More COPS, Less Crime*

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February 25, 2018

Abstract

I exploit a natural experiment to estimate the causal effect of police on crime. The American Recovery and Reinvestment Act increased funding for the Community Oriented Policing Services (COPS) hiring grant program from less than $20 million over 2005-2008 to $1 billion in 2009. Hiring grants distributed in 2009 were allocated according to an application score cutoff rule, and I leverage quasi-random variation in grant receipt by comparing the change over time in police and crimes for cities above and below the threshold in a difference in differences framework. Relative to low-scoring cities, those above the cutoff experience increases in police of about 3.2% and declines in victimization cost-weighted crime of about 3.5% following the distribution of hiring grants. The effects are driven by large and statistically significant effects of police on robbery, larceny, and auto theft, with suggestive evidence that police reduce murders as well. Crime reductions associated with additional police were more pronounced in areas most affected by the Great Recession. The results highlight that fiscal support to local governments for crime prevention may offer large returns, especially during bad macroeconomic times.

JEL Classification: K42, H76.
Keywords: Police, crime, deterrence.

*I am grateful to Ilyana Kuziemko and Alex Mas, who provided considerable advice and encouragement on this project. I thank Jessica Brown, John Donohue, and Felipe Goncalves, who read earlier drafts and offered valuable insights and criticisms. Amanda Agan, Leah Platt Boustan, Mingyu Chen, David Cho, Janet Currie, Will Dobbie, Hank Farber, Paul Heaton, Andrew Langan, David Lee, Chris Neilson, David Price, Mica Sviatschi, Danny Yagan, Owen Zidar, and seminar participants at Princeton University and the 2018 ASSA/Econometric Society Annual Meetings provided helpful comments. I also benefitted from discussions with John Kim and Matthew Scheider at the COPS Office. I acknowledge financial support from a Princeton University Graduate Fellowship and the Fellowship of Woodrow Wilson Scholars. Any errors are my own.

1 Introduction

Provision of public safety is a central responsibility of local governments. Crime victimization is estimated to cost Americans over $200 billion per year and public spending on police protection exceeds $100 billion annually (Chalfin 2016). Consistent with canonical models of the economics of crime such as Becker (1968), which predict that police presence reduces crime by deterring potential offenders, hiring police is the main policy instrument used by local governments for crime prevention. The causal effect of expanding police forces on crime rates is, therefore, a parameter of substantial interest for policymakers. In practice, estimating this effect is made difficult by the fact that police hiring decisions are endogenous to local crime conditions, which introduces simultaneity bias in OLS estimates (Klick and Tabarrok 2010).

In this paper, I exploit a unique natural experiment generated by the distribution of grants to hire over 7,000 police officers to estimate the causal effect of police on crime. In February 2009, President Obama signed into law the American Recovery and Reinvestment Act (ARRA), which provided for over $490 billion in stimulus spending between 2009 and 2011. ARRA allocated about $2 billion to the Department of Justice (DOJ), a large share of which was used to finance a reinvigoration of the DOJ's police hiring grant program. The Community Oriented Policing Services (COPS) hiring program, which covers the salary cost of new police hires for local law enforcement agencies, was a cornerstone of President Clinton's Violent Crime Control and Law Enforcement Act of 1994. Between 1995 and 2005, the COPS hiring program spent almost $5 billion to help local police departments hire about 64,000 officers (Evans and Owens 2007). Allocations for the program fell from over $1 billion per year in the late 1990's to almost zero in the years 2005–2008. The injection of Recovery Act funding restored the COPS hiring program budget to $1 billion in fiscal year (FY) 2009.

Grants issued in 2009 were allocated according to an application process. Law enforcement agencies applied for funds and the COPS office scored the applications and determined grant amounts. The funding rules generated application score thresholds, above which cities received hiring grants and below which cities did not. I compare the change over time in police and crime for municipalities whose application scores were above and below the threshold. Specifically, I estimate difference in differences models with city and year fixed effects and city-specific linear trends. Using a 2004-2014 panel of 4,327 cities and towns, I show that treatment and control cities follow similar trends in police and crime prior to the program. Beginning in 2009, however, police levels increase while crime declines in cities with application scores above the threshold. My baseline difference in differences estimates indicate that police rates increase by 3.2% while victimization cost-weighted crime rates decrease by 3.5% following the distribution of the 2009 hiring grants. The corresponding IV estimate, obtained by instrumenting the police rate with an interaction between a treatment indicator and a post-program indicator, suggests that each additional sworn officer reduces victimization costs by about $352,000. The implied elasticity of cost-weighted crime with respect to police is -1.17, which is large relative to most existing estimates in the literature.

Though noisier, the results are nearly identical when using only cities with application scores very close to the cutoff, for whom the assumption that grants are randomly assigned is most plausible. Further, the first stage and reduced form estimates are largest when using the true score thresholds, rather than placebo thresholds, to identify the treatment and control groups. This results suggests that crossing the threshold, and thereby receiving hiring grant funding, rather than differences in application scores per se, explains the post-program divergence for the treatment and control groups. I also demonstrate that neither differential exposure to the Great Recession nor different levels of other ARRA funding can account for the results.

Consistent with the existing literature, I find that violent crime is more responsive than property crime to increases in police force size (Chalfin and McCrary 2018). IV estimates imply crime-police elasticities of about -1.3 for violent crime -0.8 for property crime. Declines in robbery and auto theft are particularly pronounced, with the point estimates suggesting that an additional police officer prevents 1.9 robberies and 5.1 auto thefts. I also find evidence that police reduce murders. The coefficient is imprecisely estimated but significant at the 10% level, with the point estimate suggesting that each officer prevents 0.11 murders and thereby that one life can be saved by hiring about 9.5 additional police officers.

Using a subsample of cities that report arrests to the FBI, I find little evidence that arrests increased with the program-induced police force expansions. The lack of arrest rate increases suggests that a deterrence, rather than incapacitation, mechanism underlies the crime reductions. Additionally, by comparing changes in crime for non-applicant jurisdictions near treatment and control cities, I find no evidence for geographic spillovers or displacement associated with the local police increases.

An analysis of treatment effect heterogeneity reveals that the impact of police on crime is largest among cities more exposed to poor macroeconomic conditions during the Great Recession. The elasticity of victimization costs with respect to police is about -0.7 for cities with the smallest 2007-2009 unemployment increases but about -1.4 for cities with the largest 2007-2009 unemployment increases. This pattern of results is consistent with the hypothesis that fiscal distress caused cities to employ fewer than the optimal number of officers, which may explain the large estimated treatment effects.

A back of the envelope calculation suggests that the ARRA hiring program added about 9,450 officer-years at a total cost of about \$1.75B, suggesting that the hiring grants are cost-effective if the annual social benefit attributable to a marginal police officer exceeds \$185,000. My baseline estimate is about \$350,000, suggesting a favorable benefit-cost ratio for program spending. The program fails a cost-benefit test under more conservative assumptions about the crime reduction benefit, however.

The rest of the paper proceeds as follows. Section 2 provides a brief literature review and institutional background on the COPS hiring program. I describe the data in Section 3 and explain the empirical strategy in Section 4. Results are presented in Section 5. In Section 6, I conduct a brief cost-benefit analysis of the hiring program. Section 7 concludes.

2 Background

2.1 Research on Police and Crime

Beginning with Levitt (1997), researchers have tried to overcome endogeneity issues in estimating the police-crime relationship by relying on quasi-experimental research designs. Two strands of research comprise the bulk of the quasi-experimental literature. The first uses city level panel data and instrumental variables that predict variation in police levels at the city-year level. Some examples include Levitt (1997), who relies on the timing of mayoral election years, and Evans and Owens (2007), who rely on COPS hiring grants during the 1990's as instrumental variables. The second exploits sharp microtime series variation within cities, such as increased police deployments following terror attacks, notably Di Tella and Schargrodsky (2004), Klick and Tabarrok (2005), and Draca, Machin and Witt (2011).[1]

Quasi-experimental studies typically document that police reduce crime, although estimated magnitudes vary widely. Further, the literature is not without potential flaws. Binary instruments, such as election years, discard much of the variation in police rates and are often weak by modern standards. Studies instrumenting police levels with federal grants (Zhao, Scheider and Thurman 2002, Evans and Owens 2007, Worrall and Kovandzic 2010) typically lack a clear control group and suffer from the possibility that such grants are targeted where they are most needed or most likely to succeed, either of which would violate the exclusion restriction. My paper contributes to this strand of literature by employing a cleaner identification strategy as well as studying a larger fraction of U.S. cities and a different time period.

Papers using within-city variation in police deployments provide convincing evidence that police deter property crimes. However, these studies typically estimate effects specific to single jurisdictions, raising questions of external validity (Klick and Tabarrok 2010). Further, the deployment increases under study typically do not approximate increases in force size or policing intensity that are realistic for long run policy decisions (Blanes and Mastrubuoni 2017). Finally, scholars have documented that neighborhood crime declines caused by temporary increased policing may be offset by crime displacement (Blattman, Green, Ortega, and Tobon 2017; Ho, Donohue, and Leahy 2014).

2.2 History of COPS Hiring Program

In September 1994, President Bill Clinton signed into law the Violent Crime Control and Law Enforcement Act, the largest federal crime bill to date. The bill authorized $8.8B in spending on grants for state and local law enforcement agencies between 1994 and 2000 and established the office of Community Oriented Policing Services (COPS) to administer the new grant programs. A key tenet of the crime bill was the creation of the COPS Universal Hiring Program (CHP), which covered 75% of the cost of new police hires for grant recipients. The stated goal of the hiring grant

[1] Another noteworthy study is the recent paper by Chalfin and McCrary (2018). The authors posit that OLS estimates are biased by measurement error in police levels rather than simultaneity bias and estimate crime-police elasticities corrected for measurement error.

program was to put 100,000 new police officers on the street.[2]

CHP funding exceeded $1B in fiscal years 1995–1999, but appropriations fell considerably in the early 2000's. Less than $200M was allocated for the hiring program in 2003–2004, and less $20M was appropriated in each year 2005–2008 (James 2013). The program was defunded due both to the retreat of crime as a central policy issue and to questions over the program's effectiveness (Evans and Owens 2007). Reports authored by the Heritage Foundation in 2001 and 2006, for example, argued that hiring grants did not reduce crime because grants were used to supplant other expenditures rather than to expand police forces.

Funding for the hiring program saw a dramatic resurgence in 2009 with President Obama's signing of the American Recovery and Reinvestment Act (ARRA), which provided $2B in new funds to the Department of Justice, with $1B earmarked specifically for the COPS hiring program. The funding was seen both as a precautionary measure for keeping crime rates low in the face of a worsening economy and as a means to create or preserve as many as 5,000 police officer jobs across the country. Following the injection of ARRA funds in FY2009, congressional appropriations exceeded $140M annually between 2010 and 2013, a large increase from 2004–2008 funding levels (James 2013). Hiring grants awarded in FY's 2009–2011 were also more generous than in previous years, covering 100%, rather than 75%, of entry-level salary and fringe benefits for hires or rehires for three years.[3]

2.3 Details of COPS 2.0

ARRA hiring grants were distributed based on an open solicitation application process. Any state, local, or tribal agency with primary law enforcement responsibility was eligible to apply for funding. Applicant agencies provided an array of statistical information, such as indicators of fiscal health, local unemployment and poverty rates, and local crime rates. Applicants also provided answers to several open-ended essay style questions detailing their usage of community policing strategies and requested a specific number of officers for which they required funding.[4]

The COPS office assigned each applicant a *fiscal need* score and a *crime* score. Program doc-

[2] See `http://www.justice.gov/archive/opa/pr/Pre_96/October94/590.txt.html`.
[3] The program reverted to covering 75% of salary and benefits beginning in 2012.
[4] See `http://www.cops.usdoj.gov/pdf/CHP/e05105273-CHP.pdf`.

umentation indicates that these scores were generated by ranking applicants on each application question then weighting each question to obtain an overall ranking. I was unable to replicate the score generation process due to my inability to observe a large share of the application materials.[5] The two component scores were added to create an aggregate application score. Table A-2 shows the relationship between city characteristics and application scores in 2009. Unsurprisingly, higher-scoring cities are larger, poorer, and have significantly higher crime rates.

Applications were funded in descending order of the application score until funding was exhausted and two distributional rules were met. The COPS office was required to allocate at least 1.5% of total CHP funding to each state and was required to distribute at least 50% of all funding to jurisdictions with populations exceeding 150,000. These distributional considerations generated different score cutoffs depending on state and size category. For applicants in states that initially received more than $5 million in total funding, the cutoff was 65.75 for small agencies (population under 150,000) and 68.75 for large agencies (population over 150,000). For applicants in states that would not meet the required 1.5% using these cutoffs, the relevant threshold is the application score of the last agency funded in that state (Cook, Kapustin, Ludwig and Miller 2017).

A similar application process has been repeated each year since 2009. In this paper, I focus on the 2009 application round because of its magnitude. Total program spending was more than three times higher in 2009 than in any year 2010–2014. 46% of all funded applications and 49% of all officers granted over the 2009–2014 period occurred in 2009. Further, focusing on the ARRA grant round allows for a very simple and transparent difference in differences approach with clearly defined treatment and control groups. Studying additional grant rounds, and in particular dealing rigorously with repeat applicants, complicates the empirical analysis significantly but yields minimal payoff.[6]

[5] Municipal level employment and financial data, for example, are publicly available on an annual basis for only a small fraction of cities.

[6] In an earlier version of this paper, available at http://www.princeton.edu/~smello/papers/copsjan2017.pdf, I estimated effects for all grant rounds jointly using stacked panels, following the approach in Cellini, Ferreira and Rothstein (2010). I found crime-police elasticities of -1.36 for violent crime and -0.84 for property crime, which are nearly identical to those obtained here.

2.4 Research on the COPS Program

Several existing papers have studied the first iteration of the COPS hiring program during the 1990's. The most noteworthy paper on the topic is the careful and well-regarded study by Evans and Owens (2007). Papers by the Zhao, Scheider and Thurman (2002) and Worrall and Kovandzic (2010) also study the original COPS program and employ similar research designs.

In the first part of the paper, Evans and Owens (2007) examine whether COPS grants increased police forces. Using a twelve-year (1990-2001) panel of 2074 cities, they regress sworn officers per 10,000 residents on the lagged number of officers granted by the COPS office per 10,000 residents in panel data models, finding that local police forces increased by 0.7 sworn officers for each granted officer. In the second part of the paper, the authors instrument the police rate with the lagged grant rate in 2SLS regressions where the crime rate is the outcome of interest, finding that increases in police are associated with statistically significant declines in robberies, assaults, burglaries, and auto thefts.

Relative to Evans and Owens (2007), my contribution is as follows. First, I improve on their identification strategy. The application-based grant allocations allow for the use of rejected applicants as a control group. I argue that the set of applicants denied funding is a better control group than the broader set of cities who report crimes to the FBI. I also use graphical analysis to check parallel trends assumptions and show results using only a subsample for whom grant offers are plausibly randomly assigned. Second, I study a wider range of cities. Much of the existing research on police effectiveness has focused on large cities, while Evans and Owens (2007) study about 2,100 cities with populations greater than 10,000. I study all applicant cities and towns with populations exceeding 1,000, which results in greater coverage of U.S. municipalities. And third, I study a different era of the program. Evans and Owens (2007) examine the introduction of the COPS program in the mid 1990's, when crime rates were high and crime in general was a central policy issue. The stated goal of the program was to induce large increases in police forces across the country. My focus is the reinvigoration of the program following the injection of ARRA funding. The goal of COPS 2.0 was to preserve law enforcement jobs and prevent a rise in crime due to worsening economic conditions. The poor fiscal health of many cities during this period, combined with a lower program budget than during the original

COPS period, generated a highly competitive application process. The different context, various program changes, and the availability of a cleaner identification strategy warrant a new evaluation. Further, this paper contributes to a broader literature on the effectiveness of ARRA spending and offers insights on the relative benefits of including law enforcement funding in stimulus packages.

Two additional studies authored concurrently with mine bear mentioning here. Weisburst (2017) uses COPS funding over the period 1994–2014 as an instrument to estimate the effect of police on crime using a panel of cities. Although the author does not explicitly rely on rejected applicants as a control group, she does control for the presence of grant applications at the city-year level. Results presented in Weisburst (2017) are very similar to mine. She finds that hiring grants increase police forces by about 0.65 and estimates crime-police elasticities of -1.28 for violent crime and -0.73 for property crime.

The COPS office also funded a study of the 2009 hiring grant program, authored by Cook et al. (2017). This paper implements a regression discontinuity design to estimate the effect of grant receipt in 2009 on police forces and crime rates in 2009–2012. The authors find that at the cutoff, cities experience increases in police per capita of 2.1% and declines in violent (property) crimes per capita of 9.2% (3.6%) in 2010 relative to 2008, with implied crime-police elasticities of -4.4 and -1.7. The estimates are relatively imprecise, however.

3 Data

3.1 Grants Data

The COPS office provided information on applications and grants awarded for 2009-2014 in response to a Freedom of Information Act (FOIA) request. For each program year and applicant law enforcement agency, the data include the corresponding application score and information on the grant received in terms of both the number of officers funded and dollar value. Agencies are identified in the applications data by an agency name and a 7-character ORI (originating agency) code, which is also used to identify agencies in the FBI datasets discussed below.[7]

Raw application scores in 2009 ranged from 15-100 with a mean of about 50. I compute the score

[7] A number of ORI codes were present in the applications data but not in the FBI data. Where possible, I corrected the codes by matching on name with the FBI datasets. 184 of the 4,327 agencies in the main sample (4.25%) are assigned a different ORI code from that reported in the applications data. See the Appendix for more detail.

thresholds following Cook et al. (2017) as described above in Section 2.2. I then standardize both the application scores and cutoffs so that the score relative to the threshold is measured in standard deviations. Figure 2 displays the distribution of application scores relative to the cutoff as well as the fraction of applicants that received hiring grants in each score bin of width 0.25. No agency with a score below the threshold was funded, while 99% of agencies with scores above received hiring grants. The RD estimate of the effect of crossing the threshold on funding probability using the Imbens and Kalyanaraman (2012) [IK] optimal bandwidth and a triangular kernel yields a coefficient (standard error) of 0.948 (0.019).

3.2 FBI Data

Data on police employees and reported crimes are from the FBI's Uniform Crime Reporting Data System (UCR). I obtained the agency-level *Law Enforcement Officers Killed in Action* (LEOKA) files for 2002–2014 from the National Archive of Criminal Justice Data (NACJD) website. The data files report each agency's number of sworn officers and civilian employees as of October for each year. Criminal offenses known to police are reported in the UCR *Return A* file, which provides monthly counts of index I crimes for all reporting agencies. Index I crimes include the core violent (murder, rape, robbery, aggravated assault) and property (burglary, larceny, motor vehicle theft) crimes. Michael Maltz, a criminologist at the Criminal Justice Research Center at the Ohio State University, maintains an updated version of the Return A file, and the COPS office provided his version of the data for this study.[8] Because police officers counts are reported annually, and many agencies report their full-year crime counts once rather than report each month individually, I aggregate the crime counts to the agency-year level. For city population, I use a smoothed version of the measure reported in the UCR files.[9]

Prior research has noted the existence of record errors in the FBI datasets (Evans and Owens 2007, Chalfin and McCrary 2018, Maltz and Weiss 2006).[10] As such, the data require thorough

[8] Maltz's data is identical to the publicly available version on the NACJD website except that he (1) has identified reasons for missing values and (2) has identified certain zeroes or extreme values as outliers. My own examination of the data revealed that many record errors remained in his version and I further cleaned the data as described in the Appendix.

[9] Chalfin and McCrary (2018) note that the UCR population measure tends to jump discontinuously around census years. For this reason, I follow their procedure and smooth the population measure using local linear regression. For more detail, see the Online Data Appendix.

[10] For example, reported violent crimes in Boulder, CO for the period 2007–2011 are 219, 202, *952*, 210, 246. Police

cleaning before use. I implement a regression-based approach similar to that used in Evans and Owens (2007) to identify record errors and extreme outliers. The procedure is described in more detail in the Appendix. Values identified as errors are recoded to missing, then all missing values due either to outlier status or non-reporting are imputed using backwards/forwards filling and linear interpolation.[11] I cleaned the crime data for 2002–2014, but only use years 2004–2014 in the analysis because a large fraction (over 17%) of the crime data was imputed for 2002-2003 via backfilling. In the main analysis sample, 1.5% of police observations and 8.8% of crime observations are imputed.[12]

Empirical studies of public safety typically focus on crimes per 10,000 residents as the outcome of interest, showing results separately for each type of crime. To simplify the presentation of results, I focus primarily on a single index outcome which I term the cost-weighted crime rate or crime costs per capita. One could focus on the total crime rate, but this measure heavily weights property crimes relative to violent crimes. While property crimes are nearly six times more common than violent crimes, the average violent crime is about seventeen times more severe based on existing victimization cost estimates (Cohen and Piquero 2009). I follow Autor, Palmer and Pathak (2017) and compute the cost-weighted crime for city i in year t as

$$y_{it} = \$67{,}794 \times \text{Violent Crimes}_{it} + \$4{,}064 \times \text{Property Crimes}_{it}$$

where \$67,794 and \$4,064 are the direct costs of the average violent and property crimes based on the estimates in Cohen and Piquero (2009). Note that one could instead compute this measure as the cost-weighted sum of each individual crime type. However, such a measure would weight murder 35 times more heavily than all other crime types, despite the fact that murder is the crime type with the greatest year-to-year variability (McCrary 2002). Weighting the violent and property crime counts by the category average costs compromises by weighting up violent crimes but not excessively

in Lansford, PA for 2006–2010 are 4, 3, *40*, 9, 9.

[11] For example, if a city's first year of nonmissing violent crime is 2005, the 2005 value is imputed for the years 2002–2004.

[12] Figure A-2 illustrates the relationship between treatment status and imputation. Treatment group cities are slightly less likely to have imputed police values prior to 2006 and after 2012. There is no discernible relationship between crime imputation and treatment status. Table A-6 shows that results are nearly identical when replacing imputed values to missing.

weighting the highest variance crime types.

3.3 Other Data Sources

Standard demographic and economic information are not available at the city-level on an annual basis. I obtained demographic information from two sources. To examine city-level characteristics at the time of the program, I use demographic information, as well as employment rates and median family income, from the 2009 American Community Survey collected at the FIPS place code level. To use as controls in the regressions, I obtained data at the county-year level from several sources. I computed percent black, percent Hispanic, and percent young male (age 15-29) from the intercensal county population estimates maintained by the SEER program at the National Institutes of Health. County-level income per capita was obtained from the Bureau of Economic Analysis and county-level unemployment rates were obtained from the Bureau of Labor Statistics Local Area Unemployment Statistics data files. I use county-level percent black, percent Hispanic, percent young male, log per capita income, and unemployment rates as controls in the crime regressions.

3.4 Sample Construction

The main analysis focuses on municipal police agencies applying for COPS hiring program funding in 2009. There are 5,314 such police departments.[13] I drop 237 agencies that never report crimes to the FBI and drop an additional 229 agencies with populations below 1,000 because per-capita measures are much noisier, and often orders of magnitude higher, below this threshold. Among the remaining 4,848 departments, I require that an agency report police and crimes at least once prior to 2008 and after 2010, report positive police at least once and positive crimes at least once, and report police and crimes each for at least four years. The analysis sample is comprised of 4,327 agencies, which is 81% of all applicant municipal police departments and 89% of applicant municipal police departments that ever report to the UCR and have populations above 1,000. The most binding sample restriction was crime reporting pre and post 2009. Figure A-1 shows the relationship between the application score and inclusion in the sample. Comfortingly, sample inclusion is not discontinuous at the funding cutoff.

[13] Municipal police comprise 74% of all applicants. The remainder were sheriff's and regional police departments (18%), school police departments (5%), tribal agencies (1.4%), and special agencies(1.3%).

3.5 Characteristics of Analysis Sample

The sample includes 4,327 police departments, 18% (791) of which scored above the threshold in 2009. The total population served by such departments is 142.6 million as of 2008, about 47% of total U.S. population in that year. The sample includes at least one department from all 50 states and the District of Columbia. 1,588 counties (53% of all U.S. counties) are represented. Table A-1 provides examples of cities in the sample at quantiles of the size distribution.

Characteristics of the sample, measured at the time of the program, are presented in Table 1. The average city has about 30,000 residents (median $\approx$ 10,000), an unemployment rate of nearly 7.5%, and median family income of $50,000. Cities typically employ about 23 sworn officers per 10,000 residents and face cost-weighted crimes per capita of about $556. Cities above and below the application score threshold differ on most observable characteristics. High-scoring cities have larger populations, higher unemployment rates, lower family incomes, and larger nonwhite populations. High scoring cities employ three additional officers per 10,000. Violent and property crime rates are about 60% larger in the average high-scoring city.

Over 98% of cities above the threshold were offered hiring grants. The average grant funded 1.7 officers per 10,000 residents, about 6% of current force size in a typical winning department, and carried a dollar value of $29 per city resident, or about $67,000 per funded officer per year.

Figure 3 illustrates the relationship between the application score and select city characteristics at the time of the program. Consistent with the summary statistics, city size, police rates, crime rates, and unemployment rates all increase with the application score. Further, all four measures appear to increase discretely at the threshold, with RD estimates statistically significant for population and the unemployment rate. I return to this point in Section 4.2.

Figure 4 illustrates trends in police and crime for cities above and below the threshold. Specifically, I plot average police per 10,000 residents and crime costs per capita for the two groups in each year. The above-cutoff (treatment group) means are normalized to be equal to the below-cutoff (control group) means in 2008 to adjust for level differences. The figure foreshadows the main results. Police rates (Panel A) in treatment and control cities follow similar trends prior to the program but diverge sharply beginning in 2009, with police rates increasing slightly in high-scoring cities but declining sharply in low-scoring ones. A similar, but inverse, divergence occurs in crime costs per capita (Panel B), with treatment cities experiencing reductions in crime relative to the control group beginning in 2009.

4 Empirical Strategy

4.1 Difference in Differences

I leverage the natural experiment created by the 2009 hiring grant application process using a difference in differences design. The spirit of the analysis is to compare the change over time in police and crime for cities with application scores above the funding cutoff (treatment group) and cities below the funding cutoff (control group). Under a set of identifying assumptions discussed below, differential changes in crime in treatment and control cities can be attributed to differential changes in police, and the ratio of Δcrime and Δpolice is an estimate of the causal effect of police on crime.

Specifically, I estimate the following first stage equation:

$$Police_{it} = \beta^{FS} High_i \times Post_t + \phi_i + \kappa_t + \lambda(t)_i + \epsilon_{it} \tag{1}$$

$Police_{it}$ is sworn officers per 10,000 residents in city i in year t. $High_i$ indicates that city i's 2009 application score exceeded the threshold and $Post_t$ is an indicator for $t \geq 2009$.[14] ϕ_i is a city fixed effect, which absorbs level differences across cities. κ_t is a year fixed effect and $\lambda(t)_i$ is a city-specific linear trend. I include city-specific trends to account for heterogeneity in pre-program trends, which vary widely given the distribution of city sizes in the sample. In the estimation, I also allow κ_t to vary across city size groups, so that κ_t adjusts for common deviations from trend among cities of similar size.[15] Standard errors are clustered at the city-level. β is a difference in differences estimate capturing the extent to which changes in police from pre to post 2009 differ for treatment and control cities. We can also think of β is also an intent-to-treat estimate of the effect of a 2009 hiring grant offer on police force size.

[14] I consider 2009 a post-program year because hiring grant funding was distributed in the summer of 2009 and police is measured in October.

[15] The size groups are 1,000-2,500; 2,500-5,000; 5,000-10,000; 10,000-15,000; 15,000-25,000; 25,000-50,000; 50,000-100,000; 100,000-250,000; >250,000. Cities appearing in multiple groups are placed in the group they appear most often.

I then estimate the corresponding reduced form equation,

$$Crime_{it} = \beta^{RF} High_i \times Post_t + \phi_i + \kappa_t + \lambda(t)_i + \epsilon_{it} \tag{2}$$

where $Crime_{it}$ is crime cost per capita in city i in year t. β captures the extent to which treatment and control cities differ in their crime rates in the post period relative to the pre period. The Wald IV estimate of the effect of police on crime is the ratio $\frac{\beta^{RF}}{\beta^{FS}}$. In practice, I obtain IV estimates via 2SLS, estimating the equation

$$Crime_{it} = \beta Police_{it} + \phi_i + \kappa_t + \lambda(t)_i + \epsilon_{it} \tag{3}$$

using $High \times Post$ as an instrumental variable for $Police$.

To be clear, the identifying assumption is not random assignment of grant offers. Rather, the assumption is that police and crime would have trended similarly in grant-winning and grant-losing cities in the absence of the program (Yagan 2015). This assumption could be violated in one of two important ways. First, treatment and control cities could be trending differently prior to the program. I test for this possibility directly by estimating a fully dynamic specification of (1)-(2),

$$Y_{it} = \theta_t High_i \times \kappa_t + \phi_i + \kappa_t + \lambda(t)_i + \epsilon_{it} \tag{4}$$

Here, θ_t measures the treatment-control difference in each year. If trends in high-scoring and low-scoring cities diverge prior to the program, the θ_t's for $t < 2009$ will differ from zero.

The second threat to identification is that treatment status could be correlated with other shocks occurring exactly at the time of the program. One cause for concern is the fact that the program's timing coincided with the ramp up of the Great Recession. The nationwide unemployment rate increased from 5% in January 2008 to a peak of 10% in October 2009 and remained above 9% through most of 2010. Standard models of the economics of crime (e.g. Becker 1968) predict that crime rates increase as economic conditions worsen, a relationship verified empirically by Raphael and Winter-Ember (2001). The identifying assumption may be violated if high-scoring cities experience different macroeconomic shocks than low-scoring ones.[16] In the main specification, I control for county-level

[16] One should note that local fiscal conditions played a role in determining grant allocations, as discussed in Section

unemployment rates to partially address this concern. As a robustness check, I also present results identified only by comparing cities with similar unemployment rate shocks. Specifically, I bin cities into ten deciles of the change in the unemployment rate from 2005–2007 to 2008–2011 and estimate regressions with recession decile $\times$ year fixed effects, which has almost no impact on the results.

A second concern is that the program scale-up occurred as part of the larger American Recovery and Reinvestment Act, a broad-based stimulus package which allocated over $490 billion between 2009 and 2011 for an array of programs to support the struggling economy.[17] Correlation between treatment status and ARRA funding could violate the identifying assumption. I address this potential issue in two ways. I collect data on grants and contracts issued as part of ARRA from the Federal Procurement Data System (FPDS) and aggregate local ARRA spending to the ZIP code-year level. I match these data to the subset of cities in my data that I could match to ZIP codes and control for local ARRA spending in the regressions. I also show that although there is no difference in local ARRA funding among cities within a narrow bandwidth of the threshold, the main results hold when considering only such cities.

4.2 Why Not Regression Discontinuity?

A regression discontinuity (RD) design would seem appropriate given the application score-based funding allocations. One could look for a discontinuity in the pre-post change in police (first stage) and crimes (reduced form) at the score threshold and obtain a causal estimate of the effect of police on crime by dividing the reduced form by the first stage.

In practice, the RD design is not well suited to this context for several reasons. First, a key identifying assumption of the RD design is violated. As discussed in Section 3.5 and illustrated in Figure 3, cities just above the threshold differ from those just below on several dimensions at the time of application. In particular, city size, police per capita, cost-weighted crime per capita, and the local unemployment rate all appear to increase discontinuously at the application score threshold, with the RD estimates statistically significant for population and unemployment. The difference

2, so we might expect high-scoring cities to be more severely affected by the recession. Given the findings in the literature, this should bias the reduced form relationship between grant receipt and crime rates towards zero.

[17] See https://www.cbo.gov/publication/42682.

in differences approach, which includes city fixed effects, relies only on a parallel trends assumption.

Second, an RD design introduces concerns over statistical power. Power depends largely on sample size and the variability of the outcome of interest – one can reliably detect smaller effect sizes as N grows and as the variance of y shrinks. Relative to most applications of the RD design, the available sample for studying the COPS program is quite small. My sample includes 4,327 applicant cities, with only about 2,100 within one standard deviation of the cutoff and only about 1,100 within 0.5 standard deviations. Further, the most natural specification would use changes in police and crime rates as the outcomes of interest, both of which exhibit significant variability relative to effect sizes one would expect. For example, my difference in differences estimate of the effect of grant receipt on cost-weighted crimes per capita is -25, while the standard deviation of changes in cost-weighted crimes per capita is 211.

In Appendix B and Table A-3, I formalize this concern with explicit power calculations. The calculations indicate that even under very generous assumptions, an RD is not sufficiently powered for an analysis of violent crimes or cost-weighted crimes based on the variability in the outcome and small sample sizes. The RD is barely powered to examine police and property crime outcomes, but the closeness suggests that under more realistic estimation approaches (for example, allowing functional forms to vary on either side of the cutoff), the design lacks sufficient statistical power for even these less noisy outcomes.

I do, however, use insights from the RD literature to probe the robustness of my difference in differences estimates. I show that results hold when considering only cities in a narrow bandwidth around the score threshold, for whom the assumption of random assignment of grant offers is most credible. I also illustrate that results in the primary specification are not attainable when replacing the true cutoffs with placebo thresholds. Finally, I present simple regression discontinuity estimates in Figure A-3. Consistent with the arguments above, the RD estimates are quantitatively similarly to the difference in differences estimates but much less precisely estimated.

5 Results

Figure 5 plots the coefficients on interactions between a high score indicator and year fixed effects. Coefficients are normalized to 2008. I present the corresponding regression coefficients in Table A-4.

Circles plot the results where the dependent variable is sworn officers per 10,000 residents. Coefficients hover near zero prior to 2008, indicating that treatment and control cities follow similar trends prior to the program. However, coefficients become positive and statistically significant beginning in 2009. Relative to low-scoring applicants, cities above the threshold employ nearly one additional sworn officer per 10,000 in 2010.

As a placebo check, I repeat the dynamic first stage specification where civilian employees per 10,000 and log police expenditures per capita are the dependent variables of interest. Civilian employees are reported in the LEOKA dataset, while I obtained data on police spending from the Annual Survey of Governments.[18] Treatment and control cities exhibit no measurable difference in civilian employment or expenditures both before and after 2009.

Squares in Figure 5 plot the results where the dependent variable is victimization cost-weighted crime per capita. The coefficients follow an inverse pattern to those for police. Pre-period coefficients are near zero and statistically insignificant, again indicating parallel trends prior to application. Relative to low-scoring cities, high-scoring cities experience a decline in cost-weighted crimes beginning in 2009. One year out from the program, crime cost per capita is about \$31 lower in treatment cities. As of 2010, the implied Wald estimate is that one additional sworn officer reduces victimization costs by \$310,000 (\$31 $\times$ 10,000 to account for the different denominators). Scaling by the pre-program means for marginal cities, this estimate corresponds to an elasticity of about -1.1.

Figure A-6 illustrates the sensitivity of the results to the inclusion or exclusion of city-specific trends. The figure suggests that parallel pre-trends hold in either case, although the pre-period coefficients are larger when trends are excluded. I opt for using city-trends in the main estimates both to be conservative and because their inclusion improves the statistical precision of the first-stage relationship between grant receipt and police per 10,000.

Table 2 presents the main difference in differences estimates. The first stage estimate, presented in Column 1, suggests that police rates increase in treatment cities by 0.723 sworn officers per 10,000 over the period 2009–2014. The estimate is highly significant, with an F-statistic of 20.96, indicating

[18] Note that these results use a subset of the data because only a subset appear in the ASG. See the Table notes.

that the interaction $High \times Post$ satisfies the instrument relevance condition by conventional standards. The reduced form estimate, shown in Column 2, indicates that relative to the control group, treatment cities experience reductions in cost-weighted crime per capita of \$25.43 in the post-program period. The estimated coefficient is statistically significant at the 1% level. Columns 3-4 show OLS and IV estimates of the effect of police on crime. The OLS estimate illustrates the standard simultaneity bias result. The coefficient is positive and statistically significant, implying that more police are associated with a slight increase in crime costs. On the other hand, the IV estimate indicates that an additional officer per 10,000 reduces cost-weighted crime per capita by \$35.17. The implied elasticity of victimization costs with respect to police force size is -1.17.[19]

5.1 Robustness

5.1.1 Relevance of Application Score Thresholds

While the identification strategy does not require random assignment of grant offers, one could make the case that grant offers are approximately randomly assigned for cities close to the cutoff due to the inherent randomness of the exact threshold locations (Lee and Lemieux 2010). Motivated by this observation, I repeat the first stage and reduced form estimates using only cities within varying bandwidths of the threshold. The results are presented in Panel A of Figure 6. In both cases, the point estimates are quite similar regardless of the bandwidth. When using only cities within 0.25 standard deviations of the threshold ($N = 558$), the first stage and reduced form coefficients are 0.65 and -26.87, while the coefficients using the full sample are 0.723 and -25.43. Estimates using the narrower bandwidths are less precise, however, due to shrinking sample sizes. Still, the similarity of the main estimates to those obtained using a sample for whom the assumption of random assignment is plausible lends further credibility to the results.

I also test whether exceeding the score threshold, whose location is plausibly random, rather than simply having a high application score, drives the police increases and crime declines. Specifically, I estimate the first stage and reduced form equations coding cities as treated if their score was above

[19] Table A-5 examines the sensitivity of the IV estimate to including controls in the regressions. Results are similar when including and excluding the basic controls and when adding a control for population.

the cutoff + p, where p is a perturbation. If crossing the threshold, rather than the score itself, is the relevant distinction, the estimates should be largest (in absolute value) when using the true cutoff. As shown in Panel B of Figure 6, this is indeed the case. Both the first stage and reduced form coefficients are larger when using the true threshold than using narrowly perturbed thresholds in either direction. The reduced form estimate is largest when using the cutoff + one standard deviation, but the estimate is very noisy given that only 102 cities are considered treated under this placebo cutoff.

5.1.2 Accounting for Differential Recession Exposure

In Section 4, I highlighted that the acceleration of the Great Recession coincided with the timing of the program and, given the application score inputs, treatment cities may be differentially affected by the recession. Although the main results condition on county-year level unemployment rates and per capita income, I present a further robustness check here. Specifically, for each city, I compute the change in the county unemployment rate from 2005–2007 to 2008–2010. I then bin cities into deciles of this change and estimate regressions with recession decile $\times$ year fixed effects. Results from this exercise are presented in Table 3. In Column 1, I estimate the main difference in differences specification with the unemployment rate on the left hand side. The estimate indicates that treatment cities are indeed more exposed to the poor macroeconomic conditions, with unemployment rates increasing by 0.8 percentage points in 2009-2014 relative to the control group. Once one conditions on recession decile $\times$ year effects, however, the relationship between treatment status and recession exposure disappears, as indicated in Column 2. Columns 3-4 demonstrate that the IV estimate of police-crime relationship is unaffected by the inclusion of the recession $\times$ year effects. In other words, the results are unchanged when identifying effects only off cities who experience similar recession exposure, suggesting that the differential exposure of the treatment group does not drive the results.

5.1.3 Accounting for Differential Stimulus Spending

The second, and related, identification concern was that treated cities may receive differential amounts of non-COPS ARRA funding. If high-scoring cities received more aid, the stimulus funding, rather than increased police, could explain the crime declines in treatment cities. I collected data on all ARRA grants and contracts from the Federal Procurement Data System and aggregated by ZIP code, year, and originating federal agency (DOJ versus non-DOJ).[20] I then aggregated to the FIPS place code level and matched the ARRA funding data to the 3,277 cities in the sample that could be matched from their place codes to a set of ZIP codes.

Using these data, I repeat the main specification but control for log per capita non-DOJ ARRA spending at the city-year level. Table 4 presents the results. Column 1 repeats the main specification from Table 2. Column 2 presents the corresponding estimate using only the 3,277 cities matched to ZIP codes, with the point estimate changing very little relative to the main specification. Column 3 adds a control for log local ARRA spending per capita. Again, the coefficient on police is very similar, suggesting that differential stimulus spending cannot explain the crime declines in treated cities.

Figure A-7 plots log per capita ARRA funding over the period 2009–2013 as a function of the application score. DOJ-originating funding increases discontinuously at the threshold, lending credibility to the FPDS data and the matching process. On the other hand, non-DOJ funding is smooth through the cutoff. As shown in Figure A-8, there is no disparity in local ARRA spending among treatment and control cities close to the threshold. The IV estimate is of similar magnitude using only such cities, however, suggesting that differential stimulus spending cannot explain the results.

5.2 Results by Crime Type

In the main analysis, I focus on cost-weighted crime per capita both to simplify presentation and because this variable captures the relevant outcome for policymaking. Also of interest, however, are results broken down by crime type. Figure 7 shows the effect of exceeding the cutoff over time on the index crime categories. Violent crime is the sum of murder, rape, robbery, and aggravated assault. Property crime is the sum of burglary, larceny, and auto theft.[21] In both cases, the pattern is quite similar to that for cost-weighted crime. Treatment and control cities follow similar trends in the pre-period, but a difference emerges beginning in 2009. Corresponding regression results, shown in Table A-4, indicate that relative to cities below the cutoff, those above experience declines in

[20]See https://www.fpds.gov/fpdsng_cms/index.php/en/.

[21]For crime type definitions, see https://www2.fbi.gov/ucr/cius_04/appendices/appendix_02.html.

violent (property) crimes of 3.72 (14.25) per 10,000 in 2010.

IV estimates for the index crime categories, as well as for individual crime types, are presented in Table 5. Each regression is identical to that in Table 2, Column 4, except that crimes per 10,000 is the outcome of interest. The estimates indicate that each additional sworn officer is associated with 4.27 fewer violent crimes and 15.39 fewer property crimes. Implied elasticities are -1.3 and -0.81, which conforms to a consistent finding in the literature that crime-police elasticities are larger for violent than for property crimes (Chalfin and McCrary 2018). My estimated magnitudes are larger than most in the literature, however. For example, Evans and Owens (2007), find elasticities of -0.99 and -0.26.

Among violent crimes, the results are negative and statistically significant for murder, rape, and robbery, while the estimate is not significant for assault. I find that an additional officer prevents .11 murders, .53 rapes, and 1.98 robberies. While robbery accounts for just 15% of all violent crimes, it accounts for nearly half of the estimated impact of police on violent crime. This result is in line with Evans and Owens (2007), who find that robbery responds most to police increases in terms of elasticities, and with Abrams (2012), who finds that robbery is a particularly deterrable crime type. The estimated impact of police on murder is also noteworthy. Due to the high variability in murder rates, statistically significant estimates of the effect of police on crime, even at the 10% level, are rare in the literature. Although not precisely estimated, the point estimate implies that one life can be saved by hiring about 9.5 new police officers.

Among property crimes, the estimates indicate that police are associated with statistically significant declines in larceny (-15) and auto theft (-5.15). I find that police increase burglaries, although the coefficient is not statistically different from zero. Consistent with existing studies, the effect on auto thefts is particularly strong, implying an elasticity of -3.35. The estimate similar to that in Lin (2009), who finds an elasticity of about -4, but larger than most existing work.

5.3 Geographic Spillovers

Analyses of place-based policies often examine whether treatment effects spillover to neighboring regions. For example, increased police in one jurisdiction may reduce crime in neighboring jurisdictions by increasing the probability of apprehension near town borders. Alternatively, increased police in one jurisdiction may displace criminal activity to neighboring areas (Blattman, Green, Ortega and Tobon 2017). If local police increases carry positive or negative (i.e. displacement) spillover effects, one needs to take such spillovers into account when considering the aggregate welfare consequences associated with a program such as COPS.

Although a rigorous examination of spillovers is beyond the scope of this paper, I present a simple test here. Starting with the full sample of municipal police departments with valid crime data ($N = 12{,}245$), I divide cities into four groups: losing applicants ($N = 3{,}536$), winning applicants ($N = 791$), non-applicants in the same county as a losing applicant ($N = 2{,}837$), and non-applicants in the same county as a winning applicant ($N = 3{,}673$). Non-applicants in counties with no applicants are dropped, leaving a sample of 9,369 municipalities. I then estimate a dynamic difference in differences specification (equation 4), interacting indicators for each group with the year effects. As in the main analysis, I normalize coefficients to the losing applicants.

The estimates allow a simple comparison of changes in crime for jurisdictions near treated cities and jurisdictions near control cities. Figure 8 shows the results. Relative to the large reduction in crime in treated cities (circles), there is little change in crime in non-applicants near treated cities (squares) and non-applicants near control cities (diamonds). The similarity in the trends for jurisdictions near treated and control cities suggests that geographic spillovers associated with local police increases were negligible.

5.4 Mechanisms

As with other crime control policies, police hiring may reduce crime through two channels – deterrence or incapacitation. Standard economic models of crime, such as Becker (1968), predict that police *deter* crime by raising the expected cost associated with criminal behavior, causing fewer potential offenders to engage in crime. However, police may also increase the number of individuals detained or incarcerated, which would reduce crime by *incapacitating* potential offenders. By which mechanism police reduce crime is of considerable interest because incapacitation is associated with increased incarceration costs in addition to the police wage bill.

In practice, my estimates almost surely identify a combination of deterrence and incapacitation effects (Chalfin and McCrary 2017). To get a sense of the relative importance of the two mechanisms, I examine whether COPS-induced police force increases were associated with increases in arrest rates. As highlighted in Owens (2012), the intuition behind this test is that for police to have an incapacitation effect, hiring police must increase the number of arrested potential offenders. Hence, one can rule out that incapacitation plays a large role in the estimated crime declines if arrests do not increase along with the manpower increases.[22]

For this exercise, I rely on data from the UCR *Arrests* file, which reports yearly arrest counts by offense category at the agency level. Not all agencies that report crimes also submit arrest data and I use a sample of 3,914 (of 4,327) cities with valid arrest data. Table 6 reports IV estimates of the effect of police increases on arrests per 10,000. I show results separately for violent and property crime arrests. For reference, because the sample is slightly different, I also show the corresponding estimates for violent and property crimes when using the arrests sample.

Columns 1-2 indicate that an additional officer is associated with 4.4 fewer violent crimes and .17 additional violent crime arrests. The estimated impact on violent arrests is not statistically significant and implies a small arrest-police elasticity of .17. Similarly, columns 3-4 demonstrate that an additional officer reduces property crimes by 18 and reduces property arrests by .5. The arrest impact is again not statistically different from zero. On net, the evidence suggests that arrests did not increase with the police force expansions, which is consistent with a deterrence mechanism underlying the estimated crime reductions.

5.5 Treatment on the Treated Program Effects

The first stage regression of police per 10,000 residents on $High \times Post$ recovers an intent-to-treat estimate of the effect of a hiring grant offer on police force size. The estimate is an ITT, rather than a treatment on treated (TOT) estimate, because control cities can receive hiring grants during

[22] To the extent that police reduce crime by means other than incapacitation or deterrence, the arrest rate test cannot distinguish between deterrence and other non-incapacitation explanations. For example, police may substitute from arresting offenders to activities that raise money for the municipal government. However, existing work such as Goldstein, Sances and You (2018) highlights that such activities typically do not enhance public safety. Additionally, much of the existing work on the police-crime relationship, such as Owens (2012) and Weisburd (2016) has highlighted the importance of deterrence as a mechanism by which police reduce crime.

later funding rounds, eroding the disparity in treatment status between high and low scoring cities. Note that such an erosion has no bearing on the estimated police-crime relationship. Control cities becoming treated impacts both the first stage and reduced forms, and the IV estimate is a TOT estimate of the effect of police on crime. However, one may also be interested in the TOT effect of hiring grants on police force size. For example, to estimate the total number of officers added by program, one should use the TOT rather than the ITT.

A very simple estimate of the TOT can be obtained by scaling the pre-post (ITT) difference in police by (one minus) the fraction of control cities who are ever treated in the post-period.[23] 11% percent of control cities are treated at some point over 2010–2014. Hence, a TOT estimate is $0.723/0.89 = 0.81$ sworn officers per 10,000 added by each grant offer. Alternatively, one can deal more rigorously with the dynamic relationship between police and grants and estimate TOT effects at years 1,2,..,5 since a grant offer. I estimate dynamic TOT effects using a recursive method outlined in Cellini et al. (2010). The intuition of the strategy is as follows. The treat-control difference in police in 2009 is both an ITT and TOT estimate of the effect of grants on police in the year of grant receipt. In 2010, the treat-control difference is an ITT estimate because some control cities become treated. One can estimate directly the extent to which the disparity in treatment status erodes. Further, the 2009 ITT offers an estimate of the increase in police in 2010 for control cities that become treated in 2010. Hence, an estimate of the TOT in 2010 is the 2010 ITT estimate minus the fraction of control cities who become treated multiplied by the 2009 ITT estimate.

To operationalize this intuition, I estimate the following two equations:

$$Funded_{it} = \pi_t \times High_i \times \kappa_t + \kappa_t + \phi_i + \epsilon_{it}$$

$$Police_{it} = \theta_t^{ITT} \times High_i \times \kappa_t + \kappa_t + \phi_i + \epsilon_{it}$$

The π_t's measure the relationship between crossing the threshold in 200 and grant receipt in each year. The θ_t^{ITT}'s are ITT estimates of the effect of crossing the threshold in 2009 on police, identical

[23] Figure A-4 shows the fraction of cities in the treatment and control group applying for (Panel A) and receiving (Panel B) hiring grant funding in each program year.

to those presented in Figure 5. The TOT estimates are then

$$\theta^{TOT}_{2009} = \theta^{ITT}_{2009}$$

$$\theta^{TOT}_{2010} = \theta^{ITT}_{2010} - \pi_{2010}\theta^{TOT}_{2009}$$

$$\theta^{TOT}_{2011} = \theta^{ITT}_{2011} - \pi_{2010}\theta^{TOT}_{2010} - \pi_{2011}\theta^{TOT}_{2009}$$

and so on. To obtain standard errors, I bootstrap the TOT estimation procedure using 500 iterations of city-level resampling.

Results are presented in Table 7, with the corresponding estimates shown graphically in Figure A-9. Cities below the cutoff in 2009 are about 7% more likely to receive treatment in 2010 than those above, indicating that the 2010 ITT is an underestimate of the one-year TOT effect. Correspondingly, the 2010 TOT estimate is 0.972, compared with an ITT estimate of 0.935. On the other hand, cities above the threshold in 2009 are slightly *more* likely to receive additional funding in each year 2011–2014. As a result, the TOT estimates become slightly smaller then the ITT estimates beginning in 2012. On net, this exercise suggests that the ITT estimates are a reasonably good approximation to TOT effects, which is unsurprising given the relatively small treatment-control differences in grant receipt during 2010–2014 as compared to 2009.

5.6 Heterogeneity

An analysis of treatment effect heterogeneity may offer insights as to why the estimated impacts of police on crime are so large relative to the literature.[24] To get a sense of the estimates we might expect, consider a model of optimal police force size. Cities hire police x to minimize total costs, which is the sum of victimization costs, $v \times c(x)$, where v is the cost associated with each crime and $c(x)$ is the number of crimes as a function of police, and the cost of employing police, $w \times x$, where w is the wage. In other words, the city's problem is

[24] One possibility is that I use smaller cities than most existing studies, and treatment effects are larger in these cities. Figure A-10 demonstrates that this is not the case. While police forces increase most for small cities, crime rates also decrease most. There is no clear relationship between city size and the treatment effect or crime-police elasticity.

$$\min_x \; vc(x) + wx$$

The first order condition for an interior solution is $-vc'(x) = w$. My IV estimate of $-vc'(x)$ is about \$350,000 with a lower 90% confidence bound of \$96,508 (Table 2). The average police officer wage for cities in my sample is about \$67,000, while the true marginal cost of adding an additional officer is thought be around \$130,000. The large estimated marginal benefit relative the marginal cost appears inconsistent with optimization at the city level. Municipalities ought to have hired police until the marginal benefit equals the wage.

One potential explanation could be that cities were forced away from their optimal police levels due to fiscal stress and tightening budgets during the Great Recession. To test for this, I compute each city's change in the unemployment rate from 2007-2009, δ_i, to proxy for recession exposure. I then examine heterogeneous effects by δ using both a nonparametric and parametric strategy. For the nonparametric approach, I split the sample into quintiles of δ_i and interact the quintiles with the instrument, $High \times Post$, in the first stage and reduced form. For the parametric approach, I simply interact the instrument linearly with δ_i.

Panel A of Figure 9 shows the first stage and reduced form effects. On average, police increases and crime reductions associated with the crossing the threshold are larger for cities with more severe recession exposure. Such a pattern is apparent from both the parametric and nonparametric approaches. The estimated effect of grant receipt on police per 10,000 is about 0.5 for cities in the bottom quintile but over 1 for cities in the top quintile. Corresponding effects on crime cost per capita are -\$12 for cities in the bottom quintile and -\$50 for cities in the top quintile.

Panel B converts the first stage and reduced form estimates into IV estimates of the effect of police on crime that vary by recession exposure. The figure highlights that while both the first stage and reduced form effects are largest for cities enduring worse economic conditions, the reduced form increases (in absolute value) more dramatically than does the first stage with δ. Hence, the treatment effect of additional police is largest for the cities most exposed to the recession. The parametric approximation implies that the return to an additional officer is close to zero for cities with increases in unemployment of around 2 percentage points, while the return is around \$60 per capita in cities with 8 percentage point increases in the unemployment rate. Overall, the evidence suggests that the returns to additional police were highest for cities under more fiscal distress, which is consistent with the hypotheses that the recession forced cities below their optimal police levels.

6 Cost-Benefit Analysis

Given that police added by the program reduced crime, a natural question is whether the COPS hiring program passes a cost-benefit test. The average grant carried a dollar value of \$295,974 per 10,000 residents (recall that grants covered three years of salary). If one uses the simple TOT estimate above, a reasonable estimate of the number of officer-years per 10,000 residents added by the program is 0.8 officers $\times$ 4 years = 3.2 (four years because grants covered three years salary with the expectation that the officer would be retained for a fourth year). Hence, police forces increased by one for each \$92,492 in grant funding. About \$874.4M was allocated to cities in my sample in 2009, implying that 9,454 officer-years were added by the ARRA funding round. After accounting for deadweight loss associated with raising government revenue, the federal cost is in the range of \$1.14B. Most estimates in the literature suggest that the annual cost of a fully-equipped police officer is around \$130,000, which implies that local governments spent an additional \$600M on the estimated police increases. Hence, a reasonable estimate of the program's total cost is about \$1.75B.

Given estimates of total cost and officer-years added, the program is cost-effective if the social value added by one officer-year exceeds \$1.75B / 9,454 = \$185,107. The IV point estimate in Table 2 indicates that each officer-year contributes \$352,000 in social benefit from crime reduction. Under this assumption, the program easily passes a cost-benefit test. If one instead uses the lower 95% confidence bound, the social benefit associated with each officer is around \$54,000 and the program appears cost-ineffective.

Alternatively, one could estimate the social value per officer by summing the estimated coefficients for each individual crime type in Table 5, weighting by the associated social cost for each crime type. Such a computation is sensitive both to the coefficients and crime cost estimates used. Further, given incredibly high social costs associated with murder, such a computation is especially sensitive to the estimated murder effect. At a VSL estimate of $5 million, the point estimate in Table 5 implies that an officer provides $535,000 in social benefit due to homicide reduction alone. On the other hand, using the cost estimates in Chalfin (2016), the social benefit per officer attributable to the robbery, larceny, and auto theft reductions is $160,548, which is close to but does not exceed the required $185,000. On net, the evidence suggests that the program is cost-effective, but it is difficult to say for sure.

As a component of the American Recovery and Reinvestment Act, COPS program funding was intended, at least in part, to create or save police officer jobs. The degree to which ARRA spending increased employment has been the subject of much debate. The academic literature has focused on estimating the cost per job created by the Recovery Act, relying on cross-state variation in the generosity of transfers received from the federal government. Despite apparently similar methodologies, existing estimates vary widely. Chodorow-Reich, Feiveson, Liscow and Woolston (2012) estimate a cost per job-year of $26,000, with most job-creation in the private sector. Conley and Dupor (2013) find that most jobs created were in government and estimate cost per job-year of $200,000. My analysis implies a cost per job-year of $92,500, which is on the larger end but certainly within the range of existing estimates. Given the reasonable cost per job-year and the large ensuing crime reductions, the benefit-cost ratio associated with police hiring grants may compare favorably with other stimulus spending. Such programs may be more politically feasible, as well, since spending under the heading of crime reduction is more likely to gain bipartisan support than many federal programs.[25]

7 Conclusion

In this paper, I exploit a natural experiment to circumvent the endogeneity of police hiring and estimate the causal effect of police on crime. My identification strategy relies on the fact that COPS hiring grant funding in 2009 was distributed through an application process. I compare the change over time in police and crime in cities with application scores above and below the funding threshold, with the underlying premise that rejected applicants are a valid control group for accepted ones. Studying dynamics

[25]See, e.g. *Bipartisan House group seeks to bolster nation's police forces with COPS bill*, Mile Lillis for `thehill.com`, 5/14/2011.

non-parametrically, I show that police and crime follow similar trends in high and low scoring cities prior to 2009, but trends diverge as high scoring cities receive hiring grant funding. The corresponding instrumental variables estimates imply that an additional officer per 10,000 residents reduces victimization costs by about $35 per capita, with an implied crime-police elasticity of -1.17. The estimated magnitude suggests that expanding the police force is easily cost-effective for the average city in my sample.

The main results are robust to a series of specification checks, including relying on only cities with scores close to the threshold and therefore for whom the assumption of randomly assigned treatment is plausible. An examination of individual crime types reveals that the treatment effects are larger for violent than for property crimes and most pronounced for robbery and auto theft. I also find evidence that treatment effects are largest for cities most exposed to poor macroeconomic conditions during the Great Recession. Such a result is consistent with the theory that fiscal distress caused cities to reduce their police forces below optimal levels, which could explain the large magnitudes of my estimates relative to the literature.

Whether the COPS hiring program passes a cost-benefit test depends on the social benefit attributable to an additional officer year. The point estimate in my main specification implies that the program is easily cost-effective. I estimate that one officer-year was added for every $95,000 spent by the federal government and that the social benefit associated with the ensuing crime reduction on the order of $350,000. Under more conservative assumptions, the program fails a cost-benefit test. The results highlight that fiscal support to local governments for crime prevention may offer large returns, especially during bad macroeconomic times.

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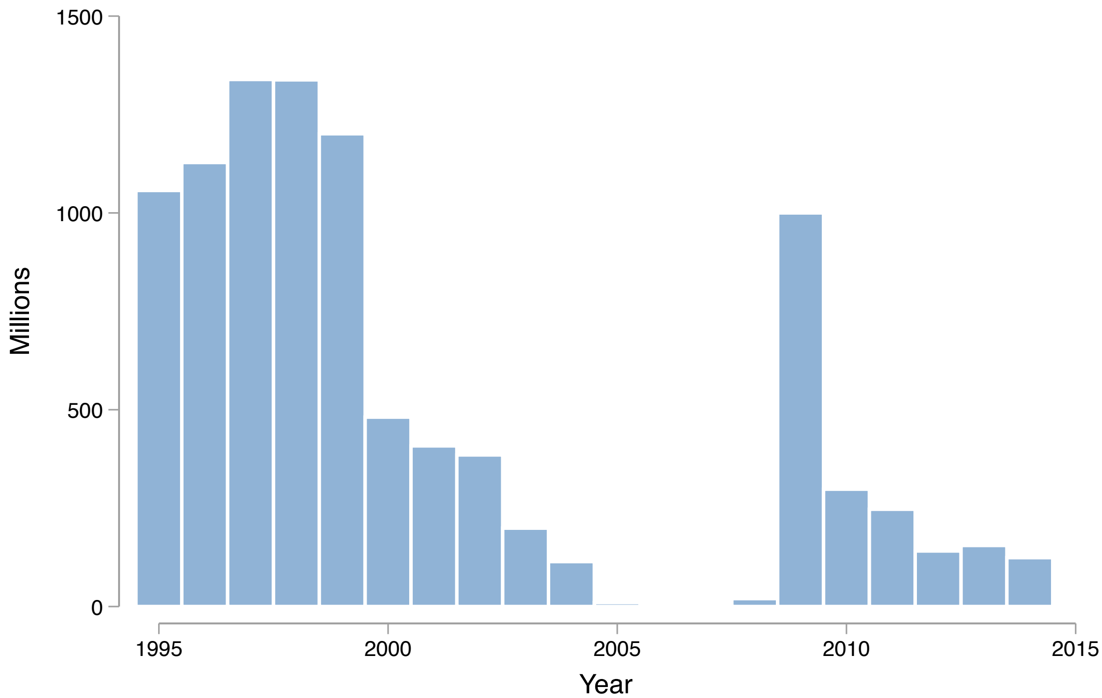
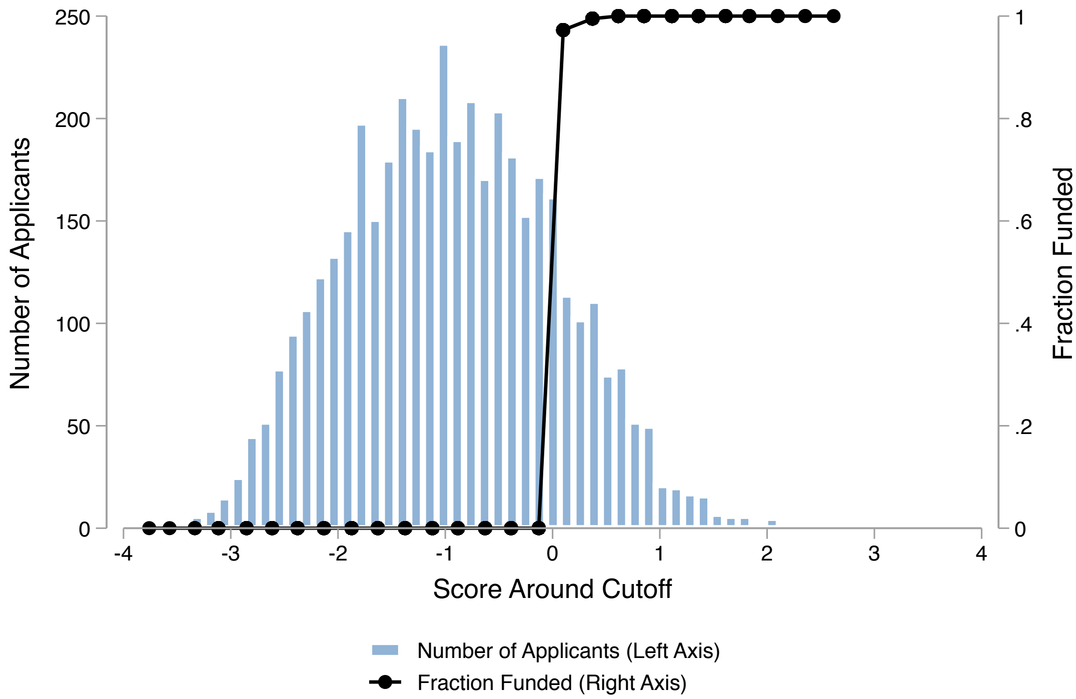
Figure 1: COPS Hiring Program Funding Over Time



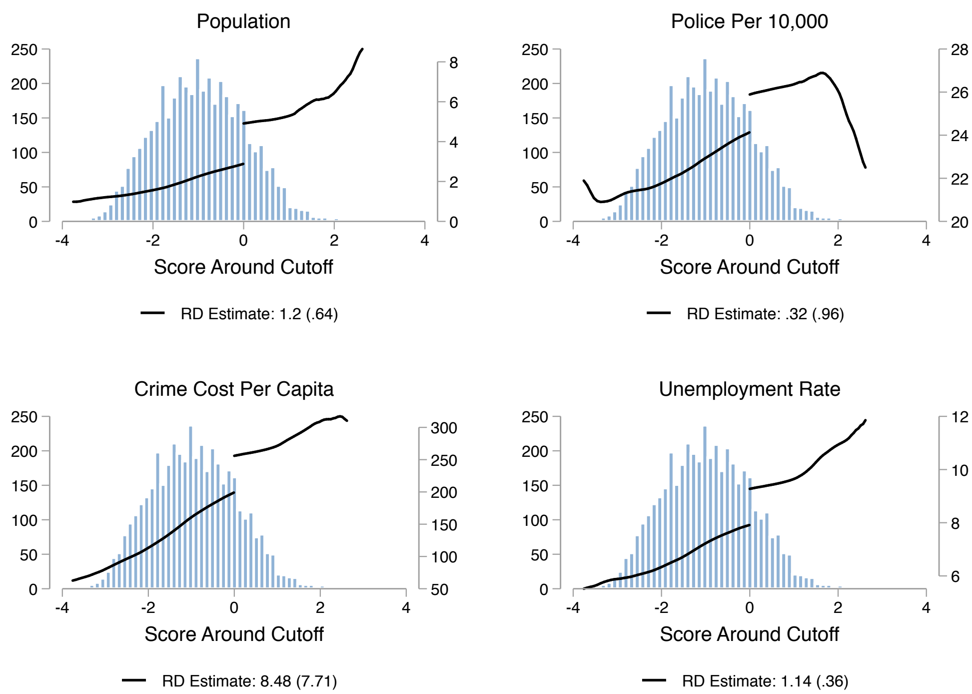
Notes: Historical appropriations data from James (2013).

Figure 2: Distribution of Application Scores and Funding Probability



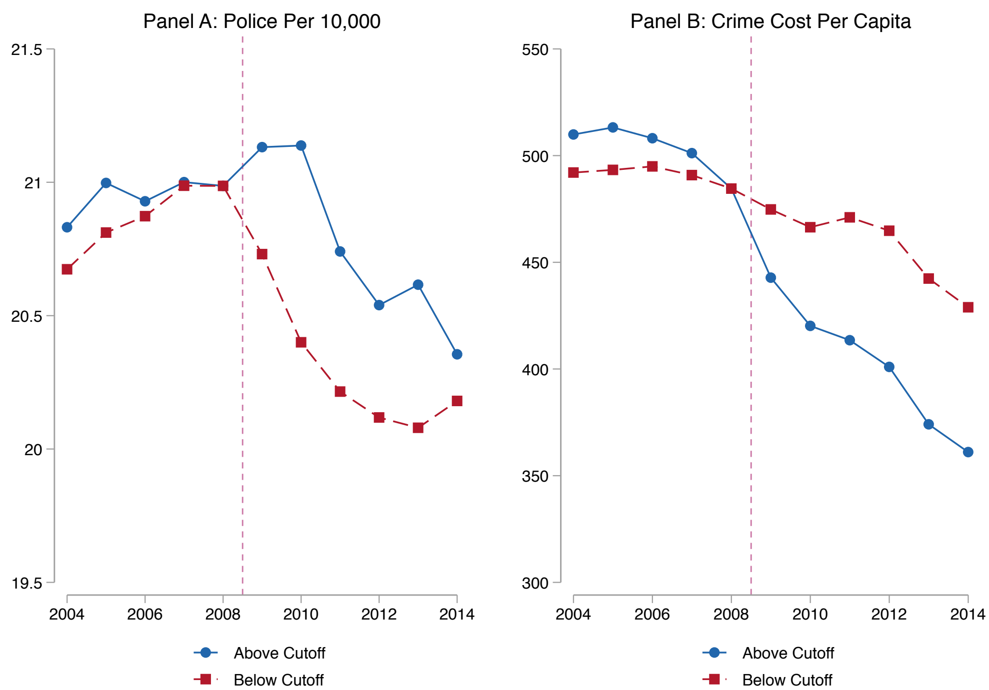
Notes: An observation is a city. Figure plots of histogram of the 2009 application score relative to the cutoff (left axis). The application score is standardized, so the units are standard deviations. Figure also plots the fraction of applicants in each bin (width=0.25 score points) that received a hiring grant (right axis).

Figure 3: Baseline Characteristics by Application Score



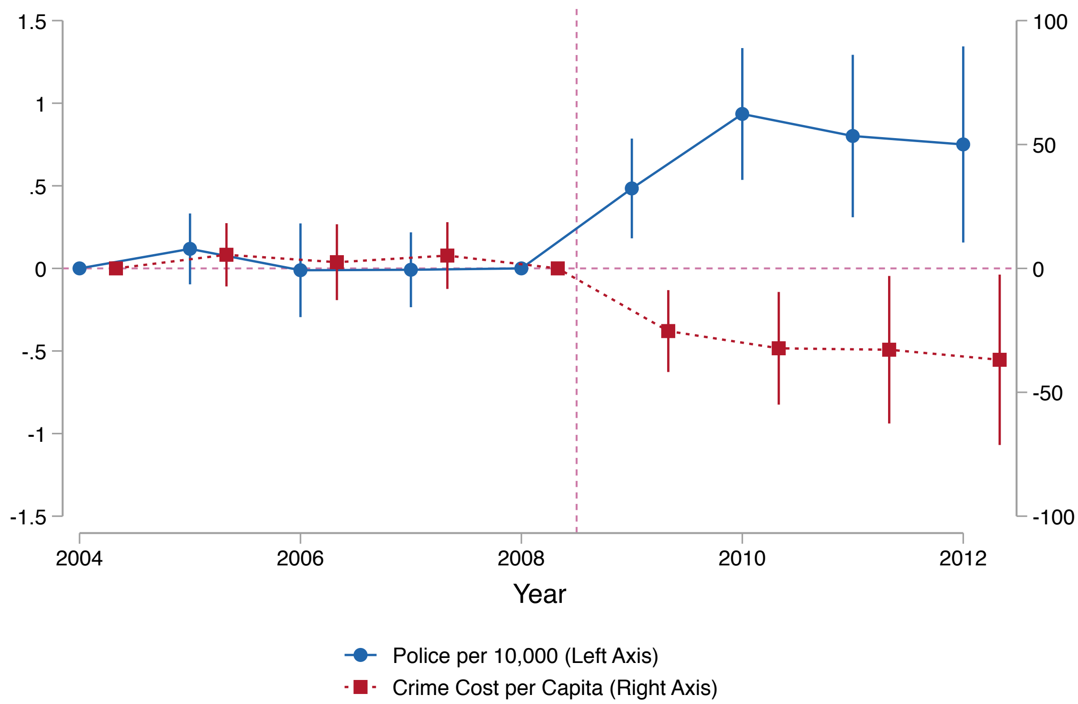
Notes: Each panel plots local linear regression fits of the denoted outcome (right axis) estimated separately for cities above and below the threshold over a histogram of the application score (left axis). Legend denotes the RD estimate using a triangular kernel and the IK optimal bandwidth. Population in ten thousands. Population, police, and crimes are from the UCR and measured in 2008. Unemployment rate is from the ACS and measured in 2009.

Figure 4: Trends in Police and Crime by Treatment Status (Raw Data)



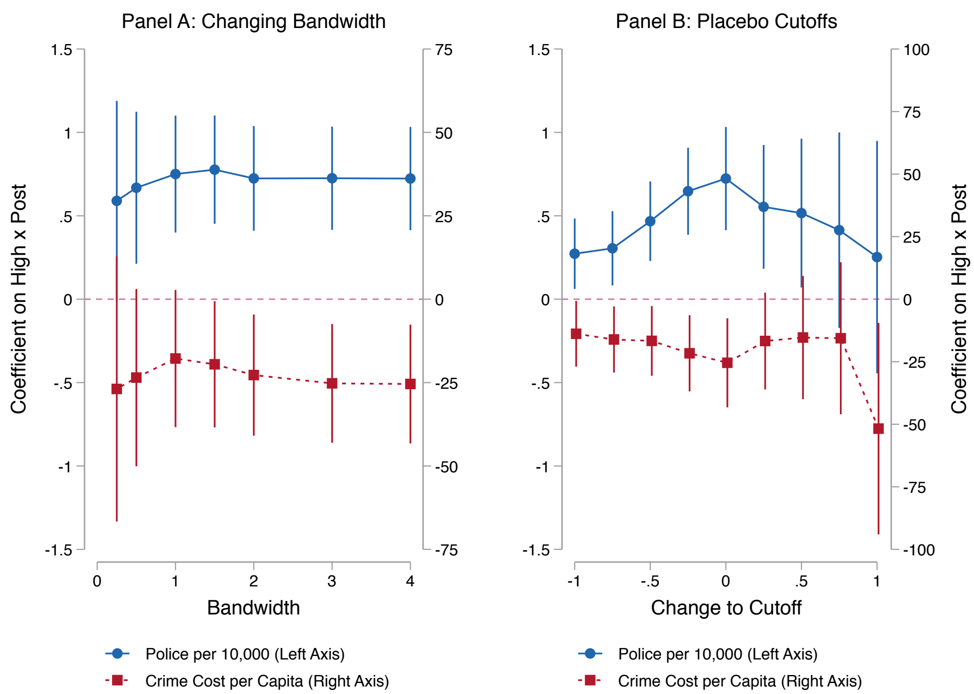
Notes: Figure plots annual averages of police per 10,000 (Panel A) and crime costs per capita (Panel B) by treatment status (above or below the cutoff). Treatment groups means are normalized to be equal to the control group in 2008.

Figure 5: Effect of Exceeding the Threshold on Police and Crime



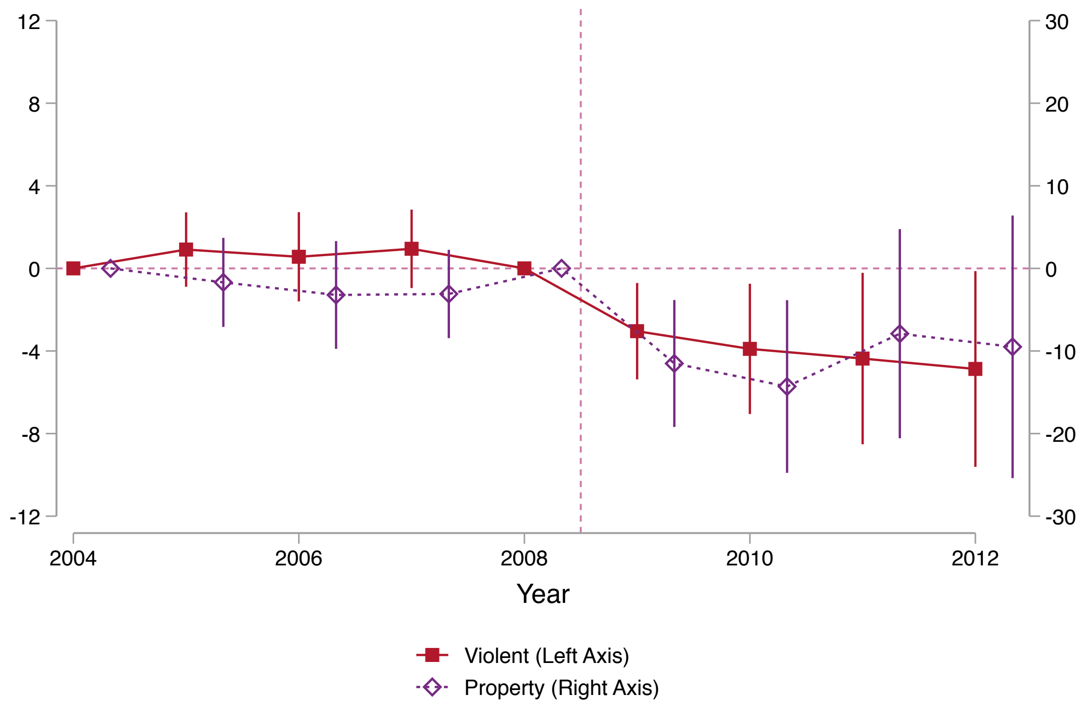
Notes: Figure plots coefficients on interactions between year indicators and an indicator for whether the 2009 application score exceeded the threshold. Regressions also include city fixed effects, year × size group fixed effects, and city-specific linear trends. 95% confidence intervals are constructed from standard errors clustered at the city level.

Figure 6: Sensitivity of First Stage and Reduced Form Estimates



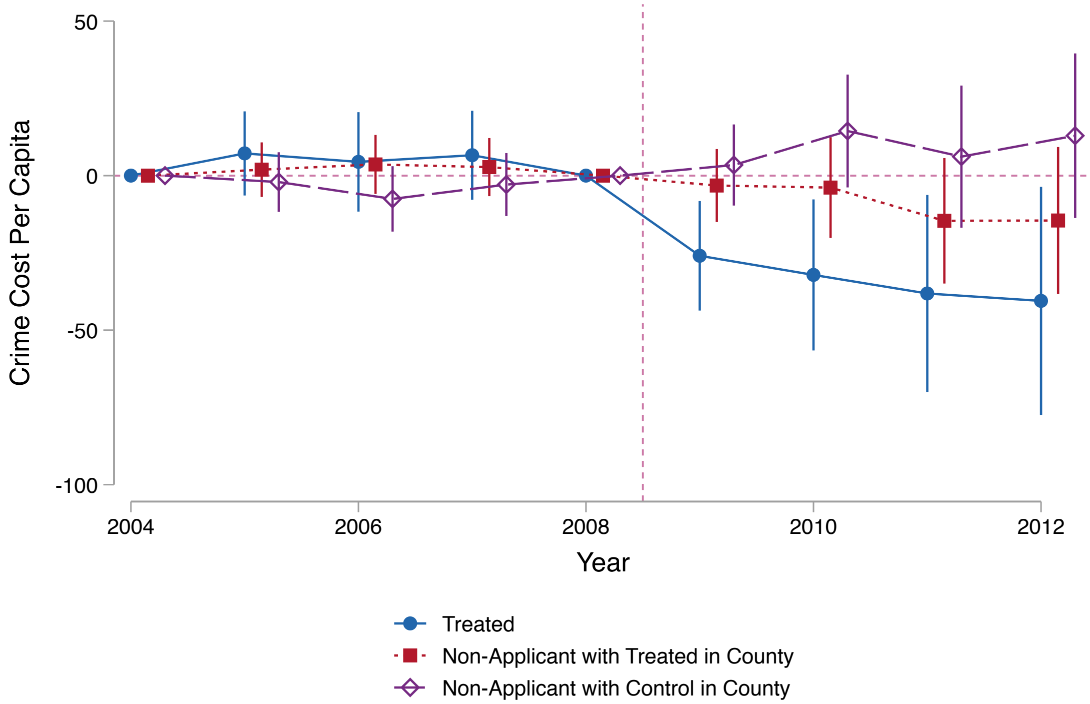
Notes: Figures plot coefficients and 95% confidence intervals on $High \times Post$ from regressions where police per 10,000 (crime cost per capita) is the outcome of interest. Regressions include controls, city fixed effects, year $\times$ size group fixed effects, and city-specific linear trends. Panel A plots coefficients when only departments within the denoted bandwidth are used. Panel B plots coefficients when using perturbed score cutoffs (i.e., the coefficient at -0.5 is the coefficient obtained when treating the cutoff as if it were 0.5 points below the true cutoff).

Figure 7: Effect of Exceeding the Threshold on Violent and Property Crimes



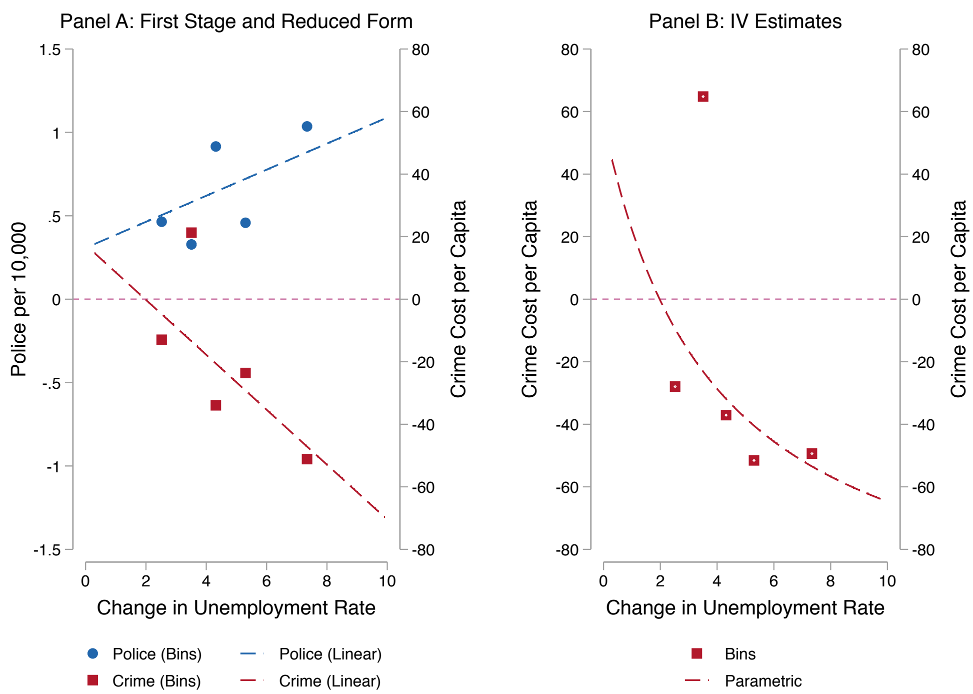
Notes: Dependent variable is crimes per 10,000. Figure plots coefficients on interactions between year indicators and an indicator for whether the 2009 application score exceeded the threshold. Regressions also include city fixed effects, year $\times$ size group fixed effects, and city-specific linear trends. 95% confidence intervals are constructed from standard errors clustered at the city level.

Figure 8: Testing for Geographic Spillovers



Notes: Dependent variable is crime costs per capita. Figure plots coefficients on interactions between treatment status indicators and year effects. Cities are grouped into four categories: winning applicants (treated) (N=791), losing applicants (control) (N=3,536), non-applicants in the same county as a treated city (N=3.673), and non-applicants in the same county as a control city (N=2,837). Coefficients are normalized to the losing applicants. Each regression includes controls, size × year effects, city trends, and city fixed effects.

Figure 9: Heterogeneous Effects by Recession Exposure



Notes: Change in Unemployment Rate is the 2007-2009 change in the local unemployment rate. See text for additional details on computation.

Table 1: Summary Statistics for Applicant Cities

	Above Cutoff	Below Cutoff	Total
Population (Ten Thousands)	6.996	2.467	3.295
	(21.74)	(15.29)	(16.74)
Unemployment Rate	9.552	6.976	7.447
	(4.020)	(3.127)	(3.454)
Family Income (Ten Thousands)	3.960	5.334	5.083
	(1.112)	(2.164)	(2.082)
Percent Black	20.76	7.753	10.13
	(22.51)	(12.38)	(15.59)
Percent Hispanic	15.19	10.05	10.99
	(20.67)	(14.92)	(16.25)
Percent Young Male	23.54	21.60	21.95
	(5.874)	(6.909)	(6.773)
Police Per 10,000	26.10	22.69	23.32
	(10.94)	(11.26)	(11.28)
Violent Crimes Per 10,000	93.20	56.83	63.47
	(51.00)	(42.35)	(46.24)
Property Crimes Per 10,000	497.4	267.6	309.7
	(228.2)	(162.0)	(197.1)
Crime Cost Per Capita	834.0	494.0	556.2
	(395.3)	(322.0)	(361.3)
Officers Funded Per 10,000	1.679	0	0.307
	(1.601)	(0)	(0.943)
Funding Per Capita	29.60	0	5.411
	(23.83)	(0)	(15.32)

Notes: Number of observations: 791 (above); 3,536 (below); 4,327 (total). Standard deviations in parentheses. Population, police, and crime are from the 2008 Uniform Crime Reports. Demographic and economic information are from the 2009 American Community Service (FIPS place code level).

Table 2: Difference in Differences Estimates

	(1) Police	(2) Crime	(3) OLS: Crime	(4) IV: Crime
High x Post	0.723***	-25.43***		
	(0.158)	(9.083)		
Police			2.198***	-35.17**
			(0.710)	(15.19)
Mean	22.85	689.23	689.23	689.23
Elasticity	-	-	.07	-1.17
F-Stat	20.96	-	-	-
Controls	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327
Observations (City-Years)	47597	47597	47597	47597

Notes: Standard errors clustered at the city-level in parentheses. Police is sworn officers per 10,000 residents. Crime is cost-weighted crime per capita. Elasticity computed using pre-program means for marginal cities. Regressions include city fixed effects.

Table 3: Accounting for Differential Recession Exposure

	(1) UER x 100	(2) UER x 100	(3) IV: Crime	(4) IV: Crime
High x Post	0.797***	0.0405		
	(0.0845)	(0.0380)		
Police			-39.32**	-42.67**
			(15.86)	(17.18)
F-Stat	-	-	19.89	19.34
Controls	No	No	No	No
Size x Year Effects	Yes	No	Yes	No
Recession Decile x Year Effects	No	Yes	No	Yes
City Trends	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327
Observations (City-Years)	47597	47597	47597	47597

Notes: Standard errors clustered at the city-level in parentheses. UER $\times$ 100 is the unemployment rate (on a scale from 0-100). Mean unemployment rate in 2008 is 5.9. Mean unemployment rate in 2010 is 9.6. Police is sworn officers per 10,000 residents. Crime is cost-weighted crime per capita. Regressions include city fixed effects.

Table 4: Accounting for Other ARRA Spending

	(1) Crime	(2) Crime	(3) Crime
Police	-35.17**	-36.79**	-37.52**
	(15.19)	(16.98)	(17.18)
F-Stat	20.96	16.88	16.66
Controls	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes
City Trends	Yes	Yes	Yes
ARRA Spending	No	No	Yes
Clusters (Cities)	4327	3277	3277
Observations (City-Years)	47597	36047	36046

Notes: Standard errors clustered at the city-level in parentheses. Table presents IV estimates. Dependent variable is cost-weighted crime per capita. Column (1) is the same as Column (4) in Table 2. Column (2) repeats the specification from Column (1) using only cities matched to ZIP codes. Column (3) adds a control for log non-DOJ ARRA spending per capita at the city-year level. Regressions include city fixed effects.

Table 5: IV Estimates by Crime Type

	(1) All Violent	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) All Property	(7) Burglary	(8) Larceny	(9) Auto Theft
Police	-4.265**	-0.107*	-0.532**	-1.984***	-1.309	-15.39**	2.747	-14.96***	-5.149***
	(2.022)	(0.0601)	(0.227)	(0.554)	(1.683)	(6.674)	(2.048)	(5.494)	(1.341)
Mean	75.16	.42	3.85	10.79	59.69	436.05	86.83	311.27	35.15
Elasticity	-1.3	-5.84	-3.16	-4.2	-.5	-.810	.72	-1.1	-3.35
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327	4327	4327	4327	4327	4327
Observations (City-Years)	47597	47597	47597	47597	47597	47597	47597	47597	47597

Notes: Standard errors clustered at the city-level in parentheses. Table presents IV estimates. Dependent variable is crimes per 10,000 residents. First stage F-statistic is 20.96. Regressions include city fixed effects.

Table 6: IV Estimates, Crimes and Arrests

	(1) Violent Crimes	(2) Violent Arrests	(3) Property Crimes	(4) Property Arrests
Police	-4.377**	0.173	-18.28**	-0.498
	(2.093)	(0.690)	(7.256)	(2.002)
Mean	75.52	23.02	439.74	76.77
Elasticity	-1.31	.17	-.940	-.15
Controls	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Clusters (Cities)	3914	3914	3914	3914
Observations (City-Years)	43054	43054	43054	43054

Notes: Standard errors clustered at the city-level in parentheses. Dependent variable is crimes (arrests) per 10,000 residents. Each column presents a 2SLS regression where $High \times Post$ instruments for police per 10,000. Sample is the subset of the main sample with valid arrest reporting data. Regression include city fixed effects.

Table 7: Dynamic TOT Effects of Grant Offers on Police

		Police per 10,000	
Year	Funded	ITT	TOT
2009	.99***	.484***	.484***
	(.004)	(.154)	(.146)
2010	-.076***	.935***	.972***
	(.007)	(.204)	(.204)
2011	.05***	.801***	.851***
	(.009)	(.251)	(.252)
2012	.049***	.75**	.742**
	(.009)	(.303)	(.292)
2013	.079***	.936***	.864***
	(.012)	(.34)	(.327)
2014	.06***	.578	.43
	(.01)	(.366)	(.328)

Notes: Dependent variable is police per 10,000 residents. Standard errors for ITT estimates are clustered at the city level. Standard errors for recursive TOT estimates are bootstrapped using 500 iterations of city-level resampling. All regressions include city fixed effects, size group × year fixed effects, and city trends. See text for details on computation of the TOT estimator.

Appendix

A Data

A-1 Grants Data

The grants data provided by the COPS office included applicant names and ORI codes as well as application scores and grant amounts. A number of ORI codes, however, could not be linked to the FBI data. 619 of 7,167 applicants in 2009 had ORI codes ending in "ZZ", which are not valid FBI codes. It appears that the COPS office assigned these fake "ZZ" ORI codes to applicants who either did not know their ORI code or did not have an ORI code. For each applicant with an ORI not appearing in the crimes reported dataset, I searched the crimes dataset and updated the code wherever possible. I updated 521 codes, 461 "ZZ" codes and 60 non-"ZZ" codes. 4% of agencies in the analysis sample (184 of 4,327) have updated codes.

A-2 FBI Sample Creation

Sample construction begins with the 2005 *Law Enforcement Agency Identifiers Crosswalk* (ICPSR 4634), which maps FBI ORI codes to information on government and agency type as well as county and place FIPS codes. I updated the directory to include 178 agencies that appear in both the grants data and the FBI crimes reported data but not the original LEAIC file. After dropping state and special police departments (such as tribal and school departments), the directory includes 15,153 agencies. I then clean the data for the subset of these agencies that meet the following conditions:

1. Report positive population at least once prior to (inclusive) 2008 and at least once after (inclusive) 2010.
2. Report police and crime at least once prior to (inclusive) 2008 and at least once after (inclusive) 2010.
3. Report population, police, and crimes at least four times over 2002–2014.

There are 12,740 such agencies that meet these conditions prior to cleaning and 12,351 such agencies after cleaning. The main analysis focuses on municipal police departments in cities with at least 1,000 residents. There are 8,752 such departments in the list of 12,351 agencies. The 4,327 of the 8,752 agencies that applied for a 2009 hiring grant comprise the main sample.

A-3 Cleaning the FBI Data

As noted in Chalfin and McCrary (2018), the annual city population reported in the FBI files tends to jump discretely around census years. I replace the reported population with a smoothed version. Specifically, I fit the population time series for each city using local linear regression with a bandwidth of two, and replace the reported population with the fitted values.

To identify extreme outliers and record errors in the FBI data, I follow a procedure similar to Evans and Owens (2007) and Weisburst (2017). For each city, using the years 2002–2014, I fit the time series of police, violent crimes, property crimes, violent crime arrests, and property crime arrests using a local linear regression with bandwidth two. I then compute the absolute value of the percent difference between the actual and predicted values, $\delta(y_{it})$. I then recode the observation as missing if δ exceeds a specific threshold.[26]

[26] In practice, I add one to each value to avoid dealing with zeroes. The percent difference between two values is always exactly 2 when one of the values is zero. The original values are used once outliers have been determined.

The thresholds are the 99th (police) and 97.5th (crimes or arrests) percentiles of the within-size group distributions δ, where the size categories are 1,000-2,500; 2,500-5,000; 5,000-10,000; 10,000-15,000; 15,000-25,000; 25,000-50,000; 50,000-100,000; 100,000-250,000; >250,000. Cities appearing in multiple groups are placed in the group they appear most often. I chose the thresholds by manually checking the data for a random subset of 250 cities. About 1% of police observations and about 2.5% of crime observations appeared to be mistakes. I use the within-group distributions of δ because the δ's tend to be more dispersed for smaller than larger cities, but my manual inspection suggested the error rate is uncorrelated with city size.

Observations missing either due to nonreporting or outlier status are then imputed using a combination of backwards/forwards filling and linear interpolation. For example, if a city's first year of nonmissing police is 2007, then that city's police value in 2007 is imputed in 2004–2006. If a city has nonmissing police in 2009 and 2011 but not 2010, the 2010 value is linearly interpolated. I opt for imputation, rather than leaving values as missing, so that estimated year effects do not reflect compositional changes.

Finally, as an empirical caution against results being driven by outliers not detected using the strategy above, I winsorize the police and crimes per 10,000 prior to the analysis. Specifically, I winsorize the bottom and top 1% of values within each size group (i.e. observations in group g with police per 10,000 below the 1st percentile in that group have their police per 10,000 replaced to equal the 1st percentile). This procedure is, again, an empirical caution and has little impact on the results. See Table A-6 for more details.

B Power Calculations

Suppose we want to estimate the effect of a hiring grant offer on $\Delta(y)$ (for example, the change in police and crime rates). If grants are randomly assigned, the minimum detectable effect size (MDE) for significance level α and power κ is

$$MDE = (t_{\alpha/2} + t_{1-\kappa}) \times \sqrt{\frac{1}{D(1-D)} \frac{\sigma^2_{\Delta(y)}}{N}}$$

where D is the fraction of cities assigned to treatment.

Now suppose grants are allocated according the score discontinuity. Schochet (2009) shows that under the assumption that $\Delta(y)$ is a linear function of the application score absent the discontinuity, the MDE in a regression discontinuity design is

$$MDE = (t_{\alpha/2} + t_{1-\kappa}) \times \sqrt{\frac{1}{D(1-D)} \frac{\sigma^2_{\Delta(y)}}{N} \frac{1}{(1-\rho^2)}}$$

where ρ is the correlation between the score and treatment status. The fraction $1/(1-\rho^2)$ is referred to as the RD *design effect*. Note that the linearity assumption is very restrictive. Under less restrictive assumptions about the relationship between the score and the outcome, the MDE will be strictly larger. The main intuition of the above formula is that MDE is decreasing in N but increasing the outcome variability.

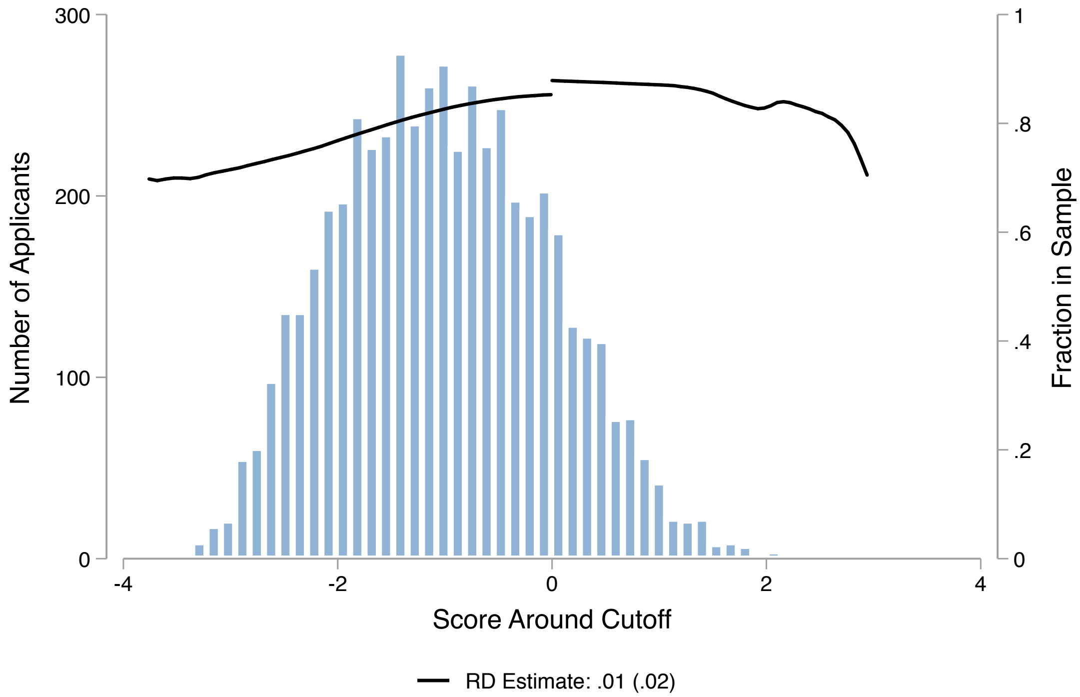
Following convention, set $\alpha = 0.5$ and $\kappa = 0.8$ so that $t_{\alpha/2} + t_{1-\kappa} = 2.8$. When computing the MDE for an RD design, we must take note of the fact that typically only observations within a certain bandwidth of the score threshold are used in estimation. For a given score bandwidth, D, N, ρ, and $\sigma^2_{\Delta y}$ are observable. Assuming a bandwidth of 1 (so cities within one standard deviation of the threshold are used), the MDE's are:

1. *Police*: 0.6 (DD Estimate = 0.723).
2. *Crime Cost*: 29.11 (DD Estimate = -25.43).
3. *Violent Crime*: 4.09 (DD Estimate = -3.09).
4. *Property Crime*: 11.7 (DD Estimate = -11.13).

For reference, I note the difference in differences estimate from the main specification in parentheses. At a bandwidth of 1 and a correctly specified linear score–outcome relationship, an RD is sufficiently statistically powered to detect the DD estimates for police and property crime (albeit narrowly), but not for crime costs or violent crimes. The RD design is underpowered for all outcomes when bandwidth less than one are used. Further, as mentioned above, these MDE's are lower bounds of the true MDE because of the (almost certainly) incorrect linearity assumption. I show the MDE calculations for each outcome and bandwidth in Table A-3.

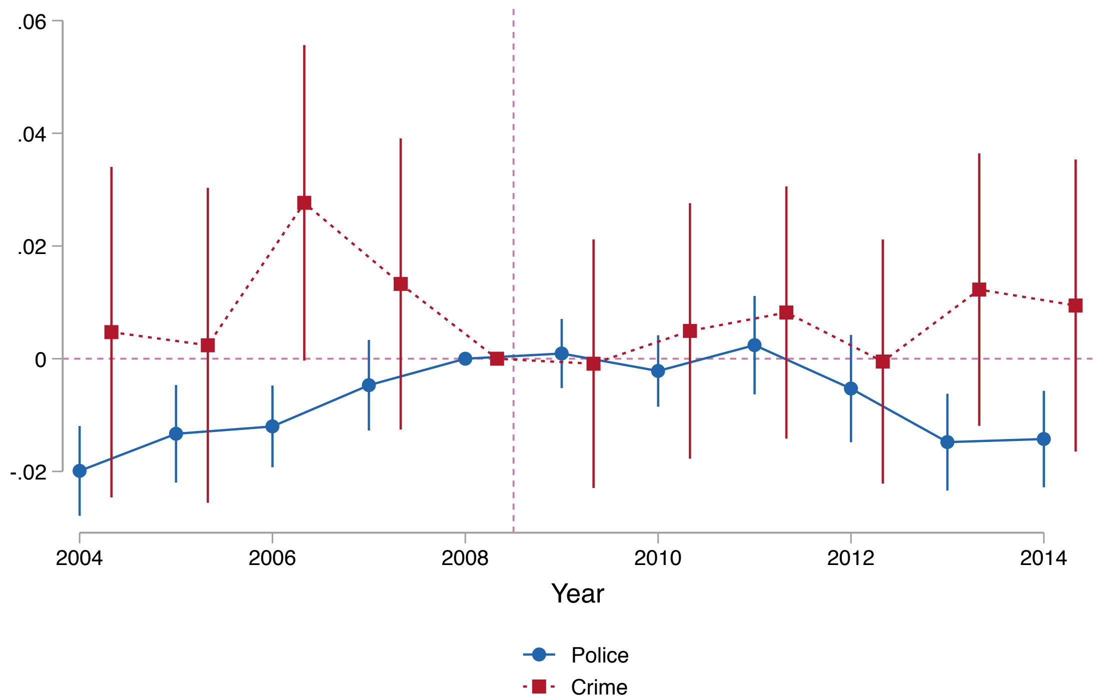
C Appendix Figures and Tables

Figure A-1: Probability of Sample Inclusion by Application Score



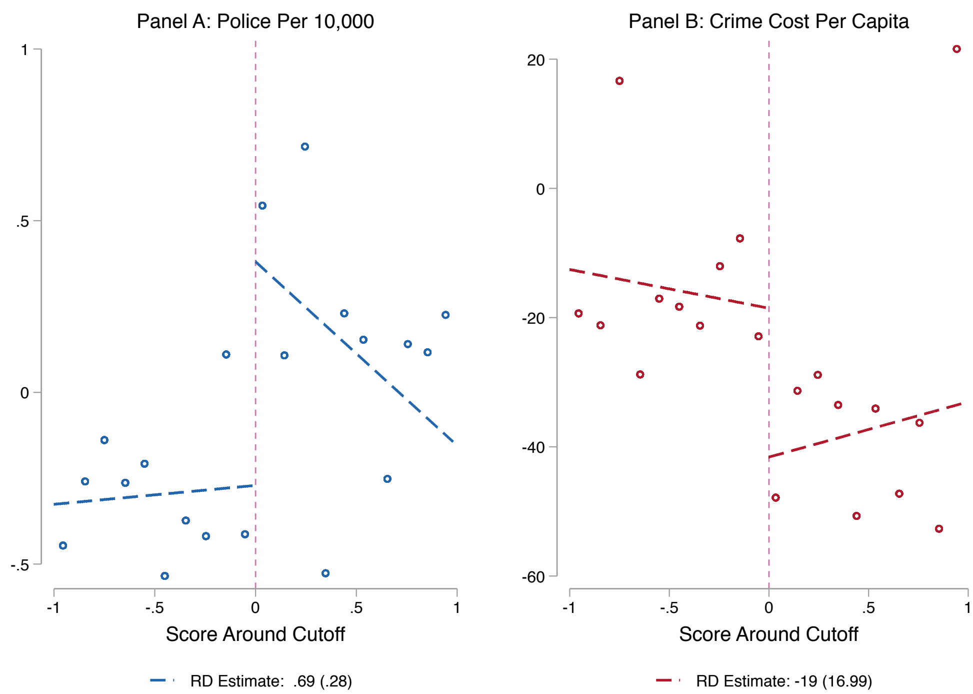
Notes: Sample is 5,314 municipal police departments applying for a hiring grant in 2009. Figure plots local linear regression fits of an indicator for being in the sample against the application score relative to the cutoff (right axis), laid over a histogram of the application scores (left axis). Legend shows corresponding RD estimate using the IK optimal bandwidth and a triangular kernel.

Figure A-2: Data Imputation by Treatment Status



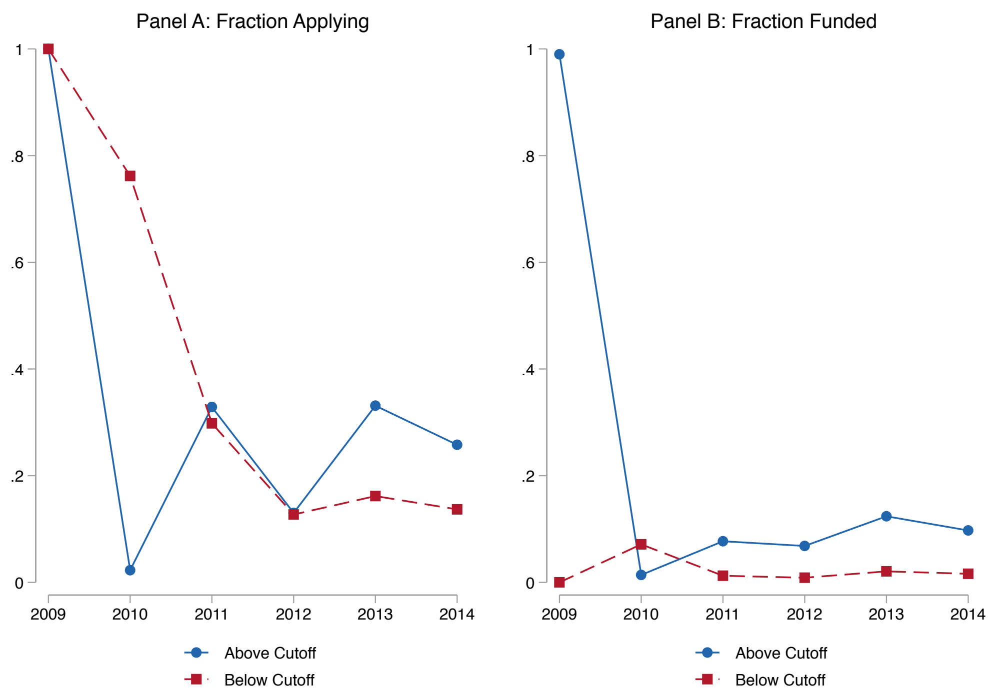
Notes: Figure plots coefficients and 95% intervals on interactions between a high score indicator and year effects. Standard errors clustered at the city-level. Regressions include city fixed effects and size × year fixed effects. Dependent variable is an indicator for police (crime) being imputed. City as coded as having crime imputed if either violent or property crime is imputed.

Figure A-3: Changes in Police and Crime by Application Score (2008–2009)



Notes: Figure plots local means (bin width equals .1 score points) of the 2008-2009 change in police (crime). Dashed lines denote linear fits, estimated separately for cities above and below the threshold. Legend indicates the RD estimate (standard error) when using the IK bandwidth and a triangular kernel.

Figure A-4: Application and Funding Rates by 2009 Treatment Status

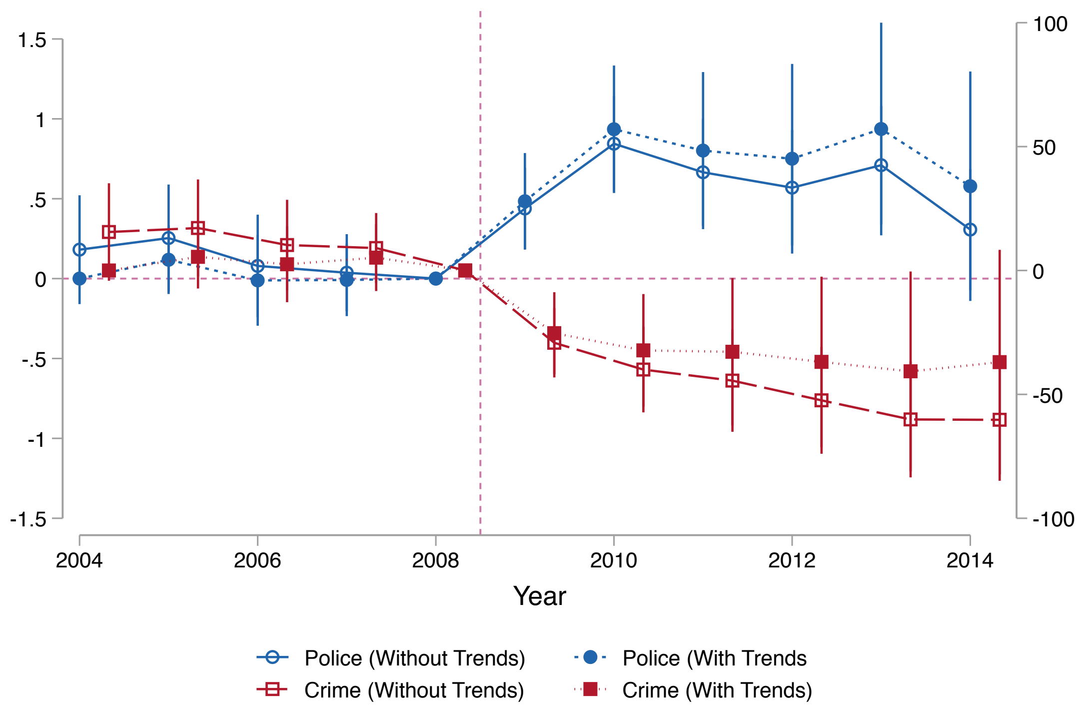


Notes: Figure plots the fraction of cities applying (Panel A) and receiving funding (Panel B) by year and by whether the 2009 application score exceeded the cutoff.

Figure A-5: First Stage Placebo Tests

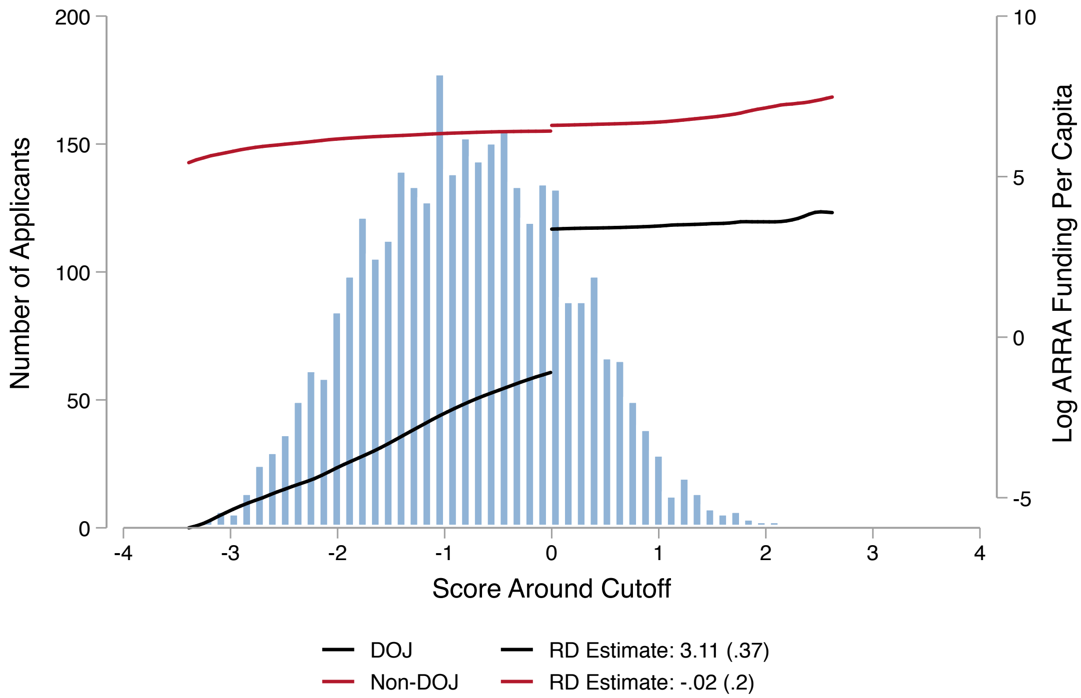
Notes: Sample is 2,075 agencies in main sample that could be matched to the Annual Survey of Governments (ASG). Civilians refers to civilian police employees reported in the UCR *LEOKA* files. Expenditures is direct expenditures reported in the ASG.

Figure A-6: Dynamic Estimates with and without City Trends



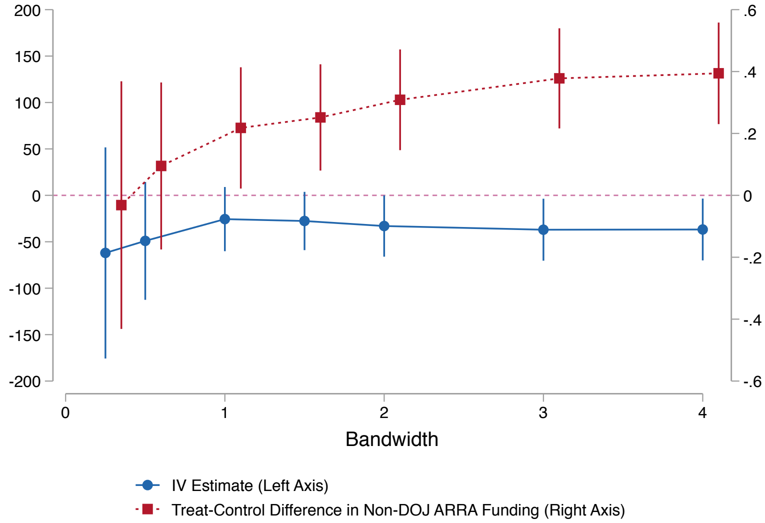
Notes: Same as Figure 5 except that results are presented when city-specific trends are excluded (hollow circle/squares) and included (solid circles/squares). Estimates with city trends are the same as Figure 5.

Figure A-7: Total ARRA Funding By Source, 2009–2013.



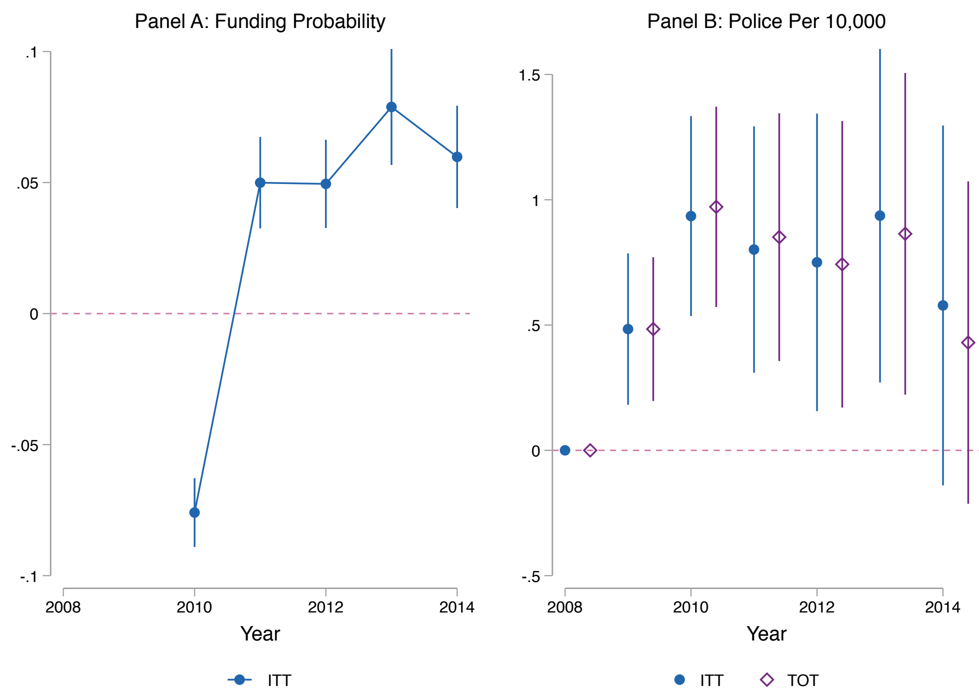
Notes: Sample is 3,227 agencies in main sample that could be matched to ZIP codes. Dependent variable is log ARRA funding per capita by source (DOJ versus Non-DOJ) at the FIPS place code level for the period 2009-2013, computed from FPDS data. Legend displays RD estimates using using the IK optimal bandwidth and a triangular kernel.

Figure A-8: IV Estimates and ARRA Funding Differences by Bandwidth



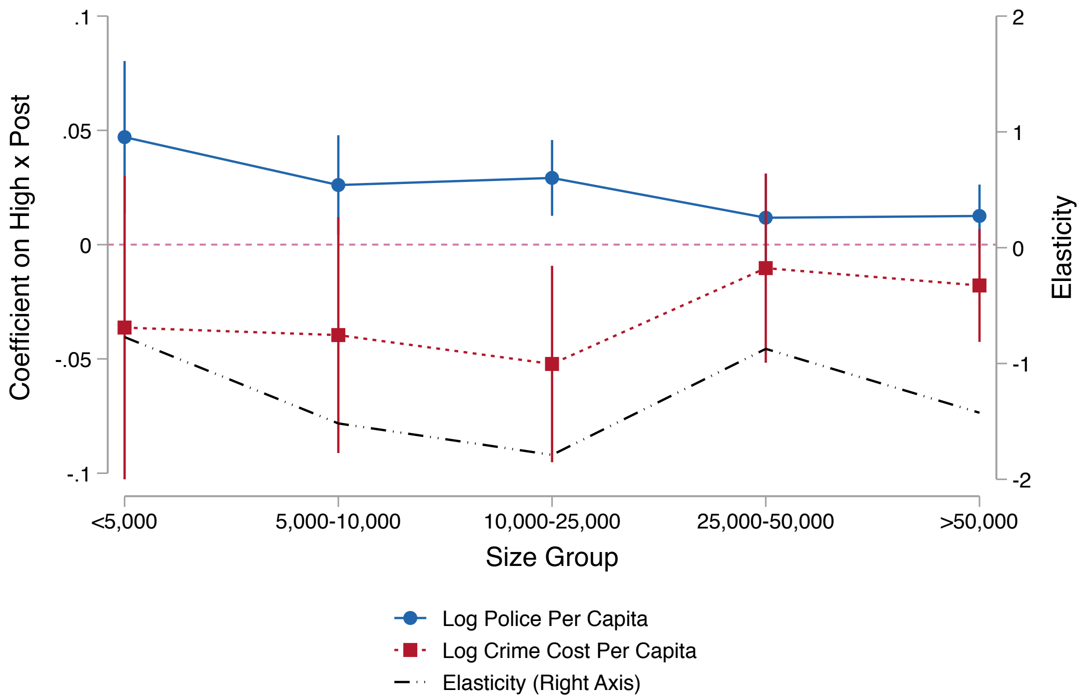
Notes: Sample is 3,227 agencies in main sample that could be matched to ZIP codes. Blue dots show IV estimates from main specification when only cities within the indicated bandwidth are used. Red squares show the coefficient on a regression of log total non-DOJ ARRA funding per capita on a high score indicator (estimated at the city, not the city-year level).

Figure A-9: Dynamic TOT Estimates of Effect of Grants on Police



Notes: Panel A plots estimates of the effect of exceeding the cutoff in 2009 on future funding. The coefficient for 2009 is 0.99 (0.0035) and is not shown for scaling purposes. Panel B plots ITT estimates (same as Figure 5) and TOT estimates. See text for details.

Figure A-10: Heterogeneous Effects by City Size



Notes: Figure plots reduced form and first stage estimates when using only cities in the denoted size group. I use a log specification here to account for differing means across groups. Note that main results using logs are very similar to those using rates as shown in Table A-7. Elasticity (right axis) is the ratio of the reduced form and first stage coefficients.

Table A-1: Sample Police Departments

ORI Code	City	Size Percentile	Population	Police	Crime Costs
NC05202	Maysville, NC	0	992	35	682
NY05139	Quogue Village, NY	1	1,086	133	337
AL02904	Coosada, AL	5	1,491	27	412
MD00807	Rising Sun, MD	10	2,063	31	962
OH02701	Gallipolis, OH	25	4,056	34	3,688
IL05008	Peru, IL	50	9,953	25	206
IL06003	Collinsville, IL	75	25,746	17	262
KS04609	Shawnee, KS	90	60,674	15	211
MO01002	Columbia, MO	95	99,941	15	488
TX22001	Arlington, TX	99	372,418	16	635
NY03030	New York, NY	100	8,244,256	43	486

Notes: Cities are eligible for inclusion in the sample if their population was above 1,000 more often than not over 2002-2014. Hence, there are some city-year observations with populations below 1,000.

Table A-2: Relationship Between Application Scores and Baseline Characteristics

	(1) All Municipal	(2) In Sample
Log Population	0.156***	0.213***
	(0.0135)	(0.0118)
Unemployment Rate	0.0267***	0.0309***
	(0.00388)	(0.00380)
Log Family Income	-0.650***	-0.502***
	(0.0449)	(0.0404)
Percent Nonwhite	0.0126***	0.00840***
	(0.000722)	(0.000743)
Percent Young Male	-0.00819***	-0.00639***
	(0.00161)	(0.00146)
Log Police Per Capita	-93.85***	14.77
	(12.94)	(11.80)
Log Violent Crime Per Capita	20.91***	23.60***
	(4.102)	(3.996)
Log Property Crime Per Capita	11.14***	18.39***
	(1.531)	(1.064)
Mean	.19	.21
R-Squared	.47	.57
Observations (Cities)	4598	4327

Notes: Robust standard errors in parentheses. Dependent variable is the standardized 2009 application score. Note that the mean is not zero because standardization is to the universe of applicants (i.e. including non municipal agencies).

Table A-3: Regression Discontinuity Power Calculations

		MDE when Bandwidth Equals					
Outcome	DD Estimate	0.25	0.5	1	2	3	4
Police	0.723	1.11	0.79	0.6	0.5	0.483	0.482
Crime Cost	-25.43	70.54	48.97	37.69	31.77	30.12	30.03
Violent Crime	-3.08	9.86	6.91	5.3	4.47	4.23	4.22
Property Crime	-11.13	29.46	20.79	15.19	12.41	11.76	11.73

Notes: See Appendix B for detail. Table shows the minimum detectable effect (MDE) for a regression discontinuity design under a linearity assumption where the outcome is change in police (crimes) per 10,000 and the denoted bandwidth is used to construct the sample. Column 2 shows the corresponding difference in difference estimate from the main specification.

Table A-4: Dynamic Difference in Differences Estimates

	(1) Police	(2) Crime Cost	(3) Violent	(4) Property
High x 2005	0.114	5.241	0.874	-1.684
	(0.109)	(6.520)	(0.920)	(2.756)
High x 2006	-0.0252	1.547	0.425	-3.281
	(0.145)	(7.866)	(1.111)	(3.337)
High x 2007	-0.0206	4.324	0.825	-3.115
	(0.116)	(6.901)	(0.974)	(2.734)
High x 2009	0.491***	-24.20***	-2.875**	-11.60***
	(0.154)	(8.461)	(1.195)	(3.924)
High x 2010	0.948***	-31.03***	-3.717**	-14.35***
	(0.202)	(11.60)	(1.612)	(5.343)
High x 2011	0.823***	-31.59**	-4.180**	-8.008
	(0.250)	(15.25)	(2.127)	(6.473)
High x 2012	0.779**	-36.44**	-4.794**	-9.694
	(0.302)	(17.65)	(2.432)	(8.118)
High x 2013	0.964***	-41.12**	-5.463*	-10.04
	(0.339)	(20.75)	(2.837)	(9.524)
High x 2014	0.607*	-37.91	-5.080	-8.531
	(0.366)	(23.37)	(3.190)	(10.69)
Mean	22.85	686.74	75.16	436.05
Controls	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327
Observations (City-Years)	47597	47597	47597	47597

Notes: Standard errors clustered at the city-level in parentheses. Dependent variable is police/crimes per 10,000 residents (columns 1,3-4) and cost-weighted crimes per capita (column 2). Regressions include city fixed effects. Regressions are identical to those graphed in Figure 5 and Figure 7 except that they include controls.

Table A-5: Sensitivity of IV Estimates to Controls

	(1) Crime	(2) Crime	(3) Crime
Police	-39.32**	-35.17**	-35.96**
	(15.86)	(15.19)	(15.52)
Mean	686.74	686.74	686.74
Elasticity	-1.31	-1.17	-1.2
F-Stat	19.89	20.96	20.36
Controls	No	Yes	Yes
Population as Control	No	No	Yes
Size x Year Effects	Yes	Yes	Yes
City Trends	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327
Observations (City-Years)	47597	47597	47597

Notes: Standard errors clustered at the city-level in parentheses. Dependent variable is crime cost per capita. All regressions include city fixed effects. Column 1 is the same as Table 2 except without controls. Column 2 is identical to Table 2. Column 3 adds population as a control.

Table A-6: Sensitivity of IV Estimates to Data Cleaning

	(1) Main	(2) No Winsorizing	(3) No Imputation	(4) No Cleaning
Police	-35.17**	-36.84**	-35.41**	50.62
	(15.19)	(16.55)	(17.06)	(76.25)
Mean	686.74	694.02	690.88	689.15
Elasticity	-1.17	-1.22	-1.18	1.7
First Stage Beta	.72	.73	.74	-.5
F-Stat	20.96	19.24	17.83	.46
Controls	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327
Observations (City-Years)	47597	47597	43026	44603

Notes: Standard errors clustered at the city-level in parentheses. Dependent variable is crime cost per capita. All regressions include city fixed effects. Column 1 is the same as Table 2. Column 2 uses non-winsorized crimes and police. Column 3 replaces imputed values to missing. Column 4 uses the raw data (no outliers deleted).

Table A-7: IV Estimates by Crime Type (Logs)

	(1) All Violent	(2) Murder	(3) Rape	(4) Robbery	(5) Assault	(6) All Property	(7) Burglary	(8) Larceny	(9) Auto Theft
Log Police	-1.352**	-2.768***	-2.970**	-2.294***	-0.732	-1.024**	-0.565	-1.334**	-1.552*
	(0.588)	(0.961)	(1.203)	(0.821)	(0.629)	(0.498)	(0.657)	(0.561)	(0.819)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Size x Year Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
City Trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Clusters (Cities)	4327	4327	4327	4327	4327	4327	4327	4327	4327
Observations (City-Years)	47597	47597	47597	47597	47597	47597	47597	47597	47597

Notes: Same as Table 5 except using a log-log specification. That is, the dependent variable is log crimes per capita and police is log sworn officers per capita. The first stage F-statistic is 25.4.