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ABSTRACT

Several theories suggest that pay raises below a reference point will reduce job performance. Final offer arbitration for police unions provides a unique opportunity to examine these theories, as the police officers either receive their requested wage or receive a lower one. In the months after New Jersey police officers lose in arbitration, arrest rates and average sentence length decline and crime reports rise relative to when they win. These declines are larger when the awarded wage is further from the police union's demand. The findings support the idea that considerations of fairness, disappointment, and, more generally, reference points affect workplace behavior.

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I. Introduction

While much of our understanding of labor markets derives from the idea that workers respond to incentives, an important theoretical literature stresses psychological and non-market factors as determinants of employee performance.¹ There is growing evidence that employees are less satisfied not just with low pay, but with pay below a reference or “fair” wage.² Although there is provocative evidence from laboratory experiments (e.g. Fehr and Gächter [2000]), there is little field evidence about whether these deviations also reduce productivity. This paper assesses the relationship between pay raises, anticipated pay raises, and employee performance in an actual labor market.

Final offer arbitration of wage disputes provides an attractive real-world laboratory to investigate how on-the-job performance of labor market participants responds to changes in compensation relative to a reference point. In final offer arbitration (FOA), disputing parties submit offers to an arbitrator who is constrained to choose one of the disputant’s offers in a binding settlement. Final offer arbitration is commonly employed in public sector unions. 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, when arbitrators rule against the union, the size of the loss incurred.

There are several reasons why arbitration systems are attractive for this study. First, because there is often divergence between what the union asked for and what they actually received, I can assess the effect of deviations of awarded pay from pay demands on productivity. Second, arbitration rulings have a surprise component, as the arbitrator’s preferred award may be unknown. In fact, the equilibrium outcome in standard theories of final offer arbitration—for example Farber

¹ A prominent example is the fair wage-effort hypothesis of Akerlof and Yellen [1990].

² Adams [1965] is a classic study on equity theory that experimentally links higher pay with higher effort. More recently, surveys of employers suggest that deviations from reference wages affect worker morale and that managers

[1980]—is for the arbitrator to select the winning party at random. This is because the offers the disputing parties submit to the arbitrator serve as sufficient statistics for information relevant to police compensation. Consistent with this prediction, I cannot reject that cities in which the arbitrator ruled against the 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 of bargaining units and their employers.

Police performance declines sharply when officers lose arbitrations. The per capita number of crimes cleared (solved) by arrest are 12 percent higher in the months following arbitration when arbitrators ruled in favor of the police officers, relative to when arbitrators ruled for the municipal employer. Felony arrests in cities where police unions lost are also associated with lower incarceration probabilities and shorter jail sentences, even after conditioning on detailed charged crime categories, suggesting that police may reduce their cooperation with prosecutors following arbitration losses. Additionally, I show that union losses are associated with a 5.5 percent increase in reported crime rates in the months following arbitration decisions.

The degree to which an arbitration decision is considered a win or a loss may depend on employee expectations upon entering arbitration. Numerous laboratory experiments provide evidence that utility depends not only on actual outcomes, but also on what could have occurred in a different state of the world [Bateman et al. 1997; Mellers, Schwartz and Ritov 1999; Thaler 1980]. This idea has been posited in numerous paper from the theoretical literature [Gul 1991; Kahneman and Tversky 1979; Koszegi and Rabin 2005]. While the experimental literature offers important evidence that effort depends on a worker's pay relative to a reference point, there are few studies addressing this question using market data.³

are reluctant to lower wages [Bewley 1999; Blinder and Choi 1990; Kaufman 1984; Agell and Lunborg 1995]. Brown et al. [2005] present survey and experimental evidence that relative pay matters for employee satisfaction.

³ Two studies addressing this question are Cappelli and Chauvin [1991] and Verhoogen et al. [2003]. Both studies use variation in local labor market conditions in the location of plants within firms to test whether plants located in areas with better economic conditions experience lower dismissal rates. Both studies find a moderate relationship

I find that police performance depends on the awarded pay raise relative to expectations. The change in performance of police officers following an arbitration loss depends not only on the amount of the pay raise, but on the counter-offer that was demanded but never implemented as well. Therefore, comparisons of pay raises to counterfactuals influence police effort when they lose. By contrast, counterfactual comparisons are not relevant when police win in arbitration, signifying that these workers are prone to a form of loss aversion. On the whole, these results highlight the importance of managing and, in particular, lowering employee expectations prior to manipulating wage policy in organizations.

II. Conceptual Framework of Final-Offer Arbitration

Before proceeding, it is helpful to review the context surrounding the arbitration cases under analysis, as well as to outline a conceptual framework of final offer arbitration. Between 1978 and 1996 the default procedure for dispute resolution between police bargaining units and their employers in New Jersey was final offer arbitration.⁴ 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 the contract expiration date, and, if an agreement was not reached 60 days before that date, parties must begin arbitration proceedings. Between 1976 and 1996, approximately nine percent of contract expirations of police officers resulted in arbitration over salary demands.

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 theoretical

between local labor market conditions and the extent of dismissals and absenteeism. Rizzo and Zeckhauser [2003] find that target incomes are a good predictor of future hourly earnings of physicians, but not of hours worked.

⁴ Ashenfelter and Dahl [2005] and Lester [1984] review public sector dispute resolution procedures in New Jersey.

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.⁵ While this model may not be a literal description of reality, it provides a useful framework for thinking about the problem negotiating parties face in 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 the 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 common distribution for all arbitrators.⁶ The "facts" of the case enter into the model through the mean, μ , of r_a . If the police are relatively productive and they deserve a sizable pay raise, then μ will be large, but how large depends on how the population of arbitrators value 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:

$$EU(r_e, r_u) = P * U((1 + r_e) * w) + (1 - P) * U((1 + r_u) * w),$$

$$EV(r_e, r_u) = P * V((1 + r_e) * w) + (1 - P) * V((1 + r_u) * w).$$

⁵ Another classic model of FOA, developed by Gibbons [1988], in which the arbitrator learns from the proposed offers, will lead to similar intuitions.

⁶ 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.

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

- (i) *If disputing parties are equally risk-averse, the winner in arbitration is determined by a coin toss.*
- (ii) *If parties exhibit constant absolute risk aversion, the arbitrator will be more likely to rule in favor of the more risk-averse party. However, the probability of an employer win is fixed, and is therefore invariant to the facts of the case.⁷*
- (iii) *The offer spread ($r_u - r_e$) is a function of σ , the uncertainty about the arbitrator's preferred award.*

Prediction (i) can be understood by recognizing that information from the case that the arbitrator uses to make a decision is taken into account in the disputants' final offers. The arbitrator selects an offer based on forecast error that is uncorrelated to the facts of the case. If the union is more risk-averse than the employer, then prediction (ii) implies that while we may not observe an equal proportion of wins for police and employers in the data, cities where police won and lost in arbitration should not be systematically different. In Section IV, I offer evidence that arbitrator rulings are in fact orthogonal to the facts of the case. Prediction (iii) implies that as the arbitrator's preferred award becomes more unpredictable, the offer spread increases.

The model is silent as to why disputes ever reach arbitration. 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 or experience mental rigidity in negotiations, then there may be an incentive to enter into arbitration proceedings. Under these scenarios, there may be variation in the likelihood that the employer is selected, depending on the size of the offers. For example, the model described above can be easily modified to allow parties to observe the true μ with measurement error. Parties will submit offers that are too high or too low than would be otherwise optimal, depending on the error realization. In this case, the probability that the arbitrator rules in favor of the employer will depend

on measurement error. As I will discuss later, there is evidence that higher average offers are associated with higher probabilities of employer selection, suggesting that parties vary in the conservativeness of their offers. But because there is no relationship between the arbitration outcome and past performance, it appears that the factors leading parties to submit more or less realistic offers are not systematically related to historical measures of police performance.

III. 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, the 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.⁸ 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 through the share of the working day spent actively policing.

Arrests represent costly effort for the police officers involved, due to the energy expended both in the act of arrest and in the subsequent paperwork. However, under some 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 on this measure. I will also consider measures of performance that may be 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 my analysis because of sporadic data availability in the case of sentencing and the extent of noise in the case of the crime rate.

⁷ Prediction (ii) follows from equation (8) of Farber [1980].

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 was used in Ashenfelter and Dahl [2005]. The data describe 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 information on whether the arbitrator ruled in favor of the municipal employer or bargaining unit. I match arbitration cases to monthly clearance and crime data from the FBI Uniform Crime Reporting System (UCR) data files for 1976 through 1996. Some of the cases are dropped from the analysis because they lie too close to one another, resulting in overlapping event-study windows. The Data Appendix explains the sample selection criteria in more detail. The resulting data set contains 383 arbitration cases from 255 different cities over salary disputes.

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 and incarceration, as well as sentence length, depending on the outcome of arbitration. A disadvantage to the OBTS data is that they are available for only a limited number of years.

IV. Sampling Scheme and 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 me to estimate the effect of arbitration rulings on outcomes after controlling for year \times month, 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. Initially, the analysis only includes cities that experienced arbitration and, for these cities, only months that are contained in the arbitration window. I then consider specifications with an augmented sample, consisting of the initial sample and a comparison group of

⁸ 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.

cities that never underwent arbitration, in order to facilitate the estimation of state-wide time patterns in clearances and crime.

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, but the findings are robust to the use of different lengths of the arbitration window.

A city in which the arbitrator ruled in favor of the police bargaining unit will be denoted as a “Union” city. Likewise, a city in which the arbitrator ruled in favor of the municipal employer will be denoted as an “Employer” city. In the simplest estimator, I compare the average difference in clearances in Union and Employer cities prior to arbitration to the average difference in clearances after arbitration. This difference-in-difference estimator measures the impact of arbitration rulings on performance in Union cities relative to Employer cities. In richer models, I control for time and arbitration window dummies and also allow for the additional comparison group of non-arbitrating municipalities to help estimate the time effects.

Table I 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 the pre-arbitration period for cities in which the police union won in arbitration, and the third column provides pre-arbitration information on cities in which the police union lost in arbitration. Because cities tend to be small, there are relatively few monthly crimes and clearances.⁹ Cities experience an average of 65 violent crime clearances per 100,000 residents per month, amounting to approximately 14 violent crime clearances per month. Because of the presence of zeros in the data, especially in narrow categories of crime, I chose to analyze per capita levels, rather than percentage changes or logs.

Column (1) shows that the employers only won 34 percent of their cases. Therefore, it does not appear that arbitrators are indifferent between the offers of the two parties. However, it is

⁹ The cities under analysis have an average population of 21,345 (median of 12,331). On average, bargaining units consist of 43 police officers, or about 87 percent of the police force in a given municipality.

possible that union negotiators are more risk-averse than city negotiators and therefore submit more conservative offers. Column (4) presents the difference in means between Union and Employer cities in the pre-arbitration period. Consistent with prediction (ii) of the theory of FOA described above, the means do not reveal much of a difference in crime rates, per capita clearances, or other characteristics of union win and union loss municipalities in the pre-arbitration period suggesting no obvious relationship between the arbitrator decision and information available to the arbitrator at the time of arbitration. However, there is a relationship between the size of the offers and the probability of an employer win. This relationship suggests that, conditional on the facts of the case, parties may submit offers of varying sizes, perhaps due to heterogeneity in risk-aversion or divergent beliefs. On average, larger offers lead to a higher probability of employer selection. But the degree to which offers are out-of-line with the arbitrator's preferred award is uncorrelated with city characteristics prior to arbitration.

V. The Effect of Arbitration Rulings on Clearance Rates

V. A. Graphical Evidence

In order to determine how arbitration outcomes affect the number of crimes cleared by arrest, I begin by comparing the average number of clearances in the months prior to arbitration to the number of clearances in the months after arbitration for Union and Employer cities. I present these averages in Figure I for the grand total of clearances using a relatively long (23,23) month arbitration 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.¹⁰ The plot suggests that prior to arbitration Union and Employer cities had similar monthly clearance rates, but that after arbitration the clearance rates in these two types of cities diverged, with police forces in Union cities clearing more crimes by arrest. This clearance rate differential appears to emerge around four months after arbitration, peaking at seven months, and persisting for approximately 22 months. Visual inspections of Figure I reveals that Union and Employer cities do

¹⁰ The sample means, as in most estimates in this paper, are weighted by population of the jurisdiction in 1976.

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 or her decision rule even after conditioning on the final offers of the disputing parties.¹¹

By adding arbitration window and time (month \times year) fixed-effects, I allow for arbitrary unobserved heterogeneity across arbitration windows—to hold constant permanent differences in clearance rates in cities around the time of arbitration—and I allow for a general time pattern in clearances in the state over the sample period. In order to facilitate the estimate of the time effects, I include a comparison group of 197 cities that never underwent arbitration with police unions. These additional cities are included in the sample for the entire 1976-1996 period. The sample therefore consists of cities that underwent arbitration in months within the arbitration window and cities that did not undergo arbitration for all months in the 1976 to 1996 interval.

Figure II is the regression-adjusted version of Figure I. To construct the figure, I estimate:

$$(1) \quad y_{tbc} = \alpha + \psi_{FE} + \delta_{\tau} * \text{UnionWins}_b + \beta_{\tau} * \text{EmployerWins}_b + \varepsilon_{tbc}, \quad \tau = -23, \dots, 23,$$

$$\psi_{FE} = \eta_t + \gamma_b + \nu_c,$$

where y_{tbc} denotes clearances per 100,000 capita in time period t (month \times year), time since arbitration τ , arbitration window b , and city c . The term ψ_{FE} denotes the collection of fixed-effects included in the model, consisting of arbitration window fixed-effects (γ_b), month \times year fixed-effects (η_t), and city fixed-effects (ν_c). Note that the arbitration window fixed-effects absorb the city fixed-effects in cities that experienced arbitration, as they are specific to the time period around the arbitration date. Because of the inclusion of the arbitration window fixed-effects, it is not possible to identify parameters β_{τ} and δ_{τ} for each of the months relative to arbitration. Therefore, β_0 and δ_0 are excluded from the model, and the remaining coefficients should be interpreted

¹¹ Formally, I cannot reject that union win and union loss cities have the same pre-arbitration trends in clearances at conventional levels of significance.

relative to clearances in the arbitration month. The estimated coefficients $\hat{\beta}_\tau$ and $\hat{\delta}_\tau$ ($\tau = -23, \dots, 23$) are plotted against event-time in Figure II.

Figure II confirms that the pattern observed in Figure I is unaffected by regression-adjustments. 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 union win and union loss municipalities in clearances after arbitration.

I conduct inference by estimating the cumulative effect of arbitration rulings on clearance rates over each of the post-arbitration months. Using shortened (12,23) windows, in order to allow for more arbitration cases in the analysis, I fit the following model to the data:

$$(2) \quad y_{tbc} = \alpha + \psi_{FE} + \xi_\tau + \theta_\tau * \text{UnionLoses}_b + \varepsilon_{tbc}, \tau = 1, \dots, 23. ^{12}$$

The estimate $\hat{\theta}_\tau$ is the estimated gap in per capita clearances between Union and Employer cities in month τ after arbitration, relative to the average gap in clearances between Union and Employer cities during the entire pre-arbitration period. A negative value of $\hat{\theta}_\tau$ means that the gap in the clearance rate between Union and Employer cities in the τ th month after arbitration is wider than the average gap in the clearance rate 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 $\hat{\theta}_\tau$ to obtain the total unexplained gap in the number of clearances between Union and Employer cities j months after arbitration:

$$\hat{\omega}_j = \sum_{\tau=1}^j \hat{\theta}_\tau, j = 1, \dots, 23.$$

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.

The plot of $\hat{\omega}_j$ along with a 90 percent confidence interval is presented in Figure III. Because there is autocorrelation in monthly clearances within municipalities, standard errors are

clustered within the arbitration windows. In Figure III, the Union/Employer clearance rate gap is significantly larger following arbitration than in the months before arbitration. The plotted $\hat{\omega}_j$ points are negative and downward sloping. The decline in clearances in Employer municipalities relative to Union municipalities begins after the second month, although I cannot reject that $\hat{\omega}_j$ is significantly different than zero at the 10 percent level until four months after arbitration.¹³ The post-arbitration difference in clearances between Employer and Union communities appears to persist for approximately one year and the cumulative difference totals to more than 225 crimes cleared by arrest per 100,000 capita.

V. B. Regression estimates

Table II reports parametric regression estimates corresponding to the (12,12) arbitration window. Column (1) reports the change in the clearance rate from the pre- to the post-arbitration period for Union and Employer cities. As this model is regression-unadjusted, the estimates can be interpreted as simple differences in means. The estimates in column (1) imply that when arbitrators ruled in favor of the union, police forces obtained on average 4.99 more monthly clearances per 100,000 capita after arbitration than before arbitration. A union loss is associated with a reduction of 6.79 monthly clearances per 100,000 capita. In column (2) the sample is weighted by population in the jurisdiction in 1976. As is evident, weighting does not have a substantial effect on the magnitude of the estimates.

Column (3) presents estimates from a model that controls for time effects as well as arbitration window and city fixed-effects. As in the statistical models that were used to construct Figures 2 and 3, I incorporate a comparison group of 197 cities that never underwent arbitration in order to facilitate the estimation of the time effects. Adding the comparison group and controlling for fixed-effects does not change the basic conclusions from the regression-unadjusted specifications: that police wins are associated with increases in clearances after arbitration, while police losses are

¹² Estimates are robust to the use of alternative arbitration windows.

¹³ One reason that it takes a few months for this difference to emerge may be that it takes time to develop cases leading to arrest.

associated with declines. Note that because there are no arbitration cases that lead to no decision, it is not possible to separately identify the effect of winning in arbitration, losing in arbitration, and simply finishing arbitration, irrespective of the outcome. 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 II corresponds to the change in the Union/Employer clearance rate differential between the post-arbitration and pre-arbitration periods. In column (3) this change is estimated as 15.71 clearances per 100,000 capita with a t-ratio of 4.19. This estimate implies that the difference in the number of monthly clearances per 100,000 capita between Union and Employer cities widened by 12 percent after arbitration.

The post-arbitration Union/Employer differentials in clearances are present and roughly of the same magnitude for both violent and property crimes (see Table II, columns (4)-(9)). Table III presents estimates of post-arbitration clearance rate differentials by specific crime type. To the extent that officers may exercise discretion on whom to arrest, they do not appear to alter enforcement in murder and rape cases following arbitration rulings. However, arbitration rulings have a large effect on clearances of assault and robbery crimes as well as all categories of property crimes.

VI. The Effect of Arbitration Rulings on Crime Rates

Table IV shows that the changes in post-arbitration clearance rates are not being driven by changing crime rates. In fact, clearances and crime rates move in the opposite direction after arbitration. Column (6) shows that post-arbitration months in Employer cities are associated with 19.86 additional monthly property crimes per 100,000 capita (t-ratio = 1.78), whereas union arbitration wins are not associated with a significant change in the property crime rate following arbitration. There appears to be no relationship, however, between the arbitration decision and the violent crime rate. 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 -15.46 in column (6), it is imprecise, having a standard error of 13.96. Nevertheless, the 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 estimates reported in Table II, as, mechanically, police make more clearances when crime rates are higher.

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. A cumulative plot of post-arbitration crime rates for Union cities relative to Employer cities, analogous to Figure III, shows that Employer cities experienced elevated crime rates in the post-arbitration months relative to Union cities. However, the confidence interval is very wide and the cumulative estimates are never significantly different than zero. Recalling Table IV, however, there is a significant change in the crime rate in Employer cities after arbitration. Therefore, it is instructive to make fewer demands on the data and simply plot the cumulative difference in Employer city crime rates at each post-arbitration month relative to the average crime rate in Employer cities during the entire pre-arbitration period. To construct this figure, I estimate:

$$(3) \quad y_{mt\tau} = \alpha + \psi_{FE} + \xi_{\tau} * \text{UnionWin}_b + \theta_{\tau} * \text{EmployerWin}_b + \varepsilon_{mt\tau}, \tau = 1, \dots, 23.$$

For each post-arbitration date I calculate the cumulative excess number of crimes reports up to that date in Employer municipalities relative to the average crime rate during the entire pre-arbitration period:

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

Estimates of $\hat{\omega}_j$ are plotted in Figure IV for total crime reports. Inspection of Figure IV shows that there were more than 600 excess crime reports per 100,000 capita in Employer cities in the 23 months after arbitration. Crime reports appear to rise 5 months after arbitration and are statistically distinguishable from zero at the 10 percent level of significance during months 6 through 16.

The estimates on the crime outcome 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. While the point estimates are somewhat imprecise, they suggest that the elasticity of crime with respect to clearances is approximately -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 as reported in Levitt (1997) and McCrary (2002). The increase in crime observed after police arbitration losses may occur either through criminals' response to the reduced presence of police, or through a containment channel—more clearances result in fewer free potential criminals.

The estimate on reported crime due to a losing arbitration outcome can also be used to bound how much the employer (taxpayers) are willing to pay in order to reduce crime. A back-of-the-envelope calculation implies that the willingness to pay to prevent a single crime is bounded above at \$487, which is a very low quantity.¹⁴ To put this figure into perspective, assume, as before, that the elasticity of crime with respect to police officers is -0.3. In a typical town in the sample, hiring one additional police officer at \$50,000 per year would result in a decline of 9 crimes, amounting to \$5,560 per crime prevented. It may be that the willingness to pay to reduce crime is low because most of the excess crimes reported following arbitration losses are property crimes and may be relatively minor in nature. It could also be that city managers failed to recognize the social costs associated with police arbitration losses. If this is the case, then the ratio of the true willingness to pay to prevent crime and the one implied by the calculation above may be considered a measure of the ignorance of employer negotiators.

While there were statistically significant increases in reported crime in Employer 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 Union cities, which is negligible but estimated with large standard errors. The noisiness that is inherent in the crime

¹⁴ We can suppose that a risk-neutral employer compares the expected payoff from entering into arbitration to the expected payoff from settling with the union. Consider the case of a typical town in the sample, which has a population of 21,345 and 50 police officers with salaries of \$50,000 per year. Using a probability of a union arbitration win of 0.66 if the employer does not settle with the union, it reveals that it is unwilling to pay more than $2 \times \text{wage bill} \times (\text{union offer} - \text{employer offer})$, or \$75,000, in order to prevent 154 crimes from occurring.

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.

VII. The Effect of Arbitration Rulings on Sentencing Outcomes

In this section I ask how arbitration affects defendant outcomes through the courts, focusing in particular on the probability of conviction and incarceration and on the sentencing of arrestees. These measures are informative of how the “quality” of policing may have changed after arbitration. I will consider whether the elevated number of clearances following police arbitration wins is the result of police targeting a different mix of crimes depending on the arbitration ruling and whether sentencing outcomes of arrestees depends on the arbitration outcome because police collect less evidence or present lower quality evidence to the prosecutor following losses.

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 disposition. 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. Ultimately, I compare

sentencing outcomes of individuals who were arrested for felonies in Union and Employer cities between 1987 and 1990 and obtained final disposition between 1989 and 1990 in municipalities that experienced arbitration between 1988 and 1989. Because the data files are organized by date of disposition, I am necessarily missing defendants who were involved in prolonged trials and whose cases may have been 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 Union and Employer cities.

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 V 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 defendants, year and season effects, year of final disposition dummies, and arbitration window dummies.

The difference-in-difference estimates on conviction probabilities in columns (1) and (2) show that the probability of conviction for individuals arrested in Union cities did not change from the pre- to the post-arbitration period relative to Employer cities. These estimates suggest that the rising number of arrests in Union municipalities following arbitration is not the result of police arresting the innocent. Defendants who were arrested after an arbitration decision were no less likely to be convicted if the arbitrator ruled in favor of the police relative to when the arbitrator ruled against the police.

The estimates in column (3) of Panel A indicate that defendants arrested following union wins were more likely to be incarcerated as compared to defendants arrested after union losses. In column (3), the probability of incarceration increased by 0.076 points in Union cities relative to Employer cities in the 12 months after arbitration relative to the 12 months before arbitration. This estimate has a t-ratio of 3.55 and corresponds to approximately a 22 percent increase in the

incarceration probability in Union cities from the pre- to the post-arbitration period relative to the change in the incarceration probability in Employer cities. The effect of arbitration on the incarceration outcome is large and suggests that there were important differences in policing strategies depending on the arbitration outcome in the cities in this sample. One reason that there could be such a large increase in the probability of incarceration is that police in Employer cities shifted their arrests towards less-serious offenders. However, when conditioning on charged offense categories in column (4), the difference-in-difference estimate of a union win versus a union loss declines by only 0.022 points. It appears that defendants in Union cities have a higher incarceration probability for a given crime charged suggesting that, perhaps, police collected less evidence, or provided less evidence to prosecutors, following arbitration losses.

VIII. Employment, Overtime and Turnover

The observed changes in clearance and crime rates after arbitration are not the result of changes in the number of police. Table VI shows that employment at Union cities did not change significantly relative to employment in Employer 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 Union and Employer 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 Employer 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.

It is possible, though, 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 changing prices. If the labor demand curve is downward sloping, then the increase in wages associated with a union win would lead the municipal employer to cut overtime hours, resulting in fewer clearances, in contrast to what actually occurs. Additionally, 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 a 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.

Two additional mechanisms that may account for post-arbitration changes in productivity are increased turnover and adverse selection of officers following union losses.¹⁵ There are several reasons why these mechanisms fail to explain the patterns in clearance rates seen in the data. First, the turnover rate of police officers is typically low, as compared to other occupations; typical officers have 10 years of seniority [Aamodt 2004]. Second, collective bargaining agreements would not permit a city to substitute higher for lower skilled officers. If the increase in clearances following police wins is coming from new and relatively skilled recruits, these recruits would have to fill existing vacancies. But, as already mentioned, there is no evidence that employment levels changed after arbitration. Finally, police are highly responsive to arbitration rulings even though the gaps between the disputing party's offers are often not large, averaging around 1.5 percent. The pronounced response to arbitration rulings

¹⁵ 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, as in Weiss [1980]. However, McCrary [2003] finds no evidence that the introduction of 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.

with relatively small spreads suggests that psychological factors may have influenced the quantity of effort supplied by the officers following arbitration. I now turn to this question.

IX. Reference Point Comparisons and Police Performance

In Bewley's [1999] study, managers report that morale is hurt when pay raises are lower than expectations. The consequences of low morale can be significant, resulting in a mood that is not conducive to work and work environments where employees are unwilling to make sacrifices for the organization. Employee reactions to lower than expected pay may represent a response to a perceived insult, or simply disappointment. Under both of these scenarios, productivity depends on changes in pay relative to a reference level. A central goal of this study is to examine whether, in fact, the degree to which workers reduce effort following arbitration depends on the size of the loss that is incurred.

IX. A. Clearances depend on the comparison of awarded pay to pay demands

As mentioned in the Introduction, determining whether productivity depends on pay raises in relation to a reference point is typically challenging because reference points are unobserved. A natural reference point candidate is the "fair wage," or what other police officers earn in similar circumstances. Such a wage may be difficult to calculate because, to do so, one must have the same information on police performance and city characteristics that was available to the parties at the time of arbitration, or some notion of the right comparison city. However, because information on each party's offer in arbitration is available, this calculation is unnecessary in this case. Unions engage in arbitration precisely because, from their point of view, the employer's offer is not the fair offer. Therefore, in the case of a union loss, the gap between the union's demand and the award (the employer's offer) is a measure of the degree of the loss. A simple test of whether comparisons of pay to reference points affect police performance is to determine whether reductions in clearances following arbitration become more pronounced as the gap between the union demand and the arbitrator award widens. Such a finding would suggest that counterfactuals, which have no material effect on the police officers, affect workplace behavior.

A convenient way to implement the test is to plot the relationship between post-arbitration changes in performance and the deviation of the award from the average of the offers. This way, one can see how performance responds to both the distance between the union demand and the award, in the case of a police loss, and the distance between the award and the employer offer, in the case of a police win.¹⁶ In Figure V, I examine this relationship. For each of the 383 arbitration cases, I calculate the change in the average clearance rate from the 12 months before to the 12 months after arbitration. I then non-parametrically estimate the expected change in clearances conditional on the distance of the arbitrator award to the average of the final offers, allowing for a break at zero, using a local linear smoother. I term this conditional expectation the “effort-response” function.

The effort-response function provides support for the reference point hypothesis. In interpreting Figure V it is useful to note that when the award is greater than the average of the final offers the union has won arbitration, and when the award is lower than the average of the final offers the employer has won arbitration. The noteworthy feature of this plot is that when police lose in arbitration there is a pronounced positive relationship between the distance of the award from the average of the final offers and the change in clearances following arbitration. This relationship indicates that police effort following arbitration depends on the gap between the pay raise that the union demanded in negotiations and the actual award.

Theories of reference-dependent preferences emphasize that losses resonate more than gains [Kahneman and Tversky 1979]. If the observed positively sloped effort-response function occurring when police lose has a psychological explanation, as would be the case if comparisons of awards to reference points lead police to reduce performance due to considerations of fairness or disappointment, then we should expect this function to flatten when the award meets or exceeds the reference point, in other words, when police win. Indeed, the figure shows that as the difference between the award and the average of the offers crosses the zero threshold, there is a marked jump in the effort-response function, but the slope of the curve flattens considerably.

¹⁶ Another way to think about this approach, from the theoretical perspective, is that the arbitrator forms a preferred (fair) offer by determining what other police forces would earn in similar cities. In expectation, the average of the parties equilibrium offers

Column (1) of Table VII confirms that the degree of loss is strongly related to the change in post-arbitration clearances in a parametric model with covariates. This column presents estimates from a parsimonious parameterization of the effort-response function:

$$(4) \quad y_{mtcb} = \alpha + \psi_{FE} + \delta_1 \text{post}_\tau + \delta_2 (\text{post}_\tau \times \text{LOSS SIZE}_b) + \varepsilon_{mtcb},$$

where $\text{LOSS SIZE} = (\text{union demand} - \text{arbitrator award})$. The estimate on the coefficient on LOSS SIZE is -10.31, with a t-ratio of 6.49, signifying that the gap between the award and the union demand has a very strong relationship to the change in the post-arbitration clearance rate.¹⁷ This relationship implies that if two unions each receive a 5 percent raise, but one asked for 15 percent while the other asked for 6 percent then, *ceteris paribus*, the union that demanded the smaller amount will clear approximately 90 more crimes per 100,000 capita each month following arbitration than the union that demanded a larger raise.

The flat portion of the effort-response function in Figure V, when the award exceeds the average of the offers, suggests that the size of the award alone does not drive post-arbitration productivity. Formally, I make this inference by regressing the per capita clearance rate on a post-arbitration dummy and a post-arbitration dummy interacted with the size of the award. The second and third columns of Table VII show that the estimated coefficient on this interaction is small in magnitude and statistically indistinguishable from zero. Police receiving larger pay raises are not exerting more effort than those receiving smaller pay raises following arbitration. The mechanism underlying the post-arbitration effects is clearly more complicated than a simple effort/wage relationship. At first blush, this conclusion may be considered to be at odds with the results showing that there are positive productivity effects arising from union wins. In fact, the positive productivity effect following union wins comes about from the jump in the effort-response function at the point where the difference in the award and the average of the offers is zero. This discontinuous jump in

is the arbitrator's preferred award.

¹⁷ 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 Employer cities across arbitration years. While it is not possible to completely discount this possibility, I have experimented with the inclusion of interacting $\text{post arbitration} \times \text{Employer Win}$ and $\text{post arbitration} \times \text{Union Win}$ with time trends, which do not qualitatively change the results of Table VII.

the effort-response function, which I will term the “Vince Lombardi” effect, suggests that losing arbitration affects productivity, even when the stakes are trivially small and may be indicative of a “warm glow” that comes from winning arbitration or a cloud that comes from losing.¹⁸

In order to estimate the Lombardi effect, I fit a model that allows post-arbitration productivity to depend on a cubic term in the size of the loss and an indicator for whether there as a police win:

$$(5) \quad y_{mtcb} = \alpha + \psi_{FE} + \rho_1 \text{post}_\tau + \rho_2 \text{post}_\tau \times \text{UnionWin}_b + (\text{post}_\tau \times \text{p(LOSS SIZE}_b)) + \varepsilon_{mtcb},$$

where $\text{p(LOSS SIZE}_b)$ denotes a cubic polynomial in LOSS SIZE. The coefficient ρ_2 determines the change in performance when the loss size is zero and the police win in arbitration, relative to when the loss size is zero and the police lose in arbitration. If ρ_2 is positive, then winning in arbitration increases clearances even if the difference between the offers is trivially small. In column (4) of Table VII, the Lombardi effect is estimated as 13.38, with a t-ratio of 2.52. This estimate suggests that police incorporate reference points which are based on categories (win versus loss) as well as pay. However, this result must be interpreted with some care. As noted earlier, because there are no arbitration cases that lead to no-decision, it is not possible to disentangle the effect of an arbitration win, arbitration loss, and simply finishing arbitration. Therefore, while the Lombardi effect shows that winning is important, it should be interpreted as the productivity response of a win *in relation* to a loss. For example, it could be that losing in arbitration leads to productivity declines, wining arbitration does not lead to productivity gains, while just finishing arbitration leads to gains in productivity. In this case, the Lombardi effect reflects the productivity decline associated with a small loss.

IX. B. Expectation-Based Counterfactuals

There is considerable support in the psychology literature for the idea that comparisons to expectation-based counterfactuals matter for emotional responses to outcomes and decisions.¹⁹ The

¹⁸ Professional football coach Vince Lombardi is attributed to have said that “winning isn’t everything, it’s the only

pain or pleasure following an outcome depends on the utility of the outcome, comparisons of actual to counterfactual outcomes, and the surprise associated with the actual outcome. For example, if the employees propose a pay raise that has no chance of selection, then they may not react as adversely as in the case that the proposed pay raise has a positive probability of selection. A structured way to account for both anticipation and the gap between the award and the union demand is to model police performance as depending on the difference between the award and the rationally expected award, where the expected award depends on the probabilities of winning and the offers presented. This specification is in line with the modeling approach of Koszegi and Rabin's [2005] who argue that reference points are best thought of as rational expectations. Taking this line of thought seriously, I can use variation in the probability of employer wins across arbitration cases to test whether deviations in the arbitrator award from the expected award can account for differences in post-arbitration relative to pre-arbitration clearance rates, even after controlling for the distance between the union demand and the arbitrator award.

Divergent beliefs, political economy, differences in degrees of risk-aversion across parties, changes in arbitrator behavior over time, and mental rigidity in negotiations are all reasons that may lead parties to submit offers with varying probabilities of selection. If parties vary in the conservativeness of their offers, then the probability of winning will vary across cases as well. If this is the case, then the mean of the final-offers can be used to predict the probability of an employer win, as noted by Ashenfelter and Bloom [1984]. Conditional on the facts of the case, the higher the average of the offers, the higher is the probability of employer selection. Following Ashenfelter and Bloom's [1984] and Ashenfelter and Dahl's [2005] approach, if one assumes that the arbitrator's preferred award is normally distributed, then the probability of employer selection is a probit function depending of the average of the offers. Therefore, the starting point for this analysis is to estimate the predicted probability of employer arbitration wins using a probit model, using as an explanatory variable the average of the final offers. Using these predicted probabilities, I calculate

thing.”

¹⁹ For example, see Mellers et al. [1999].

the expected award for each arbitration case: $\text{expected award}_b = \hat{p}_b \text{ employer offer}_b + (1 - \hat{p}_b) \text{ union offer}_b$, where \hat{p}_b is the predicted probability of an employer win in arbitration case b . I then construct the difference between the expected award and the arbitrator award.

For ease of exposition, in the context of a union loss I will call the gap between the union demand and the award the “loss size,” and the gap between the expected offer and the award the “deviation from expectation.” The finding presented in column (5) of Table VII is that, in cases of union losses, when both the loss size and the deviation from expectation are included in the model (after being interacted with a post-arbitration dummy), the deviation from expectation explains substantially more of the variation in the magnitude of the post-arbitration declines in clearances than the loss size. In fact, loss size has no significant relationship to clearances after controlling for the deviation from the expected award. The reason that the coefficient on the loss size is large in column (1), but is virtually zero in column (5), is that the loss size and the deviation from the expected award are highly correlated. The deviation from the expected award captures both the notion that some awards are more anticipated than others and the size of the loss. 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. If the union submits an offer that is very large but has a low probability of being selected, then the productivity response will be limited.²⁰ These findings validate the idea that effort depends on the degree to which pay raises fall below expectations, in this case rational expectations. By contrast, when the pay raise exceeds expectations, there is no statistically distinguishable effect on the performance of police officers, as seen in column (6). The lack of a relationship in this case signifies that loss aversion is an important determinant of workplace performance.

²⁰ Another way to understand this finding is that the degree to which a loss affects performance is effectively mediated by the size of the offer. Large losses, which occur when the gap between the union demand and the award is large, do not hurt as much when the average of the offers is sizable relative to when the average is smaller.

IX. C. Adaptation and Dynamics

An important aspect of the findings that remains to be discussed concerns the productivity dynamics following arbitration. Figure I shows that the productivity effects arising from arbitration decisions are persistent, peaking at seven months following arbitration, and appearing to reach a new steady-state after about a year. It is useful to interpret these dynamics through the lens of the theory of the “hedonic treadmill,” which holds that subjective well-being (SWB) adjusts to current circumstances [Brickman and Campbell (1971)]. This proposition implies that life events induce transitory deviations of SWB from baseline levels. How quickly SWB returns to baseline following an event depends on the kind of event considered and the research design of the study. Studies of lottery winners [Brickman et al. 1978] and people with severe spinal cord injuries [Silver 1982] show that adaptation to these extreme life-events occurs within a short time-frame, typically within a year. By contrast, a more recent literature has found that repeated spells of unemployment [Lucas et al. 2004] and changes in marital status [Lucas et al. 2003] appear to have long-lasting effects on life satisfaction. Nevertheless, it may be considered puzzling that people adapt to some major life events relatively quickly, but the reduced performance of police officers following an arbitration loss persists for over a year. One important difference between the context of this study and the ones reviewed above is that, in this study, police reactions to adverse or positive events may have been augmented by social interactions. The literature on group polarization suggests that members of group discussions advocate more extreme positions than individuals who do not participate in group discussion. Additionally, if there are strong feedback effects in the transmission of job satisfaction across individuals due to social interactions, one-time shocks to morale may, in theory, be highly persistent.

X. 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 final-offer arbitration suggest that arbitration rulings are orthogonal to the facts of the case because the information that is relevant to compensation is already incorporated into the final offers. In addition, 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 final-offer arbitration awards are low quality because they lie outside the range of negotiated settlements. This study shows that final-offer arbitration can have additional inefficiencies arising from 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, 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. That is, are the effects of falling below a reference point amplified when the resulting disappointment affects an entire group of workers? 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 final offer arbitration to increase expected productivity by making offers that arbitrators are unlikely to select. 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, for example negotiations that involve a single and discrete high-stakes issue.

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 information on whether the arbitrator ruled in favor of the municipal employer or police bargaining unit.

FBI UCR:

I match arbitration to monthly clearance and crime data from the FBI Uniform Crime Reporting System (UCR) 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 because reporting of municipal names was not uniform in the NJ PERC data file and the UCR files 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, like county and census population size, to match the cities. Several municipalities that did undergo arbitration are dropped from the analysis because they lie too close to one another, resulting in overlapping event-study windows. The exact number depends on the length of the arbitration window and the rules used to determine eligibility for inclusion in cases with overlapping windows is described below. There are an additional 104 arbitration cases on non-compensation disputes that are not used in the main analysis. One arbitration case is dropped because the city never

reported to the FBI in the period of arbitration. After these exclusions, 383 arbitration cases remain.

As described in the text, I include a comparison group consisting of municipalities that never underwent arbitration. There are 211 municipalities that never arbitrated with police unions between 1978 and 1995. In order to preserve a balanced panel for the comparison group, I only include cities that missed reporting to the FBI in at most 10 percent of months. This filter results in the use of 197 municipalities in the comparison group. The results are robust to different selection rules.

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 an arbitration 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 who were arrested for felonies between 1987 and 1990 and 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 fractions of a year. OBTS are available from ICPSR.

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Table I
Sample characteristics in the -12 to +12 month event time window

| | (1) | (2) | (3) | (4) |
|---|--------------------|-----------------------------------|------------------------------------|--|
| | Full-sample | Pre-arbitration: Employer wins | Pre-arbitration: Employer loses | Pre-arbitration: Employer win- Employer loss |
| Arbitrator rules for employer | 0.344 | | | |
| Final Offer: Employer | 6.11 [1.65] | 6.44 [1.54] | 5.94 [1.68] | 0.50 (0.18) |
| Final Offer: Union | 7.65 [1.71] | 7.87 [2.03] | 7.54 [1.51] | 0.32 (0.18) |
| Population | 21,345 [33,463] | 22,893 [34,561] | 20,534 [32,915] | 2,358 (3,598) |
| Contract length | 2.09 [0.66] | 2.09 [0.64] | 2.09 [0.66] | 0.007 (0.071) |
| Size of bargaining unit | 42.58 [97.34] | 41.36 [53.33] | 43.22 [113.84] | -1.86 (15.66) |
| Arbitration year | 85.56 [4.75] | 85.85 [5.10] | 85.41 [4.56] | 0.436 (0.510) |
| Clearances per 100,000 capita | 120.31 [106.65] | 122.28 [108.76] | 118.57 [104.35] | 3.71 (9.46) |
| Violent crime clearances per 100,000 capita | 64.79 [71.28] | 65.29 [72.64] | 63.16 [66.79] | 2.14 (6.11) |
| Property crime clearances per 100,000 capita | 55.51 [58.72] | 56.99 [58.61] | 55.42 [61.43] | 1.57 (4.92) |
| Crime reports per 100,000 capita | 444.03 [364.23] | 453.06 [411.99] | 439.75 [309.80] | 13.30 (35.92) |
| Violent crime reports per 100,000 capita | 95.49 [103.16] | 95.31 [101.78] | 92.90 [98.61] | 2.41 (9.44) |
| Property crime reports per 100,000 capita | 348.45 [292.10] | 357.65 [335.62] | 346.72 [242.84] | 10.93 (28.71) |
| Number of arbitration cases | 383 | 132 | 251 | |

Standard errors are in parentheses. Standard deviation are in brackets. In the full sample, observations are municipality \times month cells for the 12 months before and the 12 months after arbitration. The offers are percentage changes from wages in the previous contract. t-tests involving time-invariant city characteristics in column (4) are conducted on month only. For other characteristics, namely clearance and crime rates, t-tests 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. The full-sample in column (1) contains 9,538 observations. There are 210 arbitration cases missing information on number of police officers in unit. 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.

Table II
Event study estimates of the effect of arbitration rulings on clearances;
-12 to +12 month event time window

| | All clearances | | | Violent crime clearances | | | Property crime clearances | | |
|------------------------------------|--------------------|--------------------|--------------------|--------------------------|------------------|-------------------|---------------------------|------------------|-------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| Constant | 118.57 (5.12) | 141.25 (9.94) | | 63.16 (3.13) | 75.10 (6.86) | | 55.42 (2.88) | 66.15 (4.55) | |
| Post-arbitration × Employer win | -6.79 (2.62) | -8.48 (2.20) | -9.75 (2.70) | -2.54 (1.75) | -3.10 (1.35) | -3.77 (1.78) | -4.26 (1.62) | -5.39 (2.25) | -4.45 (1.87) |
| Post-arbitration × Union win | 4.99 (2.09) | 7.92 (2.91) | 5.96 (2.65) | 4.17 (1.53) | 5.62 (1.95) | 5.31 (1.42) | 0.819 (1.24) | 2.31 (1.58) | 2.19 (1.37) |
| Row 3 – Row 2 | 11.78 (3.35) | 16.40 (3.65) | 15.71 (3.75) | 6.71 (2.32) | 8.71 (2.37) | 9.08 (2.26) | 5.08 (2.04) | 7.69 (2.75) | 6.40 (2.30) |
| Employer Win (Yes = 1) | 3.71 (9.46) | -2.81 (14.92) | | 2.14 (6.11) | -5.73 (9.53) | | 1.57 (4.93) | 2.92 (7.51) | |
| Fixed-effects? | | | Yes | | | Yes | | | Yes |
| Weighted sample? | | Yes | Yes | | Yes | Yes | | Yes | Yes |
| Augmented sample? | | | Yes | | | Yes | | | Yes |
| Mean of the Dependent variable | 120.31 [106.65] | 120.31 [106.65] | 130.82 [370.58] | 64.79 [71.28] | 64.79 [71.28] | 72.15 [294.78] | 55.51 [58.72] | 55.51 [58.72] | 58.63 [180.55] |
| Sample Size R^2 | 9,538 0.0008 | 9,538 0.005 | 59,137 0.63 | 9,538 0.0007 | 9,538 0.0078 | 59,135 0.59 | 9,538 0.001 | 9,538 0.0015 | 59,136 0.55 |

Standard errors, clustered on the intersection of arbitration window and city, are in parentheses. Standard deviations are in brackets. The dependant variable is clearances per 100,000 capita. When indicated, the sample is weighted by population size in 1976. Observations are municipality × month cells. There are 383 arbitration cases under analysis. The samples in models (1), (2), (4), (5), (7), and (8) consist of municipalities that underwent arbitration, limited to months that are in the arbitration window—12 months after and 12 months before arbitration. The remaining models augment that sample with data on 197 municipalities that never underwent arbitration with police departments over wage disputes in the sample period. These additional municipalities are included for all months between 1976 and 1996. The employer-win main-effect is absorbed by the arbitration window dummies and is therefore omitted from models (3), (6), and (9). “Fixed-effects” consist of month × year effects (252), arbitration window effects (383), and city effects (452). 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.

Table III
Event study estimates of the effect of arbitration rulings on clearances by specific crime category; -12 to +12 month event time window

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
|---------------------------------|-------------------|-------------------|--------------------|--------------------|---------------------|--------------------------------|--------------------|
| | Murder clearances | Rape clearances | Assault clearances | Robbery clearances | Burglary clearances | Motor vehicle theft clearances | Larceny clearances |
| Post-arbitration × Employer win | -0.042 (0.064) | -0.075 (0.091) | -4.82 (1.41) | -0.376 (0.194) | -1.25 (0.589) | -0.156 (0.156) | -3.04 (1.53) |
| Post-arbitration × Union win | -0.011 (0.057) | -0.043 (0.132) | 2.67 (1.51) | 1.16 (0.536) | 0.551 (0.527) | 0.271 (0.237) | 1.37 (1.20) |
| Row 2 – Row 1 | 0.030 (0.087) | 0.033 (0.160) | 7.48 (2.03) | 1.53 (0.582) | 1.80 (0.778) | 0.428 (0.278) | 4.41 (1.92) |
| Mean of the Dependant Variable | 0.184 [3.33] | 1.15 [26.53] | 68.83 [288.31] | 1.98 [30.69] | 13.74 [110.82] | 3.85 [56.25] | 41.05 [123.58] |
| Sample Size | 59,137 | 59,135 | 59,137 | 59,137 | 59,136 | 59,137 | 59,137 |
| R^2 | 0.11 | 0.18 | 0.56 | 0.65 | 0.20 | 0.13 | 0.60 |

Standard errors, clustered on the intersection of arbitration window and city, are in parentheses. Standard deviations are in brackets. Observations are municipality × month cells. The sample is weighted by population size in 1976. There are 383 arbitration cases under analysis. The sample is municipalities that underwent arbitration, limited to months that are within the arbitration window, augmented with 197 municipalities that never underwent arbitration. Municipalities that never underwent arbitration are included for all months between 1976 and 1996. All models include month × year effects (252), arbitration window effects (383), and city effects (452). 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.

Table IV
Event study estimates of the effect of arbitration rulings on crime;
-12 to +12 month event time window

| | All crime | | Violent crime | | Property crime | |
|------------------------------------|--------------------|--------------------|-------------------|-------------------|--------------------|--------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Constant | 612.18 (63.98) | | 150.26 (23.23) | | 461.81 (42.00) | |
| Post-arbitration × Employer win | 26.86 (25.29) | 24.68 (14.68) | 7.75 (7.85) | 4.87 (4.70) | 19.19 (18.17) | 19.86 (11.19) |
| Post-arbitration × Union win | 7.64 (16.24) | 6.68 (11.42) | 7.07 (5.46) | 2.49 (4.46) | 0.170 (11.68) | 4.40 (7.87) |
| Row 3 – Row 2 | -19.21 (30.06) | -18.01 (19.12) | -0.68 (9.56) | -2.38 (6.63) | -19.02 (21.60) | -15.46 (13.96) |
| Employer Win (Yes = 1) | -31.81 (84.42) | | -20.43 (27.57) | | -11.35 (59.50) | |
| Fixed-effects? | | Yes | | Yes | | Yes |
| Mean of the dependent variable | 444.03 [364.23] | 519.42 [2037.4] | 95.49 [103.16] | 98.26 [363.76] | 348.45 [292.10] | 421.28 [1865.8] |
| Sample size | 9,528 | 59,060 | 9,529 | 59,085 | 9,537 | 59,119 |
| R^2 | 0.001 | 0.54 | 0.007 | 0.76 | 0.0003 | 0.42 |

Standard errors, clustered on the intersection of arbitration window and city, are in parentheses. Standard deviations are in brackets. Observations are municipality × month cells. The dependent variables are crime reports per 100,000 capita. The sample is weighted by population size in 1976. There are 383 arbitration cases under analysis. The sample is municipalities that underwent arbitration, limited to months that are within the arbitration window, augmented with 197 municipalities that never underwent arbitration. Municipalities that never underwent arbitration are included for all months between 1976 and 1996. “Fixed-effects” consist of month × year effects (252), arbitration window effects (383), and city effects (452). 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.

Table V
Arbitration decisions and sentencing outcomes
Panel A: Sentencing

| | OLS: Conviction (Yes = 1) | | OLS: Incarceration (Yes = 1) | |
|------------------------------------|---------------------------|---------------------|------------------------------|---------------------|
| | (1) | (2) | (3) | (4) |
| Post-arbitration × Employer win | 0.0190 (0.0606) | 0.0186 (0.0328) | -0.0115 (0.0405) | -0.0009 (0.0378) |
| Post-arbitration × Union win | 0.0161 (0.0273) | 0.0080 (0.0238) | 0.0645 (0.0295) | 0.0535 (0.0261) |
| Row 2 – Row 1 | -0.0028 (0.0599) | -0.0106 (0.0368) | 0.0760 (0.0215) | 0.0543 (0.0226) |
| Charge dummies? | | Yes | | Yes |
| Mean of the dependent variable | 0.791 [0.407] | 0.791 [0.407] | 0.318 [0.466] | 0.318 [0.466] |
| N | 6,685 | 6,685 | 6,685 | 6,685 |
| R ² | 0.041 | 0.135 | 0.050 | 0.111 |

Panel B: Outcomes conditional on conviction

| | Incarceration (Yes = 1) | | Sentence | |
|------------------------------------|-------------------------|---------------------|---------------------|--------------------|
| | (1) | (2) | (3) | (4) |
| Post-arbitration × Employer win | -0.0321 (0.0430) | -0.0156 (0.0435) | -0.1866 (0.1629) | 0.0878 (0.1704) |
| Post-arbitration × Union win | 0.0771 (0.0291) | 0.0722 (0.0254) | 0.6382 (0.2234) | 0.4964 (0.1844) |
| Row 2 – Row 1 | 0.1092 (0.0250) | 0.0878 (0.0264) | 0.8249 (0.1589) | 0.4086 (0.1277) |
| Charge dummies? | | Yes | | Yes |
| Mean of the dependent variable | 0.402 [0.490] | 0.402 [0.490] | 1.658 [3.861] | 1.658 [3.861] |
| Sample Size | 5,289 | 5,289 | 5,162 | 5,162 |
| R ² | 0.054 | 0.128 | 0.031 | 0.334 |

Standard errors, clustered on arbitration window, are in parentheses. Standard deviations are in brackets. 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. There are 85 charge dummies, which indicate the crime for which the defendant was charged. All models include a constant, year and month of arrest dummies, year of final disposition 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 fractions of a year. Conditional on conviction, the average sentence is 1.66 years (std. dev. = 3.86). Conditional on incarceration, the average sentence is 4.28 years (st. dev. = 5.22). Author's calculation based on NJ PERC arbitration data matched to arrestees from the Offender Based Transaction Statistics.

Table VI
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 × Employer Win | 2.50 (7.46) | 0.76 (2.78) | 3.16 (3.49) | 6.31 (4.27) | 5.50 (2.90) | 2.97 (3.25) |
| Post Arbitration × Union win | -0.87 (6.78) | 0.826 (2.82) | -0.66 (2.67) | 0.16 (2.54) | -0.16 (1.75) | -0.20 (2.44) |
| Row 2 – Row 1 | -3.37 (5.90) | 0.07 (3.56) | | -6.15 (2.88) | -5.66 (4.19) | -3.17 (3.56) |
| Employer Win (Yes = 1) | -1.14 (24.85) | | | 3.30 (8.39) | | |
| Log Population | 17.15 (6.43) | | | 1.32 (1.35) | | |
| Pop 75 th %tile × Post-Arbitration × Employer Win | | | -4.06 (5.24) | | | 0.01 (2.51) |
| Pop 75 th %tile × Post-Arbitration × Union Win | | | 2.31 (3.82) | | | 4.24 (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 [107.79] | 260.78 [107.79] | 260.78 [107.79] | 42.77 [37.57] | 42.77 [37.57] | 42.77 [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 are 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. 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 VII
Heterogeneous effects of arbitration decisions on clearances by loss size, award, and deviation from the expected offer; -12 to +12 month event time window

| | (1) | (2) | (3) | (4) | (5) Police lose | (6) Police win |
|--|------------------|-----------------|------------------|-----------------|--------------------|-------------------|
| Post-Arbitration | 5.72 (2.31) | -8.17 (9.58) | 12.99 (8.45) | -7.42 (4.76) | 4.97 (3.14) | 7.30 (4.17) |
| Post-Arbitration × Award | | 1.23 (1.16) | -1.00 (0.98) | | | |
| Post-Arbitration × Loss size | -10.31 (1.59) | | -10.93 (1.89) | | -0.20 (4.54) | |
| Post-Arbitration × Union win | | | | 13.38 (5.32) | | |
| Post-Arbitration × (expected award-award) | | | | | -17.72 (7.94) | 2.82 (4.13) |
| Post-Arbitration × p(loss size) [^] | | | | Included | | |
| Sample Size | 59,137 | 59,137 | 59,137 | 59,137 | 52,857 | 55,879 |
| R^2 | 0.63 | 0.63 | 0.63 | 0.63 | 0.60 | 0.62 |

Standard errors, clustered on the intersection of arbitration window and city, are in parentheses. Standard deviations are in brackets. Observations are municipality × month cells. The sample is weighted by population size in 1976. The dependant variable is clearances per 100,000 capita. Loss size is defined as the union demand (percent increase on previous wage) less the arbitrator award. Amongst cities that underwent arbitration, the mean loss size is 0.489 with a standard deviation of 0.953. The expected award is the mathematical expectation of the award given the union and employer offers and the predicted probability of an employer win. The predicted probability of an employer win is estimated with a probit model using as predictors year of arbitration dummies, the average of the final offers, log population, and the length of the contract. See text for details. The samples in models (1)-(4) consist of the 12 months before to the 12 months after arbitration, for jurisdictions that underwent arbitration, as well as all jurisdictions that never underwent arbitration for all months between 1976 and 1996. The sample in model (5) consists of cities where the union lost in arbitration and the comparison group of non-arbitrating cities. The sample in model (6) consists of cities where the union won in arbitration and the comparison group of non-arbitrating cities. All models include month × year effects (252), arbitration window effects (383), and city effects (452). Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports.

[^]p(loss size) denotes a cubic polynomial in loss size.

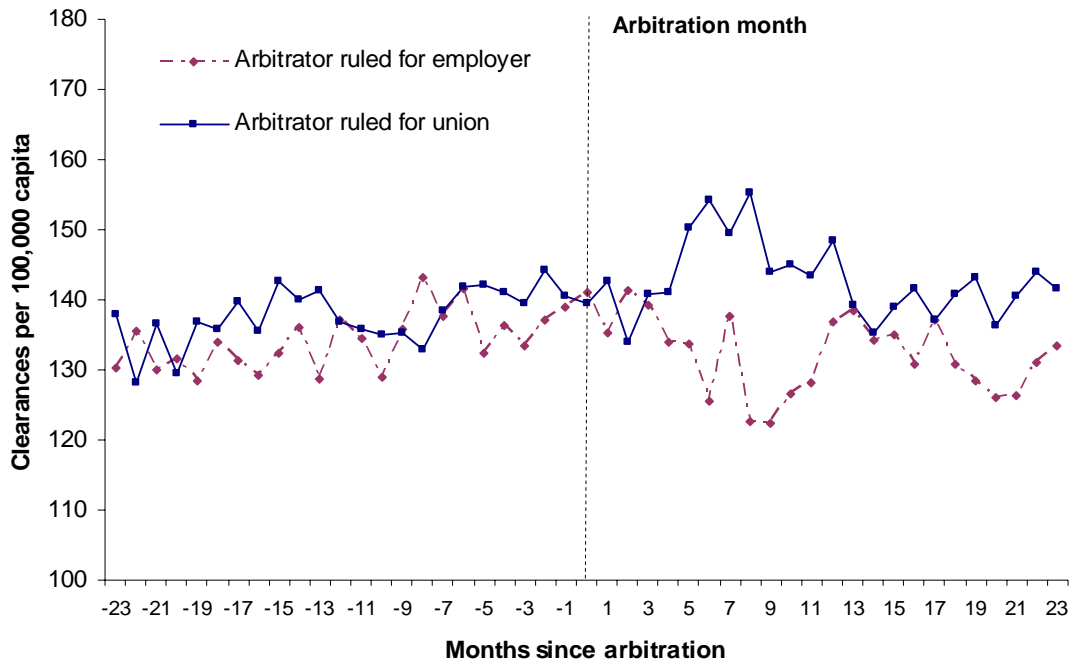


Figure I
Month-by-month comparison of Union and Employer city average clearance rates

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 is weighted by 1976 population. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

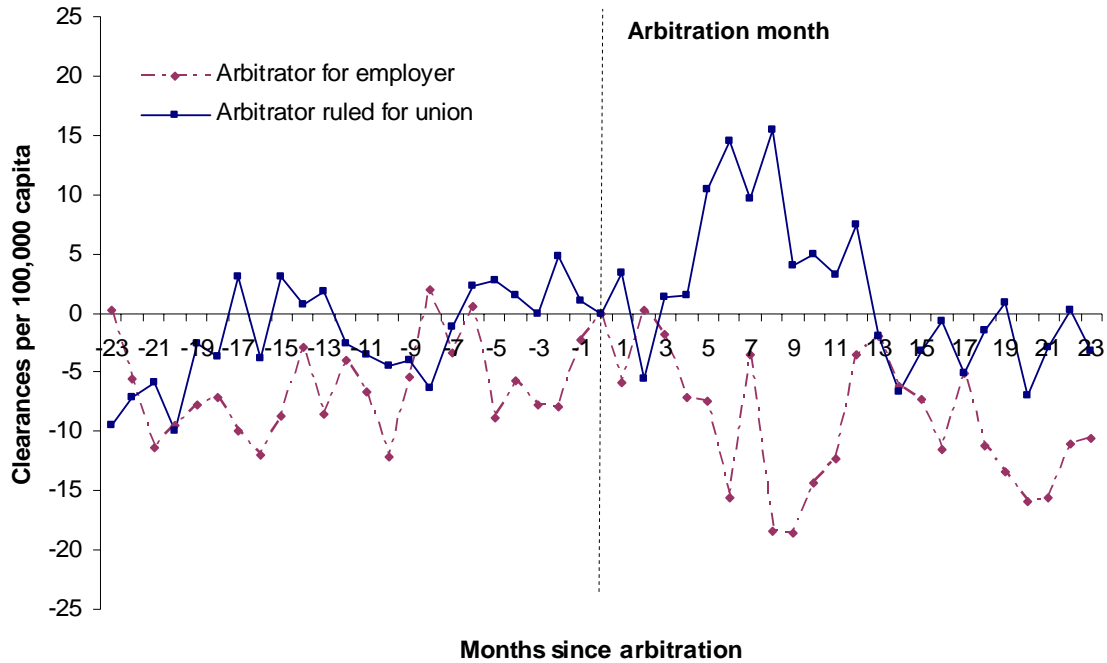


Figure II
Regression-adjusted event study estimates of the effect of arbitration rulings on per capita clearances

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 Union and Employer cities. Regression model includes controls for year \times month of arbitration dummies, arbitration window fixed-effects, and city fixed-effects for the group of cities that never underwent arbitration. 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 is weighted by population in the jurisdiction in 1976. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

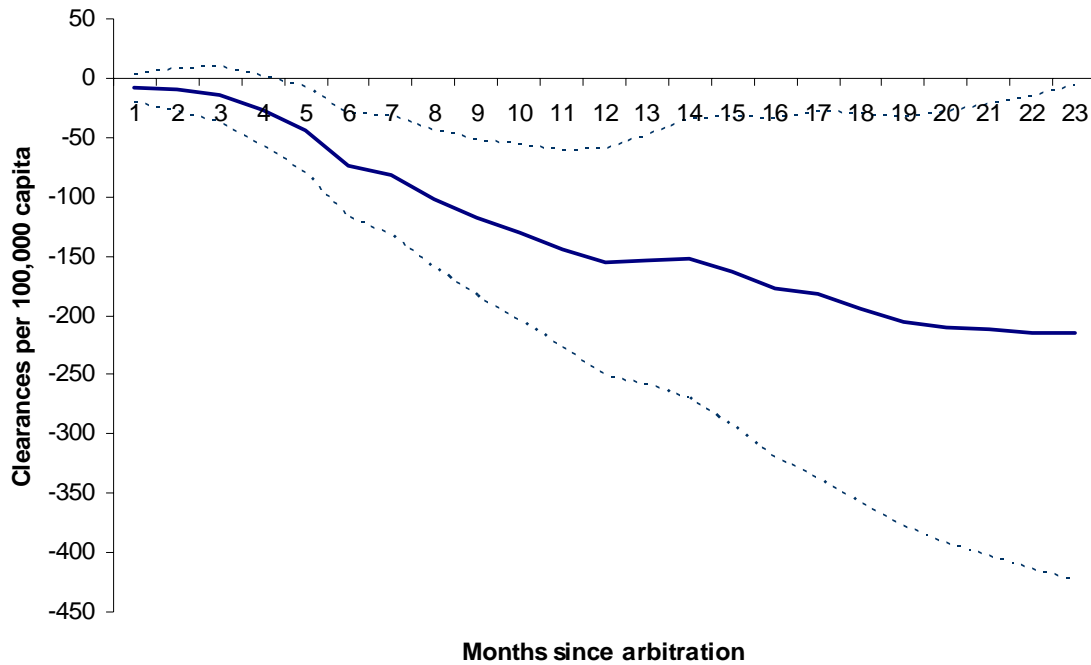


Figure III
Regression-adjusted estimates of the cumulative difference in clearances between Employer and Union cities in post arbitration months relative to the entire pre-arbitration period

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 \times month of arbitration dummies, arbitration window fixed-effects, and city fixed-effects for cities that never underwent arbitration. The dotted lines are the 90 percent 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 is weighted by population in 1976. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

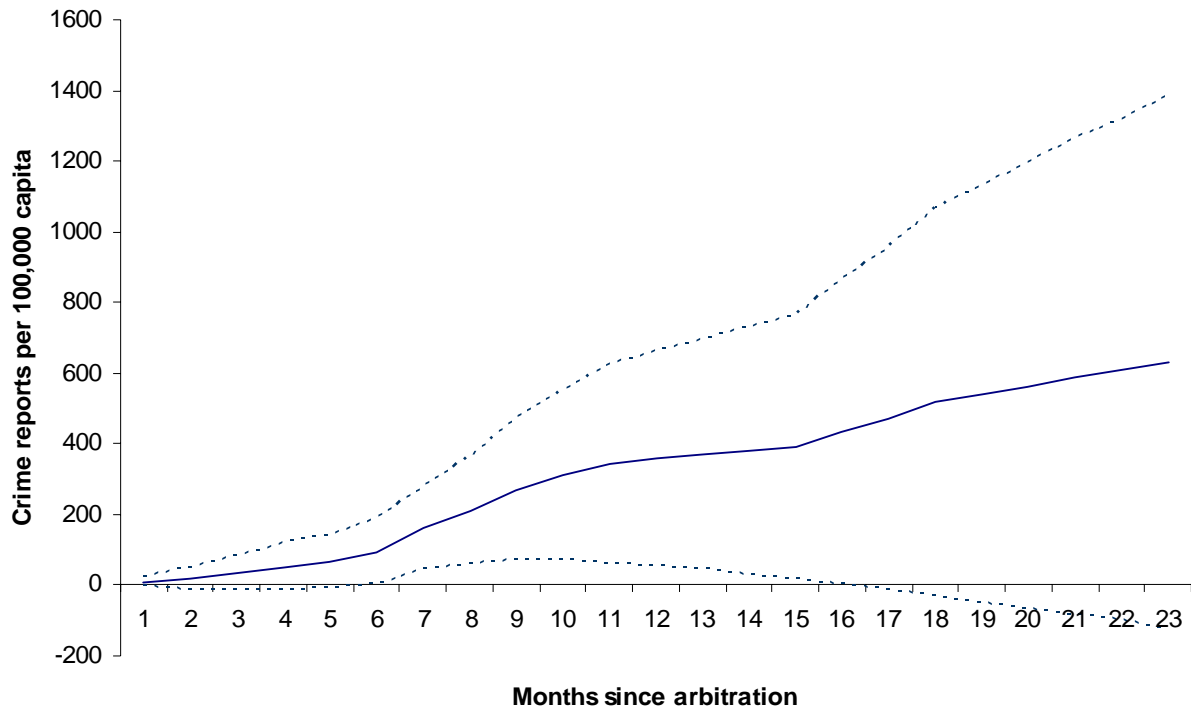


Figure IV
Regression-adjusted estimates of the cumulative effect of union losses on crime

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 an employer win dummy are cumulated and plotted. Regression model includes controls for year and month of arbitration dummies as well as arbitration window fixed-effects. Cities that never underwent arbitration are also included and are each assigned a fixed-effect. The dotted lines are the 90 percent 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 is weighted by population in 1976. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996.

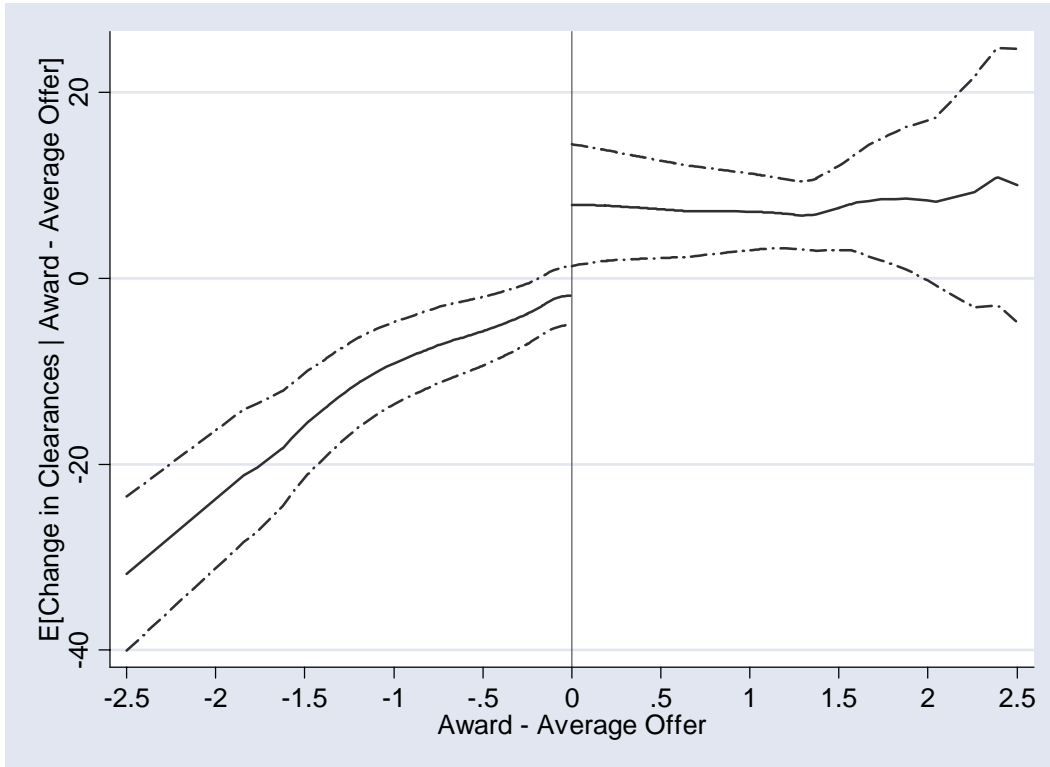


Figure V
Estimated expected change in clearances conditional on the deviation of the award from the average of the offers

Local-linear estimates of the expected change in clearances conditional on the gap between the arbitrator award and the average of the final offers for 383 arbitration cases from the 12 months prior to arbitration to the 12 months after arbitration. Dotted-line is the 90 percent confidence band. Sample is weighted by population in the jurisdiction in 1976. Data span the years 1976 through 1996 for arbitration cases occurring between 1978 and 1996. Author's calculation based on NJ PERC arbitration cases matched to monthly municipal clearance rates at the jurisdiction level from FBI Uniform Crime Reports.