# The Effects of COPS Office Funding on Sworn Force Levels, Crime, and Arrests

*Evidence from a Regression Discontinuity Design*

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The authors are listed alphabetically, and all contributed equally to this study. This paper was supported by grant # 2011CKWX0098 from the U.S. Department of Justice Office of Community Oriented Policing Services, by operating support to the University of Chicago Crime Lab from the Joyce, MacArthur, and McCormick foundations, and by visiting scholar awards to Ludwig from the Russell Sage Foundation and Sciences Po LIEPP. Thanks to Matthew Scheider for very helpful discussions and to Roseanna Ander for all of her support for this project. We are grateful to Seth Bour, Stephen Coussens and particularly Samantha Edds for their outstanding help assembling and analyzing the data. All opinions and any errors are our own.

Recommended Citation: Cook, Philip, Max Kapustin, Jens Ludwig, and Douglas Miller. 2017. "The Effects of COPS Office Funding on Sworn Force Levels, Crime, and Arrests: Evidence from a Regression Discontinuity Design." Washington, DC: Office of Community Oriented Policing Services.

Published 2017

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# I. Introduction

Crime is a major social problem, with social costs in developed countries equivalent to as much as 10 percent of GDP (Anderson 1999; Entorf and Spenger 2002; Ludwig 2006).[1] But controlling crime also imposes costs on society, a problem that has become of growing concern within the United States following the dramatic rise in incarceration rates over the past 40 years. Moreover, both of these problems—crime (especially violent crime) and crime control—are very regressive in their direct impacts. For example, African Americans represent 13 percent of the total population in the United States but account for nearly 50 percent of homicide victims[2] and nearly 40 percent of all state prisoners.[3]

The persistence of high levels of violent crime in disadvantaged urban areas, together with the substantial social harms of incarceration, have led to a surge of policy interest in alternative ways to control crime. Unfortunately, there are few alternative strategies that have much rigorous empirical support for their effectiveness—and for those that do, few have been shown to work at large scale. Indeed, the frequently-cited "proven" programs tend to rely on either changes to how government agencies work, or on fairly specialized inputs, both of which raise challenges for scale-up.[4]

This paper presents new evidence about one particularly promising approach to controlling crime that does not rely (at least directly) on incarceration and, unlike many other approaches, would seem to be relatively easy to scale: hiring more police. The canonical economic model of crime from Becker (1968) predicts that police deter criminal behavior by increasing the expected costs of punishment to potential offenders. Police may also reduce crime by making arrests that result in the incarceration and hence incapacitation of active offenders, or by engaging in preventive problem-solving activities. While the

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[1] Despite the drop in crime rates during the last two decades, our rates of crime–particularly the most socially costly violent crimes–remain far above those of other developed nations. For example, the homicide rate per 100,000 in 2010 in the United States (4.8) was four times that of the United Kingdom (1.2) and fully six times that of Germany or Japan (0.8). Crime remains an even bigger problem in many developing country contexts, with 2010 homicide rates in places like El Salvador and Honduras equal to 66 and 78 per 100,000–that is, the country-wide homicide rate in those countries is about what we see among the highest-risk population in the United States.

[2] Figure is for 2014; see CDC 2015, http://webappa.cdc.gov/sasweb/ncipc/mortrate10_us.html.

[3] Figure is for 2013; see Table 11 in Carson 2015, http://www.bjs.gov/content/pub/pdf/p14.pdf.

[4] See, for example, the list of what have been deemed to be "proven programs" by the Coalition for Evidence-Based Policy; http://evidencebasedprograms.org/about/crimeviolence-protection. There are a few exceptions, such as raising the compulsory schooling age or increased taxes on alcohol (see Cook, Ludwig, and McCrary 2011 for a review), but political constraints seem likely to limit the amount of additional crime reduction we may be able to achieve through changes in those policies.

United States has increased spending on police over the past several decades, the growth in police per capita pales in comparison to the growth in corrections expenditures (Cook and Ludwig 2011). The prospect of diminishing marginal returns raises the possibility of shifting resources away from imprisonment towards police to reduce crime at no extra cost. And one thing that almost every police department in the country knows how to do is hire more police.

Yet the effects of additional police spending in the United States remain uncertain. A blue-ribbon panel convened by the National Academy of Sciences to examine the evidence on policing was agnostic about whether police spending has any systematic relationship with crime (Skogan and Frydl 2004). Many leading criminologists tend to be more negative. For example, policing expert Lawrence Sherman has argued, "The weight of the research shows that the effect of police on crime depends heavily on what police actually *do*, rather than how many are on the payroll" (2002, p. 385).

However, studying this question is not empirically straightforward. One key challenge, as Chalfin and McCrary (forthcoming) show, is overcoming measurement error in the key explanatory variable of interest (police per capita). Another challenge is the problem of identification, which stems from the possibility that the number of police in an area may be both cause and consequence of local crime rates (Fisher and Nagin 1978).

In this paper we present new estimates for the effects on crime from adding more police, which we believe have an unusually strong claim to identification. Specifically, we take advantage of a regression discontinuity (RD) design created by the U.S. Department of Justice's COPS Hiring Program (CHP) in 2009.[5] That year, the COPS Office received over $8 billion worth of requests for the roughly $1 billion of funds available, under an expanded version of the CHP called the COPS Hiring Recovery Program (CHRP). Proposals were scored based on a formula related to local crime rates and budget conditions, and for the most part funded in descending rank order until funding ran out.[6] We use this discontinuity in police spending across jurisdictions to look for similar discontinuities in crime and, to determine whether deterrence is a key mechanism, on overall arrest rates as well.

We show that the CHP grant competition increased the size of police forces in 2009 relative to 2007–2008 by almost 2 percent in jurisdictions with scores near the funding threshold, though our estimates are statistically insignificant and of smaller magnitude in later years. We also find suggestive evidence that this discontinuity in police spending led to declines in total Uniform Crime Report (UCR) Part 1 offenses relative to 2007–2008 of approximately 5 percent from 2010–2012. Finally, we find some evidence that arrests decreased in 2010 among funding recipients, consistent with the conclusion reached by Owens (2013) that deterrence or some other preventive policing activities may be a key

[5] Operated by the Office of Community Oriented Policing Services (COPS Office), the program awards grants to state, local, and tribal law enforcement agencies for the hiring of full-time sworn officers.

[6] Several complications to this system were introduced by efforts to ensure adequate funding to each state and to police agencies of differing size. We discuss these features of the scoring system and their implication for our analysis later in the paper.

mechanism. Because these results do not take into account subsequent rounds of CHP funding in 2010–2012, they are likely to understate the effects of policing grants on crime.

# II. Previous Studies

A large literature in criminology dating back decades has found little systematic relationship between police spending and crime using cross-sectional data or repeated cross-sections. For example, Cameron's (1988) widely cited review noted that 18 of the 22 studies examined found a null or positive relationship between police spending and crime. Franklin Zimring's (2011) account of the causes of New York City's miraculous crime drop since 1990 gave little credit to the one-third increase in the size of the police force that occurred over the 1990s, arguing that the research on police spending is "mixed."[7]

The identification challenge to this line of research comes from the simultaneity that arises when policymakers respond to high levels or positive changes in crime rates by devoting more spending to police. The influential paper about this simultaneity problem by Franklin Fisher and Daniel Nagin as part of the National Academy of Science's 1978 review of deterrence research pushed most economists out of this business for decades.

A major advance in research on this topic came with Levitt (1997), who exploited variation in police spending due to the timing of electoral cycles for identification and found a more pronounced negative relationship than previous studies. Levitt's estimated elasticity of violent (property) crime with respect to police was around -1.0 (-0.3). While Levitt's estimates turned out to be less precisely estimated than originally reported (McCrary 2002), and hence statistically insignificant, this seminal paper set off a search for other "natural experiments" to identify the effect of police on crime.[8] Levitt (2002) himself utilized the number of firefighters as an instrument, with strong results. In this same spirit, DeAngelo and Hansen (2014) investigated the effects of an abrupt 36 percent reduction in the Oregon State Police capacity engendered by a voter referendum on taxation; the number of traffic citations issues fell in proportion, and highway traffic fatalities increased 12 to 14 percent. Several subsequent studies have used changes in police deployment patterns following terrorist attacks to try to identify the relationship between police resources and crime. Di Tella and Schargrodsky (2004) use data from Buenos Aires,

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[7] For additional reviews see Eck and Maguire (2000), and Sherman (2011). Other studies have used time-series variation for identification. Marvell and Moody (1996) used a Granger-causality approach with panel data, while Corman and Mocan (2000) use monthly data from New York City, exploiting the fact that there is some delay between changes in crime and when the NYPD can have new officers hired, trained and deployed, to find a significant negative relationship between police and burglary (but not other crimes).

[8] A different solution to this statistical challenge, implemented largely by criminologists, has been field experiments. These experiments have not attempted to assess the marginal product of additional resources in reducing crime, but rather have tested the effects of changing how police use the resources they already have (Sherman and Rogan 1995; Cohen and Ludwig 2003; Skogan and Frydl 2004; Braga 2008; Kennedy 2011; Sherman 2011). In practice, getting big-city police departments to make sustained changes to their standard operating procedures can be challenging.

Argentina before and after a terrorist attack against a Jewish center that subsequently led to increased police presence at Jewish and Muslim religious centers throughout the country. They find an elasticity of car thefts with respect to police on blocks where such religious centers were located of -0.33, with no such relationship found on nearby blocks.[9] Klick and Tabarrok (2005) use police redeployments in Washington, DC after terror alerts, but do not have data on local-area police presence and so cannot estimate an elasticity. More recently, Draca, Machin, and Witt (2011) use data from increased policing in central London after two waves of bombings or attempted bombings, to estimate a crime—police elasticity of -0.3 to -0.4, with generally similar elasticities for violent crimes and property offenses.

A key challenge for this type of study design is the possibility of correlated shocks: in addition to changes in police presence after a terrorist attack, it is reasonable to believe that there will be changes in such crime-related variables as commercial activity, pedestrian presence and vigilance, or the use of private security guards. Draca et al. try to overcome this problem by taking advantage of a second source of variation that comes from the termination of the stepped-up policing program, which may be less likely to coincide with other changes in local criminogenic conditions. How the Draca et al. results translate to the U.S. context remains unclear. Moreover, their paper does not generate a specific estimate for the elasticity of murder with respect to police, which as Chalfin and McCrary (forthcoming) note is particularly important in the U.S. context given that a majority of the social costs of crime are accounted for by murder. This is particularly true for America's cities, where questions about how to improve public safety are most pressing.

A very different approach is taken by Evans and Owens (2007), who use variation across jurisdictions in the size of police-officer hiring grants awarded through the COPS Hiring Program under the Clinton Administration.[10] They show that the size of these grant awards are correlated with the prior levels but not trends in crime in recipient jurisdictions, and present difference-in-difference estimates that compare trends in crime before and after the hiring grants in areas that received larger versus smaller grants. Their preferred point estimates of elasticities for crime with respect to policing levels are -0.26 for property crimes and -0.99 for violent crime. In a follow-up study, Owens (2013) shows that arrests as well as crime rates declined in areas with relatively larger CHP grants, suggesting that deterrence rather than incapacitation was the key mechanism through which police reduce crime. This is an important finding that suggests putting more resources into policing can help reduce crime without exacerbating the problem of "mass incarceration."[11]

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[9] They do not examine other crimes.

[10] A related analysis using data from the Street Crime Initiative in England and Wales found that the police districts that received funding (10 of 43) enjoyed a reduction in robbery that was large enough to pass the cost-benefit test (Machin and Marie 2011).

[11] Relatedly, Cook and MacDonald (2011) find that investments in private security following the creation of a business improvement district in Los Angeles has the effect of reducing crime and also reducing arrests.

With the publication of a new study by Chalfin and McCrary (forthcoming), the literature has in effect come full circle by returning to an analysis of naturally occurring variation in police spending rather than focusing on just part of the variation that can be explained by some natural experiment. The authors argue that the null or perversely signed results from regressing crime on police result from the fact that statistics on the number of police employed by a city are highly error-prone. They compare two measures of the number of sworn officers, the UCR measure (based on reports sent to the FBI by local police departments) and the Annual Survey of Government Employment and Payroll (ASG) conducted by the U.S. Bureau of the Census and based on reports on city employment submitted by a city official outside of the police department. Chalfin and McCrary analyze the two series for 242 large cities for the period 1960–2000; while they are highly correlated in levels, the correlation in year-to-year percentage changes is just 0.22. They note that if the measurement errors in these two series are independent, that one can serve as an instrument for the other in a classic errors-in-variables analysis. Once they "solve" the errors-in-variables problem, they generate findings suggesting that growth in police manpower reduces both violent crime rates (elasticity = -0.34) and property crime rates (elasticity = -0.17), both highly significant. While they admit that there may indeed be some simultaneity bias, the potential mechanism is not obvious: in the regression they lag the independent variable (growth in police) and include state-year fixed effects.

The Chalfin-McCrary approach has the clear advantage of using the full range of natural variation, rather than the more idiosyncratic and limited variation from a natural (or true) experiment. Their estimates tend to be smaller than those reported by Evans and Owens (2007), but are much more precisely estimated. If the only source of bias is simultaneity—that cities tend to hire more police in reliable anticipation of an increase in crime—then we can sign the bias (negative) and treat the results as a lower bound on the true prevention effects.

But in fact there is a more far-reaching problem with natural-variation data, namely that if we do not know the causes of the variation, it is hard to rule out correlated processes that may have either positive or negative effects on crime. For example, the growth in police may be the result of growth in city revenues, which may also be used to finance expansions of other programs that affect crime rates (such as regulation of alcohol sales, or truancy-prevention programs). We conclude that while Chalfin and McCrary have made an important contribution, there remains great value from studying large-scale natural experiments that involve known processes that affect police resources.

# III. COPS Hiring Program Grants Under the Obama Administration

## COPS Hiring Recovery Program background

On February 13, 2009, under the American Recovery and Reinvestment Act of 2009, P.L.111-5, the COPS Office was awarded $1 billion in funding for the COPS Hiring Recovery Program (CHRP) with the goal of creating roughly 5,000 law enforcement positions funded for a period of three years at the state, local, and tribal levels.[12] The CHRP was introduced to alleviate some of the pressure faced by law enforcement agencies (LEA) across the nation from the deteriorating fiscal climate. Unlike previous iterations of COPS hiring grant programs (the CHP) that included mandates regarding local match (25 percent) and salary caps ($75,000),[13] grants provided to LEAs under CHRP would cover the full entry-level salary and fringe benefits of every officer hired for a total of three years.[14] Officers could be newly hired, previously laid off, or scheduled to be laid off. By increasing the capacity of local law enforcement through the hiring of additional officers, the CHRP sought to "increase their community policing capacity and crime prevention efforts."[15]

## Program eligibility

Between March 16 and April 14, 2009, the period of time in which the CHRP application was open, the COPS Office received 7,272 applications requesting $8,322,695,721 to hire 39,346 officers. While the CHRP was open to any state, local, or tribal law enforcement agency, only those agencies with primary law enforcement authority (the "first responders" for their jurisdiction) and full arresting and police powers were eligible to apply.[16] An eligible LEA could request funding for "career law enforcement

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[12] American Recovery and Reinvestment Act of 2009, H.R. 1—16.

[13] OAG 1994, "Grant Funding," http://www.justice.gov/opa/pr/Pre_96/October94/590.txt.html.

[14] A LEA could hire officers at a higher rate than entry-level salary and fringe benefits, as was often the case with re-hires and preventions of future lay-offs, but any costs exceeding those levels must be covered by the LEA.

[15] COPS Office, "CHRP Background and Award Methodology," accessed November 9, 2016, http://www.cops.usdoj.gov/default.asp?Item=2267.

[16] Start-up LEAs, consortiums of LEAs, and non-Federally Recognized Tribes' LEAs were also ineligible to apply.

officers" in three hiring categories:[17] (1) new-hire, full-time sworn officers; (2) rehiring of laid-off sworn officers (as a result of budgetary restrictions),[18] or (3) "rehiring" of sworn officers slated to be laid off in the future (as a result of budgetary restrictions).[19]

While LEAs were not capped in the number of officers for whom they could request funding, grantees were required to commit to funding these positions for a period of 12 months following the completion of the 36-month grant. Additionally, the non-supplanting requirement mandated that any funds disbursed for the purposes of hiring additional officers must be used to supplement the LEA's budget for sworn officers.[20] Using CHRP funds to supplant law enforcement budgets was expressly prohibited; any additional officers hired as a result of a CHRP grant must be above and beyond the number of sworn officers that would have been employed had the grantee not received COPS funding.[21]

Of the 7,272 initial applicants, 7,202 eligible applicants make up the sample used for the purpose of this analysis.[22] Their applications are summarized in table 1. The average agency had 74 officers and requested funds to hire five additional officers. 1,046 agencies received CHRP funding of almost $1 billion. Agencies were alerted of their awards in July 2009, with an official award start date of July 1, and the public announcement coming July 28. Agencies had to send a form to the COPS Office signed by top government executives and law enforcement agencies with financial and programmatic authority in their jurisdiction within 90 days of receiving the award. Once the COPS Office received the signed documents, funds were made available, although most hiring began with the official grant announcement.

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[17] As defined in the "COPS FY2009 Application Guide: COPS Hiring Recovery Program," a career law enforcement officer is "a person hired on a permanent basis who is authorized by the law, or by a state, local, or tribal agency, to engage in or oversee the prevention, detection, or investigation of violations of criminal laws" (http://www.cops.usdoj.gov/pdf/e03094188-CHRP-v-2.pdf).

[18] The two main differences between the hiring of new entry-level officers and the re-hiring of previously laid-off officers were (1) the elimination of lag time between grant awards and officer deployment and (2) that re-hires were generally not for entry-level positions.

[19] Grantees were required to continue funding the positions slated for future lay-offs out of their local law enforcement budget until the date of the scheduled lay-offs.

[20] https://cops.usdoj.gov/Default.asp?Item=2265

[21] Other unallowable costs for which requests would not be funded included: requests for equipment, training, vehicles, existing locally-funded officers, salary and fringe benefits higher than an agency's entry-level salary and fringe benefits, civilian/non-sworn personnel, part-time officers, overtime costs, furloughed officers, and contractual arrangements for law enforcement services.

[22] 7,203 agencies submitted eligible requests, while 58 had been ineligible to apply and 11 withdrew their funding requests. (One agency withdrew after final analysis, which is still included in these totals.)

## Selection criteria

### *Application questions, weighting, and scoring*

To apply for funds, LEAs were required to respond to a series of questions regarding their locality. Basic information regarding the jurisdiction, its law enforcement capacity, and the population it served was collected, cleaned, and verified by the COPS Office (see appendix A). In the demonstration of need for federal assistance, each agency was also asked a series of questions related to its fiscal health, crime rates, and community policing efforts.

As funding for the CHRP was directed to the COPS Office through the American Recovery and Reinvestment Act, with the goal of creating and preserving jobs in those jurisdictions most affected by the recession, significant weight was given to fiscal health questions, accounting for 50 percent of the overall ranking. Within the fiscal health section, questions were further broken down into the categories of agency fiscal health (18 percent of overall), local government fiscal health (13 percent), and community socioeconomic health (19 percent). In line with the COPS Office's mission of promoting public safety through community policing, crime rate questions accounted for 35 percent of the overall ranking and community policing questions accounted for 15 percent. Each application question was scored according to a CHRP rubric using a 100-point scale and the weights described above.

After each question was assigned a score, these were combined to create composite ranking scores for each LEA in each of the three categories: fiscal health, crime rates, and community policing. The three composite scores were then summed to determine the overall ranking score for each applicant.

### *CHRP state minimum and population size requirements*

As a stipulation of funding, the COPS Office was required to disburse at least $5 million to each state or territory from which eligible applicants submitted proposals. To satisfy the state minimum requirement, the highest ranked agencies in each state were identified and awarded up to $5 million. Only after each state minimum requirement had been met were the remaining funds allocated.

Another stipulation of the CHRP was that half of all funds needed to be distributed to agencies serving populations greater than 150,000 ("large agency") and the other half to agencies serving populations less than 150,000 ("small agency"). After allocating funds to satisfy the state minimum requirement, agencies were pooled across states, separated into groups based on their size, and ranked in descending order within each group. Remaining funds were awarded to agencies with the highest scores until half of all available CHRP funds were disbursed in each of the two groups.[23]

[23] Because the state minimum requirement had to be met before further disbursement of funds, agencies in 27 states were awarded funds ahead of higher-scoring applicants from other states.

### *Award capping*

With no prior information on the number of officers a LEA might seek funding to hire/retain, the COPS Office did not cap the number of officers an applicant could request. However, to ensure the varying needs of law enforcement groups across the country were met, the COPS Office, in consultation with the Attorney General's office, established a capping system that imposed a ceiling on the potential size of a given award. It was believed that without a cap on the number of officers a LEA could receive, a few agencies would have received nearly all of the available CHRP funds.

After all eligible applicants received an initial ranking, the COPS Office limited the awards of grantees so that no agency could receive more than 5 percent of its current total force strength, up to a maximum of 50 officers.[24] However, every eligible agency qualified to receive at least one officer.

[24] For the purposes of determining total force strength, part-time officers were counted as one-half of a full time officer in the summation. Of the 85 applicants with more than 1,000 sworn officers, 24 requests were affected by the 50 officer cap.

# IV. Data

In this section we describe our different data sources on officers, crime, and arrests, which cover the years 2005–2012. While it has long been known that UCR crime and arrest data contain considerable measurement error, Chalfin and McCrary (forthcoming) show that the same is true for UCR data on sworn force size as well. As a result, we employ extensive data cleaning methods to detect potential outliers in these data, which we detail in appendix B.

## Sworn officer force data

We follow Chalfin and McCrary (forthcoming) and use two different data sources to create a measure of the number of police officers in a given jurisdiction in a given year: the UCR and the ASG. These two data sources are not entirely comparable; for example the UCR covers more agencies and measures police as of October 31, while the ASG is a sample and measures employment as of the end of March. Since the UCR has much greater coverage among our applicant cities than does the ASG, we rely on the UCR as our default data source. Nevertheless, as Chalfin and McCrary show, there is useful "signal" that can be combined from both datasets.

Since the UCR measures police force strength as of the end of October, any hiring by CHRP grantees that occurred between July and October 2009 could have increased sworn force size during that year of our analysis. Therefore, for the purposes of documenting the effects of the CHP program on the number of officers in a jurisdiction, 2009 is considered a "post-treatment year." We also obtained quarterly grant progress reports from the COPS Office and found that 47 percent of all agencies with 2009 awards hired all officers awarded by September 30, 2009, and 74 percent of agencies hired all officers award by the end of 2009, meaning many had completed their hiring by the end of October.

## Crime data

Our main outcome measures are UCR Part 1 offenses.[25] The UCR data collection program was designed for national-level crime trend analysis,[26] and has several well-known sources of measurement error when analyzed at the level of the individual law enforcement agency. Maltz and Weiss (2006) describe

[25] Part 1 offenses consist of manslaughter, forcible rape, robbery, and aggravated assault (summing to violent crimes), as well as burglary, larceny, and motor vehicle theft (summing to property crimes). Our models also use the index violent crimes, property crimes, and total crimes.

[26] "Uniform Crime Reporting," accessed November 9, 2016, https://www.fbi.gov/about-us/cjis/ucr.

some of the problems, including substantial nonresponse (due to voluntary reporting), misreporting, and gaps in the data. Maltz generously shared with us his version of the UCR data through 2012.

Unlike with officer data, UCR crime rates capture outcomes measured over the course of the entire calendar, rather than a single snapshot as of the end of October. It is therefore less clear whether we should count 2009 as a "post-treatment" year from the perspective of measuring impacts on our main crime outcomes. Even jurisdictions that used their CHRP grants to hire officers very quickly would have experienced a majority of the calendar year at their pre-CHRP staffing levels. Additionally, a Bureau of Justice Statistics study on police academy training found the median duration of basic training was 18 weeks, and ranged anywhere from 4 weeks to 6 months.[27] Slightly over 70% of all officers awarded are new hires (instead of frozen layoffs or previously laid off officers) and require academy training, suggesting that the full effect of any additional officers hired with COPS Office funding in 2009 would not be felt until 2010 at the earliest.

## Arrest data

For purposes of understanding the mechanisms through which additional police may reduce crime, we also examine the effects of CHRP grants on the number of arrests made within a jurisdiction. As Owens (2009) notes, a decline in arrests accompanying a decline in crime provides suggestive evidence that more police reduce crime through some form of general deterrence. This issue is also important for cost-benefit analysis, to determine whether the costs of additional incarceration accompany the costs of additional police.

We use UCR arrests for Part 1 offenses as our primary measure of arrest activity within a jurisdiction. The UCR arrest data are considered more error-prone than the crime data. As Maltz (1999) notes, the UCR only reports arrest data when it is sent in for both juveniles and adults. As a result, detecting outliers and nonreporting, particularly in smaller jurisdictions, is more difficult with the arrest data.

## Population data

Our final dataset contains the number of people served by each agency, which is used to calculate outcomes measured on a per capita basis. Although the UCR records this information for each agency that reports to the FBI, the accuracy of these data is not ideal.[28] As a result, we use cleaned UCR population data generously provided to us by Michael Maltz, which he obtained from the National Archive of Criminal Justice and subjected to additional data cleaning.

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[27] "Training / Academy Life," accessed November 9, 2016, http://discoverpolicing.org/what_does_take/?fa=training_academy_life.

[28] In UCR data obtained directly from the FBI, most agencies experienced large population increases from 2008 to 2009, followed by decreases of a similar magnitude in 2010.

# V. Estimation Approach

Our study provides new evidence on the effects of police spending on crime by exploiting a natural experiment generated by the CHRP grants awarded by the Obama administration. In what follows, we focus on the initial funding competition carried out in 2009, which disbursed a total of $1 billion. This was followed by smaller grant competitions that awarded $300 million in 2010, almost $200 million in 2011, and just over $100 million in 2012. Agencies that did not win an award in 2009 could reapply for funding in subsequent years, and many did so successfully. As a result, the estimates we present do not capture the full effect of CHRP funding on crime, a point we plan to address in future versions of this work.

As discussed in Section III, agencies were assigned a score based on a weighted average of their responses to a variety of questions concerning their financial health, community policing practices, and local crime rates, relative to their similarly-sized peers. After funds were disbursed to ensure that each state received at least $5 million, the remaining funds were allocated to agencies in descending order of their assigned score. Funds were distributed until both small (< 150,000) and large (> 150,000) agency groups each received $500 million.

Because of the state minimum funding requirements and the separate grant competitions for large and small agencies, our setting is slightly more complicated than a standard regression discontinuity (RD) design.[29] Specifically, depending on their state and size, the cutoff that determines an agency's funding status can differ. For an agency located in a small state that was only awarded $5 million in funding, the relevant cutoff is the score received by the last agency funded in that state. For an agency located in a state that received more than $5 million in funding, the relevant cutoff is a function of agency size: because small and large agencies were funded separately in descending order of their scores until funding ran out, there are both a small (65.75) and large (68.75) agency cutoff score.

The key assumption underlying this design is that, for agencies close to a cutoff, the outcomes we measure must not differ systematically with their position above or below it except for the effect this has on their likelihood of receiving a CHRP grant.[30] That is, small differences in agency scores are "as good as" randomly assigned near the cutoff and result in exogenous variation in funding for officers.

We use the data described above to estimate equation (1) below, where $Y_{it}$ is our dependent variable of interest for agency *i* in year *t*, which could be sworn force size, crime, or arrests per capita, and $D_i$ is an

[29] After extensive conversations with administrators at the COPS Office, we are able to perfectly reconstruct the process by which agencies were awarded funding.

[30] This is equivalent to assuming smoothness in potential outcomes near the cutoff (Porter 2003).

indicator for being above or below the cutoff. We include a flexible control for a normalized version of an agency's final ranked score, $Z_i$.[31]

$$(1) \qquad Y_i = \beta_0 + \beta_1 D_i + f(Z_i) + \varepsilon_i$$

In a very small handful of cases, agencies with scores below the cutoff received CHRP funding while those with scores above the cutoff did not.[32] For this reason our setting is, in a strict sense, a "fuzzy" RD, where having a score above the cutoff generates a discontinuity in the probability of receiving a CHRP grant in 2009. Our estimate for $\beta_1$ in equation (1) is essentially an intention-to-treat (ITT) estimate.

One estimation issue is how to control for the function $f(Z_i)$ in equation (1). Rather than imposing a functional form assumption, we use a nonparametric RD approach refined by Porter (2003). This model uses local linear regressions (Fan 1992) to produce estimated treatment impacts from the difference in the left and right side limits of discontinuity. We estimate this for a chosen bandwidth and use triangle kernel weights. This means we estimate a kernel-weighted linear regression once to the left of the CHRP cutoff and once to the right of the cutoff, then difference the results for the treatment impact (in our case focusing on the means). Our use of triangle kernels is based on work by Cheng, Fan, and Marron (1997) showing them to be optimal, if not very sensitive to choice of kernel (Fan and Gijbels 1996). Our standard errors are derived from Porter (2003).

[31] $Z_i$ is calculated by subtracting the relevant cutoff from an agency's score and dividing by the standard deviation of all scores.

[32] Four agencies had scores below the cutoff but received COPS funding in 2009: Vermillion Police Department, City of Martin, and Mitchell Department of Public Safety in South Dakota, and the Brownsville Police Department in Tennessee. Two agencies had scores above the cutoff but did not receive COPS funding in 2009: Huron Police Department in South Dakota, and Hughes Police Department in Arkansas.

# VI. Results

As noted above, we focus in this report on using the 2009 CHRP funding competition as our "natural experiment" to estimate effects on crime. Because it is possible that there could be some lag in the effects of changing police resources or practices on crime, we examine the effects of winning a 2009 CHRP grant on crime in 2009 and in subsequent years for which we have crime data (2010–2012). We begin by presenting estimates for how receipt of a COPS Office hiring grant changes police spending, and then turn to estimated effects on crime.

## Effects of CHRP grants on sworn force size

In figure 1, we present the results of being above versus below the CHRP funding threshold on the likelihood of receiving an award. We show this figure to confirm that we have been able to reproduce the CHRP funding process with our data and estimation model. Receipt of a CHRP grant is on the y-axis and the running variable along the x-axis is our standardized 2009 CHRP grant application score. The figure also shows the nonparametric estimate for the function relating the agency's normalized ranking score to the dependent variable, $f(Z_i)$, as well as the implied discontinuity (that is, our estimate for $\beta_1$ from equation 1 above) estimated using a bandwidth of 0.8 standard deviations from the scoring cutoff. So that readers have some sense for the raw data underlying this figure, it also includes cell means (shown in triangles) and their 95 percent confidence intervals (bars) from grouping the data into 10 evenly distributed bins on each side of the cutoff.

We see in Figure 1 that the probability of receiving an award jumps from essentially 0 (0.013) to almost 1 (0.981). The discontinuity is not exactly equal to one due to a handful of cases where agencies below the cutoff were funded and agencies above the cutoff were not, as described above. Figure 2 reproduces this exercise using CHRP grant award amount as the dependent variable. The mean funding amount for those below the cutoff is $1,922, and jumps to $622,155 at the funding threshold. Both estimates in Figures 1 and 2 are highly significant.

To determine which form of the outcome variable will allow us to best detect the effect of COPS funding on officers (and, later, crime and arrests), we estimate several "placebo" regressions where being above the cutoff for COPS funding in 2009 is the "treatment" and different measures of sworn force size in the pre-treatment year of 2008 are the outcomes.[33] These results are displayed in appendix table 1. On the basis of this exercise, we chose the difference in log officers per capita in a given year relative to log officers per capita in a baseline year (here, 2005–2006) as our preferred specification, shown separately in appendix figure 1. This specification yields the cleanest "zero" estimate during the pre-treatment

[33] The findings are qualitatively similar using outcomes in 2007.

period and allows us to choose a model based on its performance in a setting where the expected impact is known, thereby reducing bias introduced by potential specification mining.

In figure 3, we show the estimated effect of being above the CHRP cutoff on sworn force size in the first "post-treatment" year of 2009, as measured by log officers per capita in 2009 relative to the most recent pre-treatment baseline of 2007–2008. Agencies were notified of their CHRP application outcome in July 2009, which gave them some time to hire new officers or prevent layoffs before they reported their sworn force size to the UCR by October 31, 2009. We estimate a discontinuity in officers per capita relative to the baseline of 1.9 percent ($p = 0.033$).

One question is the degree to which the effects of these grants on sworn force size persist over time. These effects may dissipate if local governments reduce their own spending on police and shift those resources across other public functions. Such a shift is a standard prediction of public finance, although in practice many empirical studies find a so-called "flypaper" effect, where resources earmarked for a given use tend to disproportionately concentrate on that use. Even if the funds remain in police budgets, they may be put to uses other than hiring officers or forestalling layoffs, such as paying overtime or purchasing additional equipment. Finally, another reason why the estimated effect on police resources could fade over time is that agencies not receiving funding in 2009 may have successfully done so during the CHP grant competitions in 2010–2012, causing staffing levels in "control" agencies to increase.

In table 2 (columns 1–4), we show the estimated effects of being above the cutoff on sworn force size over time for all agencies. These estimates, along with 95 percent confidence intervals, are also shown visually in figure 4. The magnitude of the effect in 2010, the first full year after the 2009 CHRP funding was issued, is approximately 2 percent, very similar to the effect in 2009 though statistically insignificant. The estimates decline in magnitude in 2011 and 2012, though the confidence intervals around each are too wide to draw any conclusions about fadeout.

We also examine whether the effect of CHRP funding differed by population size or funding requested. Columns 5–8 in table 2 contain estimates using agencies serving jurisdictions with populations below 50,000. Because the bulk of agencies applying for COPS grants serve small jurisdictions, it is perhaps unsurprising that these estimates appear similar to those for all agencies. We also take advantage of the fact that agencies vary in the size of their police funding requests. Recall that agencies could request any number of officers—provided they could support their cost for one additional year after the three-year grant concluded—but the COPS Office imposed a cap on awards of 5 percent of force size, up to a maximum of 50 officers. We define an agency's potential increase in force as the maximum number of officers eligible to be awarded divided by the sworn force reported on the 2009 application. Among agencies with potential increases in force of 5 percent or more, the estimated effects of CHRP grants are considerably larger: 3.2 percent ($p = 0.018$) in 2009–2010, and declining to approximately 2 percent ($p > 0.1$) in 2011–2012. Though we cannot reject these estimates being of the same magnitude as those for all agencies, this provides suggestive evidence that agencies wishing to make larger expansions to their police forces did so upon receipt of CHRP funding.

To summarize, we document that the 2009 CHRP grant competition appears to have successfully increased the size of winning agencies' police forces in 2009. Our estimates of officer impacts beyond this point are statistically insignificant, but suggest that this effect persisted into 2010 before declining somewhat in 2011–2012. These effects also appear to be larger for agencies requesting greater increases to the size of their police forces.

## Effects of COPS Office hiring grants on crime

Table 3 and figure 5 present estimates of the effects of COPS Office funding on total UCR Part 1 crime rates from 2009–2012. In 2009, the estimated effect on total Part 1 crime rates is small and statistically insignificant. By 2010, the first full calendar year after the CHRP funding was issued, the decline in crime relative to 2007–2008 is 4.5 percent ($p = 0.052$). As discussed in Section IV, both due to the timing of when crime is measured in the UCR and delays in deploying newly hired officers, 2010 is the first year in which we expect to see an effect from grants issued in the summer of 2009. In 2011, the magnitude of the estimate increases further to 5.5 percent ($p = 0.034$). Finally, in 2012, the magnitude remains similar (5.1%) but is estimated with somewhat less precision ($p = 0.079$). These estimates appear slightly larger for smaller agencies (columns 5–8), and slightly smaller for those with potential increases in force of 5 percent or more, but as with the results on sworn force size we lack the precision to say anything more definitive.

Tables 4 and 5 and figures 6 and 7 present these estimates for violent and property crime rates, respectively. The patterns in these results are broadly similar to those for total crimes, with a handful of exceptions. Violent crime rates relative to 2007–2008 experience their largest decline (9.2 percent, $p = 0.022$) in 2010, with estimates becoming smaller and less precisely estimated thereafter. This pattern also holds for agencies with larger potential increases in force, where the estimates are also somewhat larger, unlike with total crime rates. Among all agencies, the effects on property crime rates are less precise, with none being estimated at the 10 percent significance level or lower. However, among smaller agencies, the effect on property crime rates appears to be both larger and more precisely estimated, peaking in 2012 (7.6 percent, $p = 0.038$). The effects of COPS Office hiring grant funding on violent crime rates appear to be larger, overall, than those on property crime rates, which is consistent with previous studies. And this pattern is also relevant for policy purposes since the social costs of crime are disproportionately driven by violent crime. However, as we discuss below, the sensitivity of the violent crime results to the choice of baseline year raises concerns and argues for caution in interpreting these estimates.

## Effects of COPS Office hiring grants on arrests

Our last set of results tries to shed light on the potential mechanisms through which increased police spending reduces crime. As noted previously, police may reduce crime by arresting and incapacitating offenders or by deterring criminal activity from occurring in the first place. We lack direct data on any of these mechanisms, but we can examine what happens to arrest rates in agencies above the funding

cutoff. If arrests decline alongside crime, this suggests that the deterrence and prevention activities additional officers undertake outweigh any effect on crime from apprehending more offenders. On the other hand, if additional police resources reduce crime while increasing arrests, this suggests that the effects of apprehension and incapacitation dominate those of deterrence and prevention activities. The distinction is important both for strengthening our understanding of how police reduce crime and for policy reasons. If police reduce crime and arrests simultaneously, then increased police spending may not strain courts and correctional systems, and could potentially alleviate (rather than exacerbate) concerns about America's incarceration rate.

Tables 6–8 and figures 8–10 replicate our main estimates for crime rates using arrest rates relative to 2007–2008 as our outcome variable. It is worth noting that the arrest data are missing at much higher rates than either the officer or crime data, and our estimation sample is consequently smaller. Nevertheless, the estimated effects of COPS Office funding on arrest rates show a similar pattern to those on crime rates. Despite the reduction in sample size, arrest rates in 2010 relative to 2007–2008 are 6.2 percent smaller ($p = 0.090$). As with crime rates, the effects on violent arrest rates tend to be larger than those on property arrest rates. However, these effects disappear in 2011–2012, suggesting that patterns of arrest attenuate more rapidly after receipt of COPS funding than patterns of criminal activity.

## Robustness checks

This section provides additional evidence on the sensitivity of our estimates and addresses potential concerns about our research design.

The results presented above are all estimated using a bandwidth of 0.8. As appendix table 2 shows, this bandwidth falls within the range suggested by two popular techniques for choosing optimal bandwidths in RD designs (Calonico, Cattaneo, and Titiunik 2014; Imbens and Kalyanaraman 2012). Further, in the range of bandwidths suggested by these methods (0.7 – 1.1), our results remain substantively unchanged (appendix figure 2).

A more pressing concern is the choice of baseline year used in our main specifications. We measure outcomes relative to 2007–2008 because these are the two most recent pre-treatment years. Using an earlier baseline allows us to verify whether no "effect" is measured in the pre-treatment period, though at the cost of a more recent measure; we show the results of this exercise, using 2005–2006 as our baseline, in appendix tables 3–5. For officers and arrests, these estimates show a broadly similar pattern to our main results. The estimates for violent crime rates (appendix table 4, columns 5–8), however, are substantially negative and, in 2008, statistically significant, suggesting that there may be some unmodeled heterogeneity in agencies on either side of the cutoff.

Another set of concerns center on the CHRP grant application score that serves as the running variable in our RD model. Specifically, the validity of our research design would potentially be compromised if this score was manipulated in any way, or more generally if the characteristics of agencies did not vary smoothly near the cutoff. Appendix figure 3 provides no evidence that the running variable has been

manipulated in any way; the density of application scores is smooth near the cutoff, with only a small and statistically insignificant estimated discontinuity. Appendix figure 4 shows how the three primary components of the application score—the fiscal needs, crime, and community policing scores—vary near the cutoff. Though the fiscal needs score exhibits a discontinuity, it is quite small in magnitude. The other two components appear to vary smoothly through the cutoff.

# VII. Discussion

This paper presents new estimates of the effects of increased police resources on crime and arrests in the United States. Compared to most previous work in this area, this study has both an unusually strong research design and the advantage of drawing on national data. We exploit the fact that the COPS Office awarded funding to local law enforcement agencies in 2009 based on scores assigned by a formula. Using a regression discontinuity design, we compare outcomes between agencies that just received COPS Office funding and those that narrowly missed doing so.

We find that COPS Office funding increased officers per capita in 2009 relative to 2007–2008 by almost 2 percent for agencies near the funding threshold, and led to declines in reported UCR Part 1 crimes of approximately 5 percent in subsequent years, albeit estimated with varying degrees of precision. A similar estimated decline in arrests was found in 2010, suggesting that deterrence, rather than incapacitation, may on net be the mechanism driving these results.

We have two goals for future work on this project. First, we hope to improve the precision of the estimates presented here. One way to do this is to utilize variation created by COPS Office grants awarded in 2010, 2011, and 2012. Second, we hope to better understand the unmeasured heterogeneity affecting our violent crime estimates.

Though not sufficiently strong to conclude that there are increasing returns to police spending, the results presented here provide suggestive evidence that additional police resources represent a cost-effective approach to reduce crime.

# Figures and Tables

**Table 1. Summary statistics for agencies applying to the 2009 COPS Hiring Program**

| | | Population[1] | | |
|---|---|---|---|---|
| | **All Agencies** | **0-50K** | **50-150K** | **150K+** |
| | **(1)** | **(2)** | **(3)** | **(4)** |
| Avg. Sworn Force Size | 74 | 26 | 143 | 1,114 |
| Avg. Positions Requested | 5 | 3 | 9 | 57 |
| Avg. Funds Requested ($000) | 1,131 | 516 | 1,926 | 14,682 |
| *N (Agencies Applied)* | *7,202* | *4,954* | *662* | *208* |
| Avg. Positions Awarded | 4 | 2 | 7 | 26 |
| Avg. Funds Awarded ($000) | 956 | 331 | 1,567 | 6,110 |
| *N (Agencies Awarded)* | *1,046* | *689* | *152* | *81* |

**Notes:** Data from COPS office and UCR.

[1] Agency population is most recent non-missing value reported in UCR. 1,376 agencies do not report a population from 1998-2012.

**Table 2. Log officers per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | *All Potential Increases in Force* | | | | | |
| 2009 | 0.019 | 0.009 | 0.033 | 2,163 | 0.024 | 0.010 | 0.020 | 1,724 |
| 2010 | 0.021 | 0.013 | 0.122 | 2,163 | 0.023 | 0.016 | 0.146 | 1,724 |
| 2011 | 0.012 | 0.015 | 0.420 | 2,163 | 0.017 | 0.018 | 0.347 | 1,726 |
| 2012 | 0.006 | 0.016 | 0.713 | 2,162 | 0.011 | 0.019 | 0.558 | 1,724 |
| | | | *Potential Increases in Force of 5%+* | | | | | |
| 2009 | 0.032 | 0.013 | 0.018 | 1,271 | 0.037 | 0.014 | 0.010 | 1,154 |
| 2010 | 0.032 | 0.020 | 0.108 | 1,272 | 0.038 | 0.022 | 0.083 | 1,155 |
| 2011 | 0.021 | 0.022 | 0.340 | 1,272 | 0.028 | 0.024 | 0.239 | 1,156 |
| 2012 | 0.017 | 0.025 | 0.502 | 1,273 | 0.022 | 0.027 | 0.421 | 1,156 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 3. Log total crimes per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | *All Potential Increases in Force* | | | | | |
| 2009 | -0.010 | 0.018 | 0.568 | 2,119 | -0.015 | 0.022 | 0.509 | 1,683 |
| 2010 | -0.045 | 0.023 | 0.052 | 2,116 | -0.063 | 0.028 | 0.028 | 1,680 |
| 2011 | -0.055 | 0.026 | 0.034 | 2,118 | -0.072 | 0.031 | 0.022 | 1,682 |
| 2012 | -0.051 | 0.029 | 0.079 | 2,114 | -0.074 | 0.035 | 0.035 | 1,680 |
| | | | *Potential Increases in Force of 5%+* | | | | | |
| 2009 | -0.003 | 0.028 | 0.920 | 1,240 | -0.007 | 0.030 | 0.805 | 1,126 |
| 2010 | -0.038 | 0.035 | 0.283 | 1,236 | -0.050 | 0.039 | 0.201 | 1,122 |
| 2011 | -0.047 | 0.039 | 0.230 | 1,238 | -0.058 | 0.043 | 0.178 | 1,124 |
| 2012 | -0.022 | 0.044 | 0.617 | 1,236 | -0.038 | 0.049 | 0.430 | 1,122 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 4. Log violent crimes per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | *All Potential Increases in Force* | | | | |
| 2009 | -0.043 | 0.034 | 0.214 | 2,086 | -0.047 | 0.043 | 0.269 | 1,650 |
| 2010 | -0.092 | 0.040 | 0.022 | 2,078 | -0.113 | 0.049 | 0.022 | 1,642 |
| 2011 | -0.065 | 0.044 | 0.139 | 2,068 | -0.068 | 0.055 | 0.213 | 1,632 |
| 2012 | -0.074 | 0.046 | 0.108 | 2,068 | -0.087 | 0.056 | 0.125 | 1,634 |
| | | | | *Potential Increases in Force of 5%+* | | | | |
| 2009 | -0.050 | 0.052 | 0.333 | 1,205 | -0.044 | 0.057 | 0.444 | 1,091 |
| 2010 | -0.107 | 0.060 | 0.075 | 1,198 | -0.122 | 0.066 | 0.067 | 1,084 |
| 2011 | -0.097 | 0.066 | 0.141 | 1,190 | -0.096 | 0.072 | 0.183 | 1,076 |
| 2012 | -0.092 | 0.068 | 0.174 | 1,190 | -0.099 | 0.074 | 0.181 | 1,076 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 5. Log property crimes per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| | | | | *All Potential Increases in Force* | | | | |
| 2009 | 0.001 | 0.019 | 0.946 | 2,115 | -0.001 | 0.023 | 0.963 | 1,679 |
| 2010 | -0.036 | 0.024 | 0.138 | 2,111 | -0.052 | 0.029 | 0.079 | 1,675 |
| 2011 | -0.042 | 0.026 | 0.103 | 2,110 | -0.059 | 0.032 | 0.063 | 1,675 |
| 2012 | -0.050 | 0.030 | 0.094 | 2,108 | -0.076 | 0.037 | 0.038 | 1,674 |
| | | | | *Potential Increases in Force of 5%+* | | | | |
| 2009 | 0.017 | 0.029 | 0.565 | 1,236 | 0.013 | 0.032 | 0.687 | 1,122 |
| 2010 | -0.021 | 0.037 | 0.561 | 1,231 | -0.032 | 0.040 | 0.429 | 1,117 |
| 2011 | -0.022 | 0.040 | 0.587 | 1,231 | -0.032 | 0.043 | 0.462 | 1,117 |
| 2012 | -0.015 | 0.046 | 0.745 | 1,230 | -0.033 | 0.051 | 0.520 | 1,116 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 6. Log total arrests per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| **Year** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| | | | | *All Potential Increases in Force* | | | | |
| 2009 | -0.012 | 0.030 | 0.681 | 1,689 | -0.014 | 0.037 | 0.698 | 1,332 |
| 2010 | -0.062 | 0.036 | 0.090 | 1,685 | -0.099 | 0.044 | 0.027 | 1,328 |
| 2011 | -0.009 | 0.041 | 0.816 | 1,681 | -0.028 | 0.050 | 0.576 | 1,323 |
| 2012 | 0.014 | 0.047 | 0.759 | 1,677 | -0.014 | 0.058 | 0.810 | 1,318 |
| | | | | *Potential Increases in Force of 5%+* | | | | |
| 2009 | -0.019 | 0.045 | 0.672 | 974 | -0.025 | 0.049 | 0.606 | 876 |
| 2010 | -0.087 | 0.054 | 0.110 | 971 | -0.111 | 0.060 | 0.063 | 873 |
| 2011 | 0.004 | 0.061 | 0.949 | 967 | 0.003 | 0.067 | 0.964 | 869 |
| 2012 | 0.046 | 0.069 | 0.498 | 962 | 0.039 | 0.076 | 0.606 | 864 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 7. Log violent arrests per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0–50K | | | |
|---|---|---|---|---|---|---|---|---|
| **Year** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| | | | | *All Potential Increases in Force* | | | | |
| 2009 | -0.015 | 0.043 | 0.724 | 1,645 | -0.006 | 0.053 | 0.913 | 1,288 |
| 2010 | -0.089 | 0.049 | 0.071 | 1,637 | -0.109 | 0.060 | 0.071 | 1,280 |
| 2011 | -0.007 | 0.055 | 0.896 | 1,633 | 0.009 | 0.068 | 0.893 | 1,275 |
| 2012 | 0.018 | 0.061 | 0.762 | 1,625 | 0.038 | 0.075 | 0.612 | 1,266 |
| | | | | *Potential Increases in Force of 5%+* | | | | |
| 2009 | -0.019 | 0.065 | 0.775 | 934 | -0.001 | 0.072 | 0.986 | 836 |
| 2010 | -0.142 | 0.073 | 0.053 | 924 | -0.144 | 0.081 | 0.075 | 826 |
| 2011 | -0.041 | 0.081 | 0.611 | 921 | -0.025 | 0.091 | 0.782 | 823 |
| 2012 | 0.012 | 0.090 | 0.895 | 914 | 0.021 | 0.100 | 0.837 | 816 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Table 8. Log property arrests per 100,000 residents, relative to 2007–2008**

| | All Agencies | | | | Population: 0-50K | | | |
|---|---|---|---|---|---|---|---|---|
| **Year** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** | **Estimate** | **Std. Err.** | ***p*-value** | **Obs.** |
| | **(1)** | **(2)** | **(3)** | **(4)** | **(5)** | **(6)** | **(7)** | **(8)** |
| | | | | *All Potential Increases in Force* | | | | |
| 2009 | 0.002 | 0.035 | 0.949 | 1,677 | -0.003 | 0.043 | 0.943 | 1,320 |
| 2010 | -0.047 | 0.042 | 0.263 | 1,674 | -0.085 | 0.052 | 0.099 | 1,317 |
| 2011 | -0.005 | 0.046 | 0.920 | 1,673 | -0.029 | 0.056 | 0.606 | 1,315 |
| 2012 | 0.011 | 0.054 | 0.832 | 1,667 | -0.032 | 0.066 | 0.630 | 1,308 |
| | | | | *Potential Increases in Force of 5%+* | | | | |
| 2009 | 0.006 | 0.052 | 0.908 | 963 | -0.011 | 0.058 | 0.851 | 865 |
| 2010 | -0.052 | 0.062 | 0.408 | 961 | -0.081 | 0.069 | 0.242 | 863 |
| 2011 | 0.034 | 0.068 | 0.624 | 959 | 0.023 | 0.076 | 0.756 | 861 |
| 2012 | 0.057 | 0.078 | 0.466 | 953 | 0.038 | 0.086 | 0.658 | 855 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Figure 1. Agency received COPS Hiring Grant in 2009**

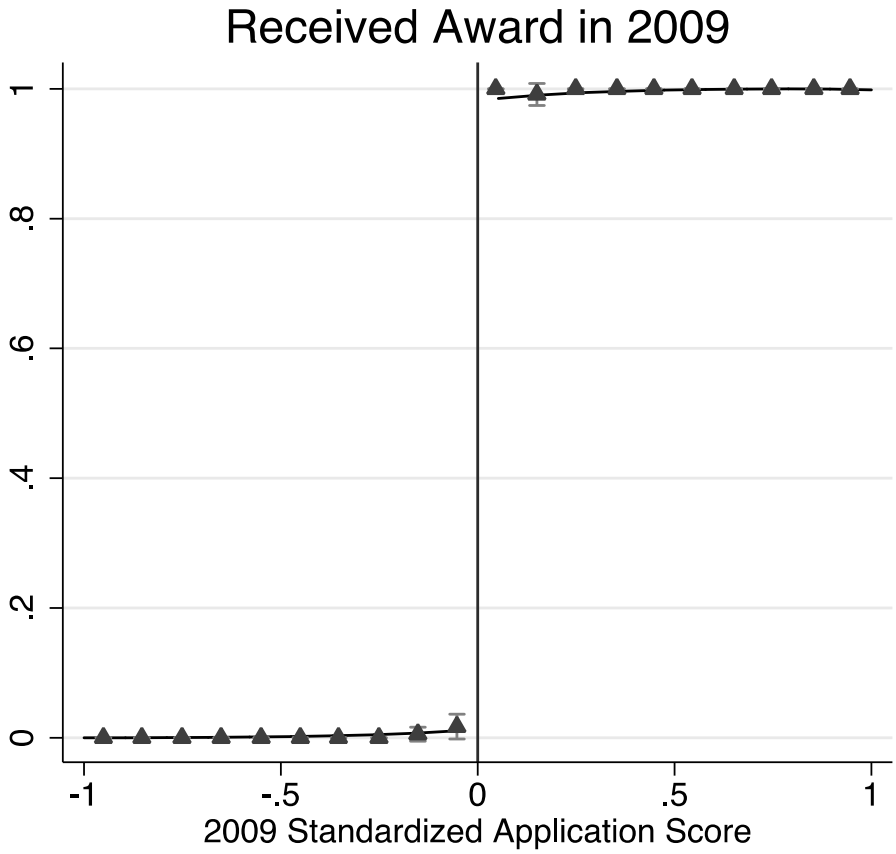


**Figure 2. Grant amount received by agency in 2009**

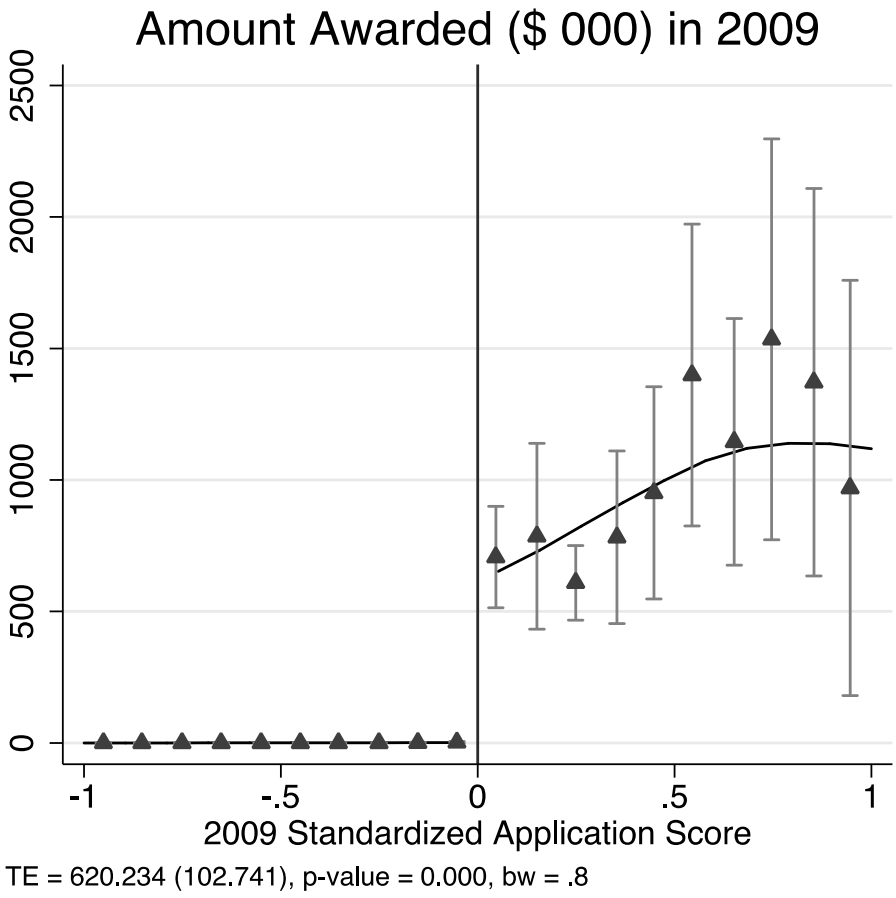


**Figure 3. Log officers per 100,000 residents in 2009, relative to 2007–2008**

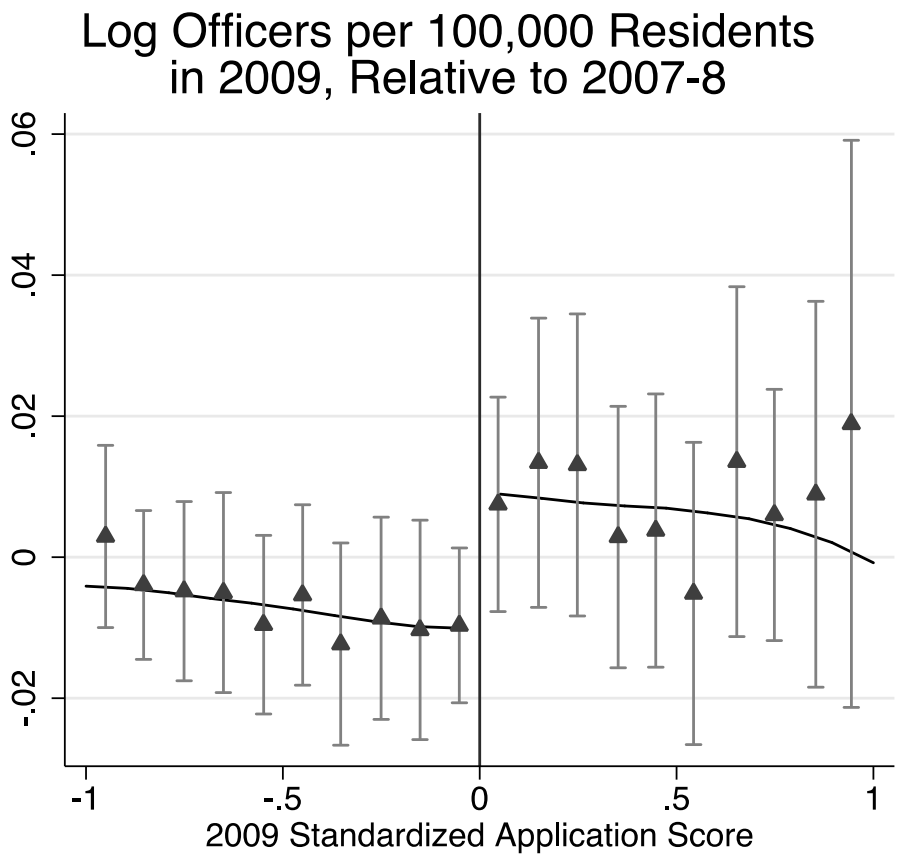


**Figure 4. Log officers per 100,000 residents, relative to 2007–2008**

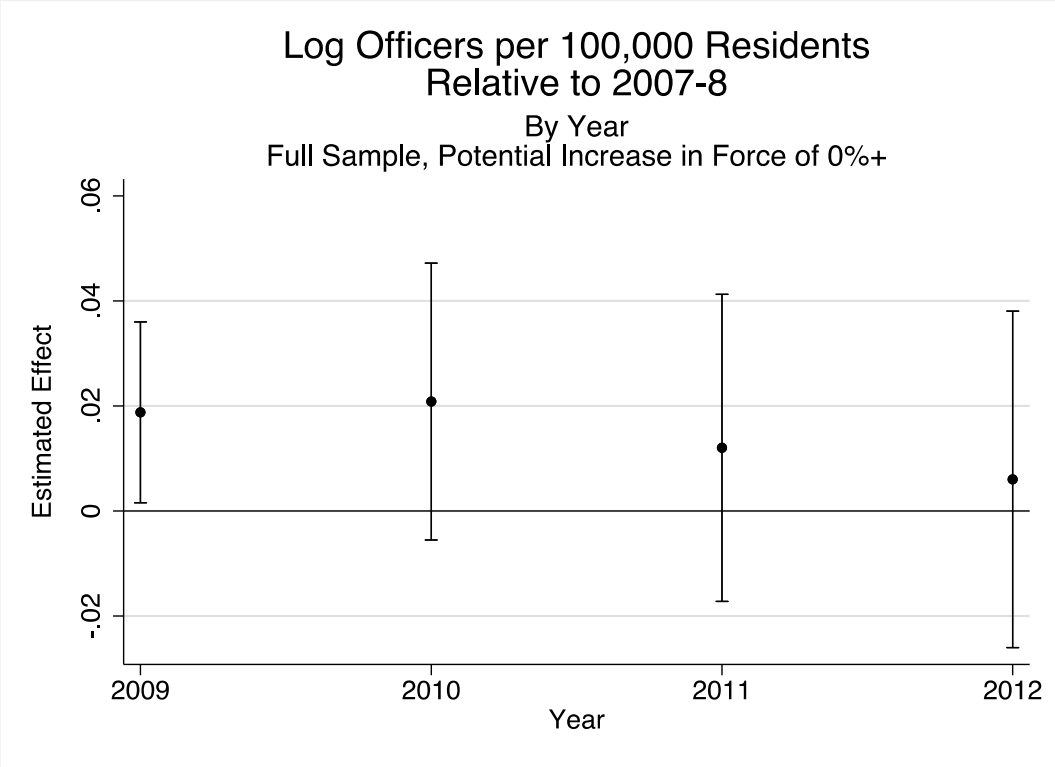


**Figure 5. Log total crimes per 100,000 residents, relative to 2007–2008**

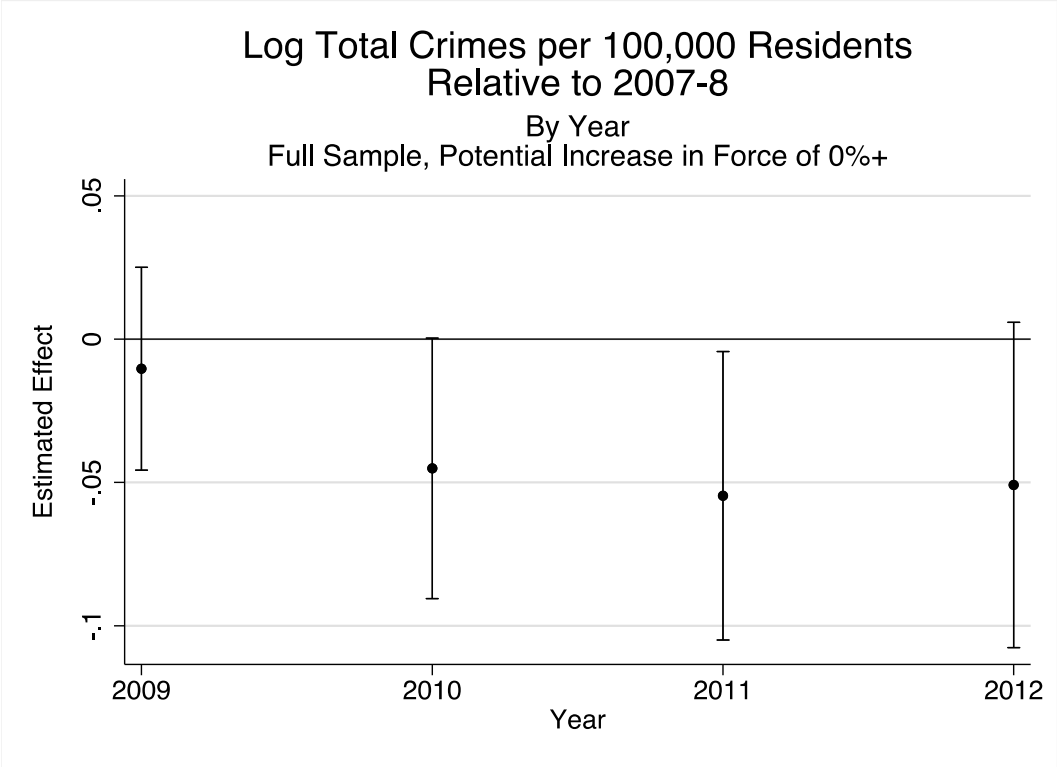


**Figure 6. Log violent crimes per 100,000 residents, relative to 2007–2008**

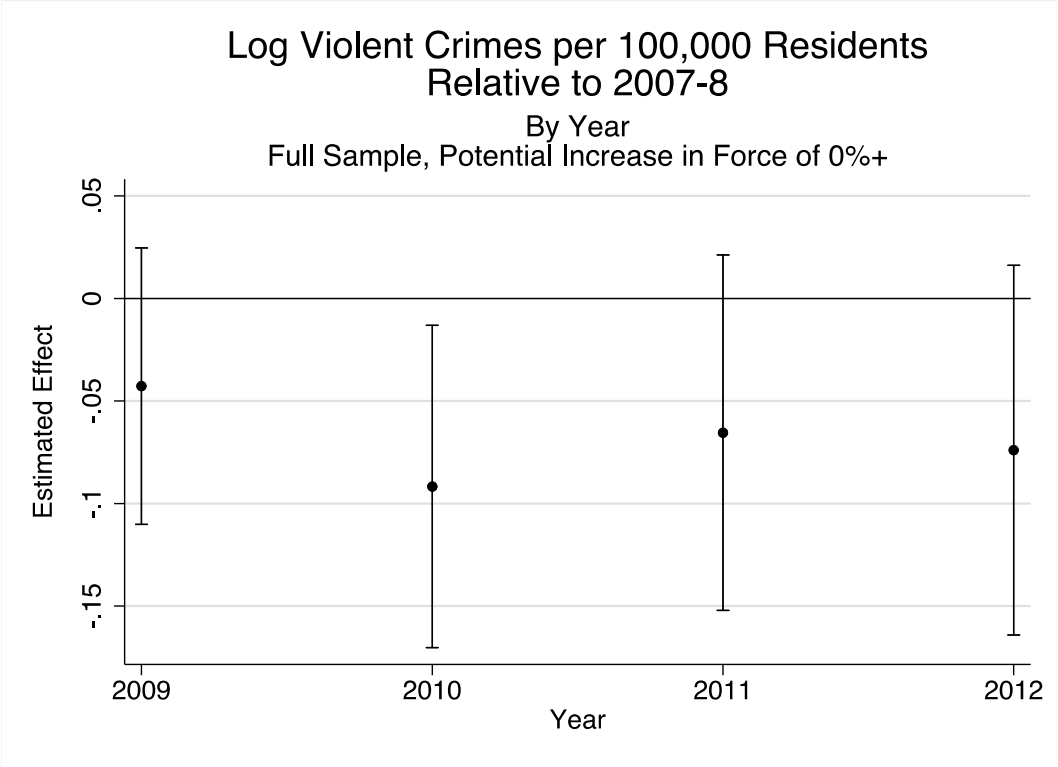


**Figure 7. Log property crimes per 100,000 residents, relative to 2007–2008**

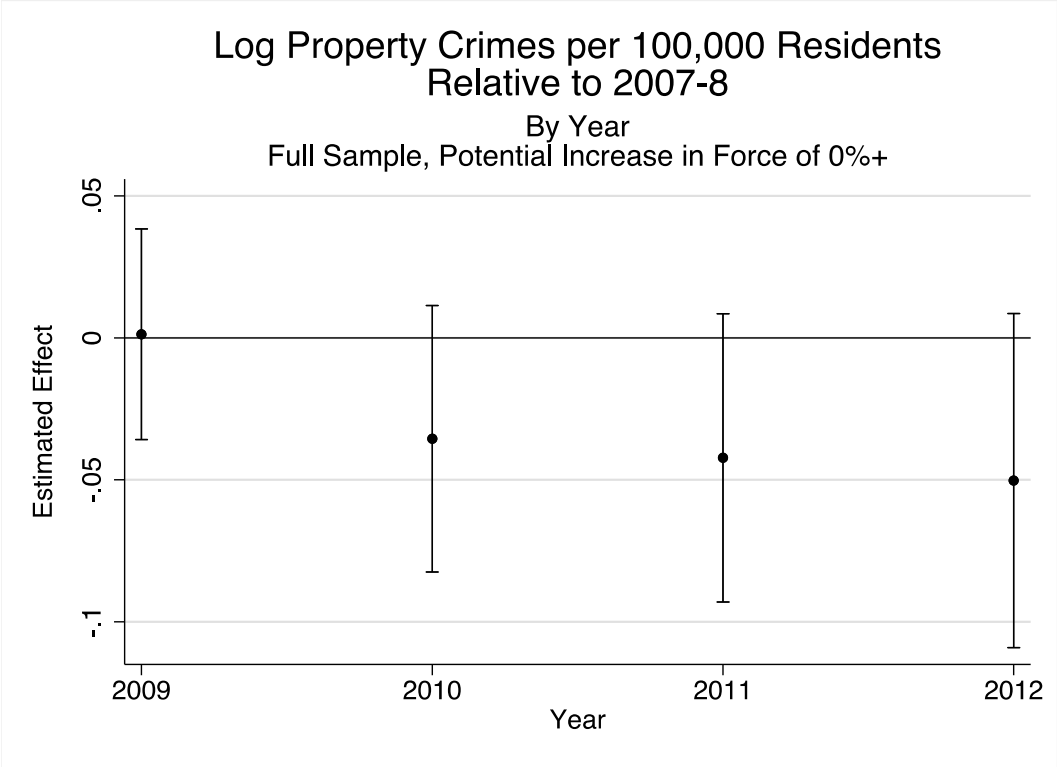


**Figure 8. Log total arrests per 100,000 residents, relative to 2007–2008**

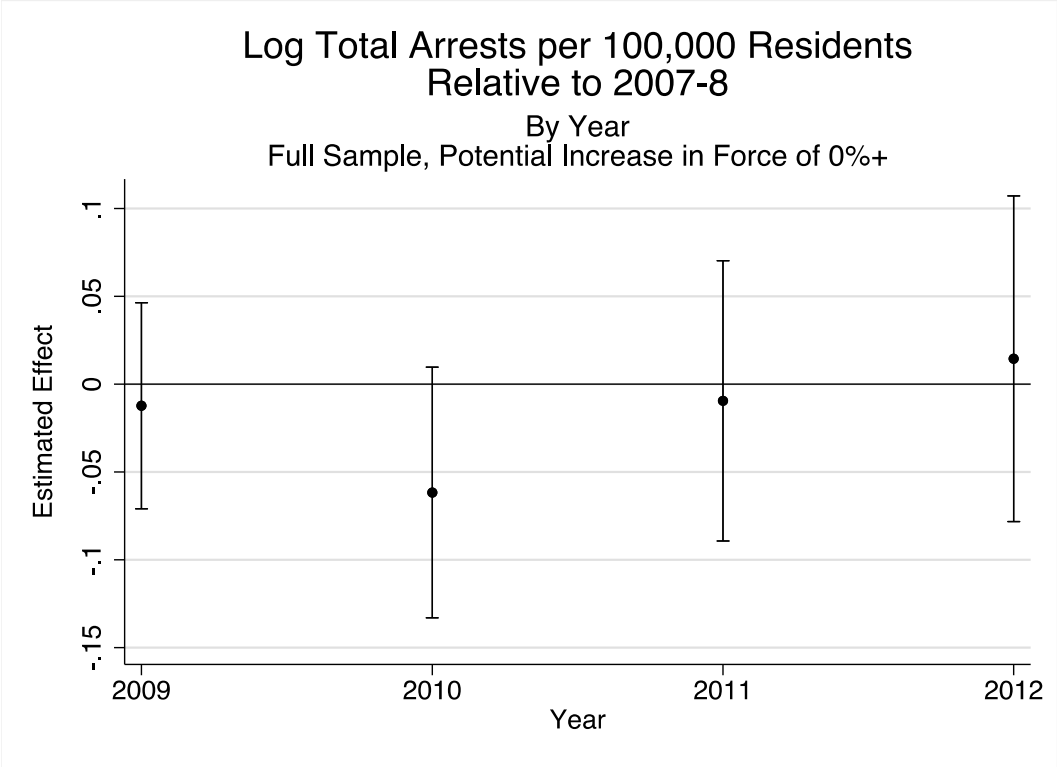


**Figure 9. Log violent arrests per 100,000 residents, relative to 2007–2008**

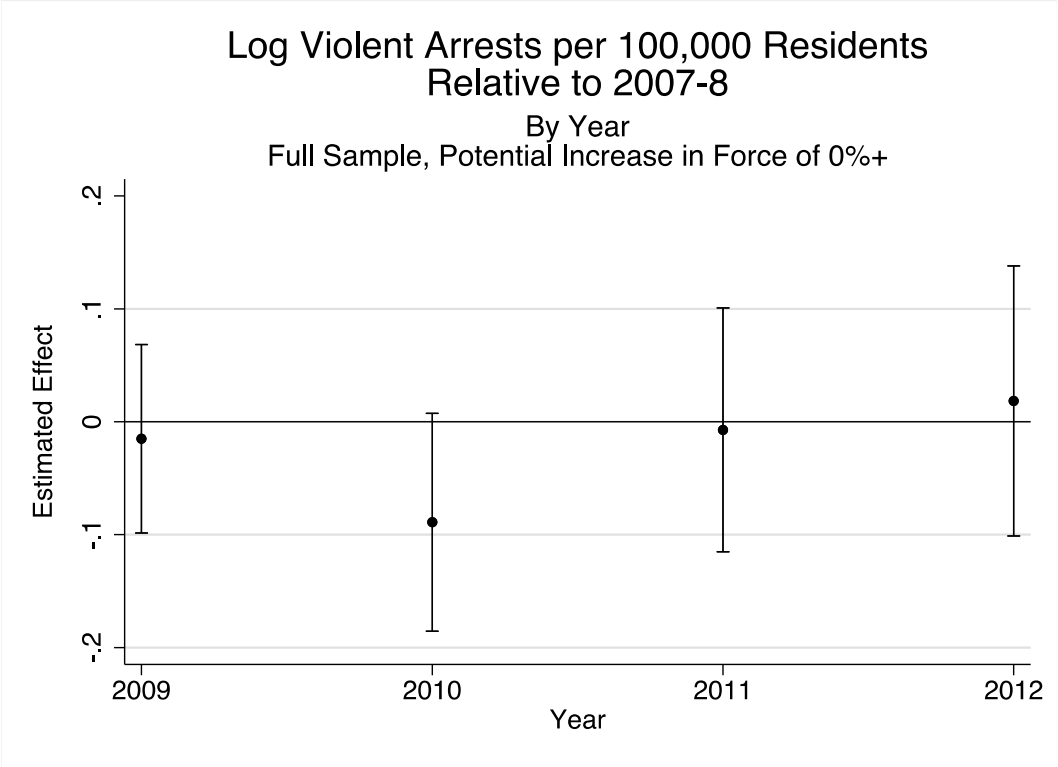
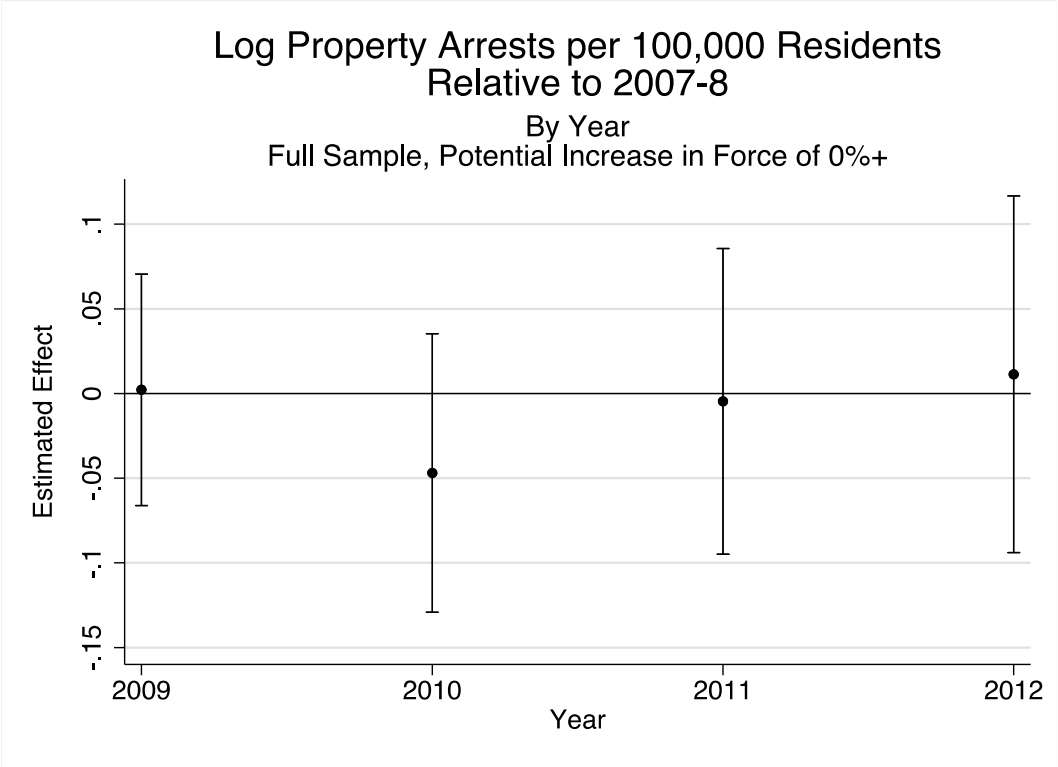


**Figure 10. Log property arrests per 100,000 residents, relative to 2007–2008**



# Appendix

**Appendix table 1. Placebo estimates of the "effect" of being above the cutoff for COPS Hiring Grant funding in 2009 on sworn force size in 2008**

| Outcome in 2008 | Estimate | Std. Err. | *p*-value | Obs. |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Officers | 39.6 | 42.2 | 0.348 | 2,200 |
| Officers per 100,000 Residents | 15.1 | 10.0 | 0.130 | 2,163 |
| Log Officers | 0.094 | 0.128 | 0.463 | 2,200 |
| Log Officers per 100,000 Residents | 0.071 | 0.041 | 0.084 | 2,163 |
| Log Officers, Relative to 2005-6 | 0.017 | 0.010 | 0.098 | 2,200 |
| Log Officers per 100,000 Residents, Relative to 2005-6 | 0.004 | 0.010 | 0.709 | 2,168 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Appendix table 2. Alternative bandwidth estimators**

| Outcome in 2008 | Bandwidth | Estimate | *p*-value | Obs. |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| *Calonico, Cattaneo, and Titiunik (2014)* | | | | |
| Officers | 0.32 | 86.8 | 0.035 | 883 |
| Officers per 100,000 Residents | 0.43 | 28.1 | 0.019 | 1,195 |
| Log Officers | 0.40 | 0.23 | 0.155 | 1,101 |
| Log Officers per 100,000 Residents | 0.41 | 0.14 | 0.009 | 1,123 |
| Log Officers, Relative to 2005-6 | 0.79 | 0.01 | 0.215 | 2,185 |
| Log Officers per 100,000 Residents, Relative to 2005-6 | 0.72 | 0.01 | 0.479 | 1,953 |
| *Imbens and Kalyanaraman (2012)* | | | | |
| Officers | 0.96 | 24.5 | 0.015 | 2,636 |
| Officers per 100,000 Residents | 1.26 | 9.4 | 0.083 | 3,318 |
| Log Officers | 1.25 | 0.02 | 0.340 | 3,349 |
| Log Officers per 100,000 Residents | 1.45 | 0.04 | 0.085 | 3,766 |
| Log Officers, Relative to 2005-6 | 0.90 | 0.01 | 0.349 | 2,476 |
| Log Officers per 100,000 Residents, Relative to 2005-6 | 1.10 | 0.01 | 0.553 | 2,947 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and bandwidths chosen by the indicated estimators. Obs = number of observations within the bandwidth around the cutoff.

**Appendix table 3. Log officers per 100,000 residents, relative to 2005–6**

| Year | Estimate | Std. Err. | *p*-value | Obs. |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| 2007 | 0.004 | 0.010 | 0.709 | 2,168 |
| 2008 | 0.012 | 0.011 | 0.294 | 2,163 |
| 2009 | 0.032 | 0.013 | 0.015 | 2,163 |
| 2010 | 0.030 | 0.016 | 0.056 | 2,163 |
| 2011 | 0.022 | 0.016 | 0.183 | 2,163 |
| 2012 | 0.016 | 0.018 | 0.366 | 2,162 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Appendix table 4. Log crimes per 100,000 residents, relative to 2005–6**

| | Total Crimes | | | | Violent Crimes | | | | Property Crimes | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| 2007 | -0.014 | 0.019 | 0.466 | 2,104 | -0.057 | 0.036 | 0.110 | 2,068 | -0.001 | 0.020 | 0.976 | 2,100 |
| 2008 | 0.002 | 0.024 | 0.938 | 2,103 | -0.088 | 0.041 | 0.034 | 2,072 | 0.019 | 0.025 | 0.461 | 2,099 |
| 2009 | -0.014 | 0.026 | 0.605 | 2,097 | -0.127 | 0.045 | 0.004 | 2,062 | 0.013 | 0.027 | 0.624 | 2,094 |
| 2010 | -0.049 | 0.029 | 0.094 | 2,094 | -0.163 | 0.048 | 0.001 | 2,059 | -0.030 | 0.030 | 0.326 | 2,089 |
| 2011 | -0.066 | 0.032 | 0.036 | 2,099 | -0.147 | 0.050 | 0.003 | 2,056 | -0.049 | 0.032 | 0.133 | 2,093 |
| 2012 | -0.066 | 0.033 | 0.049 | 2,098 | -0.153 | 0.053 | 0.004 | 2,052 | -0.047 | 0.034 | 0.175 | 2,094 |

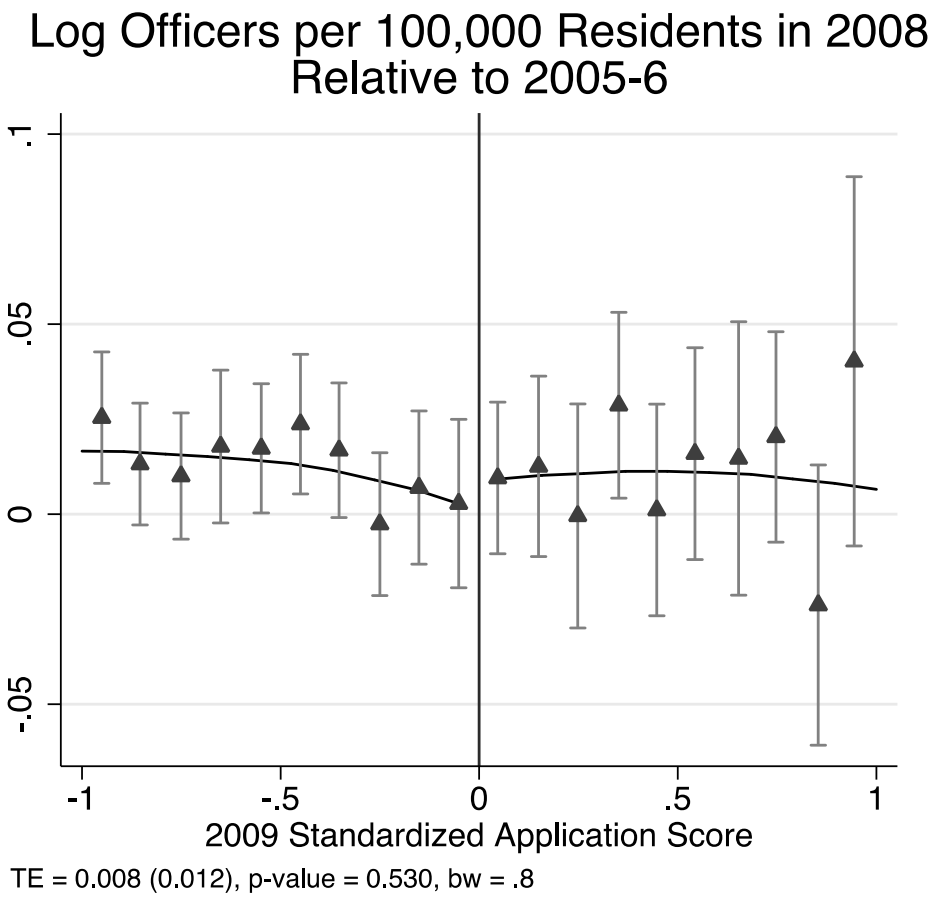
**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

**Appendix table 5. Log arrests per 100,000 residents, relative to 2005–6**

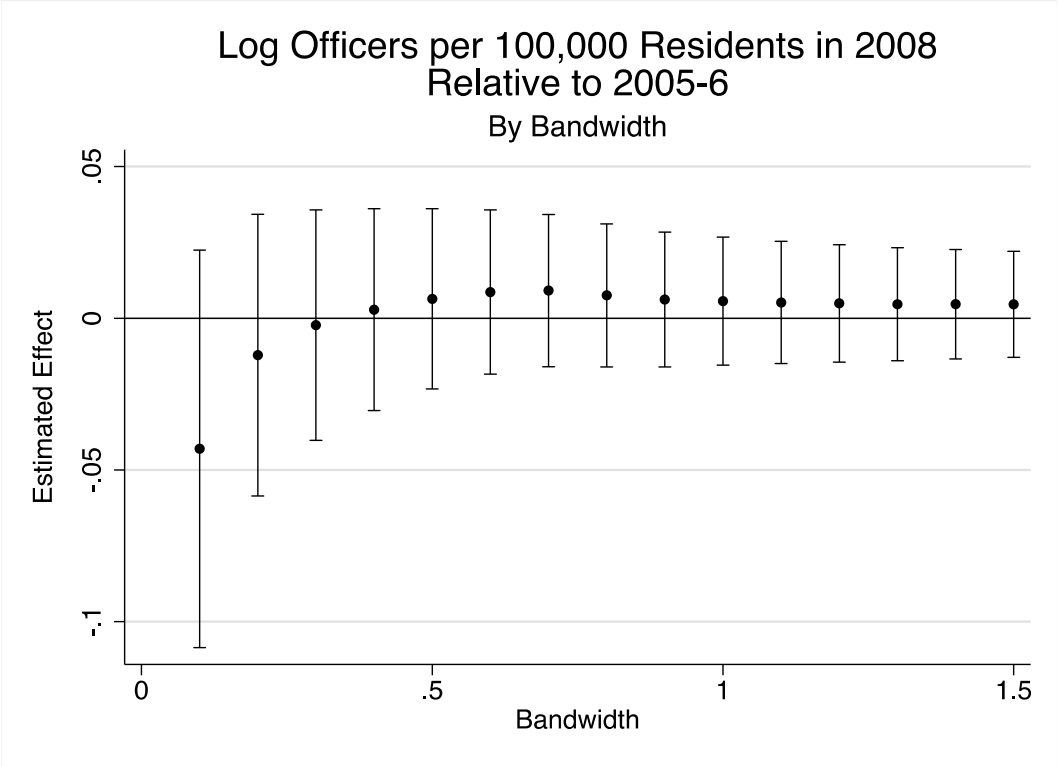
| | Total Arrests | | | | Violent Arrests | | | | Property Arrests | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Year | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. | Estimate | Std. Err. | *p*-value | Obs. |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12) |
| 2007 | -0.033 | 0.032 | 0.298 | 1,686 | 0.008 | 0.044 | 0.860 | 1,635 | -0.042 | 0.036 | 0.244 | 1,674 |
| 2008 | -0.022 | 0.040 | 0.588 | 1,682 | 0.012 | 0.053 | 0.825 | 1,628 | -0.008 | 0.046 | 0.857 | 1,672 |
| 2009 | -0.030 | 0.046 | 0.514 | 1,683 | 0.000 | 0.057 | 0.994 | 1,628 | -0.028 | 0.052 | 0.585 | 1,666 |
| 2010 | -0.080 | 0.050 | 0.110 | 1,683 | -0.081 | 0.063 | 0.200 | 1,626 | -0.060 | 0.056 | 0.284 | 1,666 |
| 2011 | -0.031 | 0.052 | 0.553 | 1,683 | 0.020 | 0.065 | 0.765 | 1,630 | -0.020 | 0.058 | 0.734 | 1,669 |
| 2012 | -0.004 | 0.056 | 0.936 | 1,677 | 0.005 | 0.070 | 0.938 | 1,618 | -0.005 | 0.062 | 0.932 | 1,662 |

**Notes:** Estimates from local linear regressions using triangle kernel weights and a bandwidth of 0.8. See text for additional details. Obs = number of observations within the bandwidth around the cutoff.

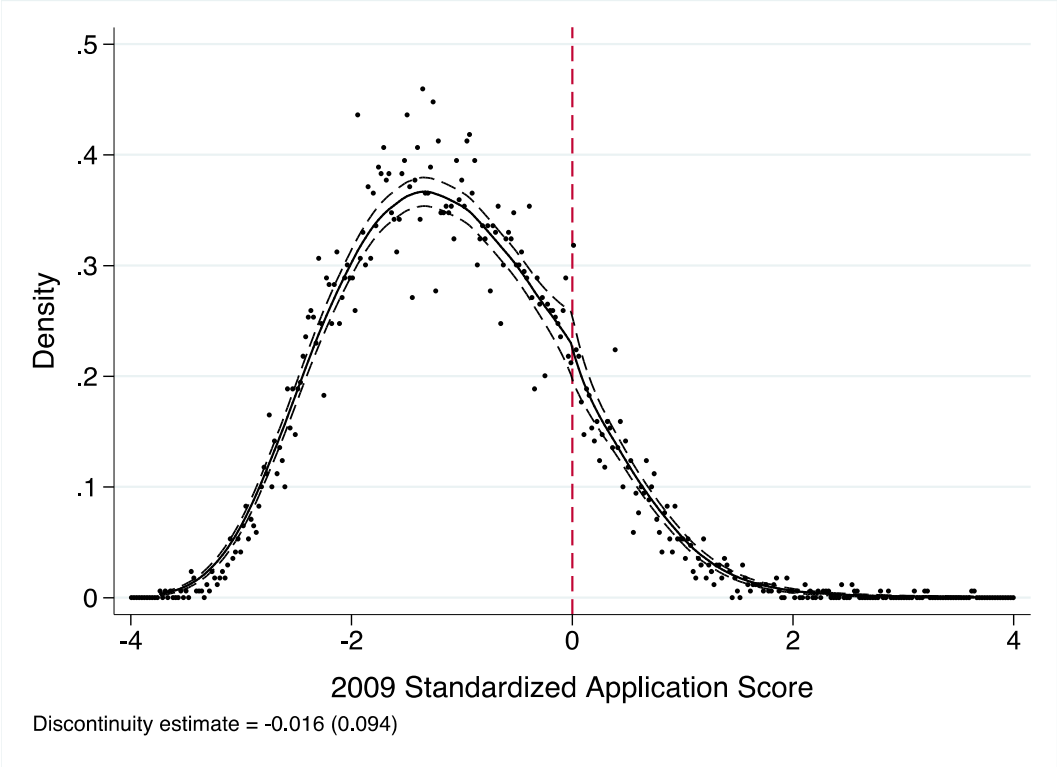
**Appendix figure 1. Log officers per 100,000 residents in 2008, relative to 2005–6**



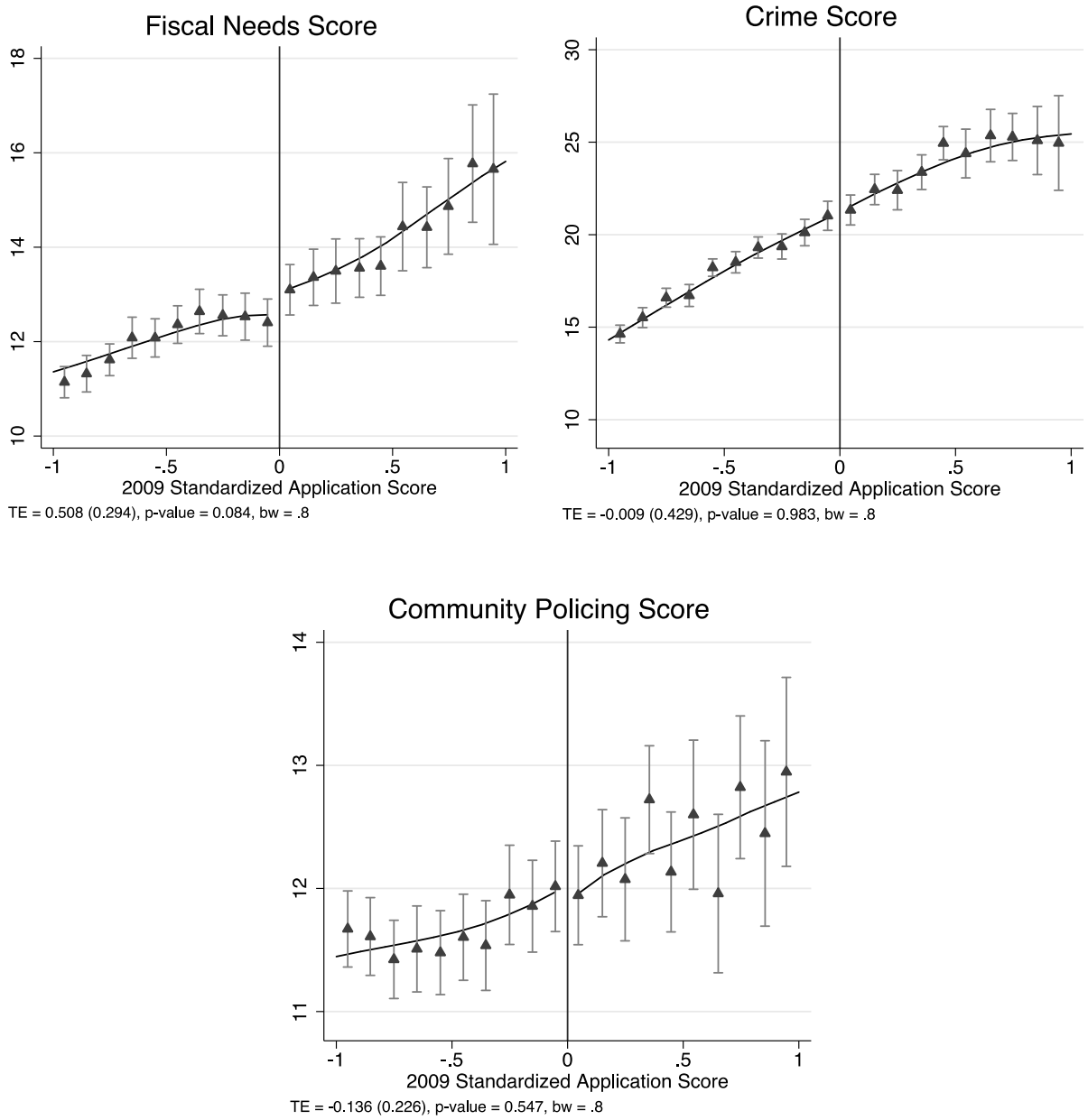
**Appendix figure 2. Bandwidth sensitivity: sworn force size in 2008**



**Appendix figure 3. Density of the running variable**



**Appendix figure 4. Continuity of COPS Hiring Grant application score components through the cutoff**



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# About the COPS Office

The **Office of Community Oriented Policing Services (COPS Office)** is the component of the U.S. Department of Justice responsible for advancing the practice of community policing by the nation's state, local, territorial, and tribal law enforcement agencies through information and grant resources.

Community policing begins with a commitment to building trust and mutual respect between police and communities. It supports public safety by encouraging all stakeholders to work together to address our nation's crime challenges. When police and communities collaborate, they more effectively address underlying issues, change negative behavioral patterns, and allocate resources.

Rather than simply responding to crime, community policing focuses on preventing it through strategic problem solving approaches based on collaboration. The COPS Office awards grants to hire community police and support the development and testing of innovative policing strategies. COPS Office funding also provides training and technical assistance to community members and local government leaders, as well as all levels of law enforcement.

Another source of COPS Office assistance is the Collaborative Reform Initiative for Technical Assistance (CRI-TA). Developed to advance community policing and ensure constitutional practices, CRI-TA is an independent, objective process for organizational transformation. It provides recommendations based on expert analysis of policies, practices, training, tactics, and accountability methods related to issues of concern.

Since 1994, the COPS Office has invested more than $14 billion to add community policing officers to the nation's streets, enhance crime fighting technology, support crime prevention initiatives, and provide training and technical assistance to help advance community policing.

- To date, the COPS Office has funded the hiring of approximately 129,000 additional officers by more than 13,000 of the nation's 18,000 law enforcement agencies in both small and large jurisdictions.
- Nearly 700,000 law enforcement personnel, community members, and government leaders have been trained through COPS Office-funded training organizations.
- To date, the COPS Office has distributed more than eight million topic-specific publications, training curricula, white papers, and resource CDs.
- The COPS Office also sponsors conferences, roundtables, and other forums focused on issues critical to law enforcement.

The COPS Office information resources, covering a wide range of community policing topics—from school and campus safety to gang violence—can be downloaded at www.cops.usdoj.gov. This website is also the grant application portal, providing access to online application forms.

This paper estimates the effects on crime from increased spending on police by studying the 2009 competition for officer hiring grants carried out by the U.S. Department of Justice's Office of Community Oriented Policing Services (COPS Office). Because the competition was oversubscribed, the COPS Office ranked applicants in descending order using a score derived from their responses to application questions and distributed $1 billion in grants until the funding was exhausted. Using a regression discontinuity (RD) design, we show that receipt of COPS Office grant funding increased the size of police forces in 2009 by almost 2 percent relative to 2007–2008. In the following years, we find suggestive evidence that total crime rates declined from their baseline levels by almost 5 percent. Finally, we find some evidence that arrest rates decreased as well, suggesting that deterrence may be a key mechanism behind this effect.

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the COPS Office Response Center at 800-421-6770.
Visit the COPS Office online at www.cops.usdoj.gov.

e111602790
Published 2017