The Effect of Police Layoffs on Crime: A Natural Experiment Involving New Jersey's Two Largest Cities

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Abstract:

The current study tests the effect of police layoffs on crime through a natural experiment involving Newark and Jersey City, New Jersey's two largest cities. In response to severe budget shortfalls resulting from the economic recession beginning in 2008, officials in both cities seriously considered police layoffs as a potential component of their cutback strategies. The Newark Police Department terminated 13% of the police force in late 2010 while Jersey City officials averted any layoffs from occurring. The current study uses monthly Part 1 crime counts spanning from 2006 to 2015 to measure the effect of the police layoffs on crime in Newark. Findings of time series generalized least squares regression models indicate the police layoffs were associated with significant increases of overall crime, violent crime, and property crime in Newark as compared to Jersey City in the post-layoffs period. Supplemental analyses found the overall crime and violent crime increases become progressively more pronounced each year following the police layoffs.

Keywords:

Police layoffs, Police force size, Natural experiment, Police budgets, Policing strategy

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Introduction

In early 2008, the United States economy began a significant downturn, representing the 10th economic recession since World War II (Wiseman, 2011). Budget shortfalls required strategic cutbacks on the part of American police agencies over the approximately 18 months the recession lasted (COPS, 2011; Wiseman, 2011). An estimated 10,000 law enforcement jobs were lost during this time period (COPS, 2011). Researchers exploring the recession's effect on policing have focused on issues such as the impact of officer layoffs on agency operations (COPS, 2011; PERF, 2010), cutback management strategies for police to consider (Wiseman, 2011), and the contextual factors associated with agencies either enacting or averting officer layoffs in the face of fiscal distress (Giblin and Nowacki, 2018). The effect of police officer layoffs on crime, to our knowledge, has yet to be subjected to empirical evaluation.

The literature on police force size provides a logical source for considering potential casual mechanisms linking police layoffs with changes in crime levels. Reviews of the literature have generally found increasing police force size does not reduce crime (Lee *et al.*, 2016; Sherman and Eck, 2002) or improve organizational performance (Skogan and Frydl, 2004). However, negative relationships between police force size and crime have been observed in certain instances (Carriaga and Worrall, 2015). It is further important to note the majority of studies on police force size have tested small, gradual changes in officer levels. Research considering abrupt, drastic reductions in police force sizes (i.e. police strikes) has found officer reductions to be significantly related to crime increases (Sherman and Eck, 2002). While research suggests police strategy is a more important consideration for crime prevention than police force size (Lee *et al.*, 2016), officer levels and changing strategy may not always be mutually exclusive. Sufficient manpower may be necessary to institute evidence-based crime prevention practices. At a time when police departments are at a cross-roads of potentially phasing out specialized units to meet budgetary constraints, this work adds to an ongoing contemporary debate about the impact of police force size on crime.

The current study seeks to contribute to the literature through a natural experiment testing the effect of police layoffs on crime. In 2008, the Newark Police Department (NPD) and Jersey City Police Department (JCPD), New Jersey's two largest police forces, both seriously considered police layoffs due to severe budget cutbacks. The police union and city officials in Jersey City came to a contract agreement to avert officer layoffs (Porter, 2011). No such agreement occurred in Newark, leading to the termination of 13% of the NPD on November 30, 2010 (Star Ledger, 2010). Our analysis found that crime significantly increased in Newark as compared to Jersey City following the NPD layoffs. We conclude the paper with a discussion of the study implications for public policy, evidence-based policing, and contemporary policing research. We begin with a review of the relevant empirical literature.

Review of Relevant Literature

Research on the relationship between police force size and crime rate has spanned decades, methodologies, and statistical inquiry. Research has occurred often enough to allow for multiple systematic reviews of the literature over time. Eck and Maguire (2006) identified 27 studies containing 89 separate tests of police force size on crime. Only about 20% of the tests indicated a negative relationship, with ~49% finding no effect and ~30% finding a positive effect. Similar findings emerged when studies employing weak research designs (e.g. cross-sectional designs, unit of analysis larger than the city-level, and those failing to addresses simultaneity between police and crime) were excluded. Eck and Maguire (2006) concluded there was no consistent evidence that increasing police officer size impacts crime. Lim *et al.* (2010) conducted a systematic review of 256 findings in 58 studies published between 1971 and 2009. They found most studies generated negative relationships between police force size and crime irrespective of their significance levels. While this would seemingly support the police force size hypothesis, Lim *et al.* (2010) determined that methodological limitations common in the original studies prevented any concrete conclusions from being reached.

Recent research has built upon the early systematic reviews by incorporating meta-analytic techniques to measure the overall effect of police force size on crime. Meta-analysis improves upon the “vote-counting” of individual study findings commonly employed in early systematic reviews. Meta-analysis combines disparate study findings into a single effect size reflecting the cumulative impact of the intervention under question (Lipsey and Wilson, 2001). Carriaga and Worrall (2015) identified 24 longitudinal studies measuring the effect of police levels on crime at a macro-level, with 12 studies providing the necessary data for inclusion in the meta-analysis. Findings of a vote-counting procedure found mixed-evidence of police level effect on crime. However, the meta-analysis found the mean effect size of sworn officers on crime was small but statistically significant, indicative of a negative relationship between police levels and crime rates.

A later systematic review and meta-analysis conducted by Lee *et al.* (2016) generated findings in contrast to those of Carriaga and Worrall (2015). Lee *et al.* (2016) analyzed 229 findings across 62 studies conducted from 1971 to 2013. The analysis also tested whether a change in the size of an existing police force had an impact on crime, but differed in selection criteria from Carriaga and Worrall (2015). Selection criteria did not require the use of longitudinal designs and specified that studies must have been conducted in the United States. Lee *et al.* (2016) used meta-regression techniques to examine changes in findings over time and identify potential reasons four such change (e.g., publication time period, statistical methodology, and variation in police force size over time).

Lee *et al.* (2016) estimate a small, nonsignificant effect size suggesting police force size to be unrelated to crime. Lee *et al.* (2016) further contextualized their findings by comparing the police force effect size to those from meta-analyses of contemporary policing strategies: problem-oriented policing (Weisburd *et al*., 2010), neighborhood watch (Bennett *et al*., 2006), hot spots policing (Braga *et al*., 2014), and focused-deterrence (Braga and Weisburd, 2012). This exercise showed that the effect of police force size was, in the words of Lee *et al.* (2016), "miniscule" in comparison to these policing strategies. The cumulative findings of their study led Lee *et al*. (2016) to two main conclusions: 1) that research on the effect of police force size on crime has "exhausted its utility" and 2) changing policing strategy is likely to have a greater impact than adding more police.

Upon further inspection of the literature, the general lack of relationship between police force size and crime may be explained by how officers allocate their time on duty. Researchers have found the majority of police officer time constitutes activities unrelated to fighting crime (Black, 1971; Cumming *et al*., 1965; Payne, 2017). Perhaps we should not expect officer levels to significantly impact the crime rate given most incidents police respond to are not criminal in nature. However, research published since Carriaga and Worrall (2015) and Lee *et al.* (2016) suggests there may be a relationship when focusing on crime type, swiftness of force level change, and deployment strategy.

While the body of empirical research has found that adding police does not universally decrease crime, it does not mean that reducing the size of the police force will be without consequence in all instances (Payne, 2017). Recent studies by Chalfin and McCrary (2018), Mello (2019), and Kaplan and Chalfin (2019) suggest the question researchers should be asking is whether police are impacting violent crime – particularly murder – and property crime in the same manner. Chalfin and McCrary (2018) incorporate various statistical models to account for simultaneity bias and suggest that police reduce murder to a greater extent than they do assault, larceny, and burglary. Similarly, Mello (2019) finds violent crime to be more responsive than property crime to increases in police force size. Specifically, one additional officer results in a decrease of 0.11 murders, 0.53 rapes, and 1.98 robberies. In a similar vein, Kaplan and Chalfin (2019) show the addition of one officer has the ability to prevent approximately seven index crimes – six property and one violent. A particularly noteworthy finding of the study is increasing the number of police does not lead to increased incarceration, representing what the authors call a "double dividend" – a decrease in crime and incarceration rates.

The aforementioned studies, including those used in the relevant meta-analyses, measured the effect of incremental force level changes on crime. The question then becomes *what happens when change is drastic, and in the negative direction*? The empirical literature on police strikes informs such research questions. Five of the six police strike studies reviewed by Sherman and Eck (2002) found major increases in both violent and property crime following police strikes (Andenaes, 1974; Clark, 1969; Makinen and Takala, 1980). The lone exception was the study by Pfuhl (1983), which found strikes across 11 cities had neither a statistically significant or systematic impact on rates of the reported crimes in question. However, Sherman and Eck (2002: 302) noted 89% of the "strike" period in Pfuhl's study consisted of non-strike days, confounding the measurement of cause and effect. Nonetheless, we should note that the overall body of research on police strikes is methodologically weak with no study rating higher than 2 on the Maryland Scientific Methods Scale (Farrington *et al.,* 2002; see Sherman and Eck, 2002: 303). As this is below the minimally interpretable research design (level 3) involving a comparable control condition it cannot be ruled out that crime would have increased in the absence of a strike.

When discussing the conflicting evidence in support of police force size as a crime control mechanism, it should be noted recent decades have seen the rise of evidence-based policing, which advocates for police strategy to be rooted in scientific evidence (Lum and Koper, 2017; Sherman, 1998). Generally speaking, proactive, focused, and place-based police interventions are more likely to result in reduced crime and disorder as compared to initiatives concentrating on individuals, or that that are reactive (Lum *et al.*, 2011; also see chapter 3 in Lum and Koper, 2017). The evidence-based policing literature supports Lee *et al.'s* (2016) conclusion that changing police strategy is likely to have a larger crime control effect than increasing the size of a police force.

An important caveat is effectively implementing evidence-based practices may require a department to employ a sufficient number of police officers. The Flint, MI police department (FPD) provides such an example, as the agency lost approximately 50% of its sworn officers (almost 300 to under 150 officers) over two decades (Terrill *et al.*, 2014). This loss of officers prevented FPD from continuing its longstanding community policing initiatives or implementing contemporary strategies such as hot spots policing, disorder reduction strategies, or CompStat due to a lack of resources, rather than resistance from police leadership or front-line personnel. Additionally, officers once assigned specifically to community policing were shifted to basic patrol following the budget cutbacks. A similar situation was observed in Newark, NJ, which directly informs the current study. In the summer of 2008, the NPD began Operation Impact, a foot-patrol saturation intervention that deployed 12 foot-patrol officers in an approximately quarter-square mile area of the city on a nightly basis. An evaluation by Piza and O'Hara (2014) found the foot-patrols generated a significant reduction in overall violence as compared to two separate control areas (Piza and O'Hara, 2014). Despite the intervention proving beneficial, Operation Impact patrols were canceled and ultimately phased out due to budget shortfalls over the looming police layoffs (see Piza and O'Hara, 2014: 713).

Conversely, natural experiments levying current events have demonstrated the benefits of *increasing* police presence at a granular level. Di Tella and Schargrodsky (2004) and Draca *et al.* (2011) measured police increases in response to terrorist attacks in Buenos Aires and London, respectively, each finding evidence of crime decreases. Klick and Tabarrok (2005) used terror alert levels in Washington D.C. to demonstrate increased officer presence resulted in significant decreases of various crime types. Simpson and Hipp (2019) explored the bi-directional relationship between crime and increased levels of police stops and foot patrols. They found a significant negative relationship between police actions and crime, with city blocks experiencing more police stops having reduced odds of burglary in subsequent time periods.

The current study contributes to the literature through a natural experiment testing the effect of police layoffs on crime. Our methodology expands upon the prior literature primarily focused on incremental changes to police force size. The natural experiment involves New Jersey's two largest cities, Newark and Jersey City. The Newark Police Department (NPD) terminated 13% of the police force on November 30, 2010 while Jersey City officials were able to avert layoffs. More detail on the negotiations preceding the officer layoffs and as well as the subsequent effect on agency practice appears below.

Methodology

Study setting

Newark and Jersey City are the two largest cities in New Jersey, with populations of 277,140 and 247,597, respectively, according to the 2010 decennial census. Ethnic minorities account for the majority of the citizenry, with only 12.2% of residents in Newark and 22.6% of residents in Jersey City identifying as White alone. In 2010, Newark and Jersey City boasted the two largest police forces in the state, employing 1,308 and 831 officers, respectively (FBI, 2011a). Both cities exhibited 2010 overall part 1-, violent-, and property-crime rates well above average for New Jersey municipalities with at least 50,000 residents (see Table 1). Nonetheless, as will be discussed subsequently, Newark and Jersey City experienced general reductions in crime during the preceding years. This is important in the context of the current study, as research suggests cities with diminishing crime rates may be more likely to consider police layoffs in the face of fiscal constraints (Giblin and Nowacki, 2018).

Table 1: Study setting characteristics

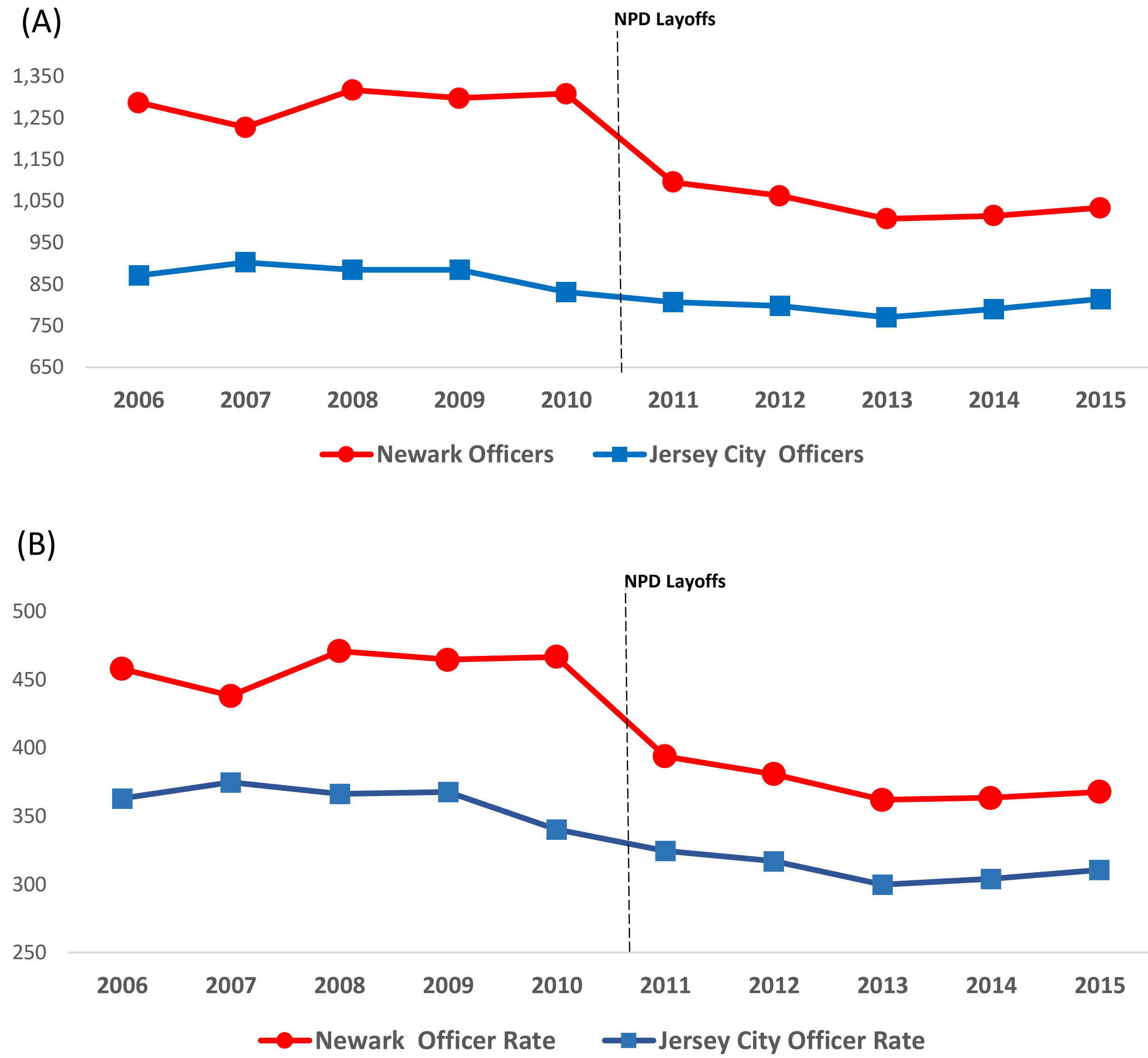
	Newark	**Jersey City**
Population	277,140	247,597
% White alone	12.2%	22.6%
# of officers pre layoffs (per 100,000 residents)	466.5	340.3
# of officers post layoffs (per 100,000 residents)	393.8	324.4
2010 Part 1 Crime Rate (per 100,000 residents)	4,313.5	3,202.7
2010 Violent Crime Rate (per 100,000 residents)	1,029.0	749.0
2010 Property Crime Rate (per 100,000 residents)	3,284.5	2,454.7

Note: According to the 2010 UCR, the 32 New Jersey municipalities with 50,000 or more residents reported an average Part 1 crime rate of 2,763.8 per 100,000 residents, a violent crime rate of 479.0 per 100,0000 residents, and a property crime rate of 2,284.8 per 100,0000 residents.

Similar to police departments around the United States, the NPD and JCPD were challenged by the economic downturn beginning around 2008. In early 2009, officials in both cities seriously considered police layoffs as a means to offset budget deficits, leading to prolonged negotiations between city officials and their respective police unions. In Jersey City, the police union ultimately agreed to a contract providing an 11% raise over 4 years instead of the previously requested raise of 13% and a higher copay for medical prescriptions, which averted layoffs. The agreement saved the jobs of approximately 10% of the JCPD (Porter, 2011).

Negotiations in Newark proved to be less fruitful. The Newark Police union agreed to about $2.7 million in concessions and $6 million in pay deferrals, but rejected the city's proposal for an overtime cap and 5 days of unpaid leave (Ford, 2010). The lack of a contract agreement resulted in the termination of the 167 most recently hired officers, approximately 13% of the NPD, which took effect on November 30, 2010. The layoffs brought the per capita police officers in the NPD much closer to that of JCPD. In 2010, NPD had over 120 more officers per 100,000 residents than JCPD (466.5 vs. 340.3). While the NPD officer rate was still larger than JCPD following the layoffs, the difference was smaller (393.8 vs. 324.4). The difference became less pronounced from 2012 through 2015, as the JCPD added officers while the NPD lost officers to attrition without making any new hires (see Figure 1).

Figure 1: Number of police officers (A) and officers per 100,000 population (B) in Newark and Jersey City.



The loss of officers adversely impacted NPD practices. Following a change in leadership in 2006, the NPD committed to a placed-based approach to crime prevention. While the aforementioned Operation Impact program epitomized this strategy, all precincts regularly engaged in hot spots policing activities. The NPD frequently deployed teams of officers with little to no responsibility for responding to calls for service to maximize officer time in hot spots.

These officers were primarily tasked with proactively policing Newark's crime hot spots for the majority of their shifts. Taken as a whole, hot spots policing activities were a mainstay throughout Newark from 2006 through 2009. These practices were largely discontinued owing to the layoffs, as officers were needed for general patrol and response to citizen calls for service.

Evaluation design

The current study takes advantage of the unique timing of the fiscal crisis, budget negotiations, and police officer layoffs to conduct a natural experiment measuring the effect of police layoffs on crime in Newark. Natural experiments share certain characteristics with quasi-experiments, particularly that the creation of experimental and control groups cannot be manipulated by researchers. However, quasi-experiments make no presumption that the allocation of the intervention occurred in any random fashion. This differs from natural experiments, in which the allocation of the experimental condition occurs in a random or *as-if random* process, despite falling outside the control of researchers (Dunning, 2008).

Demonstrating treatment occurred in a random or *as-if random* process is an important first step for any natural experiment. Without doing so, researchers risk "conceptual stretching" by applying the term "natural experiment" to studies with weaker designs (Dunning, 2012). We consider the situation observed with New Jersey's two largest police departments as occurring in an *as-if random* process. We rely on the causal-process observation method, which Collier *et al*. (2010: 184) describe as "an insight or piece of data that provides information about context, process, or mechanism." Police unions and city administrators in both Newark and Jersey City actively negotiated to avoid police officer layoffs at the same point in time, with neither the NPD or JCPD motivated to self-select into the experimental group experiencing layoffs.[1] Furthermore,

[1] We acknowledge the layoffs were technically the result of an explicit choice, specifically NPD's union not accepting the new contract offer with JCPD's union choosing the opposite. However, it is important to note police

as New Jersey's two largest cities, Newark and Jersey City are more similar to one another than any of the other municipalities in the state in regards to police force size, pre-layoff police resources, and pre-layoff crime levels.

Analytical Approach

Study period

The current study incorporates monthly crime data from 2006 through 2015 to reflect the local context in Newark. In 2006, Cory Booker began his first term as Newark Mayor, with crime control as his administration's top priority. Booker installed a new Police Director at the NPD who stressed adherence to evidence-based practices, in particular proactive, place-based crime control strategies (Jenkins and DeCarlo, 2015). In January 2016, after Booker resigned as mayor following a successful campaign for the U.S. Senate, newly elected Mayor Ras Baraka merged Newark's police department, fire department, and office of emergency management and homeland security into a single Department of Public Safety. While each entity maintained autonomy in determining strategy and daily operations, this merging of agencies allowed certain administrative tasks to be shared across the disparate public safety missions, freeing up more personnel to perform front-line duties. In recognition of these contextual factors, restricting the

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unions represent police officers, not bureaucratic police agencies. Police unions typically approach collective bargaining with the goal of preserving the labor interests of officers irrespective of the impact this has on police department operations (DeCarlo and Jenkins, 2015). In the context of the police layoffs, an outside entity (i.e., police unions) decided upon a policy that directly impacted the research subjects (i.e., police departments) which we believe is distinct from self-selection. To illustrate this point, we draw a parallel to a natural experiment ranking high on Dunning's (2008) "continuum of plausibility" regarding the *as-if random* standard: Snow's ([1855] 1965) classic natural experiment of cholera transmission in London. Snow hypothesized cholera was a waterborne infectious disease spread via household water delivered by Lambeth Waterworks, one of London's two water supply companies at the time. Snow noted the choice of water supply company was made by absentee landlords rather than the renters of the property. Therefore, renters transmitting cholera from the contaminated water of Lambeth Waterworks could not be considered as self-selecting into the experimental condition.

study period to 2006 through 2015 allows us to best isolate the effect of layoffs without the potentially confounding factors of a changing agency mission (pre-2006) or creation of the Department of Public Safety (post-2015).

Outcome measures

Our analysis incorporates a panel design to conduct a time-series generalized linear regression analysis testing the effect of police layoffs on crime rates (per 100,000 population). Panel models are considered one of the best methods for estimating causation (Campbell and Stanley, 1967; Hsiao, 1986). The NPD and JCPD provided us with copies of their monthly Uniform Crime Reports (UCR) submitted to the New Jersey State Police from January 2013 to December 2015. This totaled 240 monthly reports (120 per agency). In recognition of prior research suggesting the effect of police force differs across crime types, we summed the individual crime types to form measures of *violent crime* (murder, robbery, and aggravated assault) and *property crime* (burglary, larceny theft, and motor vehicle theft).[2] We further summed all incidents into a single *total crime* category to test the aggregate effect of the police layoffs. Due to the multiple outcome measures, our analysis incorporated a Holm-Bonferroni correction to adjust critical *p.* values in order to protect against the increased risk of Type I error that results from tests of multiple hypotheses (Holm, 1979).

Independent variables

A binary measure identifies each observation as pertaining to Newark (“1”) or Jersey City (“0”). This variable was interacted with alternate measures of the post layoff-period to

[2] In consideration of the data sources and context of the study setting, both rape and arson were excluded from the analysis. The UCR definition of rape changed in 2013, negatively affecting this measure’s reliability over the study period. Arson is not actively collected by the New Jersey State Police, given the control of arson is typically considered the purview of fire departments and not the police. This is certainly the case in Newark, with the police department excluding arson from both its internal CompStat reports as well as their public crime statistics web page: see https://npd.newarkpublicsafety.org/comstat

calculate difference-in-differences (DiD) terms used as the primary independent variable. An important consideration is how the effect of police layoffs may manifest over time. As such, we computed two DiD terms for use in separate models to test the *immediate effect* and *gradual effect* of the police layoffs. For the immediate effect model, the DiD term interacts "Newark" with a binary measure reflecting the post-layoff period (December 2010 – December 2015) to measure how the termination of officers impacted crime levels during the full post-layoff time period. For the gradual effect model, the DiD term interacts "Newark" with a 5-scale ordinal measure representing each 12-month period following the layoffs (i.e., December 2010-November 2011 = 1; December 2011 – November 2012 = 2; and so on). This model measures how crime rates changed with each passing year following the layoffs.

Control variables

We sought to control for potential confounding factors by introducing additional variables in our statistical models. We consulted prior longitudinal policing studies in selecting control variables (e.g., Braga *et al.*, 2010, 2011, 2012; Marvell and Moody, 1996; Piza and Gilchrist, 2018). The lagged outcome measure (t – 1) controls for crime rates observed during the prior month. The number of days in the month controls for the potential exposure to crime, as longer months have more daily opportunities to experience crime. To control for the potential effect of seasonality on crime, we identified the quarter of the year each month fell within. The first quarter (January – March) was the reference category. A continuous variable measuring the sequential order of the monthly time periods (January 2006 =1; February 2006=2; and so on) accounts for the linear trend in the data.[3]

[3] The reader may be concerned with whether changing sociodemographic conditions in Newark or Jersey City influenced crime rates. Given the relatively small number of observations in our database, we opted against including such controls in the main analysis to avoid potential model overfitting. However, we present results of

Results

Figure 2 displays the monthly violent and property crime rates for Jersey City and Newark over the study period. The graphs include trend lines depicting the rolling 12-month moving average, which visualizes crime rate changes from January 2006 through December 2015. In Jersey City, violent crime steadily decreased over the 10-year study period. Violent crime rates in Jersey City for most of the post NPD layoffs period were lower than at any point during the pre-layoff period. While Jersey City's property crime rate peaked in early 2009, steady declines occurred subsequently. From 2011 through 2015, the moving average of property crime rates in Jersey City was lower than any year during the pre-layoff period. This contrasts with the crime trends in Newark. Violent crime rates in Newark increased in the aftermath of the officer layoffs. While the rolling average decreased somewhat from 2014 to 2015, violent crime rates during this time frame were higher in Newark than any year prior to the layoffs. More volatility was observed for property crime rates in Newark. In the pre-layoff period, the property crime rate moving average peaked in mid-2008 before declining through the remainder of the pre-layoff period. Following the layoffs, the property crime rate moving average steadily increased through late 2012 before declining through 2015. By the end of the study period, property crime rates in Newark were generally lower than in the pre-layoffs period.

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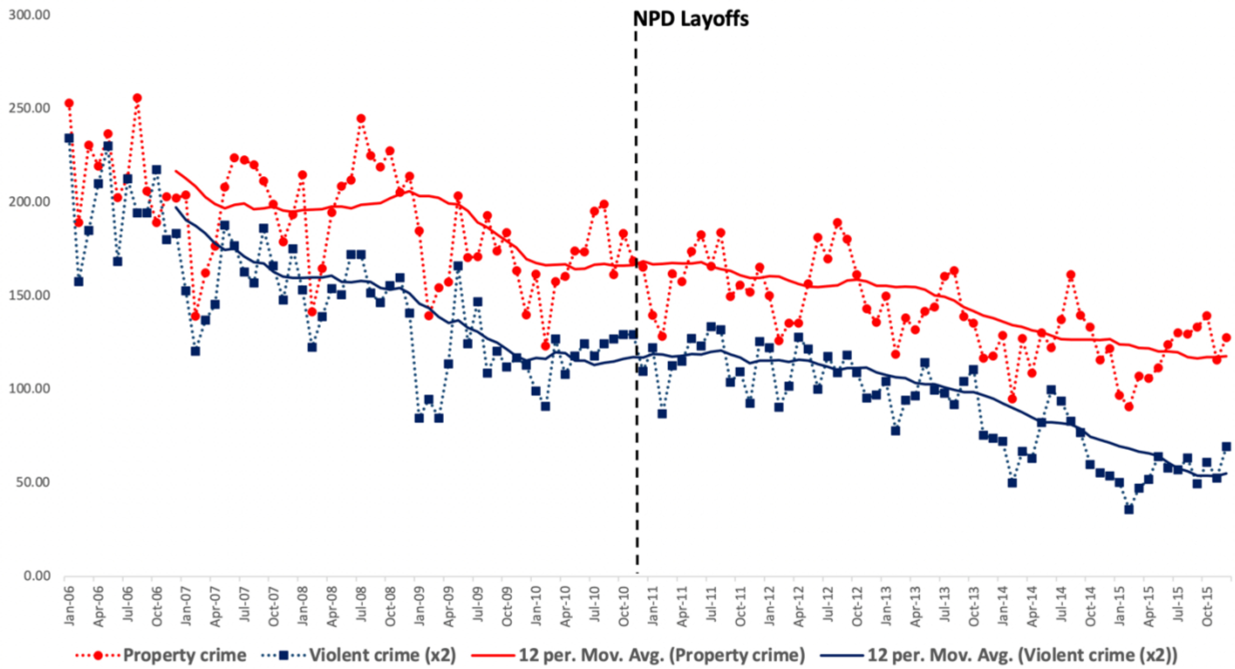
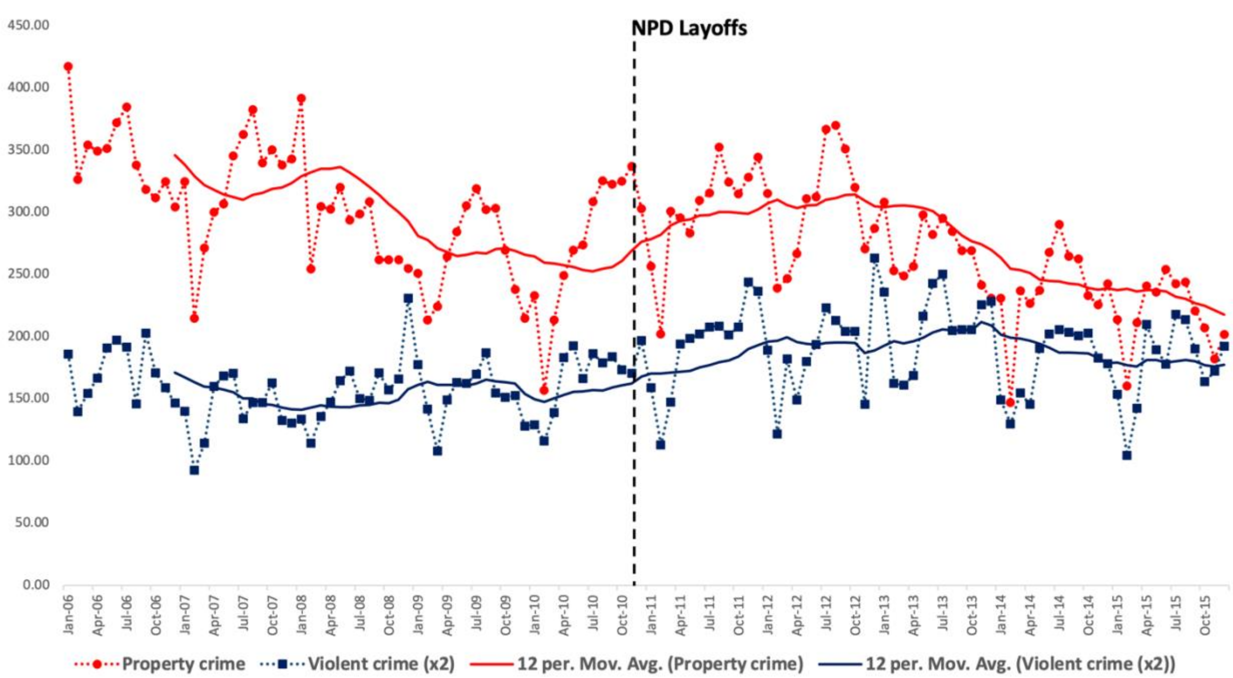
models controlling for 9 sociodemographic variables in the appendix. These results do not meaningfully differ from the main analysis presented in-text.

Figure 2: Violent crime and property crime rates (per 100,000 population).

Jersey City



Newark



Tables 2-4 display the results of the generalized linear regression models.[4] Note that *p.* values for all statistically significant DiD terms fall below the corrected ∝ values generated through the Holm-Bonferroni procedure, meaning they maintain significance after multiple comparisons are controlled for.[5] For both Newark and Jersey City, all monthly crime rates (i.e. the dependent variable) were standardized to reflect their level relative to the average city-wide rate observed across all 120 months in the study period; *b* values reflect changes in the dependent variable in terms of standard deviation increases or decreases.[6] We adjusted the interpretation of *b* for the DiD interaction terms to account for the use of a dynamic panel model that included a lagged outcome measure as a covariate. We follow the approach of Marvell and Moody (1996) by multiplying DiD *b* values by the reciprocal of one minus *b* for the lagged dependent variable [*b*DiD * (1/(1-*b*lagDV))] to interpret the true effect of the layoffs on subsequent monthly crime rates in Newark.

Table 2 reports the results of the overall crime models. In the immediate effect model, monthly overall crime rates significantly increased by 1.10 standard deviations [0.58*(1/(1-0.48))] in Newark as compared to Jersey City when considering the entirety of the post layoff period. Significant effects were also observed in the gradual effect model, with each 12-month period following the layoffs associated with a crime rate increase of 0.18 standard deviations

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[4] Data and code to replicate the generalized linear regression analysis can be downloaded at https://www.dropbox.com/s/kc0y0voa1a8b9r5/Layoffs_data.zip?dl=0

[5] Given space constraints, the Holm-Bonferroni corrected ∝ values are not presented in text, but are included in the appendix.

[6] Time series models rest on assumptions of stationarity and parallel trends between the different panels (i.e. Newark and Jersey City) during the pre-intervention period. We conducted the Levin, Lin, and Chu (2002) unit root test for panel stationarity. In all cases, the null hypothesis of panels containing unit roots (i.e. non-stationarity) was rejected: total crime (*t*= -2.11, *p*.=0.02); violent crime (*t*= -2.8514, *p*.<0.01); property crime (*t*= -1.95, *p*.=0.03). We used the ddid command in Stata to conduct a time trend test of the parallel trends assumption for the standardized crime rates used as dependent variables. In each case, the test failed to reject the null hypothesis of parallel trends: total crime (F=1.19, *p*=0.28); violent crime (F=3.30, *p*=0.07); property crime (F=2.41, *p*=0.12).

[0.09*(1/(1-0.49))] in Newark. Jointly, these models indicate that the police layoffs generated a crime increase that become progressively more pronounced over the subsequent 5 years.

Table 2. Time Series GLS Regression Model results for standardized monthly overall crime rate. 2006 – 2015.

Covariates	**Immediate Effect** *b*	**Lower**	**Upper**	***p.***	**Gradual Effect** *b*	**Lower**	**Upper**	***p.***
Newark x post layoffs	0.58	0.35	0.80	0.00	0.08	0.03	0.14	0.01
Newark	-0.29	-0.45	-0.14	0.00	-0.13	-0.26	0.01	0.07
Post layoffs	0.15	-0.08	0.37	0.20	0.33	0.11	0.56	0.00
Lagged outcome	0.48	0.39	0.57	0.00	0.54	0.45	0.62	0.00
Days in month	0.39	0.32	0.46	0.00	0.40	0.33	0.47	0.00
Quarter								
2nd	0.53	0.39	0.68	0.00	0.52	0.36	0.67	0.00
3rd	0.49	0.31	0.67	0.00	0.43	0.25	0.61	0.00
4th	0.21	0.05	0.37	0.01	0.17	0.00	0.34	0.05
Sequential order	-0.01	-0.02	-0.01	0.00	-0.01	-0.02	-0.01	0.00
Wald $(X)^2$	*1227.16*				*1122.44*			

Observations= 238
Time periods=119
Notes: Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.

The violent crime models generated similar results, though the effect of the layoffs on violence was stronger than the effect on overall crime (see Table 3). In the immediate effect model, the violent crime rate increased by 2.30 standard deviations in Newark as compared to Jersey City [1.33*(1/(1-0.42))], more than double the effect on overall crime. A significant gradual effect was also observed in Newark, with each 12-month period following the layoffs associated with a violent crime rate increase of 0.53 standard deviations [0.28*(1/(1-0.48))]. As with overall crime, the violent crime increase in Newark become more pronounced throughout the 5-year period following the layoffs.

Table 3. Time Series GLS Regression Model results for standardized monthly violent crime rate. 2006 – 2015.

	Immediate Effect				**Gradual Effect**			
Covariates	***b***	**Lower**	**Upper**	***p*.**	***b***	**Lower**	**Upper**	***p*.**
Newark x post layoffs	1.33	0.98	1.69	0.00	0.28	0.19	0.37	0.00
Newark	-0.66	-0.89	-0.43	0.00	-0.41	-0.60	-0.21	0.00
Post layoffs	-0.19	-0.51	0.13	0.24	0.39	0.10	0.68	0.01
Lagged outcome	0.42	0.32	0.52	0.00	0.48	0.38	0.57	0.00
Days in month	0.24	0.16	0.32	0.00	0.24	0.15	0.33	0.00
Quarter								
2^{nd}	0.69	0.50	0.89	0.00	0.67	0.46	0.87	0.00
3^{rd}	0.59	0.37	0.80	0.00	0.54	0.32	0.76	0.00
4^{th}	0.49	0.28	0.70	0.00	0.46	0.25	0.68	0.00
Sequential order	-0.01	-0.01	-0.01	0.00	-0.02	-0.02	-0.01	0.00
Wald $(X)^2$	*536.49*				*552.57*			

Observations= 238
Time periods=119
Notes: Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.

We present findings of the property crime models in Table 4. Similar to overall crime and violent crime, the immediate effect model indicates a significant increase in property crime rates following the police layoffs of 0.71 standard deviations [(0.33*(1/(1-0.5))] in Newark as compared to Jersey City. The DiD term did not achieve significance in the gradual effect model. The property crime analyses indicate that, while property crime rates increased in the post-layoffs period, they did not progressively worsen over time.

Table 4. Time Series GLS Regression Model results for standardized monthly property crime rate. 2006 – 2015

	Immediate Effect				Gradual Effect			
Covariates	***b***	**Lower**	**Upper**	***p.***	***b***	**Lower**	**Upper**	***p.***
Newark x post layoffs	0.33	0.13	0.54	0.00	0.04	-0.01	0.09	0.16
Newark	-0.17	-0.32	-0.03	0.02	-0.06	-0.19	0.06	0.33
Post layoffs	0.10	-0.11	0.31	0.36	0.20	-0.01	0.41	0.06
Lagged outcome	0.53	0.44	0.62	0.00	0.57	0.48	0.66	0.00
Days in month	0.41	0.34	0.48	0.00	0.42	0.35	0.49	0.00
Quarter								
2nd	0.40	0.26	0.55	0.00	0.39	0.24	0.54	0.00
3rd	0.37	0.19	0.55	0.00	0.33	0.15	0.51	0.00
4th	0.06	-0.11	0.22	0.50	0.03	-0.13	0.19	0.72
Sequential order	-0.01	-0.02	-0.01	0.00	-0.01	-0.02	-0.01	0.00
Wald $(X)^2$	*1347.75*				*1294.26*			

Observations= 238
Time periods=119
Notes: Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.

Discussion and Conclusion

The current study contributes to the literature on the effect of police force size on crime. Our findings indicate that sudden and drastic reductions in police force size via police officer layoffs can generate significant crime increases. The adverse effect of the layoffs is contextualized by translating the standardized DiD coefficients into raw per capita crime rates. With a violent crime rate standard deviation of 16.98, the adjusted *b* (12.30) indicates a monthly increase of 38.98 [16.98*2.30] violent crimes per 100,000 persons in Newark during the post-layoff period. Considering Newark's population of 277,140, this translates to approximately 108 [38.99*(277,140/100,000)] additional violent crime incidents per month resulting from the layoffs. Using a similar equation, the police layoffs resulted in approximately 103 additional property crime incidents[7] per month in Newark.

Considering the current study findings alongside the larger body of literature, it is important to note changes in police force size observed in prior research have, for the most part, been incremental. For example, Worrall and Kovandzic (2007) found COPS Office hiring grants provided funding that represented less than one half of 1% of recipient police agency budgets. This incremental addition of police officers may help explain why prior increases in police force size have generally not impacted crime. Research on police strikes show that crime significantly increases when a large proportion of police officers fail to report for duty (see Sherman and Eck, 2002: 302). While the reduction of officers in Newark was not as large as those associated with police strikes, the change in force size was sudden, with 13% of the force terminated on the same day.

We find it important to acknowledge that the termination of officers can generate poor morale amongst the entirety of a police force, which may directly impact the performance of officers and, tangentially, crime levels. The effect of layoffs on employee effectiveness was recently illustrated in a study conducted by Strunk *et al*. (2018), who found teacher layoffs in Washington State negatively impacted the performance and job commitment of the teachers ultimately retained by the school district. While we are unaware of any research that directly explores this issue in policing, we find it reasonable to assume police officers may be similarly vulnerable to stress introduced by the termination of their colleagues.

Weisburd and Telep (2010) note in a time of economic decline, police are forced to do more with scarce resources while still producing results in the most efficient ways. Evidence-

[7] Property crime rate standard deviation=52.42, adjusted *b*=0.71.

based policing allows officers to control crime and disorder in ways that are more effective and less costly than traditional response driven models (Bueermann, 2012). However, it is possible these practices may only be effectively implemented when agencies have the necessary resources to do so. A survey conducted by the Police Executive Research Forum (2010) during the recession found that 51% of reporting police agencies suffered cuts to their total funding and 59% of those agencies anticipated their budgets would see additional cuts over subsequent years. Committing to evidence-based practices may be challenging in the face of such cutbacks.

The threat of the impending layoffs led the NPD to phase out their hot spots policing activities in anticipation of the termination of the officers staffing these units. The situation in Newark may not be unique, as the disbandment of specialized units is a common response to budget cuts. For example, a report from the National Institute of Justice (Wiseman, 2011) offered unit disbandment as a cutback management strategy, citing the disbandment of mounted patrol divisions in both Boston and San Diego in response to budget cuts. The NYPD has recently reassigned its entire 600-officer plain clothes anti-crime unit following nationwide pressure to reallocate police funds to various community services and the District of Columbia disbanded its equestrian unit in response to budget cuts. While such unit contraction alleviates associated financial costs, the police obviously lose any crime control functions of these units. This may have been the case in Newark, as units solely dedicated to proactive hot spots operations were disbanded in anticipation of the layoffs.

In considering the findings of our analysis, one may ask whether a continued commitment to place-based policing by NPD could have averted the crime increase. The NPD theoretically could have remained committed to place-based policing by tasking their regular patrol officers with maintaining presence in hot spots between calls for service. This is especially the case in consideration of the Koper curve, which finds that crime reductions can occur with police patrolling hot spots in intervals as short as 15 minutes (Koper, 1995; Telep *et al*., 2014). However, conducting hot spots policing in such a manner may be easier said than done in a contemporary police agency dealing with officer layoffs. Given the time intensive nature of the duties associated with standard patrol activities, such as writing paperwork, transporting arrestees, and responding to non-crime calls for service, patrol officers may not have sufficient discretionary time to conduct proactive hot spots policing activities. This may especially be the case in police agencies suffering from police officer layoffs. For such agencies, adhering to even a Koper curve model of hot spots policing may be challenging. A study by the Police Executive Research Forum (2010) conducted during the recession found that many departments reduced the scope of their community services due to budget cuts. This exemplifies how budget constraints can negatively impact police operations, which directly relates to the issue of officer layoffs.

Despite the implications of the findings, this study, like most research, suffers from certain limitations that should be mentioned. In particular, we note prior critiques of official sources of crime data, specifically the UCR, which we used in this study. The UCR is the primary source of crime data in the U.S., and provides a readily available data source to measure the impact of NPD’s officer layoffs. However, the UCR provides an incomplete picture of crime, as Part 1 crimes are the only incidents systematically reported to the FBI (Maxfield and Babbie, 2015). Given their reliance on official crime reports, UCR data may be influenced by police officer discretion (Warner and Pierce, 1993) and citizen distrust of the police (Kirk and Matsuda, 2011). Including additional outcome measures, such as calls for service, would have helped to overcome these limitations. Unfortunately, such data was not available to us. Furthermore, we were unable to directly measure the impact of the layoffs on NPD operations. While place-based policing programs were largely disbanded, as discussed, we were not able to measure the level to which this reduced actual police officer presence in crime hot spots. Unfortunately, the NPD lacked data technologies necessary to measure police presence at such a granular level, such as automated vehicle locators (Weisburd *et al.*, 2015) or GPS trackers in officer radios (Ariel & Partridge, 2017). Future research should incorporate such data when available. We lacked access to data necessary to see how other policies and practices of the NPD changed in response to the layoffs. Given the length of our study period, the analysis would have benefitted from controlling for such potential history effects. Lastly, the study would have benefitted from qualitative data from interviews with police officers and City of Newark officials to add context to the quantitative findings. The post hoc nature of the study prevented such a multi-methods approach.

In conclusion, we feel the current study positively contributes to the literature in a number of ways. First, we took advantage of naturally occurring phenomenon in order to conduct a natural experiment involving New Jersey's two largest police forces. We feel our methodology can inform the work of policing scholars interested in studying the effect of "treatment" conditions that cannot be readily manipulated by researchers. Second, we used the causal-process method (Collier *et al*., 2010) to contextualize the police layoffs in Newark. This allowed us to more readily explore the potential causal mechanisms of Newark's crime increase. Third, the current study, to our knowledge, is the first empirical test of the effect of police officer layoffs on crime. While typically a rare occurrence, the economic downtown in the mid 2000's necessitated that a number of police agencies enact layoffs in order to balance fiscal budgets. It also adds to the ongoing national dialogue on police reform which seeks to reduce police budgets and force size. We feel this provides an opportunity for a range of natural experiments to better understand the effect of the layoffs for various reasons (e.g. economic downturn, police reform).

For example, researchers can model the effects of each defunding step in anticipation of downsizing police departments in addition to the actual layoffs. Such an approach would better allow for the type of multi-methods approach mentioned in the prior paragraph. While our focus was on crime, other potential outcomes of interest include officer productivity, officer wellbeing, citizen fear of crime, and citizen perceptions of police legitimacy. We encourage social scientists to continue pursuing this line of research.

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APPENDIX

Table A1: Holm-Bonferroni corrections for DiD interaction terms.

Rank	***H(i)***	***p.***	**Corrected ∝**
T-1	Overall crime, immediate	<0.001	0.008*
T-1	Violent crime, immediate	<0.001	0.008*
T-1	Violent crime, gradual	<0.001	0.008*
4	Property crime, immediate	0.002	0.017*
5	Overall crime, gradual	0.005	0.025*
6	Property crime, gradual	0.156	0.050

*Statistically significant following Holm-Bonferroni correction.

The Holm-Bonferroni procedure expands upon the traditional Bonferroni method for controlling for multiple statistical comparisons. The Bonferroni method adjusts the critical *p.* value through the formula $\propto/m$ where ∝ is the original critical *p.* value (i.e. 0.05) and *m* is the total number of hypotheses tested. Any hypothesis with $p. > \propto/m$ is rejected though the traditional Bonferroni approach. Given the highly conservative nature of the Bonferroni method, it minimizes Type I errors while simultaneously increasing the risk for Type II errors (Olejnik *et al.*, 1997). The Holm-Bonferroni method maintains statistical power by establishing different ∝ values for the tested hypothesis depending on the observed level of significance. Obtained *p.* values are first ordered from smallest to largest, $p(_1),\ldots,p(_m)$, and matched with the corresponding hypotheses, $H(_1),\ldots,H(_m)$. This Holm-Bonferroni procedure rejects all hypotheses with $p_{(i)} > \propto/(m\text{-i}+1)$, protecting against Type I error while maintaining statistical power through the sequential increase of the significance criterion.

Table A2. Time Series GLS Regression Model results for standardized monthly overall crime rate with sociodemographic controls included. 2006 – 2015

	Immediate Effect				Gradual Effect			
Covariates	***b***	**Lower**	**Upper**	***p.***	***b***	**Lower**	**Upper**	***p.***
Newark x post layoffs	1.10	0.68	1.53	0.00	0.35	0.23	0.48	0.00
Newark	-0.55	-0.80	-0.29	0.00	-0.53	-0.77	-0.30	0.00
Post layoffs	-0.43	-0.92	0.06	0.09	0.15	-0.30	0.59	0.52
Lagged outcome	0.35	0.25	0.44	0.00	0.34	0.24	0.44	0.00
Days in month	0.24	0.16	0.32	0.00	0.23	0.15	0.31	0.00
Quarter								
2nd	0.70	0.51	0.89	0.00	0.70	0.51	0.89	0.00
3rd	0.62	0.40	0.85	0.00	0.63	0.41	0.86	0.00
4th	0.52	0.29	0.76	0.00	0.52	0.29	0.75	0.00
Sequential order	-0.01	-0.02	0.01	0.22	-0.01	-0.03	0.00	0.08
% Black population	-0.10	-0.23	0.04	0.16	-0.14	-0.27	-0.01	0.03
% White population	0.00	-0.10	0.09	0.97	-0.02	-0.11	0.07	0.70
% Hispanic population	-0.07	-0.37	0.23	0.65	-0.23	-0.53	0.07	0.13
% Unemployed	-0.10	-0.22	0.02	0.09	0.08	-0.05	0.21	0.21
Median income	-0.20	-0.33	-0.07	0.00	-0.06	-0.21	0.10	0.48
% Under poverty	0.05	-0.17	0.26	0.67	0.27	0.04	0.49	0.02
% College	0.02	-0.24	0.28	0.88	-0.11	-0.37	0.15	0.40
% Vacant properties	0.02	-0.08	0.11	0.75	0.02	-0.08	0.11	0.74
% Owner occupied	0.09	-0.16	0.35	0.47	-0.01	-0.27	0.25	0.95
Median home value	-0.30	-0.52	-0.08	0.01	-0.02	-0.25	0.21	0.86
Wald $(X)^2$	*601.00*				*633.78*			

Observations= 238
Time periods=119
Notes: All sociodemographic variables reflect the 5-year rolling averages, as measured in the US Census Bureau's American Community Survey. Each month was assigned the value of the calendar year it falls in. Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.

Table A3. Time Series GLS Regression Model results for standardized monthly violent crime rate with sociodemographic controls included. 2006 – 2015

	Immediate Effect				Gradual Effect			
Covariates	***b***	**Lower**	**Upper**	***p.***	***b***	**Lower**	**Upper**	***p.***
Newark x post layoffs	1.10	0.68	1.53	0.00	0.35	0.23	0.48	0.00
Newark	-0.55	-0.80	-0.29	0.00	-0.53	-0.77	-0.30	0.00
Post layoffs	-0.43	-0.92	0.06	0.09	0.15	-0.30	0.59	0.52
Lagged outcome	0.35	0.25	0.44	0.00	0.34	0.24	0.44	0.00
Days in month	0.24	0.16	0.32	0.00	0.23	0.15	0.31	0.00
Quarter								
2nd	0.70	0.51	0.89	0.00	0.70	0.51	0.89	0.00
3rd	0.62	0.40	0.85	0.00	0.63	0.41	0.86	0.00
4th	0.52	0.29	0.76	0.00	0.52	0.29	0.75	0.00
Sequential order	-0.01	-0.02	0.01	0.22	-0.01	-0.03	0.00	0.08
% Black population	-0.10	-0.23	0.04	0.16	-0.14	-0.27	-0.01	0.03
% White population	0.00	-0.10	0.09	0.97	-0.02	-0.11	0.07	0.70
% Hispanic population	-0.07	-0.37	0.23	0.65	-0.23	-0.53	0.07	0.13
% Unemployed	-0.10	-0.22	0.02	0.09	0.08	-0.05	0.21	0.21
Median income	-0.20	-0.33	-0.07	0.00	-0.06	-0.21	0.10	0.48
% Under poverty	0.05	-0.17	0.26	0.67	0.27	0.04	0.49	0.02
% College	0.02	-0.24	0.28	0.88	-0.11	-0.37	0.15	0.40
% Vacant properties	0.02	-0.08	0.11	0.75	0.02	-0.08	0.11	0.74
% Owner occupied	0.09	-0.16	0.35	0.47	-0.01	-0.27	0.25	0.95
Median home value	-0.30	-0.52	-0.08	0.01	-0.02	-0.25	0.21	0.86
Wald $(X)^2$	*601.00*				*633.78*			

Observations= 238
Time periods=119
Notes: Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.

Table A4. Time Series GLS Regression Model results for standardized monthly property crime rate with sociodemographic controls included. 2006 – 2015

	Immediate Effect				Gradual Effect			
Covariates	***b***	**Lower**	**Upper**	***p.***	***b***	**Lower**	**Upper**	***p.***
Newark x post layoffs	0.32	0.03	0.60	0.03	0.05	-0.03	0.14	0.23
Newark	-0.17	-0.34	0.01	0.06	-0.09	-0.25	0.08	0.30
Post layoffs	-0.05	-0.38	0.28	0.76	0.07	-0.25	0.40	0.66
Lagged outcome	0.49	0.40	0.59	0.00	0.51	0.42	0.61	0.00
Days in month	0.40	0.33	0.47	0.00	0.40	0.34	0.47	0.00
Quarter								
2nd	0.39	0.24	0.53	0.00	0.38	0.24	0.53	0.00
3rd	0.36	0.17	0.54	0.00	0.33	0.15	0.52	0.00
4th	0.01	-0.17	0.19	0.92	-0.01	-0.19	0.17	0.93
Sequential order	0.00	-0.02	0.01	0.44	0.00	-0.01	0.01	0.53
% Black population	-0.01	-0.11	0.08	0.78	-0.03	-0.12	0.06	0.49
% White population	-0.08	-0.14	-0.01	0.02	-0.07	-0.14	-0.01	0.03
% Hispanic population	-0.19	-0.40	0.02	0.08	-0.21	-0.42	0.00	0.05
% Unemployed	0.05	-0.04	0.13	0.27	0.07	-0.02	0.16	0.11
Median income	0.01	-0.08	0.11	0.76	0.01	-0.11	0.12	0.89
% Under poverty	0.10	-0.05	0.26	0.20	0.13	-0.04	0.30	0.12
% College	-0.12	-0.30	0.06	0.20	-0.13	-0.32	0.05	0.15
% Vacant properties	-0.06	-0.13	0.01	0.11	-0.05	-0.12	0.02	0.18
% Owner occupied	0.04	-0.15	0.22	0.70	0.03	-0.15	0.22	0.72
Median home value	0.05	-0.09	0.20	0.46	0.09	-0.07	0.25	0.26
Wald $(X)^2$	*1424.20*				*1390.93*			

Observations= 238
Time periods=119
Notes: All sociodemographic variables reflect the 5-year rolling averages, as measured in the US Census Bureau's American Community Survey. Each month was assigned the value of the calendar year it falls in. Models were estimated assuming panel-specific autocorrelation and heteroscedasticity through the corr(psar1) option in Stata.