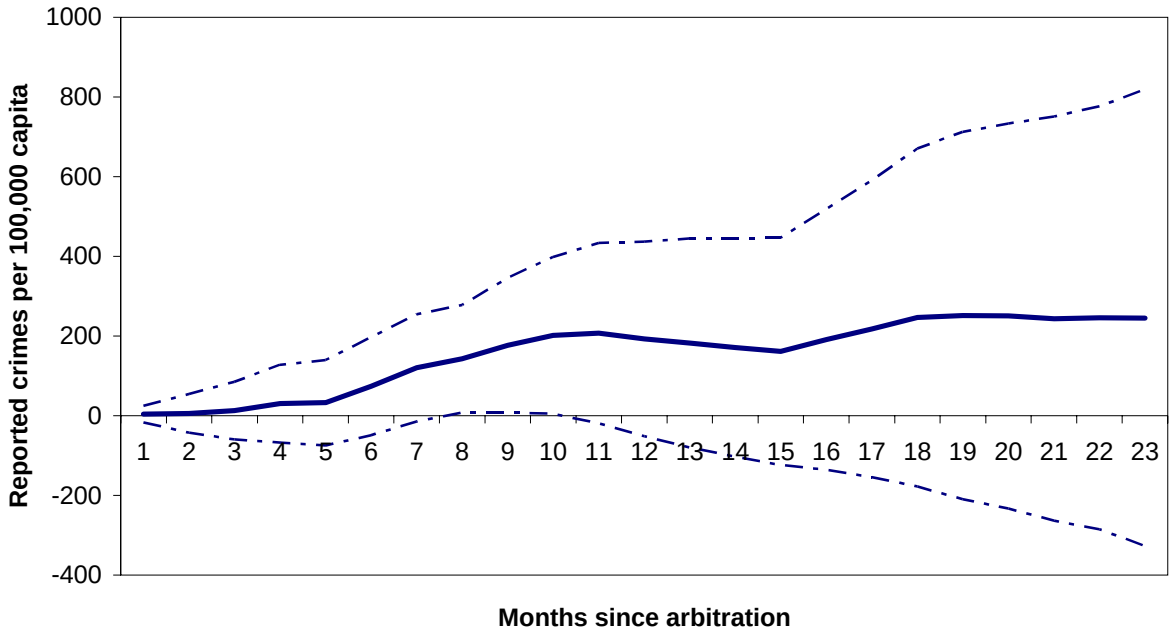
Notes: Regression-adjusted estimates based on a regression of clearances per 100,000 capita on event-time dummies interacted with indicators for whether the arbitrator ruled in favor of the union or against the union. Estimates on the interacted event-time dummies are plotted relative to the omitted month of arbitration for PBUW and PBUL cities. Regression model includes controls for year and month of arbitration dummies as well as arbitration window fixed effects. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

Figure 4: Regression adjusted event study estimates of the cumulative difference in clearances between PBUL and PBUW cities in post arbitration months relative to the entire pre-arbitration period



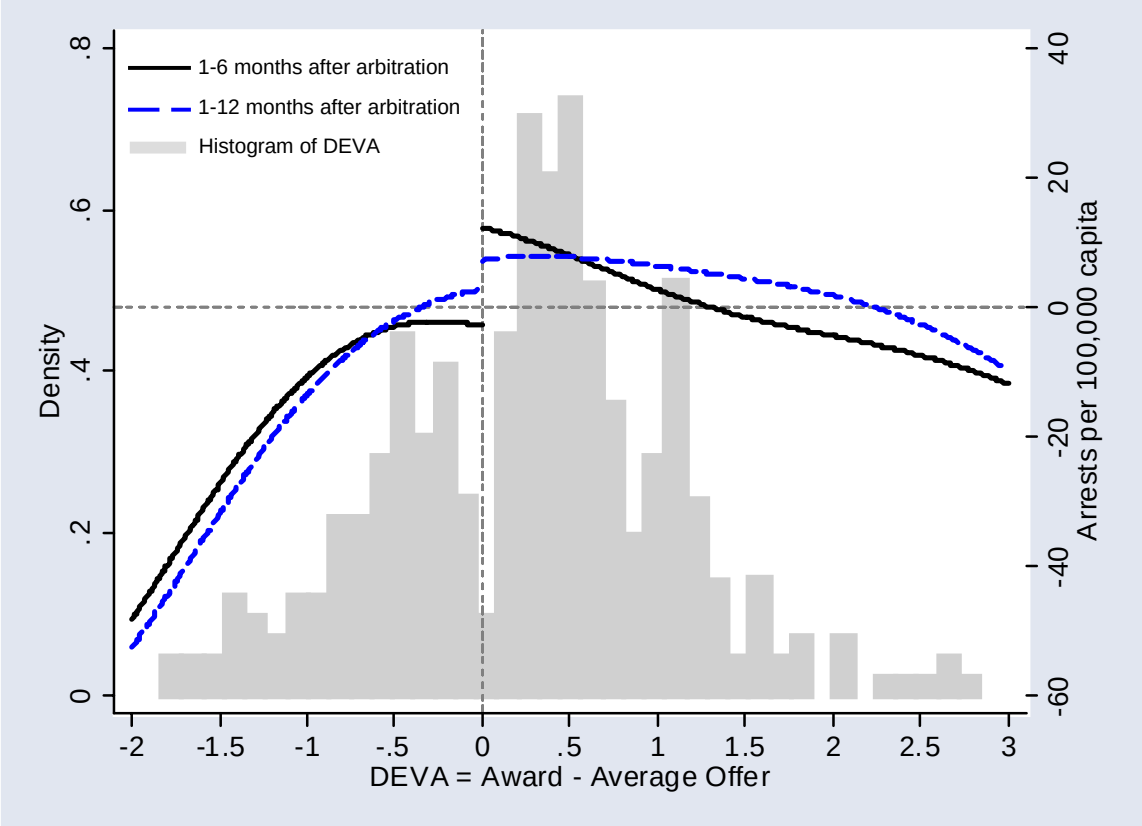
Notes: Regression-adjusted estimates based on a regression of clearances per 100,000 capita on post-arbitration event-time dummies and on post-arbitration event-time dummies interacted with indicators for whether the arbitrator ruled against the union. Estimates on the interacted post-arbitration event-time dummies are cumulated and plotted. Regression model includes controls for year and month of arbitration dummies as well as arbitration window fixed effects. The dotted lines are the 95% confidence interval. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

Figure 5: Regression adjusted event study estimates of the cumulative effect of union losses on crime.



Notes: Regression-adjusted estimates based on a regression of crimes per 100,000 capita on event-time dummies for the post-arbitration months interacted with indicators for whether the arbitrator ruled in favor of the union or against the union. Post-arbitration event-time dummies interacted with a PBUL dummy are cumulated and plotted. Regression model includes controls for year and month of arbitration dummies as well as arbitration window fixed effects. The dotted lines are the 95% confidence interval. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal crime rates at the jurisdiction level from FBI Uniform Crime Reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

Figure 6: Estimated relationship between post-arbitration clearance rates and deviation of award from the average of the offers.



Notes: Histogram is of the 348 values of DEVA, one for each arbitration case. The left axis corresponds to the density of the distribution. Solid and dotted lines are the predicted post-arbitration relationship between per capita clearances and DEVA for the 6 and 12 post-arbitration months respectively. These estimated relationships are obtained by regressing clearances per 100,000 capita on month by year dummies, 348 arbitration window dummies, a post arbitration dummy, a union arbitration win dummy and a sixth order polynomial in DEVA interacted with the post arbitration dummy. The solid and dotted lines are the estimated polynomial evaluated over values of DEVA and with a break corresponding to a union win, which occurs when DEVA is positive. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

Table 1: Sample characteristics in event time window from -17 to +17

	(1) Full-Sample	(2) Pre-arbitration: Employer Wins	(3) Pre-arbitration: Employer Loses	(4) Pre-arbitration diff: Emp. win-emp. loss
Arbitrator Rules For Emp^.	0.348	1	0	
Final Offer Employer^	6.08	6.490	5.867	0.623
	[1.70]	[1.597]	[1.722]	(0.189)
Final Offer Union^	7.66	7.921	7.518	0.403
	[1.76]	[2.097]	[1.532]	(0.197)
Population^	21,987	24,140	20,840	330.69
	[36,423]	[40,036]	[34,382]	(4101.94)
Contract length^	2.08	2.091	2.080	0.012
	[0.667]	[0.658]	[0.674]	(0.075)
Size of bargaining unit^	48.73	57.04	44.25	12.79
	[116.77]	[110.97]	[120.08]	(19.57)
Arbitration year^	1985.51	1985.8	1985.4	0.402
	[4.843]	[5.20]	[4.65]	(0.545)
Clearances	118.05	118.22	117.31	0.905
per 100,000 capita	[107.90]	[106.47]	[106.14]	(9.86)
Violent crime clearances	62.93	62.60	62.06	0.539
per 100,000 capita	[71.29]	[71.46]	[68.61]	(6.33)
Property crime clearances	55.07	55.61	55.24	0.374
per 100,000 capita	[59.50]	[56.61]	[61.92]	(5.12)
Crime reports	441.63	445.06	440.87	4.18
per 100,000 capita	[378.45]	[422.03]	[321.05]	(39.12)
Violent crime reports	94.27	92.68	92.74	-0.056
per 100,000 capita	[106.79]	[104.58]	[103.03]	(10.27)
Property crime reports	347.04	352.50	348.16	4.35
per 100,000 capita	[302.94]	[343.21]	[251.01]	(31.24)
Number of Arbitration Cases	348	121	227	

Notes: Standard errors in parentheses. Standard deviation in brackets. Observations are municipality × month cells for the 17 months after and 17 months preceding arbitration. T-tests involving time-invariant city characteristics in column (4) are conducted by regressing the city or bargaining unit characteristic on an indicator for whether the arbitration case was won by the employer for the arbitration month only. For other characteristics, namely clearance and crime rates, t-test are conducted by regressing the characteristic on a employer win indicator on all pre-arbitration months while employing robust standard errors that are clustered within the arbitration window. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance and crime rates at the jurisdiction level from FBI Uniform Crime Reports. The full-sample in column (1) has 12,083 observations. There are 191 arbitration cases missing information on number of police officers in unit.

^ The means for time-invariant city characteristics are calculated for 348 dates only.

Table 2: Event study estimates of the effect of arbitration rulings on clearances

Panel A: The dependent variable is per capita clearances in event time from -12 to +12

Exp. Variables	All Clearances			Violent Crime Clearances			Property Crime Clearances		
	(1)	(2)	(3)	(4)	(5)	(5)	(7)	(8)	(9)
Constant	142.72			75.88			66.83		
	(11.00)			(7.64)			(5.00)		
Post Arbitration	-3.96	-1.35	-7.81	-1.43	1.88	-3.03	-2.53	-3.24	-4.79
× PBUL	(3.78)	(5.40)	(4.27)	(2.36)	(3.97)	(2.66)	(1.89)	(2.08)	(2.71)
Post Arbitration	9.28	11.12	6.39	6.22	7.27	4.38	3.06	3.84	2.01
× PBUW	(3.14)	(4.34)	(2.80)	(2.10)	(3.01)	(1.86)	(1.71)	(1.89)	(2.05)
Row 3 – Row 2	13.25	12.47	14.20	7.66	5.39	7.41	5.59	7.09	6.79
	(4.91)	(6.31)	(4.24)	(3.16)	(4.48)	(2.72)	(2.55)	(2.72)	(2.44)
PBUL (Yes = 1)	0.40	6.47		-1.28	2.48		1.68		
	(16.20)	(14.79)		(10.35)	(9.24)		(7.79)		
Mnth &Yr. fe		Yes	Yes		Yes	Yes		Yes	Yes
Arbitration Wndw. Dummies {348}			Yes			Yes			Yes
Mean Dep.	144.91	144.91	144.91	76.99	76.99	76.99	67.92	67.92	67.92
Variable	[92.02]	[92.02]	[92.02]	[61.16]	[61.16]	[61.16]	[48.67]	[48.67]	[48.67]
N	8,670	8,670	8,670	8,670	8,670	8,670	8,670	8,670	8,670
R2	0.003	0.07	0.82	0.003	0.09	0.81	0.001	0.05	0.73

Panel B: The dependent variable is per capita clearances in event time from -17 to +17

Exp. Variables	All Clearances			Violent Crime Clearances			Property Crime Clearances		
	(1)	(2)	(3)	(4)	(5)	(5)	(7)	(8)	(9)
Constant	142.74			76.11			66.62		
	(11.01)			(7.90)			(4.76)		
Post Arbitration	-3.54	-1.73	-6.88	-1.59	0.32	-3.05	-1.94	-2.05	-3.82
× PBUL	(4.96)	(5.59)	(3.96)	(2.85)	(3.87)	(2.36)	(2.38)	(2.26)	(2.14)
Post Arbitration	6.96	9.87	6.18	4.85	6.56	4.62	2.10	3.31	1.57
× PBUW	(2.73)	(4.23)	(2.55)	(2.03)	(3.11)	(1.68)	(1.52)	(1.76)	(1.42)
Row 3 – Row 2	10.50	11.60	13.06	6.45	6.23	7.67	4.05	5.37	5.39
	(5.66)	(6.55)	(4.85)	(3.50)	(4.50)	(3.01)	(2.83)	(2.87)	(2.49)
PBUL (Yes = 1)	-0.228	5.19		-1.75	2.05		1.53	3.14	
	(16.43)	(15.50)		(10.80)	(9.96)		(7.56)	(6.90)	
Mnth &Yr fe		Yes	Yes		Yes	Yes		Yes	
Arbitration Wndw. Dummies {348}			Yes			Yes			
Mean Dep.	144.09	144.09	144.09	76.60	76.60	76.60	67.48	67.48	67.48
Variable	[91.70]	[91.70]	[91.70]	[61.10]	[61.10]	[61.10]	[48.22]	[48.22]	[48.22]
N	12,083	12,083	12,083	12,083	12,083	12,083	12,083	12,083	12,083
R2	0.002	0.05	0.81	0.003	0.06	0.80	0.001	0.046	0.72

Notes: Standard errors clustered on arbitration window in parentheses. Standard deviations in brackets. Number of groups in braces. Sample is weighted by population size in 1970. Observations are municipality × month cells for the 12 months after and 12 months preceding arbitration in Panel A, and for the 17 months after and 17 months preceding arbitration in Panel B. Dependant variable is clearances per 100,000 capita. All models include a constant. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. PBUW are cities where the arbitrator ruled in favor of the police bargaining unit. PBUL are cities where the arbitrator ruled against the police bargaining unit.

Table 3: Event study estimates of the effect of arbitration rulings on clearances

Panel A: The dependent variable is per capita clearances in event time from -12 to +12

Explanatory Variables	(1) Murder Clearances	(2) Rape Clearances	(3) Assault Clearances	(4) Robbery Clearances	(5) Burglary Clearances	(6) Motor Vehicle Theft Clearances	(7) Larceny Clearances
Post Arbitration × PBUL	0.034	-0.064	-2.46	-0.545	-0.873	-0.159	-3.75
	(0.122)	(0.194)	(2.42)	(0.670)	(1.016)	(0.276)	(2.21)
Post Arbitration × PBUW	0.044	-0.087	3.68	0.743	0.499	0.512	0.995
	(0.060)	(0.219)	(1.83)	(0.463)	(0.770)	(0.282)	(1.75)
Row 2 – Row 1	0.010	-0.023	6.14	1.289	1.372	0.671	4.75
	(0.089)	(0.180)	(2.39)	(0.559)	(0.873)	(0.295)	(1.99)
Mean of the Dependant Variable	0.55 [1.31]	1.721 [2.964]	65.89 [53.15]	8.83 [12.08]	15.62 [15.67]	4.051 [5.65]	48.25 [40.81]
N	8,670	8,670	8,670	8,670	8,670	8,670	8,670
R2	0.30	0.46	0.77	0.84	0.43	0.42	0.75

Panel B: The dependent variable is per capita clearances in event time from -17 to +17

Explanatory Variables	(1) Murder Clearances	(2) Rape Clearances	(3) Assault Clearances	(4) Robbery Clearances	(5) Burglary Clearances	(6) Motor Vehicle Theft Clearances	(7) Larceny Clearances
Post Arbitration × PBUL	0.015	-0.059	-2.60	-0.407	-0.937	-0.217	-2.66
	(0.089)	(0.111)	(2.10)	(0.407)	(0.674)	(0.206)	(1.68)
Post Arbitration × PBUW	-0.032	-0.107	3.57	1.19	-0.033	0.387	1.22
	(0.035)	(0.139)	(1.53)	(0.40)	(0.595)	(0.233)	(1.15)
Row 2 – Row 1	-0.047	-0.048	6.16	1.60	0.903	0.604	3.88
	(0.089)	(0.139)	(2.66)	(0.632)	(0.843)	(0.293)	(2.04)
Mean of the Dependant Variable	0.561 [1.24]	1.69 [2.93]	65.61 [53.04]	8.74 [11.95]	15.53 [16.58]	4.00 [5.51]	47.95 [40.47]
N	12,083	12,083	12,083	12,083	12,083	12,083	12,083
R2	0.32	0.43	0.76	0.83	0.42	0.391	0.73

Notes: Standard errors clustered on arbitration window in parentheses. Standard deviations in brackets. Number of groups in braces. Sample is weighted by population size in 1970. Observations are municipality × month cells for the 12 months after and 12 months preceding arbitration in Panel A, and for the 17 months after and 17 months preceding arbitration in Panel B. Dependant variable is clearances per 100,000 capita. All models include arbitration a constant, arbitration window fixed-effects and month and year dummies. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports. PBUW are cities where the arbitrator ruled in favor of the police bargaining unit. PBUL are cities where the arbitrator ruled against the police bargaining unit.

Table 4: Event study estimates of the effect of arbitration rulings on crime

Panel A: The dependent variable is per capita crime in event time from -12 to +12

Exp. Variables	All Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	636.29		158.14		478.1	
	(70.32)		(25.64)		(46.07)	
Post Arbitration	38.63	19.83	10.72	1.29	27.88	18.55
× PBUL	(23.35)	(9.51)	(7.24)	(4.85)	(16.84)	(7.29)
Post Arbitration	8.82	-1.87	7.37	-1.27	1.72	-0.547
× PBUW	(18.50)	(14.88)	(6.24)	(3.15)	(13.35)	(12.97)
Row 3 – Row 2	-29.81	-21.70	-3.35	-2.55	-26.16	-19.10
	(29.79)	(17.55)	(9.56)	(5.72)	(21.49)	(13.54)
PBUL (Yes = 1)	-28.29		-13.71		-14.55	
	(93.48)		(31.51)		(66.00)	
Mth &Yr fe		Yes		Yes		Yes
Arbitration Wndw. Dummies {348}		Yes		Yes		Yes
Mean Dep.	635.20	635.20	157.07	157.07	478.21	478.21
Variable	[435.6]	[435.6]	[134.5]	[134.5]	[329.2]	[329.2]
N	8,670	8,670	8,662	8,662	8,670	8,670
R2	0.001	0.94	0.27	0.93	0.25	0.92

Panel B: The dependent variable is per capita crime in event time from -17 to +17

Exp. Variables	All Crime		Violent Crime		Property Crime	
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	637.71		158.46		479.27	
	(69.90)		(25.83)		(45.30)	
Post Arbitration	42.36	17.79	15.14	3.85	27.04	13.92
× PBUL	(29.51)	(10.65)	(9.58)	(4.40)	(20.28)	(7.32)
Post Arbitration	4.07	4.78	6.23	0.37	-1.92	4.45
× PBUW	(20.94)	(11.27)	(5.95)	(3.73)	(15.92)	(8.41)
Row 3 – Row 2	-38.29	-13.01	-8.91	-3.48	-28.95	-9.47
	(36.19)	(17.57)	(11.27)	(6.46)	(25.78)	(12.26)
PBUL (Yes = 1)	-31.66		-16.03		27.04	
	(94.64)		(31.40)		(20.28)	
Mth &Yr fe		Yes		Yes		Yes
Arbitration Wndw. Dummies {348}		Yes		Yes		Yes
Mean Dep.	634.68	635.20	157.0	157.0	347.04	347.04
Variable	[439.5]	[435.6]	[134.9]	[134.9]	[302.9]	[302.9]
N	12,083	8,670	12,070	12,070	12,082	12,082
R2	0.001	0.93	0.003	0.93	0.001	0.001

Notes: Standard errors clustered on arbitration window in parentheses. Standard deviations in brackets. Number of groups in braces. Sample is weighted by population size in 1970. Observations are municipality × month cells for the 12 months after and 12 months preceding arbitration in Panel A, and for the 17 months after and 17 months preceding arbitration in Panel B. Dependant variable is crime reports per 100,000 capita. All models include a constant. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal crime rates at the jurisdiction level from FBI Uniform Crime Reports. PBUW are cities where the arbitrator ruled in favor of the police bargaining unit. PBUL are cities where the arbitrator ruled against the police bargaining unit.

Table 5: Arbitration and sentencing

Panel A: Sentencing outcome

Exp. Variables	OLS: Conviction (Yes = 1)		OLS: Incarceration (Yes = 1)	
	(1)	(2)	(3)	(4)
Post Arbitration	0.022	0.023	-0.006	0.005
× PBUL	(0.059)	(0.031)	(0.041)	(0.038)
Post Arbitration	0.020	0.012	0.064	0.053
× PBUW	(0.028)	(0.025)	(0.031)	(0.027)
Row 2 – Row 1	-0.002	-0.011	0.070	0.048
	(0.060)	(0.037)	(0.022)	(0.023)
Female (Yes = 1)	-0.031	-0.041	-0.159	-0.144
	(0.013)	(0.013)	(0.018)	(0.022)
Nonwhite (Yes = 1)	0.008	-0.007	0.030	0.021
	(0.009)	(0.013)	(0.018)	(0.021)
Age trend	-0.003	-0.002	-0.002	-0.0015
	(0.001)	(0.001)	(0.001)	(0.0007)
Offense Dums. {86}		Yes		Yes
Mean Dep.	0.792	0.792	0.319	0.319
Variable	[0.406]	[0.406]	[0.466]	[0.466]
N	6686	6686	6696	6696
R2	0.040	0.135	0.051	0.113

Panel B: Outcomes conditional on conviction

Exp. Variables	Incarceration (Yes = 1)		Sentence	
	(1)	(2)	(3)	(4)
Post Arbitration	-0.026	-0.009	-0.166	0.117
× PBUL	(0.044)	(0.044)	(0.168)	(0.175)
Post Arbitration	0.075	0.069	0.618	0.484
× PBUW	(0.031)	(0.027)	(0.222)	(0.184)
Row 2 – Row 1	0.101	0.079	0.785	0.367
	(0.026)	(0.027)	(0.150)	(0.123)
Female (Yes = 1)	-0.191	-0.167	-0.838	-0.676
	(0.020)	(0.024)	(0.122)	(0.048)
Nonwhite (Yes = 1)	0.032	0.032	0.403	0.310
	(0.020)	(0.021)	(0.114)	(0.112)
Age trend	-0.001	-0.001	0.117	0.005
	(0.001)	(0.001)	(0.006)	(0.006)
Offense Dums. {86}		Yes		Yes
Mean Dep.	0.402	0.402	1.66	1.66
Variable	[0.490]	[0.490]	[3.87]	[3.87]
N	5292	5292	5165	5165
R2	0.056	0.130	0.031	0.336

Notes: Standard errors clustered on arbitration window in parentheses. Standard deviations in brackets. Author's calculation based on NJ PERC arbitration data matched to arrestees from the Offender Based Transaction Statistics. Observations are individuals arrested for felonies between 1987 and 1990, obtained final disposition between 1989 and 1990, in municipalities that experienced arbitration between 1988 and 1989. There are 40 arbitration cases used in this analysis. All models include a constant, year and month of arrest dummies, year of final diposition dummies, and arbitration window dummies. For the sentence outcome, 13 offenders who received the death penalty were dropped from the sample. If the same offender appears more than once in the data, only the first offense is used. Offenders with a missing offense code or conviction code are dropped from the sample. Sentence is the maximum length of the jail sentence imposed for an offense expressed in fraction of a year. Conditional on conviction, the average sentence is 1.66 years (std. dev. = 3.87). Conditional on incarceration, the average sentence is 4.27 years (st. dev. = 5.21). PBUW are cities in which the arbitrator ruled in favor of the police bargaining unit. PBUL are cities in which the arbitrator ruled against the police bargaining unit.

Table 6: Event study estimates of the effect of arbitration rulings on the number of police officers and civilian personnel employed at agencies

Explanatory Variables	Law enforcement officers per 100,000 capita			Civilian personnel per 100,000 capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Post Arbitration × PBUL	2.50	0.76	3.16	6.31	5.50	2.97
	(7.46)	(2.78)	(3.49)	(4.27)	(2.90)	(3.25)
Post Arbitration × PBUW	-0.87	0.826	-0.66	0.16	-0.16	-0.20
	(6.78)	(2.82)	(2.67)	(2.54)	(1.75)	(2.44)
Row 2 – Row 1	-3.37	0.07		-6.15	-5.66	-3.17
	(5.90)	(3.56)		(2.88)	(4.19)	(3.56)
PBUL (Yes = 1)	-1.14			3.30		
	(24.85)			(8.39)		
Log Population	17.15			1.32		
	(6.43)			(1.35)		
Pop 75th %tile × Post-Arbitration × PBUL			-4.06			0.01
			(5.24)			(2.51)
Pop 75th %tile × Post-Arbitration × PBUW			2.31			4.24
			(3.82)			(6.45)
Year Dummies {19}	Yes	Yes	Yes	Yes	Yes	Yes
Arbitration Window Dummies {338}		Yes	Yes		Yes	Yes
Mean of the Dependant Variable	260.78	260.78	260.78	42.77	42.77	42.77
	[107.79]	[107.79]	[107.79]	[37.57]	[37.57]	[37.57]
N	676	676	676	676	676	676
R2	0.18	0.99	0.995	0.11	0.96	0.963

Notes: Standard errors clustered on arbitration windows in parentheses. Standard deviations in brackets. Number of groups in braces. Observations are municipality × year cells corresponding to the year before and the year after arbitration, excluding the year of arbitration. For a given municipality, an arbitration case that occurred within two years of another arbitration case is excluded from the sample. Sample is weighted by 1970 population. All models estimated with a constant. PBUW are cities in which the arbitrator ruled in favor of the police bargaining unit. PBUL are cities in which the arbitrator ruled against the police bargaining unit. Author's calculation based on NJ PERC arbitration cases matched to monthly employment levels at the jurisdiction level found in FBI Uniform Crime Report Law Enforcement Officer Killed and Assaulted data files.

Table 7: The Effect of Arbitration Rulings on per capita Clearances
Heterogeneous effects by offer spread and award

	Arbitration Window: -12 to +12	Arbitration Window: -17 to +17
Explanatory Variables	(1)	(2)
Post-Arbitration × PBUL × SPREAD	-11.82	-13.05
	(2.66)	(2.99)
Post-Arbitration × PBUW × SPREAD	-0.019	-0.896
	(1.85)	(2.32)
Post-Arbitration × PBUL × AWARD	-0.470	1.14
	(1.81)	(2.40)
Post-Arbitration × PBUW × AWARD	-2.195	-1.52
	(1.868)	(1.85)
Post-Arbitration × PBUL	12.30	4.74
	(13.94)	(16.44)
Post-Arbitration × PBUW	22.52	18.48
	(13.94)	(13.19)
p-value union loss*	0.000	0.000
p-value union win^	0.072	0.11
Mean of the Dependant Variable	144.91	144.08
	[92.02]	[91.70]
N	8,670	12,083
R2	0.81	0.81

Notes: Standard errors clustered on arbitration window in parentheses. Standard deviations in brackets. Sample weighted by population size in 1970. Dependant variables are number of clearances and reported crime per 100,000 capita. All models include arbitration window fixed-effects, and month and year dummies. PBUW are cities in which the arbitrator ruled in favor of the police bargaining unit. PBUL are cities in which the arbitrator ruled against the police bargaining unit. SPREAD denotes that union's offer less the employer's offer. AWARD denotes the arbitrator award. The mean of SPREAD in column (1) is 1.57 with a standard deviation of 1.39. AWARD denotes the arbitrator award. The mean of AWARD in column (1) is 7.16 with a standard deviation of 1.63. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance and crime rates at the jurisdiction level from FBI Uniform Crime Reports.

* Denotes the p-value corresponding to the test that the variables Post-Arbitration × PBUL × SPREAD , Post-Arbitration × PBUL × AWARD, and Post Arbitration × PBUL are jointly significant.

^ Denotes the p-value corresponding to the test that the variables Post-Arbitration × PBUW × SPREAD, Post-Arbitration × PBUW × AWARD, and Post Arbitration × PBUW are jointly significant.

Table 8:
The Effect of Arbitration Rulings on per capita Clearances
Heterogeneous effects by spread, award, and deviation from expected award

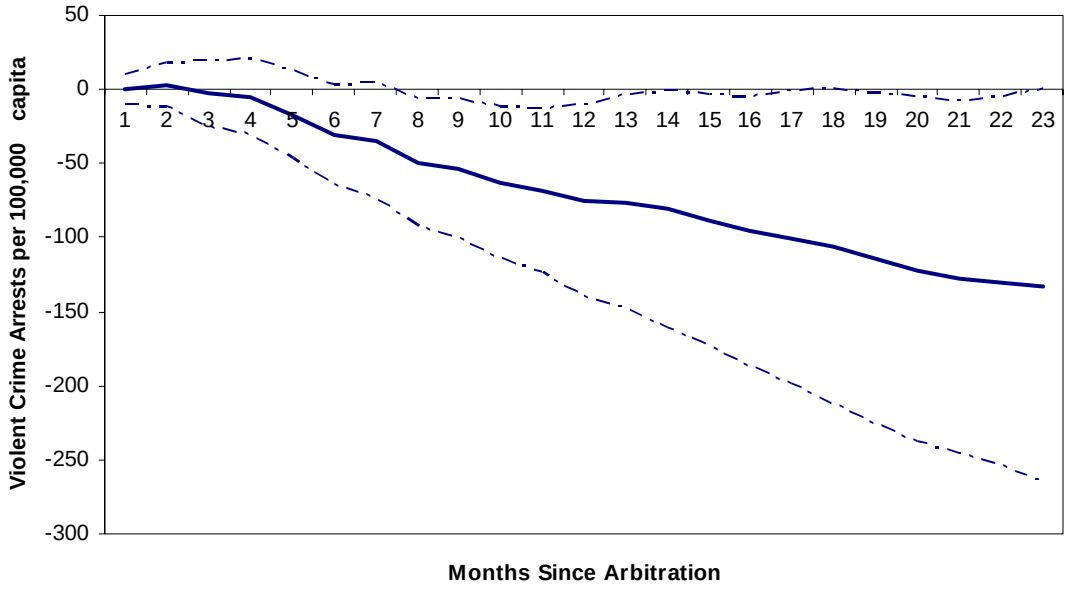
	Arbitration Window: -12 to +12		Arbitration Window: -17 to +17	
Explanatory Variables	(1)	(2)	(3)	(4)
Post-Arbitration × PBUL × DEVIATION	-21.38	-19.14	-24.15	-27.20
	(4.21)	(8.37)	(5.17)	(11.82)
Post-Arbitration × PBUW× DEVIATION	-5.25	-7.40	-0.10	-3.78
	(5.87)	(6.57)	(6.04)	(7.67)
Post-Arbitration × PBUL × SPREAD		-1.56		1.90
		(4.34)		(5.75)
Post-Arbitration × PBUW × SPREAD		-0.84		-1.26
		(2.15)		(2.85)
Post-Arbitration × PBUL × AWARD	-2.25	-2.14	-0.80	-1.09
	(1.81)	(1.85)	(2.15)	(2.02)
Post-Arbitration × PBUW × AWARD	-3.41	-3.56	-2.04	-2.40
	(2.13)	(2.07)	(2.25)	(2.25)
Post-Arbitration × PBUL	23.98	23.75	17.85	19.50
	(13.59)	(13.57)	(14.57)	(14.01)
Post-Arbitration × PBUW	28.63	30.05	20.47	23.21
	(14.17)	(13.86)	(15.14)	(15.55)
p-value union loss*	0.00	0.00	0.00	0.00
p-value union win^	0.06	0.09	0.14	0.21
Mean of the Dependant Variable	144.91 [92.02]	144.91 [92.02]	144.08 [91.70]	144.08 [91.70]
N	8,670	8,670	12,083	12,083
R2	0.82	0.82	0.81	0.81

Notes: Standard errors clustered on event window in parentheses. Sample weighted by population size. Dependant variable is clearances per 100,000 capita. All models include arbitration window fixed-effects, and month and year dummies. DEVIATION denotes the difference in the arbitrator award and the expected award. The expected award is a mathematical expectation of the award given the union and employer offers and a predicted probability of an employer win. The predicted probability of an employer win is estimated with a probit model using as predictors year and month of arbitration dummies, indicators for union and employer attorney representation, the average of the final offers, and the length of the contract. The mean of DEVIATION in column (1) is -0.028 with a standard deviation of 0.785. The mean of DEVIATION in PBUL cities is 0.79, with a standard deviation of 0.68. The mean of DEVIATION in PBUW cities is -0.46, with a standard deviation 0.39. SPREAD denotes that union's offer less the employer's offer. The mean of SPREAD in column (1) is 1.57 with a standard deviation of 1.39. AWARD denotes the arbitrator award. The mean of AWARD in column (1) is 7.16 with a standard deviation of 1.63. PBUW are cities in which the arbitrator ruled in favor of the police bargaining unit. PBUL are cities in which the arbitrator ruled against the police bargaining unit. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports.

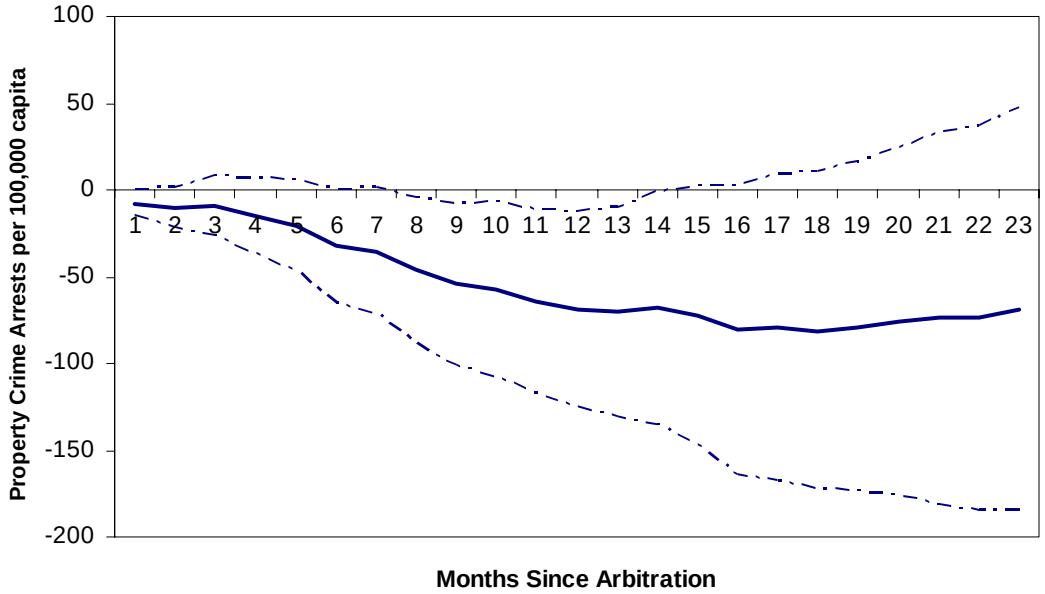
* Denotes the p-value corresponding to the test that the variables Post-Arbitration × PBUL × DEVIATION, Post-Arbitration × PBUL × SPREAD , Post-Arbitration × PBUL × AWARD, and Post Arbitration × PBUL are jointly significant in columns (2) and (4), and Post-Arbitration × PBUL × DEVIATION, Post-Arbitration × PBUL × AWARD, and Post Arbitration × PBUL in columns (1) and (2).

^ Denotes the p-value corresponding to the test that the variables Post-Arbitration × PBUW × DEVIATION, Post-Arbitration × PBUW × SPREAD , Post-Arbitration × PBUW × AWARD, and Post Arbitration × PBUW are jointly significant in columns (2) and (4), and Post-Arbitration × PBUW × DEVIATION, Post-Arbitration × PBUW × AWARD, and Post Arbitration × PBUW in columns (1) and (2).

Appendix Figure 1: Regression adjusted event study estimates of the cumulative difference in violent crime clearances between PBUL and PBUW cities in post-arbitration months relative to the entire pre-arbitration period

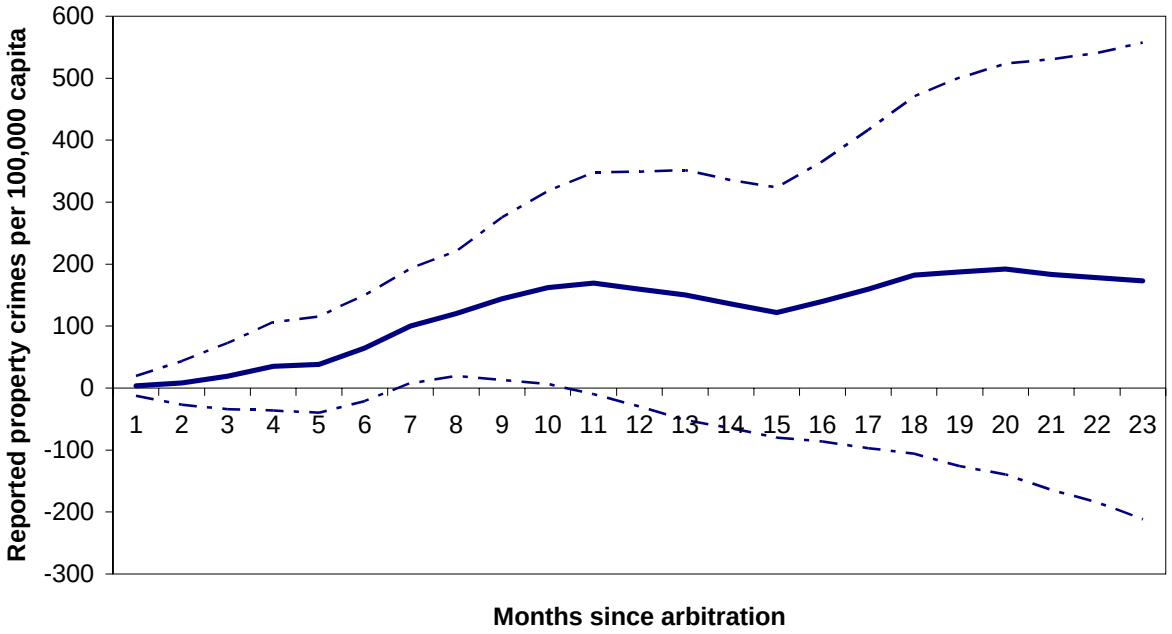


Appendix Figure 2: Regression adjusted event study estimates of the cumulative difference in property crime clearances between PBUL and PBUW cities in post arbitration months relative to the entire pre-arbitration period

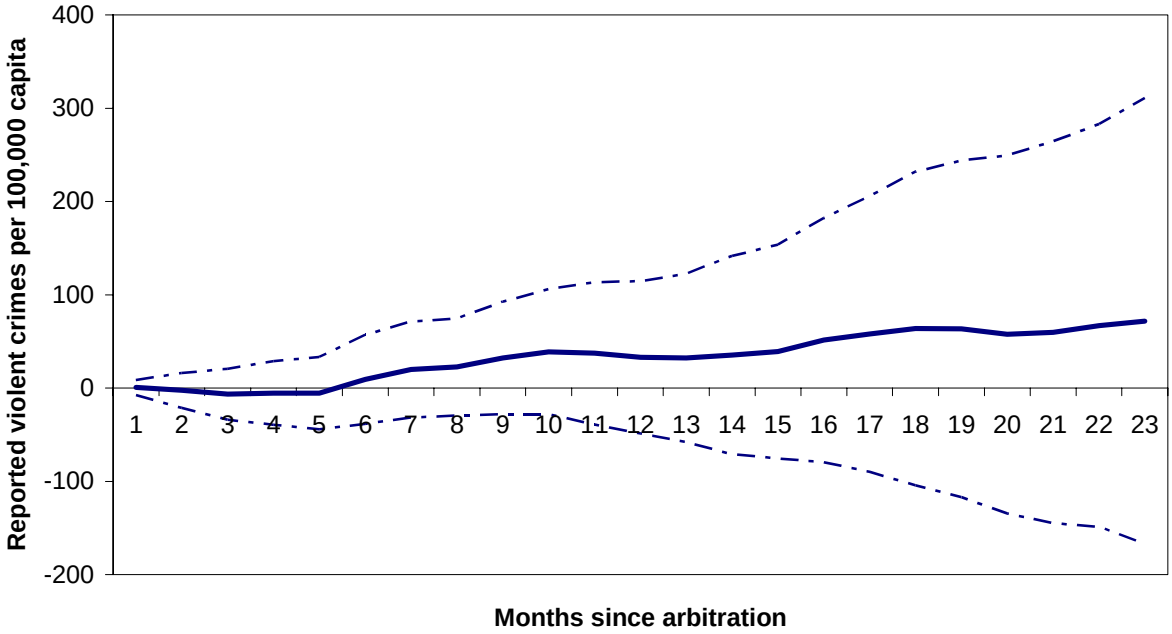


See notes for Figure 4.

Appendix Figure 3: Regression adjusted event study estimates of the cumulative difference in post- and pre-arbitration property crime rates in PBUL municipalities.

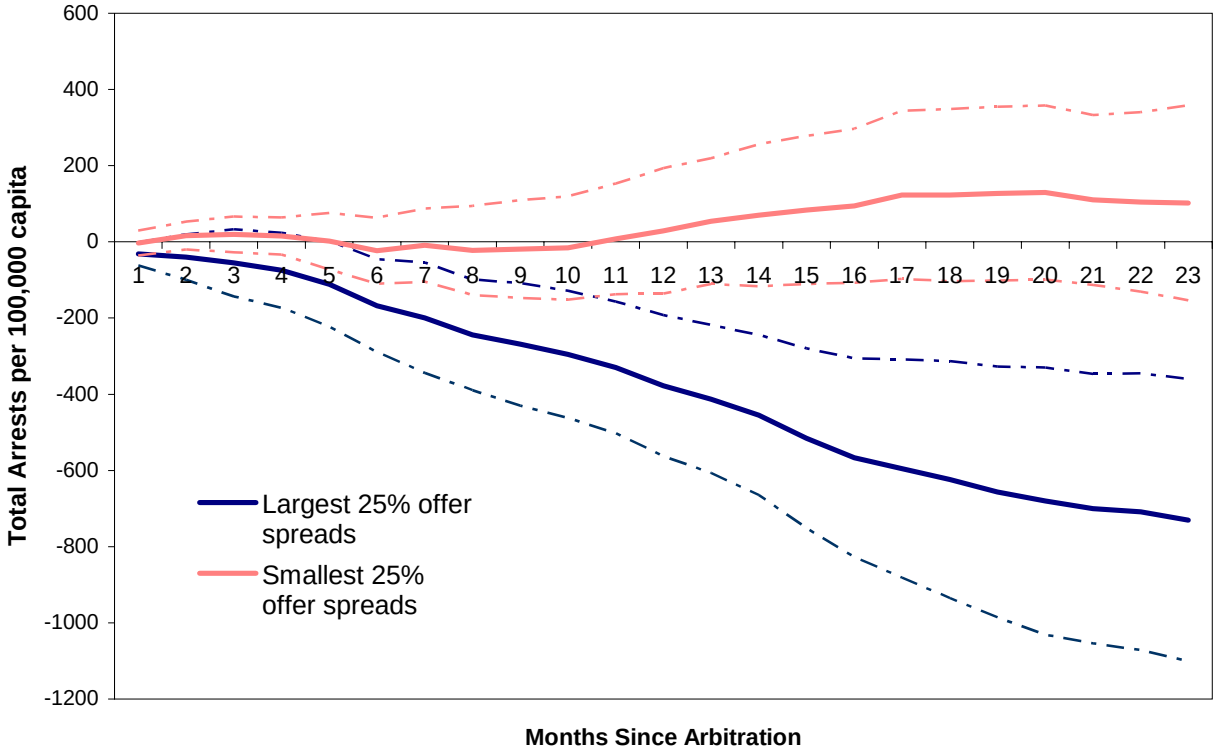


Appendix Figure 4: Regression adjusted event study estimates of the cumulative difference in post- and pre-arbitration violent crime rates in PBUL municipalities.



See notes for Figure 5.

Appendix Figure 5: Regression adjusted event study estimates of the cumulative difference in total clearances between PBUL and PBUW cities in post-arbitration months relative to the entire pre-arbitration period by size of offer spread



Notes: Regression-adjusted estimates based on a regression of clearances per 100,000 capita on post-arbitration event-time dummies and on post-arbitration event-time dummies interacted with indicators for whether the arbitrator ruled against the union. Estimates on the interacted post-arbitration event-time dummies are cumulated and plotted. Regression model includes controls for year and month of arbitration dummies as well as arbitration window fixed effects. The dotted lines are the 95% confidence interval. The offer spread is the union offer less the employer offer. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from uniform crime reports. Sample weighted by 1970 Census population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.