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Collective bargaining, police pay, and racial differences in police lethality rates

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ABSTRACT

This study examines the interaction effects of police collective bargaining authorization and police pay on racial differences in police-related fatalities. Using data from Fatal Encounters, the Bureau of Justice Statistics, and other publicly available databases, we applied entropy-weighted regressions to a balanced panel of 282 local police departments from 2000 to 2013 in the United States. We found that collective bargaining authorization is not directly associated with police-caused deaths. However, results indicate that higher median salaries for city police officers directly and meaningfully contribute to fewer people killed by police actions. When considering interactive effects, our findings suggest that police unionization offsets the life-saving benefits of higher relative pay, leading to more Black citizens dying from police intervention as salaries increase in agencies with collective bargaining authorization. Our findings demonstrate authorities should consider the potentially fatal and inequitable consequences for citizens during collective bargaining and salary-setting negotiations.

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Calls for increased police accountability have intensified in response to recent high-profile police killings of Black Americans, with many citing police unions as a major barrier to holding abusive officers accountable. An illustrative case for such allegations is the police union's backing of Chicago police officer Jason Van Dyke. Before being convicted for murdering Laquan McDonald, Van Dyke had a higher number of use-of-force complaints compared to most of his colleagues (Corley, 2022; Invisible Institute, 2022). The labor agreement between the Fraternal Order of Police (FOP) and the city of Chicago allows officers to delay questioning, review witness statements before being questioned, alter their statements after viewing incident videos, and expunge officer misconduct records (Emmanuel, 2015). Such contract provisions frustrate efforts to identify abusive patterns.

The benefits afforded to Van Dyke are not exclusive to Chicago officers. Union contracts offer protection for most U.S. police officers should they similarly commit misconduct or use excessive force. Over half (56%) of our nation's police officers are union members (Hirsch et al., 2023), and 41 states and the District of Columbia have extended collective bargaining rights to the police (Sanes & Schmitt, 2014).[1] Union leaders maintain union contract safeguards enable officers to protect their

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communities against crime, whereas critics contend these labor groups impede reforms needed to address police brutality (Bies, 2017; Fisk & Richardson, 2017; Segura, 2020). However, the research in this area is lacking, leaving open questions about whether police unions facilitate higher levels of force.

Explanations for unions' role in police-related fatalities (PRFs) vary but center on two competing hypotheses. According to the first hypothesis, unionization results in higher rates of police lethality as it promotes public-service values (e.g., commitment to the public interest) and increased job protections that enhance enforcement activity (Kerrissey & Schofer, 2018; Rosenfeld, 2014; Zullo, 2013). Such an increased focus on traffic stops and arrests leads to more chances for citizen resistance; thereby creating ripe conditions for police force usage (Magenau & Hunt, 1996; Parker et al., 2005). This hypothesis maintains that enforcement-focused police forces likely believe the union will shield them from punishment for injuring or killing a citizen (Alpert & MacDonald, 2001), resulting in higher police lethality rates.

Conversely, since collective bargaining agreements (CBA)s often improve pay and benefits (Brunner & Ju, 2019; Knepper, 2020), the second hypothesis predicts unionization will protect against unnecessary police force for two primary reasons. First, police forces that offer higher salaries could potentially attract a larger pool of highly educated and experienced applicants, resulting in a more skilled and selective group of officers more likely to use force appropriately (Philippon & Reshef, 2012). Second, increased compensation motivates employees to work diligently and elicits greater safety behavior and compliance with policy and law (Kudo & Belzer, 2019). Therefore, unionization theoretically can lead to less lethal police forces.

Central to both arguments is the assumption that unionization directly affects officer performance, but the presence of a police union does not guarantee performance changes. Instead, officers' enforcement efforts hinge on the collectively bargained salary relative to their expectations, as prior research shows arrest rates increase when unions secure a fair pay raise and decrease when less satisfactory salaries are negotiated (see, e.g., Mas, 2006). The few studies that examine the association between unionization and police force usage support the supposition that CBAs permit or perpetuate officer attitudes and behaviors conducive to contentious and sometimes violent police-citizen encounters (Dharmapala et al., 2022; Goncalves, 2021; Parker et al., 2005). However, only two papers considered the effects of union representation on PRFs (Cunningham et al., 2021; Dincer & Johnston, 2023). Further, none of the cited studies examined whether the gap between police pay and the market-clearing wage moderates the impact of CBAs. This underdeveloped area of study represents a crucial research gap, as collective bargaining profoundly influences officer salary and liability structures that ultimately impact the quality of police services.[2]

We attempt to advance this literature using syncretic theorizing and empirical modeling. Our study assesses the interactional effects of collective bargaining and relative police wages on both overall and race-specific PRFs by applying entropy-weighted regressions to a balanced panel of 282 local police departments from 2000 to 2013.[3] We begin with an overview of the socializing effects of police unionization and collective bargaining in the US. Next, we discuss the connection between police unionization and police force usage, including lethal force. After presenting the hypotheses, study data, methods, and results, we conclude with research and policy recommendations.

The socializing effects of police unionization

Similar to labor unions across other industries (Fullagar et al., 1995), police unions convey behavior-shaping norms and beliefs (or cultural understandings) to employees and departments (Walker, 2008). While scholars debate the existence of a singular police subculture (Paoline et al., 2000), they generally agree that the occupational culture within policing promotes certain work values, including feeling 'above the law,' hypermasculinity, and embracing an 'us versus them' mentality toward citizens (Parnaby & Leyden, 2011). Most relevant of the hypothesized subcultural effects is officers' belief that order must be maintained at all costs, or else disorder will prevail – even if it means placing themselves in danger

(e.g., standing in front of fleeing vehicles or jumping onto moving cars) (Klockars, 1980). This willingness to jeopardize personal safety is noteworthy in the context of police lethality, as criminologists conclude that fatal police-citizen encounters are frequently owed to officer-initiated jeopardy or:

> The actions of officers who, without sound justification, willingly fail to take advantage of available tactical concepts like distance, cover, and concealment ... willingly abandon tactically advantageous positions by moving into disadvantaged positions without justification, or act precipitously on their own without waiting for available assistance from other officers. (Stoughton et al., 2021, p. 158)

Thus, socialization into traditional police subcultures may inform how often and how severely police apply force.

As police officers undergo this socialization process, their values become more aligned with departmental culture, and their actions more commensurate with what is rewarded (Oberfield, 2014). Moreover, police organizations tend to highly value solidarity and loyalty, putting immense pressure on officers to behave in line with their colleagues. This environment makes some officers less willing to report excessive force or intervene (Wilkins & Williams, 2008). Some members may reject these values or selectively subscribe to some beliefs. Nonetheless, research shows that officers socialized within conventional subcultural contexts usually commit to predominant organizational goals, including the type of strict law enforcement that raises the likelihood for violent police encounters (Stinson et al., 2018; White & Kane, 2013; Wood et al., 2019).

Unionization introduces an additional layer to this socialization process. According to scholars such as DiSalvo (2022) and Samuel Walker (2008), police unions not only shape police subcultures but also foster a culture of impunity. This is owed largely to the robust job protections afforded by collective bargaining agreements, which reduce officers' fear of disciplinary action, termination, or prosecution. Police unions, like other labor organizations, aim to protect their members' interests, which includes job security, working conditions, and legal protection. Aside from legislative lobbying and electioneering for supportive candidates (Perkins, 2020; Zoorob, 2018), police labor groups influence departmental policies and operations through collective bargaining (Reiss, 1992). Collective bargaining describes how union representatives negotiate labor contracts with local governments. Among other services, police unions protect frontline officers' job security by determining employment terms, ranging from discipline and appeals to promotion and salary. Activists have long criticized protections ensconced in CBAs as mechanisms shielding police fromoversight, transparency, and accountability (Fisk & Richardson, 2017; Nowacki & Willits, 2018; Walker, 2008). For example, a study of CBAs across 178 U.S. cities identified various protections restricting disciplinary action against police officers, civilian oversight, and anonymous complaints, including statute of limitations for misconduct complaints and stipulations that delay interrogations, bar public disclosure of disciplinary actions, and periodically erase disciplinary records (Rushin, 2017).

Regardless of the perceived guilt, labor unions have an obligation to provide fair representation to their members. This includes vigorously advocating for members and exhausting all available protections, even for unjustified civilian killings and sustained complaints. Unions typically pay for and facilitate legal counsel, consult with officers before post-PRF interviews occur, and invoke due process protections during questioning. When suspension, dismissal, or decertification occurs, these decisions are subject to appeal and regularly overturned (Hickman, 2006). Some police unions even successfully reduced the range of conduct for which state certifying bodies (e.g., Peace Officers Standards and Training Commissions) may discipline officers (Goldman, 2015).[4]

How unionization foments a subculture of greater force usage

Given these dynamics, along with unions' historical resistance to reforms (Scheiber et al., 2020), it's reasonable to expect that in agencies where police unions have a strong influence and prioritize the protection of their members, there could be a corresponding increase in the cultural acceptance of using force, including both less-than-lethal and deadly interventions. This does not mean that all unionized police forces are aggressive or that all police officers in unionized environments will use excessive force. However, the protective nature of unions, combined with the socialization processes that emphasize solidarity and defense against external criticism, can create conditions where the use of force is more readily accepted or rationalized. In this way, unionization threatens to foment and strengthen a subculture within police departments that is conducive to high levels of force usage and creates an echo chamber where such perspectives are amplified.

Findings from the limited quantitative research on the impact of unionization on police force usage tend to support such conclusions. Except for one study by Goncalves (2021), other studies find evidence suggesting that police unionization encourages officer attitudes and actions that increase the likelihood of violent police-citizen encounters. For studies examining non-fatal police incidents, the authors reasoned that CBA protections may contribute to increased force usage and misconduct (Parker et al., 2005; Rad, 2018; Rad et al., 2023). For example, Rad et al.'s (2023) study using an index of CBA and legal protections to analyze the relationship between unionization and police killings of civilians found a positive association. Similarly, results from Dharmapala et al.'s (2022) state-level difference-in-differences analysis show that the right to collectively bargain leads to a 40% increase in incidents of violent misconduct. These authors concluded the findings imply that officers covered under CBAs are less susceptible to the potential deterrent effects of sanctions.

Several studies have found a significant correlation between police unionization and an increase in police-caused deaths of Black citizens. In a 2014 study, Willits and Nowacki observed that officers in big city police departments with CBAs were more likely to use deadly force. In another study using annual data from 50 states from 2013 to 2019, Dincer and Johnston (2023) found that police unions' political influence and the share of LEOs covered by CBAs positively related to Black PRF rates. Likewise, an event study analysis by Cunningham et al. (2021) that leveraged discontinuities in collective bargaining rights showed a substantial increase in state-level PRFs involving non-White citizens after officers gained these rights. Moreover, they found that collective bargaining rights explained roughly 10% of non-White citizen deaths involving U.S. law enforcement between 1959 and 1988. Consistent with the time required to form a union and negotiate a contract, the noted effects emerged four years after states enacted bargaining rights. Collectively, these studies indicate that unionization leads to the increased use of potentially deadly tactics among police officers, as the influence of the union may make them less careful and feel exempt from the consequences of their actions.

Police pay, efficiency wages, and performance

Labor economists have long theorized about how relative wages can affect worker performance using efficiency wage models. The efficiency wage paradigm, grounded in wage equity and social exchange traditions, postulates a positive relationship between higher relative pay and worker performance (Akerlof, 1982; Akerlof & Yellen, 1990; Shapiro & Stiglitz, 1984). These theories maintain that organizations can induce effort exertion by offering employees a wage above the market clearing rate (Westley & Schmidt, 2006). In contrast, pay levels below the prevailing market rate can contribute to employee dissatisfaction and other negative attitudes, leading to job-shirking activities and high turnover (Akerlof & Yellen, 1986; Solow, 1979).

From this frame, when workers feel more valued, as represented by a higher relative wage, they work harder (Akerlof, 1982). Competitive wages are believed to stimulate work effort because of the increased opportunity cost of job loss, workplace cohesiveness, employee responsibility and loyalty,

and job attractiveness (Fehr et al., 2009). Higher relative wages also help organizations recruit and retain a superior pool of workers (Mühlau & Lindenberg, 2003; Stiglitz, 1985; Weiss, 1980).

Alternatively, employee reactions to lower-than-expected wages may reflect a perceived slight on their end, disappointment, or lowered morale. These associated perceptions can result in feelings inconducive to quality performance and work environments where employees are unwilling to sacrifice for the organization. In either case, this would suggest that employee productivity, and thus organizational outcomes, depends on changes in pay relative to a perceived reference level or going rate.

As part of the socialization process, union members may become especially sensitive to wage levels. Labor unions often negotiate from the position that organizations undercompensate workers, and improved pay will boost employee morale and performance. Consequently, research shows that non-union members initially may be content with their wages. Then, their opinions change after joining the union and being socialized through interactions with union leaders and fellow members (Hammer & Avgar, 2005). From this vantage point, unionization increases the likelihood of members feeling underpaid and undervalued by the public and police management, particularly since compensation is a prominent collective bargaining concern for police officers (Bolinger, 1981).

Previous studies of collective bargaining offer evidence of the theorized efficiency wage effects in law enforcement. For example, Mas' (2006) study of police unions in New Jersey between 1978 and 1995 revealed that arrest rates decreased when unions negotiated a pay raise officers deemed unfair or below market value. A subsequent study by Chandrasekher (2016) also offers evidence that changes in wages relative to a reference point affect police performance. Results of her analysis of New York City Police data from 1996 to 2007 show that substantiated misconduct complaints rose with time spent out of contract after lengthy labor negotiations resulted in the expiration of standing CBAs.

Comparisons for formulating perceived going rates

Although workers compare their wages to those paid to their coworkers (Frank, 1984), the efficiency wage literature suggests that workers mostly appraise pay fairness based on occupation-specific and local wage conditions. First, regarding occupation-specific comparisons, scholars assert that employees perceive pay equity based on salaries of similarly situated jobs in similar locations (Johansen & Strøm, 2001). However, in policing, the unique nature of the job and the corresponding training and certifications leave the profession without clear occupational equivalents (Lewin et al., 2012). Historically, salaries for other public sector workers, mainly firefighters, have served as external wage benchmarks for law enforcement. Indeed, both professions provide life-saving services supported by specialized skills accumulated through training and work experience. Nonetheless, such overly simplistic comparisons neglect critical differences in training, the scope and complexity of the jobs, salary determinations, and performance evaluations.

Given this complexity, police collective bargaining actors generally refer to agency pay structures in jurisdictions of comparable size and geographic setting. This more idiosyncratic approach attempts to account for variations in workload, job requirements, and living costs that may contribute to substantial wage differentials across police labor markets. Further, since the public sector regularly makes salary schedules publicly available (Rosenfeld, 2017), unions, officers, and civic leaders can readily compare prevailing police wages at various levels of government (i.e., local, state, and county).

Second, research also indicates that employees prefer wages commensurate with the local wage market. The local market is an economically integrated geographic area where people may reside and find employment within a reasonable distance. Unlike the two wage markets discussed above, local wage markets generally account for local economic conditions, including job opportunities and the cost of living. Such jurisdiction-specific factors likely mean those police officers looking at

local wages as benchmarks do so within locales, not across potentially disparate policy and regulatory contexts.

The current study

Our research advances the understanding of police unionization, pay, and lethality by addressing limitations in the current evidence, such as analyzing data reflecting modern police practices and criminal justice reforms while considering potential operational and policy confounders. The limitation most relevant to our study concerns the absence of research assessing whether unions' effects on police lethality rates depend on the competitiveness of collectively bargained police wages. Based on the available empirical and theoretical evidence, we suspect that unionization and relative pay jointly affect police lethality rates. We propose the following hypotheses:

H1 (Union protection hypothesis): Police unionization, through the protections offered by CBAs, leads to decreased accountability and increased use of potentially deadly tactics, as officers feel shielded from consequences due to contractual safeguards.

H2 (Economic incentive hypothesis): Higher relative wages are associated with lower rates of police lethality, as officers have more at stake from losing their jobs and are more satisfied with their employment. Further, higher pay attracts high quality officers who are more likely to possess traits and skills that enable them to resolve conflict without resorting to deadly force, even if they are more productive.

H3 (Interaction hypothesis): In non-union departments with higher pay, police are more fearful of losing their well-paid positions, leading to increased vigilance in their duties. Conversely, job security under CBAs alleviates the fear of losing a high salary in unionized departments. Therefore, we hypothesize that as pay levels increase in non-union agencies, police lethality will decrease, whereas we expect alternative or no such protective effects in unionized agencies.

H4 (Racial differential hypothesis): We anticipate that the hypothesized interaction will exert racially heterogeneous effects on police-involved deaths, as impacts on police lethality will be particularly salient for Black Americans. Black Americans are overrepresented in neighborhoods characterized by crime and disadvantage (Wilson & Weiss, 2012), which contributes to the uneven distribution of police contacts (Cooley et al., 2020). Some police contacts, like arrests, that pose a high risk of citizen flight and resistance, also increase the likelihood of police force and PRFs. Further, heightened tensions caused by mistrust between police and Black communities, stemming mostly from past injustices and high-profile incidents of police brutality, can lead to misunderstandings, fear, and aggression in police-citizen encounters (Pickett et al., 2022); thereby raising the probability of violent altercations between police and Black citizens.

Methods

Sample data

We constructed a unique police agency-level dataset using the Law Enforcement Management and Administrative Statistics (LEMAS) survey, Fatal Encounters project, and other public databases covering 2000 – 2013 (see Table 1). Agency-level PRF data came from the Fatal Encounters project (www.fatalencounters.org). Researchers, journalists, and crowd-sourced

Table 1. Collective bargaining status among sampled cities.

Authorized	No = 0	Yes = 1	Total
Year			
2000	89	193	282
2003	80	202	282
2007	96	186	282
2013	34	248	282
Total	299	829	1,128

Source. BJS LEMAS data.

contributors collaborated to compile incident-level PRF data since 2000, regardless of intent or disposition, through media reports, Freedom of Information Act requests, and police records. Details on the race/ethnicity of the deceased are missing in some cases (particularly before 2013), but Fatal Encounters data include a higher proportion of PRFs compared to other databases (Finch et al., 2021).

We generated police agency measures using LEMAS data obtained from a survey of about 3,000 state and local law enforcement agencies. The data provide information on agency personnel, expenditures, operations, equipment, and policies, with agencies employing 100 sworn officers or more being sampled with certainty and smaller ones randomly selected based on department size and type. We limited our analysis to data through 2013 because more recent LEMAS surveys lack important agency incentive and performance measures.

We obtained annual crime totals by major offense type from the FBI's Uniform Crime Report (UCR) Offenses Known to Law Enforcement data. To measure overall arrest rates, we used the UCR Arrests by Age, Sex, and Race (ASR) data that provide annual counts of total arrests. We collected data on population, income, and structural indicators from the U.S. Census Bureau's decennial and American Community Survey (ACS) 5-year estimates for principal cities in metropolitan or micropolitan statistical areas. State-level salary data for law enforcement and public safety professionals came from the BJS Occupational Employment and Wage Statistics (OEWS) program.

We merged these data using common match indicators from the 2000 and 2012 Law Enforcement Agency Identifier Crosswalk (LEAIC) files. A crosswalk based on the city, state, agency name, and assigned Originating Agency Identifier (hereafter ORI) was created to join these data with the Fatal Encounters data. Great lengths were taken to remove duplicate entries and verify that decedents died from police actions. We also excluded PRFs involving multiple agencies to prevent misattribution. To maintain consistent units of analysis, we selected jurisdictions with independent police agencies employing more than 100 sworn officers for which data was available across all sources. All departments in the Fatal Encounters dataset linkable to agency ORI codes and census-designated place identifiers were merged and included in the sample.

We implemented multiple imputation by chained equations (MICE) to preserve 297 cases (26.3%) with incomplete data. MICE replaces missing values with draws from a probability distribution, resulting in variability across the imputed data sets that represents uncertainty in the imputation process. Model estimates are then averaged across imputed datasets after performing separate statistics for each dataset (Rubin, 1987). A two-stage quadratic rule-based equation indicated that our analysis required 55 imputed datasets (see von Hippel, 2020).

Our final sample comprised a balanced four-wave panel of 282 city agencies and 1,128 city agency-year observations from 2000–2013. Spanning 33 states, the sampled municipal police agencies served geographically diverse jurisdictions of varying sizes, including the nation's largest cities.

Our study data captured nearly a quarter (23.9%) of documented citizen fatalities involving police during this period (see Table 2). Although our sample is not representative of all law enforcement agencies, it constitutes a sizable portion of fatal police-citizen encounters in the United States.[5]

Table 2. Crosstab: overall and race-specific PRF counts.

	Nationally			Sampled cities			% total PRFs covered in sample		
Year	Black victims	White victims	Total victims	Black victims	White victims	Total victims	Black victims	White victims	Total victims
2000	227	341	771	65	64	187	28.6	18.8	24.3
2003	307	428	965	67	80	219	21.8	18.7	22.7
2007	338	505	1,116	112	86	300	33.1	17.0	26.9
2013	494	768	1,634	128	120	367	25.9	15.6	22.5
Total	1,366	2,042	4,486	372	350	1,073	27.2	17.1	23.9

Fatal Encounters database.

Outcome measures

The current study defines PRFs as citizen deaths caused by official police intervention (e.g., police shootings, pursuits, Taser use, etc.). We employed an arrest benchmark to account for differential exposure to police rather than the commonly used residential population benchmark (Skogan & Frydl, 2004). Arrest rates may be confounded by bias in policing practices (Goff et al., 2016). Nonetheless, scholars, including Fryer (2019), Hickman et al. (2009), and Smith et al. (2009), recommend arrest benchmarks as a more conservative and contextualized alternative that accounts for racial disparities in police stops (Epp et al., 2014) and arrests (Kochel et al., 2011). Arrests also represent the chief formal police response to crime that carries an inherent need for some level of police force or threat of force (expressed or implied), especially since these contacts also present the most common opportunity for citizen resistance and altercations (Tregle et al., 2019; Willits & Nowacki, 2014).

Based on insights from prior research (Ross et al., 2021; Tregle et al., 2019), the overall PRF rate was calculated by dividing the number of police-caused deaths per 1,000 arrests in a given city-year ((Total PRFs ÷ Total arrests) × 1,000 arrests). We used race-specific arrest totals for Black PRF rate ((Black PRFs ÷ Total Black arrests) × 1,000 arrests) and White PRF rate ((White PRFs ÷ Total White arrests) × 1,000 arrests). We restricted our analysis to overall, Black, and White PRFs because the Uniform Crime Report (UCR) arrest data only began reporting on arrestees' Hispanic origin toward the end of the study period in 2013.

Focal measure

Following prior research using LEMAS data, we used a dichotomous indicator to quantify the effects of unionization. *Collective bargaining* (1 = yes) denotes if police departments authorized collective bargaining for sworn officers in a given city and year. Collective bargaining authorization was staggered, and some sampled agencies fluctuated between authorized and non-authorized status during the study period. This indicator takes the value of 1 in the years the collective bargaining authorization is active and 0 when inactive or never adopted (all subsequent binary measures were coded in this fashion).

Moderating measure

Research suggests that in the same way employees formulate pay equity perceptions (Goodman, 1974), determining an occupation's fair going rate requires a mix of wage market referents. Carefully defining appropriate wage markets is integral to accurate wage comparisons. Defining the market too narrowly or too broadly can lead to measurement error bias yielding results either higher or lower than the correct measurement. Therefore, we used estimates of police salaries from LEMAS, city residents' annual income from ACS, and state median salaries for police and firefighters from BJS OEWS program to develop a *fair wage index (FWI)*.

The first step of this measurement scheme involved calculating four fair wage ratios. For each wage ratio, we used the median salary of non-executive LEOs in a city for each year (i.e., frontline supervisors and entry-level officers) as the numerator. The four respective denominators represent the state median salaries for police and firefighters, median salaries for city residents in police jurisdictions, and the median salaries of non-executive LEOs clustered by city population size and region.

$$Fair\ wage\ ratio = \frac{median\ salaries\ of\ non-executive\ LEOs\ in\ city\ agencies}{median\ benchmark\ salary}$$

Fair wage ratios of 1 represent wage parity, ratios above 1 indicate officers earned higher salaries, and ratios below 1 suggest departments paid officers less than market wage. We then calculated the percent above or below parity for each wage ratio measure.

$$Percent\ from\ parity = (fair\ wage\ ratio - 1) \times 100$$

The average of these four percentages represents our moderating variable. We believe this measurement approach better captures the dimensions of public sector wage-setting that affect officers' perceptions of salary fairness than a single-item indicator. Further, what is important for our purposes is not which wage gap is most impactful, but whether we find evidence that equitable pay (in its various forms) matters in fatal police encounters.

Police agency measures

Police-related controls tapped department resources and enforcement strategies that might affect agencies' fatal force usage. *Police strength* is measured as the natural log of the number of police personnel per capita to proxy police saturation. To control for the effects of departments' racial and gender diversity, we included measures for the percent of non-White officers and female officers. *Civilianization*, operationalized as the number of nonsworn agency employees, is a measure of departments' non-sworn civilian/support components. We used a dichotomous policy indicator, *college degree requirement* (1 = associate's degree or bachelor's degree), to account for the effects of departmental college degree requirements for sworn new hires (Johnson, Johnson, & Sevigny, 2022). We accounted for *fiscal capacity*, operationalized as agency expenditures per capita (calculated as the total yearly police department expenditures in real dollars divided by the number of city residents), to represent police resource availability. Some departments have established economic incentive programs such as tuition reimbursements, bonus pay for degreed officers and those with specialized vocational skills, and merit pay for performance to attract and retain officers. Therefore, we included the four dichotomized measures, *tuition reimbursement* (1 = yes), *education incentive pay* (1 = yes), *specialized skills pay* (1 = yes), and *merit pay* (1 = yes), indicating whether agencies provided such incentives.

We also controlled for the predicted protective effects of community and problem-oriented policing activities (Gill et al., 2014; Peyton et al., 2019) with three binary measures. *Problem-oriented policing* (1 = yes) indicates whether agencies reported implementing scanning, analysis, response, and assessment (SARA)-type problem-solving operations. *Community partnerships* (1 = yes) denotes if departments had a problem-solving partnership with a local organization. Officer *performance evaluation* (1 = yes) indicates whether supervisors consider participation in collaborative problem-solving programs when evaluating officers.

Crime measures

Fatal police encounters are mostly concentrated in high crime areas often perceived as dangerous by law enforcement. Such threatening views can lead to over-policing and the

disproportionate use of force in these neighborhoods (Fridel et al., 2020; Gaston et al., 2021). For these reasons, we employed two crime measures, *violent crime rate* and *property crime rate*. We calculated violent crime rates as the rate per 100,000 of all known homicides, rapes, robberies, and aggravated assaults for each city and year, and property crime rates were computed as the rate of all reported burglaries, thefts, and motor vehicle thefts per 100,000 residents. Annual Law Enforcement Officers Killed and Assaulted (LEOKA) program data from the FBI were used to generate the rates of felonious assaults against officers per 1,000 officers for each agency, excluding accidental deaths and those related to illness or unrelated injury. We included this measure to account for the level of danger that police officers may encounter as elevated rates of felonious assaults against officers could lead to a higher likelihood of police using deadly force as a response to perceived threats.

Population and Structural measures

The next group of measures derived from Census data covered population and socioeconomic factors that may be correlated with public sector union strength, average wage levels, and violent police encounters. These include city population, percent non-White residents, percent female residents, percent foreign-born residents, percent of residents below the federal poverty level, percent of residents receiving SNAP benefits, median household income, percent bachelor-degreed residents, unemployment rate, and average household size (Goldfield & Bromsen, 2013; Holmes & Smith, 2008; Jacobs & O'brien, 1998; Klinger et al., 2016; MacDonald et al., 2001; Smith & Holmes, 2014; Terrill & Reisig, 2003) (The market wage is a function of supply and demand, and the percent with bachelor's degrees and unemployment rate measures also capture the supply side of the wage equation). We adjusted for potential regional effects (Schwartz et al., 2020) with a four-category census region indicator (0 = South, 1 = Northeast, 2 = Midwest, 3 = West).

Analytical strategy

This section details our empirical approach. Imputed and observed results were similar, so we report the more conservative imputed estimates. Between-group differences were examined with independent samples t-tests and chi-square tests as appropriate. Entropy weighted regressions were used to identify the interaction effects of collective bargaining and relative police wages on PRFs. Tolerance values for the regressors revealed no evidence of multicollinearity problems (average tolerance values [1/VIF] = .49). We also conducted supplemental analyses with alternative outcomes. Our Poisson regression coefficients are interpretable as semi-elasticities, or the percent change in PRF rates associated with a change in the explanatory variables (Wooldridge, 2010). All analyses were performed with Stata 16.1.[6]

Panel regression analysis

We model the association between agency unionization and PRF rates using entropy-weighted Poisson pseudo-maximum likelihood (PPML) regression models with multiway fixed effects. The PPML estimator renders reliable estimates robust to overdispersion and the non-trivial number of zeros on our outcomes due to the relative rarity of PRFs (Fally, 2015; Silva & Tenreyro, 2006). Even though PPML implicitly assumes the variance of the error term is proportional to the conditional mean, this estimator generally performs consistently even when confronted with a misspecified variance function (Silva & Tenreyro, 2011). Further, PPML allows for multidimensional fixed effects to control for unobserved heterogeneity.

Our PPML regression model is generally expressed as:

$$y_{icsrt} = \alpha_{c*st,r*t} + \beta \text{UNION}_{csrt} + \beta_1 \text{LWGR}_{csrt} + \beta_2(\text{UNION} * \text{LWGR}_{csrt}) + Z_k X_{icsrt} + \varepsilon_{icsrt},$$

where y_{icsrt} corresponds with the respective PRF rate i for city c of state s in region r during year t. $\alpha_{c*st,r*t}$ denotes the city × state-year and region × year FE for each period. The binary measure, β_{csrt}, distinguishes agency union status in csr and year t and β_1 reflects relative wage ratios. β_2 (UNION*LWGR$_{csrt}$) represents the interaction between these measures. Z_k denotes the vector of parameters for the control variables included in X_{csrt}. ε_{icsrt} is the residual error term. We clustered standard errors by city-state-region-year to mitigate potential heteroskedasticity and serial correlation issues (Bertrand et al., 2004).

Entropy balancing and covariate selection

We used entropy balancing to address selection bias related to observable determinants of agency unionization. Entropy balancing algorithms equalize covariate moment distributions among treatment and control groups by reweighting the moments of covariates in the control group to match the treatment group's covariate moments (e.g., means, variances, and skewness) (Hainmueller, 2012). By controlling for such systematic differences, we approximate an experimental design by creating statistically similar treatment and control conditions.

Previous studies indicate relevant factors impacting PRFs, but we are less certain about the factors predictive of police unionization. To overcome this challenge, covariate selection for the balancing equation was guided by a combination of prior research, theory, and the least absolute shrinkage and selection operator (Lasso) machine learning algorithm. After performing traditional regression and Lasso techniques, we settled on an economical and robust set of 19 measures identified with Lasso. These measures included: performance evaluation, college degree requirement, civilianization, percent non-White officers, fiscal capacity, education incentive pay, specialized skills pay, problem-oriented policing, community partnerships, violent crime rate, property crime rate, officer assault rate, percent non-White residents, percent female residents, percent foreign-born residents, percent of residents receiving SNAP benefits, percent bachelor-degreed residents, and region. Entropy weighting reduced the mean differences between the comparison samples to zero (results available upon request). As recommended (McMullin & Schonberger, 2022), entropy weights were generated separately in each panel wave. The estimated regression coefficient is interpreted as the average treatment effect on the treated (ATT) considering only untreated control group observations are weighted.

Results

Descriptive statistics

Table 3 reports imputed descriptive statistics for the study variables. We begin with the key measures. The mean rate for overall PRFs was 0.11 per 1,000 arrests, with the average Black PRF rate slightly higher at 0.09 per 1,000 Black arrests compared to the mean White PRF rate of 0.07. Approximately three-quarters (74%) of sampled agencies authorized collective bargaining during the study period, with higher percentages found in larger cities (82% for cities with at least 1 million residents; 88% for cities with more than 500,000 residents; 73% for cities with less than 200,000 residents; 65% for cities with fewer than 50,000 residents). For the 250 LEAs with collective bargaining authorization in at least one observation, the authorization was active in 83% of their observations. Among the 112 agencies without a CBA at any point, this was the case in about two-thirds (67%) of their observations. Median salaries of non-executive officers were roughly a third (33%) higher than the parity wage (M = 33.12, SD = 21.49, range = −41.05–220.91).

Sampled agencies' racial and gender composition corresponded to national estimates, with an average of 23.4% non-White officers and 10.2% female officers. Around 12% of the agencies

Table 3. Imputed descriptive statistics of selected study variables.

Variable	M/Percent	SD	Min	Max	*N*
Outcome measures					
Overall PRF rate	0.11	0.24	0	3.16	1,128
Black PRF rate	0.09	0.36	0	4.29	1,128
White PRF rate	0.07	0.41	0	11.74	1,128
Focal measures					
Collective bargaining	73.49	1.31	–	–	1,128
Fair wage index	33.11	21.49	−41.05	220.91	1,128
Covariates					
Police strength (logged)	−6.24	0.36	−7.17	−4.91	1,128
Percent of non-White officers	23.40	18.94	0	100	1,128
Percent of female officers	10.21	4.70	0	42.20	1,128
Civilianization	23.18	8.64	0	47.48	1,128
College degree requirement	11.61	0.95	–	–	1,128
Fiscal capacity	245.21	124.19	15.83	1,531.89	1,128
Tuition reimbursement	73.48	1.32	–	–	1,128
Education incentive pay	73.15	1.33	–	–	1,128
Specialized skills pay	37.11	1.44	–	–	1.128
Merit pay	34.84	1.42	–	–	1,128
Problem-oriented policing	63.75	1.44	–	–	1,128
Community partnerships	68.54	1.39	–	–	1,128
Performance evaluation	44.84	1.49	–	–	1,128
Officers feloniously assaulted	136.21	162.29	0	3,140.52	1,128
Violent crime rate	634.19	426.95	1.61	3,020.03	1,128
Property crime rate	4,580.16	2,200.19	46.19	22,615.23	1,128
Population	156,265.60	275,227.60	22,759	3,900,794	1,128
Percent non-White residents	35.08	18.03	4.55	96.31	1,128
Percent female residents	51.42	1.46	46.55	57.02	1,128
Percent foreign-born residents	32.45	29.99	1.13	98.49	1,128
Percent of residents below the federal poverty level	15.44	6.55	2.19	40.15	1,128
Percent of residents receiving SNAP benefits	1.32	0.81	0.15	5.52	1,128
Median household income	46,417.91	14,856.07	24,409	122,080	1,128
Percent bachelor-degreed residents	17.95	9.27	3.22	59.44	1,128
Unemployment rate	8.23	3.24	2.30	26.38	1,128
Average household size	2.70	0.38	1.89	4.63	1,128

$N_{max} = n$ (282 cities) $\times$ t (4 waves).

required a 2-year college degree, with 74% offering tuition reimbursement and 37% providing education incentive pay. The mean agency expenditure per capita was $245.21, and over a third offered specialized skills pay (37%) and merit pay (35%). The average officer assault rate was 136.2 per 1,000 officers. The average violent crime rate was 635.9 per 100,000 residents, while the average property crime rate was 4,586.3 per 100,000, with population estimates ranging from 22,759 to 3,900,794. The demographic composition of the sampled cities included, on average, 35.1% non-White residents, 51.4% female residents, and 32.5% foreign-born residents, with approximately 15.4% of city residents below the poverty level and nearly 18% being 4-year college graduates (descriptives for the full set of measures are shown in Table 3).

T-test and chi-square test results show significant differences between unionized and non-unionized agencies (only measures with significant variation across comparison groups are presented). Beginning with t-test results displayed in Table 4, as predicted, we find that unionized agencies generally offered salaries much farther above the fair market wage than non-unionized agencies. Unionized agencies provided lower police coverage per capita. However, they reported higher expenditures and a larger percentage of non-White officers. Cities with unionized police forces experienced lower property crime rates, but their unemployment rates, percent of immigrant-residents, average household size, and median household incomes were relatively higher, though more residents received SNAP benefits on average. For the outcome measures, the mean differences were similar before and after entropy weighting, suggesting that observed or unobserved heterogeneity bias is minimal.

Table 4. T-tests of means in unionized and non-unionized police agencies.

	Unionized ($n = 829$)		Non-unionized ($n = 299$)		
Variable	Mean	SD	Mean	SD	t-value
Fair wage index	35.57	22.19	26.33	17.77	6.44***
Police strength (logged)	−6.27	0.36	−6.15	0.33	−4.89***
Percent non-White officers	24.35	19.93	20.79	15.63	2.78**
Fiscal capacity	257.39	131.65	211.36	92.89	5.56***
Property crime rate	4,232.73	1,951.82	5,576.44	2,518.10	−9.35***
Percent female residents	51.31	1.42	51.73	1.54	−4.38***
Percent foreign-born residents	37.22	29.71	19.24	26.68	9.21***
Percent residents receiving SNAP benefits	1.41	0.86	1.08	0.57	5.96***
Median household income	47,837.44	15,378.62	42,482.14	12,505.91	5.41***
Unemployment rate	8.42	3.33	7.71	2.89	3.30**
Average household size	2.73	0.40	2.62	0.31	4.08***

$N = 1{,}128$. *$p < .05$ **$p < .01$ ***$p < .001$.

Table 5. Chi square of independence test.

	Unionized		Non-unionized		
College degree requirement	*N*	%	*N*	%	Total
No	274	91.64	723	87.21	997
Yes	25	8.36	106	12.79	131
Total	299	100	829	100	1,128
χ^2 (1) = 4.19; $p = 0.041$					
Education incentive pay					
No	96	32.14	207	24.94	303
Yes	203	67.86	622	75.06	825
Total	299	100	829	100	1,128
χ^2 (1) = 5.82; $p = 0.016$					
Specialized skills pay					
No	209	69.87	500	60.31	709
Yes	90	30.13	329	39.69	419
Total	299	100	829	100	1,128
χ^2 (1) = 8.60; $p = 0.004$					
Merit pay					
No	159	53.18	576	69.48	735
Yes	140	46.82	253	30.52	393
Total	299	100	829	100	1,128
χ^2 (1) = 25.73; $p = 0.000$					
Region					
South	239	79.93	209	25.21	448
Northeast	7	2.34	173	20.87	180
Midwest	16	5.35	192	23.16	208
West	37	12.37	255	30.76	292
Total	299	100	829	100	1,128
χ^2 (1) = 279.44; $p = 0.000$					

*$p < .05$ **$p < .01$ ***$p < .001$.

Significant chi square results in Table 5 show that college degree requirements, education incentive pay, specialized skills pay, merit pay, and region were significantly positively related to collective bargaining authorization.

Table 6 displays the arrest-based PRF rates for sampled agencies by authorization status between 2000 and 2013. Sample and national PRF rates trended upward regardless of union status. National growth in overall and race-specific PRF rates ranged between 162.2% and 177.9%, with the sharpest increases observed for White PRF rates. Apart from Black rates, the growth in PRF rates was about

Table 6. National and sample PRF arrest-based rates by unionization status.

National rates					
Year	2000	2003	2007	2013	% change
Overall PRF rate	0.006	0.007	0.008	0.014	162.2
Black PRF rate	0.006	0.008	0.009	0.016	175.9
White PRF rate	0.004	0.004	0.005	0.010	177.9
Sample rates					
Year	2000	2003	2007	2013	% change
Overall PRF rate					
0 = non-unionized	0.042	0.051	0.085	0.113	168.4%
1 = unionized	0.087	0.108	0.142	0.219	151.5%
Black PRF rate					
0 = non-unionized	0.039	0.037	0.071	0.060	55.6%
1 = unionized	0.091	0.096	0.155	0.253	178.0%
White PRF rate					
0 = non-unionized	0.026	0.029	0.041	0.124	368.6%
1 = unionized	0.045	0.061	0.061	0.113	150.1%

Fatal Encounters database and UCR arrest data.

30 to 218 percentage points greater for sampled agencies not authorizing collective bargaining relative to authorized agencies. In contrast, the growth percentage over the study period for Black PRF rates was over three times lower for non-unionized agencies than unionized agencies.

Table 7 reports the estimates from the PPML regression models. We begin with the significant main effects detected in all models, whereby a 1% FWI increase corresponds with a 1% decrease in overall rate ($B = -0.01$, $S.E. = 0.02$) and a respective 4% and 5% decrease in the rate of Black ($B = -0.04$, $S.E. = 0.01$) and White ($B = -0.05$, $S.E. = 0.01$) people dying from police actions. Our collective bargaining (CB) measure was not significantly related to the outcomes.

Table 7 also shows interaction analysis results. Significant interaction coefficients emerged in the overall and Black lethality models. FWIs ranged between −41.1% and 220.9% throughout the study period. Given our focus on racial differences, we graphically depict the marginal effects of collective bargaining across different fair wage index levels to better illustrate the race-specific interactive relationships.

For overall police lethality, marginal effects analysis reveals the significant interaction is driven by the effects in both unionized and non-unionized agencies. A move from the lowest to highest FWI level contributed to PRF rate reductions of 1 to 1.9 percentage points in unionized agencies. For non-unionized agencies, the interaction became significant once median wages transitioned from below to above parity. Between the lower and upper bounds of this range, overall police lethality rates dropped an associated 6.1 points.

Table 7. Effects of unionization and fair wage gap on PRF rates (total arrest benchmark).[abcd]

	Overall PRF rate		Black PRF rate		White PRF rate	
	N = 1,128		*N* = 1,128		*N* = 1,128	
	B	95% CI	*B*	95% CI	*B*	95% CI
Collective bargaining	−0.10 (0.18)	−0.46, 0.26	0.60 (0.51)	−0.40, 1.59	−0.93 (0.55)	−2.02, 0.15
FWI	−0.01 (0.01)*	−0.03, −0.00	−0.03 (0.01)**	−0.05, −0.01	−0.05 (0.01)**	−0.07, −0.02
CB × FWI	0.02 (0.01)*	0.00, 0.04	0.10 (0.03)**	0.04, 0.14	0.02 (0.01)	−0.00, 0.05

*$p < .05$, **$p < .01$, ***$p < .001$.
[a]Robust standard errors in parentheses
[b]Coefficients reflect semi elasticities.
[c]Regressions included controls for measures described in the Data and sample section.
[d]Regressions included city, state, region, and year fixed effects.

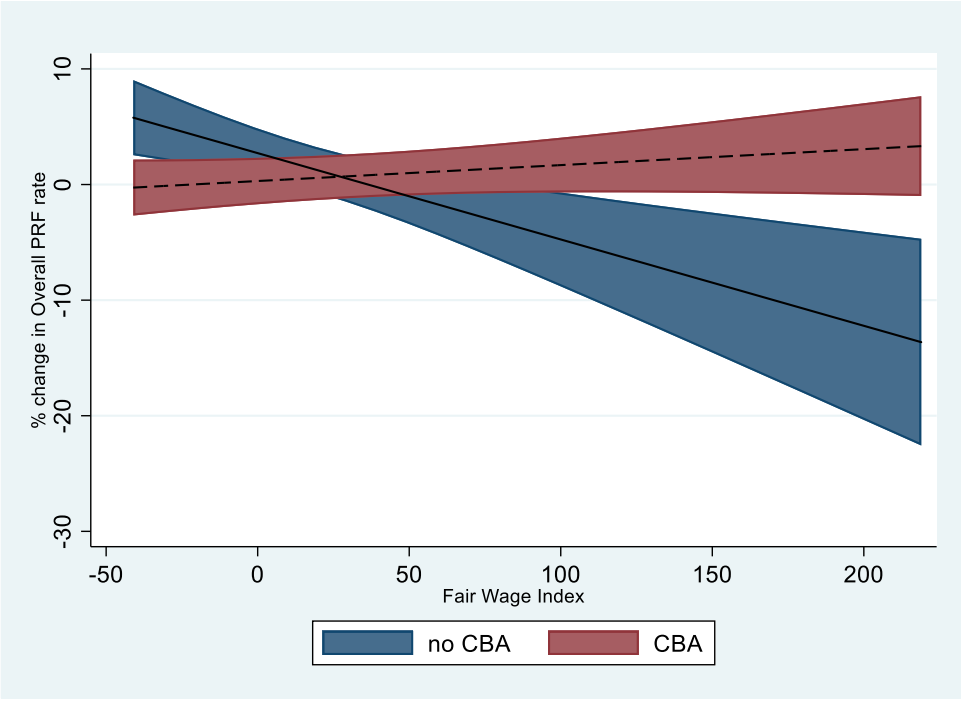

Figure 1. Interaction: marginal effect on Black PRF rates (Black arrest benchmark) (95% CI).

As shown in Figure 1, positive but insignificant marginal effects for unionized agencies underpin the significant interaction for Black PRFs. However, statistically meaningful marginal effects emerged at the lowest and highest FWI levels in agencies without CB authorization. In these agencies, Black PRF rates decreased significantly as agency FWI scores increased from −41% to −31% below wage parity, and then again from 39% above wage parity throughout the FWI. The confidence intervals reveal the difference between the two groups' average marginal effects became statistically significant at the 109% FWI level.

Concerning the White PRF model, the interaction term did not reach statistical significance but approached this threshold with a *p*-value of .08. Per Kingsley et al.'s (2017) recommendations, we delved deeper into this relationship as underlying marginal effects could be statistically different from zero for some values of the moderating variable. In contrast to the Black PRF models, we found significant marginal effects for unionized agencies (see Figure 2). The marginal effects of unionization were significantly negative for White PRFs when wages closely approached and traveled above parity. Within this range, estimates show that collective bargaining authorization results in a decrease of 1.4 – 6.7 percentage points in the White PRF rates. Although significant across the same FWI scores, the decrease for non-authorized agencies was respectively 0.9 – 3.6 points lower than the corresponding drop in unionized agencies.[7]

Supplemental analysis with alternative outcomes

To account for varying levels of risk for physical altercation, we replicated our analysis using the number of arrests for felony or Part 1 crimes as denominators to proximate encounters with more serious offenders (Fyfe, 1980; Geller & Karales, 1981; White, 2006). We calculated overall police lethality rates by dividing the number of police-caused deaths by the number of Part 1 arrests in a given city-year and multiplying the quotient by 1,000 ((Total PRFs ÷ Total Part 1 arrests) × 1,000).

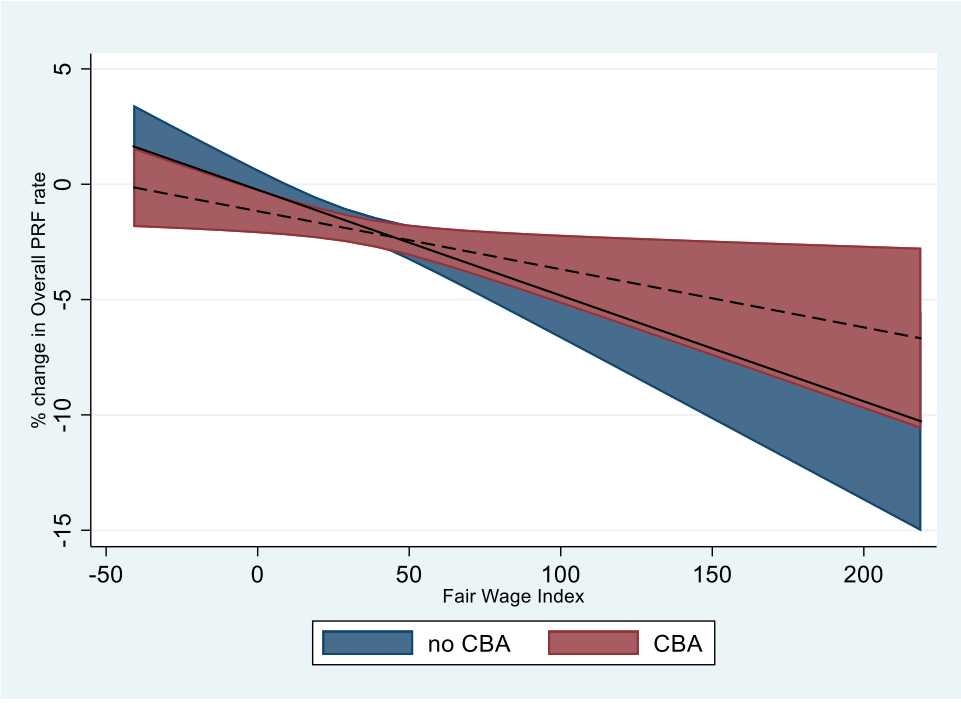

Figure 2. Interaction: marginal effect on White PRF rates (White arrest benchmark) (95% CI).

We used race-specific arrest totals for Black PRF rate ((Black PRFs ÷ Black Part 1 arrests) × 1,000) and White PRF rate ((White PRFs ÷ White Part 1 arrests) × 1,000).

Regression results with this benchmark (see Table S1 in Supplemental materials) are largely similar to those found with the total arrest denominator. Mirroring our primary findings, we did not observe significant main effects associated with collective bargaining authorization, but the FWI correlates with decreases in overall ($B = -0.03$, $S.E. = 0.01$), Black ($B = -0.03$, $S.E. = 0.01$), and White ($B = -0.04$, $S.E. = 0.01$) PRF rates. Regarding the interaction between collective bargaining and relative pay, a significant coefficient emerged in the overall and Black PRF models. Graphical illustration of the interaction results regarding Black PRFs is presented in Figure S1 in Supplemental materials. The interaction term in the White PRF model was not significant and failed to approach this threshold as seen in the primary analysis. However, we did include a graphical depiction of the interaction for White PRFs (Figure S2), as underlying marginal effects became significant at 49% above wage parity. We elaborate on the interaction results for the Black PRF model next.

Beginning with agencies unauthorized to collectively bargain, we observed meaningful reductions in Black rates across the full FWI. Figure S1 shows a declining pattern for these agencies, with marginal effects corresponding with a decrease of 17.7 percentage points in police lethality rates between the minimum and maximum FWI values. For agencies with collective bargaining authorization, the Black rates conversely increased as relative salaries rose above pay levels 9% higher than the parity score. We observed associated increases of 1.4 – 4 percentage points in Black PRF rates across this continuum. Consistent with the primary findings, the confidence bounds in Figure S1 depict significant differences in the two groups' average marginal effects, but at much lower FWI levels beginning at 69% above wage parity (compared to 109% above parity using the total arrest denominator).

We conducted additional analyses to investigate if the results are linked to changes in officer productivity or the level of danger faced by officers in the sampled jurisdictions (results not shown but are available upon request). Staying with the methods applied in the

primary analysis (except for the inclusion of population-based PRF rates as a control), we explored how variation in relative pay modifies the effects of collective bargaining authorization. We examined seven outcomes, including total arrests per 100,000 residents, total Black arrests per 100,000 Black residents, total White arrests per 100,000 White residents, Part 1 arrests per 100,000 Black residents, Part 1 arrests per 100,000 White residents, reported violent crime rate per 100,000 residents, and felonious officer assaults per 1,000 sworn officers.

These regression estimates demonstrate that neither collective bargaining, relative pay, nor their combination significantly influences arrest rates or the rate of reported criminal violence. However, we find that relative pay modifies the association between collective bargaining and assaults against officers. The plotted marginal effects of collective bargaining authorization on officer assault rates presented in Figure S3 are consistently significant across all FWI levels, with associated increases hovering around 5% points at each wage level. We also observed significantly positive marginal effects along the full FWI for non-unionized agencies.

Police officers can use lethal force if a suspect poses an imminent threat, but not all incidents meet this standard. Unfortunately, the unavailability of data during the study period prevents analysis of cases considered legally justified or involving armed decedents, making it difficult to determine the threat level faced by officers. Nonetheless, for our final supplemental analysis, we attempted to capture situations when deadly force might be necessary by applying the count of felonious assaults against officers (lethal and non-lethal) as the denominator. Using the same methodology, we explored how variation in relative pay moderates the association between collective bargaining authorization and the number of PRFs per 1,000 assaults against officers. Data on the race of the assailant are unavailable, which prevents a race-specific analysis.

PPML regression models produced significant coefficients for the FWI indicator ($B = -0.03$, $S.E. = 0.02$, $p < 0.05$) and the interaction term ($B = 0.04$, $S.E. = 0.02$, $p < 0.05$). Figure S4 shows that both unionized and non-unionized police forces experienced significant decreases in overall lethality as FWI scores increased. However, we observed steeper declines among police forces unauthorized to collectively bargain. Importantly, the confidence intervals in the last two figures overlap, indicating no statistically significant differences in the average marginal effects of these two groups.

Finally, per a reviewers' suggestion, we explored the possibility of a three-way interaction between collective bargaining, relative police pay, and agency college degree requirements. Though beyond the scope of this study, prior research provided some basis for anticipating this three-way interaction (Johnson, Johnson, & Sevigny, 2022). Similar to higher wages, law enforcement college education requirements signal professionalization and attract high-quality officers. Research indicates that police departments can enhance their efficiency and reduce the likelihood of fatal encounters, particularly with Black citizens and unarmed individuals, by hiring college-educated officers who have traits such as self-discipline, intelligence, civility, urbanity, and a heightened understanding of racial discrimination and systemic inequities (see Johnson et al., 2022 for more details). The coefficient for the three-way product term was statistically insignificant ($p = .649$) and the same key findings remained. However, we observed a significant interaction between college degree requirements and police pay ($b = -.17$; $p < .05$). As shown in Figure S5, decreasing but insignificant marginal effects for degree requiring agencies underpin the significant interaction. For agencies without a 2-year degree requirement, significantly negative marginal effects emerged across the entire FWI continuum. Considering the alternative benchmarks and outcomes, our supplemental analyses lend additional support to our primary findings.

Discussion and policy implications

The current study's findings suggest that the impact of collective bargaining on police lethality is contingent on the competitiveness of negotiated police wages. In what follows, we discuss our two key findings and potential policy directions.

Key finding 1: main effects of relative police pay

Our models show that collective bargaining authorization is not directly associated with police-caused deaths. Contrastingly, we found that higher officer pay significantly reduces police lethality rates, emphasizing the importance of competitive wages in law enforcement. Research grounded in efficiency wage theories asserts that employers paying market-clearing wages will see gains in organizational performance. A straightforward application in this study's context would imply that higher-paid police forces potentially lead to more police-citizen confrontations, as they are more likely to issue citations, make arrests, and conduct traffic stops (Brandl & Stroshine, 2012; Weisburst, 2019). However, supplemental models suggest that pay levels do not affect arrest productivity, violent crime, or officer assault rates, but contribute to fewer fatal encounters. Even in situations where lethal force is likely to be considered legally justified, our research provides evidence that this still holds true.

Policy implications regarding the effects of relative pay

A first policy implication is that more competitive police salaries contribute to lower levels of police lethality. Higher relative wages, in line with the efficiency wages tradition and our hypothesis, might enhance employee performance in several ways beyond sheer output. First, scholars have long predicted that efficiency wages can enhance job candidate quality by signaling high-quality jobs (Dal Bó et al., 2013). In policing, this could mean better-paying police forces attract applicants with more human capital and superior job skills associated with fewer fatal police encounters (see, e.g., Johnson, Johnson, & Sevigny, 2022).

Second, interdisciplinary studies suggest increased compensation bolsters employee motivation, promotes safer behavior, and encourages compliance with policies and laws. Such impacts were demonstrated in research by Kudo and Belzer (2019) on truck driver safety performance, and earlier studies by Monaco and Williams (2000) and Monaco et al. (2005) on the relationship between drivers' wages and the risk of falling asleep while driving. Applied to policing, such insights suggest higher-paid police forces should perform better both in routine and 'higher stakes' encounters, as they likely utilize practices informed by law and agency policy.

Lastly, offering competitive wages could discourage police officers from using excessive force as it would come with the risk of ruining their livelihoods (Becker & Stigler, 1974). The skillset acquired in law enforcement is not easily transferable to other professions, making dismissals especially costly for officers. Thus, since employment records follow officers throughout their careers, higher pay may incentivize them to implement crime control approaches, physical tactics, and strategies to mitigate high-liability and fatal incidents.

Taken together, the negative association between relative pay and police lethality suggests that higher-paying agencies are able to attract and employ better-quality officers. Adding to this, the bivariate correlation (results not shown) between higher wages and greater fiscal capacity may indicate that these agencies have better resources (e.g., robust vetting, data analytics, and less-than-lethal weapons). These factors, in turn, likely coalesce to promote a culture of less lethal police operations.

Key finding 2: joint effects of collective bargaining and relative police pay

While we detected positive effects of collective bargaining on Black PRF rates and negative effects on White rates, these impacts were not statistically significant. Instead, our interaction results show that the influence of police unionization depends on the relative wages offered by departments. Our race-specific models indicate that the rates of Black people killed by police in unionized departments are higher than they would have been if these unionized departments were as responsive to competitive pay as non-unionized departments. It also appears that Black PRF rates are more sensitive to these effects given the significant interaction terms and relatively broader FWI range corresponding with significant marginal effects in Black models.[9] Although the reported magnitude of these marginal effects is relatively small, the financial burden of police lethality on a city's budget (from which the payouts come), in addition to the invaluable loss of life, can still be substantial. As examples, the city of Louisville agreed to pay Breonna Taylor's family $12 million (Callimachi, 2020), and the Minneapolis city government reached a $27 million settlement with George Floyd's family for deaths caused by police officers (Karnowski & Forliti, 2021).

Policy implications regarding the racial differentials in police lethality

A second policy implication of our findings is that unionized departments are apparently immune from the effects of higher pay in reducing PRFs involving Black people. According to limited scholarship, unionized police forces are more likely to deploy lethal tactics against Black and non-White citizens. For example, Dincer and Johnston (2023) found that increased political influence among police unions and greater union density contributed to higher Black PRF rates. Likewise, Cunningham et al. (2021) observed a higher frequency of fatal police encounters involving non-White citizens after police forces attained collective bargaining rights. Both studies concluded that the added job security and CBA protections dilute the fear of sanctions, emboldening officers to use more heavy-handed tactics. Therefore, along with the intensified enforcement focus among police forces linked to unionization (Chappell et al., 2006), reduced fear of job loss can influence safety mindfulness and attention to agency policy associated with relative pay in a manner likely to impact Black communities adversely and unevenly.

Our study confirms previous findings that police unions contribute to racial disparities in police force usage, but reconciling why only unionized agencies had increased rates of Black PRFs as pay levels rose is challenging. It seems, however, that much deeper issues than commonly cited factors like structural disadvantage, disproportionate police resource allocation, and greater job security underpin our findings. Deductive evidence also points to a volatile nexus of uneven and aggressive enforcement and increased fear and distrust of the police associated with unionization.

The fraught history of excessive force and discriminatory enforcement have made many Black Americans fearful of and resistant to law enforcement (Pickett et al., 2022; Verhaeghen & Aikman, 2022). Indeed, such insights apply to both unionized and non-unionized police forces. However, experts contend that police unions' public pushback (real or perceived) against reform, including increased transparency and accountability, and focus on preserving the law enforcement rather than the service function of policing (Magenau & Hunt, 1996), may be especially harmful to unionized agencies' relations with Black communities (Walker, 2008). The impact of police unions on police culture and operations and the resulting friction between citizens and unionized police forces leads us to reason that Black citizens may be more resistant to these agencies. Offering partial support, our non-race-based interaction analysis indicated that officers in unionized agencies encountered assaults more frequently as relative salaries increased. It should be kept in mind these explanations are speculative and mostly beyond the purview of the data.

Limitations and directions for future research

Besides the need for studies of smaller jurisdictions and rural areas, several other improvements could extend this study's insights. Chiefly, the study data did not contain information about collective bargaining provisions or the share of officers who are union members. To our knowledge, no national data source currently exists that permits such nuanced analysis. Publicly available data sources like unionstats.com offer national data about police union membership density but not at the city or agency level. Compiling such information and testing how the relationships explored in this paper vary across police forces with different union densities or CBA protections would be a valuable extension. Second, PRFs are unevenly distributed across U.S. cities (Klinger et al., 2016). Thus, our city-level analysis might have masked unionization's more localized neighborhood and precinct effects, particularly given the structural and interactional heterogeneity across U.S. neighborhoods. Future assessments using neighborhood and other micro-level data are needed. Third, despite our best efforts, our results may reflect channels not grounded in collective agreements and are instead conflated with other factors. For example, our finding that collective bargaining authorization offsets the protective effects of higher salaries for Black PRFs may indicate the culture associated with police unions rather than CBAs. If a strong traditional subculture exists, officers may be highly motivated to engage the public and use force because the subculture covers them, resulting in poor accountability. Therefore, future research should account for agency disciplinary practices to help tease out possible deterrence effects (of lack thereof) that may be at play. Another limitation, due to limited data availability, is that we only examined fatal police encounters, which represent the least common police use of force outcome. Future researchers should replicate this study to analyze non-lethal outcomes of police shootings and the use of less-than-lethal interventions like tasers, chemical agents (OC spray), and physical combat techniques. Likely, these efforts will have to be local in nature, as national-level data on less than lethal force are limited. The FBI use of force data cover 40% of the U.S. population, and survey data from BJS's Police-Public Contact Survey are not jurisdiction specific. Finally, determining whether instances of deadly force by police were unwarranted or preventable is not within the scope of the study, so further research is required to better understand the circumstances surrounding these encounters.

Conclusion

Our study demonstrates that although higher relative pay for police can save lives, the implementation of collective bargaining for police can negate this effect and lead to the loss of Black lives. This suggests that unionized forces, with their accompanying cultures, may be resistant to change and too powerful, which reduces the fear of discipline or job loss. Alternatively, police are more cautious in their jobs due to the concern of losing their high pay in non-unionized departments. It is also possible that non-unionized police forces, in contrast to unionized agencies, may treat residents better when they receive higher pay due to feeling valued by their community, local government, and department. Our results are suggestive and require further investigation, but the policy relevant upshot is that police and government leaders should carefully weigh the potentially fatal and inequitable consequences for citizens during collective bargaining and salary-setting negotiations.

Notes

1. Fourteen states have established 'police bills of rights' laws granting special protections to law enforcement officers (LEOs) under investigation for misconduct (Elliott, 2021). And even in states like Georgia that prohibit police from bargaining collectively, police associations offer members legal aid, political advocacy, and other services.
2. Some instances require lethal intervention by police to protect and save lives. Considering this unfortunate reality along with the lack of nuanced PRF data, we do not argue whether PRFs represent 'good' or 'bad' police

behaviors. However, we contend that any time an officer uses life-threatening force to achieve compliance – whether justified or not – the effects reverberate throughout the police force and community.
3. Throughout this paper, terms like PRFs, police killings, and police homicides refer to citizens killed by on-duty police officers for any reason, whether intentional or accidental.
4. It warrants noting that local governments often provide disciplinary protections to employees in lieu of higher wages and benefits during negotiations of CBAs (Jones, 2024).
5. The selection into the sample was not based on PRF rates but on other, LEMAS-related sampling characteristics (see Goodison, 2022 for LEMAS more details on sampling procedures).
6. Please see Supplemental materials for more details on our multiple imputation and model specification procedures and diagnostics.
7. Similar results were found using race-specific population, total arrest, and violent arrest estimates as denominators.

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