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## **The Thin Blue Line in Schools: New Evidence on School-Based Policing Across the U.S.**

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## **Abstract**

U.S. public school students increasingly attend schools with sworn law enforcement officers present. Yet, little is known about how these school resource officers (SROs) affect school environments or student outcomes. Our study uses a fuzzy regression discontinuity (RD) design with national school-level data from 2014 to 2018 to estimate the impacts of SRO placement. We construct this discontinuity based on the application scores of nearby police agencies for federal school-based policing grants. We find that SROs do effectively reduce some forms of violence in schools, but do not prevent school shootings or gun-related incidents. We also find that SROs intensify the use of suspensions, expulsions, police referrals, and arrests of students. These effects are consistently over two times larger for Black students than White students. Finally, we observe that SROs increase chronic absenteeism, particularly for students with disabilities.

Police stationed within K-12 schools, known as school resource officers (SROs), are a common feature of American schools (Goldstein, 2020). According to federal data, about half of schools had a SRO on school grounds at least once a week during the 2017-2018 school year (NCES 2021). In the same year, a national survey found that 80% of parents supported having police officers in schools (Sawchuk et al. 2021), and some states, like Maryland and Florida, passed new laws mandating adequate law enforcement at all schools as a result of school shootings (Florida State Legislature, 2018; Maryland Association of Boards of Education, 2018). However, in the year since George Floyd's death in 2020, the US has experienced an intensified debate about the proper role of police in communities, including schools. As a result of this debate, there has been some modest reduction in the use of police in schools. According to an analysis conducted by Education Week, 33 of the United States' 13,000 school districts have eliminated their SROs, affecting about 800,000 students. Other school districts, including Chicago and Los Angeles, have significantly cut their budgets for school policing (Sawchuk et al. 2021).

The argument against police in schools often cites specific incidents of police violence against Black students in schools (Goldstein, 2020; Lee, 2015). It also links SROs to the broader concept of a school-to-prison pipeline, in which students' early experiences with school discipline and/or police in schools may directly or indirectly influence their lifetime involvement with the criminal justice system (Kupchik, 2010; Skiba, Arredondo, & Williams, 2014). Critics of SROs fear that having a police officer within a school makes it easier for a student to be formally arrested or referred to juvenile justice for minor acts of misconduct that would otherwise be handled through school discipline (Hirschfield, 2008). This criminalization of

school misconduct is particularly problematic when applied to Black students, given the stark existing racial disparities in arrest and incarceration (Theriot, 2009).

The argument for having police in schools takes multiple forms. School districts often view SROs as the first line of defense against school shootings and other acts of school violence (NASRO, 2012). SROs also aim to act as a specialized form of community policing, a model of policing designed to assign officers to permanent beats, involve citizens (or in this case, students) in decision-making, and problem-solve using non-criminal justice techniques such as mentoring and informal sanctions (Barnes, 2009; Skogan, 2006). Consistent with this logic, research has shown that SROs may improve student attitudes toward the police (Theriot, 2016) and improve student and staff perceptions of school safety (Pentek & Eisenberg, 2018).

This study presents the first school-level examination of sworn law enforcement officers across the full universe of public schools in the U.S. The data come from the 2013-14, 2015-16, and 2017-18 waves of the U.S. Department of Education's Civil Rights Data Collection (CRDC) from all public schools in the U.S. The paper uses these data to systematically compare the characteristics of schools with and without SROs to assess patterns of SRO adoption. It then uses a fuzzy regression discontinuity (RD) approach to estimate the causal effects of SRO placement on school shootings, violent incidents and crimes occurring at school, suspensions, law enforcement arrests, and other disciplinary and academic outcomes. The study pays particular attention to the possibility that SRO placement has differential effects for students of different races/ethnicities, genders, and other characteristics.

Between 2014 and 2018, around 6,000 (or 14 percent of) elementary schools and another 6,000 (or 16 percent of) middle and high schools gained new school-based police. In order to isolate exogenous variation in these SRO investments, we link each school to its closest law

enforcement agency and obtain data on school-based policing grant applications from the federal Community Oriented Policing Services (COPS) hiring program. The COPS hiring program, funded by the U.S. Department of Justice, provided 104 three-year grant awards in 2015 (59), 2016 (34), and 2017 (11) to law enforcement agencies to support expanding their SRO programs, selecting from a total of 791 eligible applicants. Our method uses the COPS hiring grant application score as a running variable, and school hiring of SROs as the treatment variable, in a fuzzy RD design to compare schools partnered with police agencies that scored just above and just below the grant award threshold.

The results from our analysis indicate that SROs noticeably change school environments and student outcomes. Contrary to frequently cited objectives of SRO programs, the introduction of a full time SRO appears to increase gun-related offenses, perhaps due to increased detection and reporting activities of the police officer within the school. It also marginally increases the likelihood of a school shooting. At the same time, the introduction of an SRO does appear to improve general student safety by decreasing non-firearm related violent offenses, such as physical attacks and fights. This benefit comes at a high cost of increased disciplinary responses both by the school and law enforcement. We find that SROs increase the incidence of in-school suspension, out-of-school suspension, expulsion, police referral and arrest, particularly in middle and high schools. For many of these disciplinary consequences, the increased use of punishment is over two times larger for Black students than White students, and significantly larger for students with disabilities than students without disabilities. SROs also increase the rate of chronic absenteeism, a likely portent of long-term educational consequences for students.

Our study is not the first to attempt to assess the effects of police in schools on student outcomes (Anderson, 2018; Gottfredson et al., 2020; Na & Gottfredson, 2011; Owens, 2017;

Sorensen, Shen, & Bushway, 2021; Weisburst, 2019; Zhang, 2019). However, this prior literature has been tightly constrained by the limited data available on SROs in schools and has therefore relied heavily on either state-level studies or on district-level funding proxies for SRO presence at schools. Using a recently released national U.S. Department of Education survey and a causal design based on COPS funding protocols, this study presents the most rigorous and broadly relevant results yet to inform future school district decision-making regarding partnerships with police.

### **Adoption of Police in Schools**

The U.S. Department of Justice defines an SRO as a “career law enforcement officer assigned in a community policing capacity to a local educational agency” (U.S. Department of Justice, 2019). Although specific responsibilities and functions of SROs vary from place to place, the “triad” concept of school-based policing divides SRO responsibilities into three main areas of: teacher, informal counselor, and law enforcement officer (NASRO, 2021). Currently, no one has a definitive answer to how many police officers are stationed in schools across the country. There is no central database for registering SROs, nor is there any uniform training or certification process for police officers to become SROs. According to the 2017-18 School Survey on Crime and Safety (SSOCS) conducted by the National Center for Education Statistics (NCES), 46.7 percent of public schools contain a sworn law enforcement officer routinely carrying a firearm, including 36.0 percent of elementary schools, 67.6 percent of middle schools, and 72.0 percent of high schools (NCES, 2021, Table 233.70).

The current prevalence of police in schools reflects steady growth over time. Over the past several decades, schools, districts, and states have invested more heavily in school-based

police. This push for police in schools began during the era of heightened juvenile crime in the 1990s, at a time when school safety was a significant concern (James & McCallion, 2013). These school safety concerns and increased demand for SROs were heightened by high-profile school shootings such as the Columbine massacre in 1999 (Curran, Fisher, & Viano, 2020). The federal government further encouraged this growth in SROs through large funding initiatives, including the COPS in Schools (CIS) program, and the Safe and Drug Free Schools and Communities Act (SDFSCA). Partly as a result of these investments, the number of police officers assigned to work as SROs increased from around 12 thousand in 1997 to around 20 thousand in 2003, according to local law enforcement agency surveys (James & McCallion, 2013).

### **Effects of Police in Schools**

This increase in SROs has only recently been followed by an increase in research on the impact of SROs in schools. (Anderson, 2018; Gottfredson et al., 2020; Owens, 2017; Weisburst, 2019; Zhang, 2019; Sorensen, Shen, & Bushway, 2021). There are two major challenges facing researchers intent on studying the impact of SROs – the lack of data at the school level and the difficulty of distinguishing causality from correlation in this context.

An early study by Na and Gottfredson (2011) used data from the U.S. Department of Education’s School Survey on Crime and Safety. This survey is a representative cross-sectional survey of school administrators which is conducted every two years. A small subsample (non-representative) of schools (N=470) were sampled in three surveys in a row (2003-4, 2005-6 and 2007-8). Na and Gottfredson found that schools that added armed security officers since the last survey recorded 29% more weapons and drug violations in the year they added the officer than schools that do not add armed security staff. One interpretation is that this finding is associated



with an increase in detection, rather than an increase in the underlying behavior. Their model has the advantage of looking at changes rather than levels, but the paper did not test whether the patterns of change before the arrival of the SRO were similar for schools that did and did not receive a police officer. The authors showed clearly that schools with SRO's looked very different than schools without SRO's.

Two key papers addressed the latter problem using exogenous variation in the presence of SROs caused by the federal Cops in Schools (CIS) grants program (Owens, 2017; Weisburst, 2019). Using data from the 2003, 2005 and 2007 National Center for Education's Statistics School Survey on Crime and Safety (SSCS), Owens (2017) showed that CIS grants were associated with reductions in recorded student misbehavior, and small increases in the likelihood that school administrators report contacting police about the incidents that are recorded. Owens (2017) also used data from the National Incident Based Reporting System (NIBRS) between 1997 and 2007 to identify delinquent events in schools that are officially reported to the police. She found that police jurisdictions that received CIS grants learned about more violent delinquency taking place in schools, along with more weapons and drug violations, and appeared to have more arrests of juveniles less than 15 years of age for delinquent acts committed in school.

One concern about Owen's work is that it lacked data on key school-related student outcomes like suspensions. Weisburst's paper in 2019 responds directly to these concerns. Like Owens, she studied the impact of the CIS program, but unlike Owens, she was able to study the impact of receiving a CIS grant on in-school suspensions, out-of-school suspensions and expulsions by school administrators using data on 7<sup>th</sup> through 12<sup>th</sup> graders from Texas public schools from the 1998-1999 school year to the 2007-2008 school year. She did not have data on student misbehavior unless it led to one of those three disciplinary actions. She also did not have

data on the presence of school resource officers at the school or district level. She found that, for middle school students, receiving a CIS grant increased the number of students who received one of the measured school discipline actions, particularly disciplinary actions associated with low-level offenses. She found that receiving a CIS grant increased suspensions and expulsions at a rate that was 50% greater for Black students than White students – a result that is consistent with the claim that police officers in schools are partially responsible for the disparate impact of school discipline on Black students. Weisburst (2019) also found that students in middle schools in a district with a CIS grant experienced a 2.5 percent reduction in high school graduation rates and a 4% decrease in college enrollment. She did not find a significant difference in these effects by race.

Although both of these papers study variation in funding for SROs, both analyses are limited to looking at the impact of CIS funding at the school district level, rather than variation in the presence of a school resource officer in a particular school. As a result, their measured effects may be substantially muted, since not all schools in a school district will receive the treatment when the federal government awards a CIS grant.

This muting is also observed in a third study, by Anderson (2018), which also looked at the increase of funding for SROs. Anderson (2018) studied the impact of NC Bill 402:8.36 which provided \$2 in state matching funds for every additional \$1 spent by school districts on SROs in schools. Between 2013 and 2018, 50 of the 110 NC school districts received matching funds. Anderson used a difference in differences design (DID) that switched on for the 2013-2014 school year for any district that ever received matching funds before 2018. The key dependent variable was a count of the number of reported offenses at the school of the 16 serious offense types that schools are required to report to the state. The pre-existing downward trend in misbehavior for schools in treatment and control school districts was similar, although the schools in the treatment

districts had fewer of the serious offenses reported than the schools in the control districts throughout the study period. He found that the funding did not lead to a further decrease in the number of reported acts for school districts that got funding relative to those that did not, a result that led him to conclude that the bill had failed to achieve its goal. As in Owens (2017) and Weisburst (2019), Anderson (2018) did not know which schools actually got an SRO within a school district.

Two other recent papers, by Zhang (2018) and Gottfredson et al. (2020), were able to actually study the presence of an SRO at the school level. Zhang (2018) used between-school variation in the presence of police in West Virginia middle and high schools from 2014-16 to study the impact of SROs on disciplinary events. Gottfredson et al. (2020) examined monthly disciplinary data over two years from 33 middle and high schools in California that increased SRO staffing compared to a matched sample of California schools that did not change SRO staffing. Zhang (2018) found that schools with a police officer for at least one year recorded roughly 35% more drug violations than schools without an officer. Gottfredson et al. (2020) found that increased SRO presence in schools increased the number of drug- and weapon-related offense reports relative to matched schools without increased SRO staffing, and increased the number of exclusionary disciplinary actions by school administrators. The results of these two studies highlight the possible tension between increased school safety – through increased knowledge of delinquent or problematic behavior by both school administrators and law enforcement – and more frequent imposition of punitive disciplinary sanctions. However, the two studies’ use of between school measures without exogenous variation – they controlled for selection with methods that used observable variables that differ between the treatment and control groups – leaves open the

possibility that the increased drug and weapon offenses caused the presence of the SROs, rather than the other way around.

The most recent study, by Sorensen, Shen and Bushway (2021) focused on within school (rather than between school) differences in the presence of an SRO in North Carolina. Sorensen, Shen and Bushway (2021) use incident-level administrative data to examine the impact of SROs on both reported disciplinary events and assigned disciplinary consequences. Specifically, they examine the impact of SROs in North Carolina middle schools on short-term student outcomes in school and in the juvenile justice system during the years 2005-2009 as well as long-term student outcomes in educational attainment and the adult criminal justice system. They found that SROs reduce serious violent behavior on school grounds, but have no effect – positive, or negative – on weapon, drug, or alcohol offenses, suggesting that the prior results showing an increase in detection might indeed have been a selection artifact. They also found that an increase in SRO presence leads to a higher chance of a referral to law enforcement, particularly for Black and Hispanic students and that SROs substantively increase the probability of receiving short-term out-of-school suspensions among Hispanic students and long-term out-of-school suspensions, transfers to alternative schools, or expulsions among Black students given a reported offense. Despite increasing referral to law enforcement per offense, an increase in SRO presence does not result in a substantially greater number of juvenile justice complaints against students. Overall, these findings imply that although SROs in North Carolina primarily seek non-criminal justice strategies to deal with student problems when asked to intervene, these strategies still disproportionately intensify the punitive environment faced by Black and Hispanic students within schools.

Despite the reliance on within school variation instead of between school differences, the Sorensen, Shen and Bushway (2021) analysis suffers from the lack of exogenous variation in the presence of school resource officers. In addition, the paper had some measurement error in the measure of SROs, a factor which could have dampened the estimated effect. Another limitation of the Sorensen, Shen and Bushway (2021) paper is that it uses variation in SROs from 10-15 years ago. Debates about defunding the police are happening now. These debates and the events that motivate them may be changing the effects of police presence on students. Theft and violent victimization rates for students have declined over 80% from 1992 to 2018, both inside and outside of schools (<https://nces.ed.gov/fastfacts/display.asp?id=49>). The potential positive effect of SROs may now be more limited, while the negative effects might still occur regularly.

The current study, which studies school-level variation in the presence of a SRO at the national level for the 2017-2018 school year represents a significant improvement over the Sorensen, Shen and Bushway (2021) study. It also represents a significant advancement over the approach of Owens (2017) and Weisburst (2019). Like these two earlier studies, we use variation induced by the federal COPS funding. However, unlike these earlier studies, we use the discontinuity at the application score cutoff in the probability of a grant award and in the probability of gaining an SRO, combined with a detailed understanding of the award process, to isolate and then analyze the impact of this plausibly exogenous variation in SROs. The prior studies were limited to looking to the link between receiving the grant and student outcomes at the school district level. The current approach also allows us to look at the direct impact of SROs at the school, rather than district, levels, a major advantage given that there is substantial variation in the use of school resource officers within districts.

## Method

### School Data

This study uses data from the 2014, 2016, and 2018 waves of the CRDC from all public schools in the U.S., excluding preschools and excluding schools with fewer than 50 students. We also restrict the sample to schools that appear in all three waves of the CRDC data. Finally, we remove schools which have missing information on school law enforcement officers. The final sample prior to linking the CRDC with law enforcement data includes 84,726 schools tracked across three survey waves.

The most critical variable for our analysis is the number of full-time-equivalent (FTE) sworn law enforcement officers reported by the school. CRDC defines a sworn law enforcement officer as a career law enforcement officer with arrest authority. They provide further clarification on this definition in the school data collection form (CRDC 2016, p. 37):

“A sworn law enforcement officer may be a school resource officer (who has specialized training and is assigned to work in collaboration with school organizations). A sworn law enforcement officer may be employed by any entity (e.g., police department, school district or school). An officer’s duties may include: motor vehicle traffic control; security enforcement and patrol; maintaining school discipline; coordinating with local police and emergency team(s); identifying problems in the school and proactively seeking solutions to those problems; training teachers and staff in school safety or crime prevention; mentoring students; teaching a law-related education course or training students (e.g., drug-related education, criminal law, or criminal prevention courses); recording or reporting discipline problems to school authorities; and providing information to school authorities about the legal definitions of behavior for recording or reporting purposes (e.g., definition assault for school authorities).”

Although technically sworn law enforcement officers encompass a broader set of police officers involved in schools, we will use this term interchangeably with SRO. Importantly, this survey differentiates the sworn law enforcement officer role (or SRO) from the security guard role, who “guards, patrols, and/or monitors the school premises to prevent theft, violence, and/or

infractions of rules” (CRDC 2016, p. 37). Based on the FTE school law enforcement officer variable, we classify schools in each year into one of three mutually-exclusive categories: (i) one or more FTE school law enforcement officers (“Full-time SRO”); (ii) more than zero but less than one FTE school law enforcement officers (“Part-time SRO”); and (iii) zero FTE school law enforcement officers (“No SRO”).<sup>1</sup>

CRDC has rich information on a variety of relevant student outcomes that may be affected by presence of an SRO. Based on prior research, we hypothesize that SROs could directly or indirectly influence levels of school violence and reported crime (Na & Gottfredson, 2011; Owens, 2016; Sorensen, Shen, & Bushway, 2021; Zhang, 2019), school climate (Devlin, Santos, & Gottfredson, 2018), disciplinary or law enforcement actions (Fisher & Hennessey, 2016; Gottfredson et al., 2020; Weisburst, 2019; Sorensen, Shen, & Bushway, 2021), and academic outcomes (Weisburst, 2019). Accordingly, we include the following dependent variables:

- **Shooting incident:** A binary indicator of whether there was at least one incident at the school that involved a shooting (regardless of whether anyone was hurt).
- **Firearm-related offenses:** The sum of incidents of robbery with a firearm or explosive device, physical attack or fight with a firearm or explosive device, threats of physical attack with a firearm or explosive device, or possession of a firearm or explosive device.
- **Other offenses:** The sum of incidents of rape or attempted rape, sexual assault, robbery without a firearm or explosive device, physical attack without a firearm or explosive device, or threats of physical attack without a firearm or explosive device.

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<sup>1</sup> In the 2018 data collection, 1.8 percent of schools did not report information on FTE school law enforcement officers. These schools were dropped from the sample.

- **In-school suspension:** Number of students receiving any in-school suspension.
- **Out-of-school suspension:** Number of students receiving any out-of-school suspension.
- **Expulsion:** Number of students receiving expulsion.
- **Police referral or arrest:** Number of students referred to a law enforcement agency or official plus the number of students who received a school-based arrest.
- **Chronic absence:** Number of students chronically absent, defined as missing more than 15 school days.
- **Grade retention:** Number of students retained a grade level.

Trends in these variables are plotted from 2014 through 2018 for disciplinary and academic outcomes, and from 2016 through 2018 for offense outcomes (the years they are available), in Appendix Figure A3. For the disciplinary and academic outcomes described above, we can also further disaggregate these outcomes by student race, ethnicity, gender, Limited English Proficiency (LEP) status, and disability status. This disaggregation allows us to test for evidence of disproportional impacts of SROs on students with different backgrounds and characteristics.

Our time-varying control variables drawn from the CRDC data include total student enrollment; proportion of students by race, gender, LEP, and disability; and number of FTE teachers, guidance counselors, security guards, nurses, psychologists, and social workers.

### **Law Enforcement Agency Data**

In addition to data on schools, we also collect data on law enforcement agencies who applied for COPS Hiring Program (CHP) grants. Through a Freedom of Information Act request to the U.S. Department of Justice, we received detailed information for all applicants to the 2015, 2016, and 2017 CHP grant cycles. The problem focus areas of these grant applications span multiple domains, including “Building Trust and Respect,” “Gun Violence,” “Drug Education



and Prevention,” “School-Based Policing,” and “Youth Crime and Delinquency.” Our final sample only includes agencies that applied for school-based policing grants. For agencies receiving such grants, the CHP requires them to submit a Memorandum of Understanding (MOU) with their school partner, and requires that any SROs that are deployed complete National Association of School Resource Officers (NASRO) basic training within nine months. CHP also requires agencies to use grant funds to either directly place a new SRO at a school, or to maintain an existing SRO position that would have been removed absent receipt of the grant.

To obtain more information about both the law enforcement agencies that did, and that did not, receive CHP grants, we linked the application and award data using Originating Agency Identification (ORI) numbers to the Law Enforcement Agency Identifiers Crosswalk data (U.S. Bureau of Justice Statistics, 2018), which provided detailed geographic and identifying information on each agency.

### **CHP Award Selection Process**

Across the three grant cycles, COPS allocated a total of approximately \$452 million in awards to CHP grant recipients. \$47 million of this funding went to agencies for school-based policing efforts. In application for CHP grants, agencies submit information on: (i) reported crime for the previous three years; (ii) current commitment to community policing; (iii) planned community policing activities; (iv) changes in budget for law enforcement agencies; and (v) poverty and unemployment rates (U.S. Department of Justice, 2017). Final applicant scores equal a weighted sum of constructed community policing scores (50 percent), crime score (30 percent), fiscal need score (20 percent), and miscellaneous bonus points.<sup>2</sup> Assignment of awards based on

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<sup>2</sup> The purpose and specification of bonus points can vary from year to year, often corresponding to current preferences of Department of Justice leadership. In many years, school-based policing applications receive a set number of bonus points, which should not affect our analysis since this is a constant number added to each agency score. Following the start of the Trump Administration, the largest bonus point

these factors occurred over two stages, aligned with certain statutory requirements. One part of the statute requires that the COPS office must allocate at least 0.5 percent of total funds to each state or territory with eligible applicants. Therefore, in the first stage, they distribute awards in descending order to the highest scoring applicants in each state or territory until that state or territory has received at least 0.5 percent of total funds. A second part of the statute requires that half of funds go to agencies serving populations of more than 150,000 (“large agencies”), and half of funds go to agencies serving populations of fewer than 150,000 (“small agencies”). Following state-by-state allocation, therefore, the second stage assigns remaining funds to the top scoring agencies in each agency size bracket until half of total funds are assigned to large agencies and half of total funds are assigned to small agencies.

This process implies that the effective cutoff score for receiving a CHP award differs by year, by state, and by agency size. Through a simulated award assignment process (see Appendix B), we determine the binding cutoff score for each year, state, and agency size. The binding cutoff score is the score above which an agency in that state, year, and agency size, would receive an award; and below which they would not. Then, we subtract this binding cutoff score from each agency’s final score to create a continuous running variable centered around zero. This centered agency score reflects how “close” the agency was to receiving an award. Appendix B describes in more detail how we simulate CHP award assignment and calculate centered agency scores. Appendix Figure A1 shows the distribution of these centered scores. Figure 2(a) plots final CHP award status by the centered agency score for school-based policing applicants, and illustrates that the probability of CHP award jumps perfectly from 0 percent to 100 percent at the discontinuity.

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allocation (25 points) went to agencies fully cooperating with Immigrations and Customs Enforcement. This systematically disadvantaged agencies in so-called “sanctuary cities”.

## **Matching Schools to Law Enforcement Agencies**

To isolate variation in SRO presence at schools due to federal COPS grants, we match each law enforcement agency in the COPS data with the school district in which it is located. We use geographic distance as a proxy for the likelihood of school-police partnerships, since there is currently no publicly available information on actual school-police partnerships or memorandums of understanding (MOUs) between schools and police agencies nationally.

We begin with the longitude and latitude for each applicant law enforcement agency from the Law Enforcement Agency Identifiers Crosswalk (U.S. Bureau of Justice Statistics, 2018).<sup>3</sup> Figure 1 presents a map of these agencies, along with information about the year of application, the number of officers requested, and whether an award was made.

We use the MABLE/Geocorr geographic correspondence engine with 2010 US Census block group information and 2014 school district information to obtain the longitude and latitude of the centroid of every block group in the nation, as well as the percentage of the population of each block group that lived in each school district (Blodgett and Meij, 2000). We identify the block group with a centroid closest to each agency that are also in the same state and county as the agency. For 276 of the 293 agencies, the nearest block group is entirely within a single school district and we therefore match that district and agency. For the 17 agencies for which the nearest block group is served by multiple districts, we then examine the three block groups nearest to the agency and make a match based on district coverage of the three block groups and based on the agency and school district names. We achieve a full match to school districts for all agencies located in the 50 U.S. states, but not for those located in U.S. territories, which are excluded from the analysis.

## **Empirical Strategy**

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<sup>3</sup> For law enforcement agencies that did not have latitude or longitude information from the Law Enforcement Agency Identifiers Crosswalk, we manually searched for the addresses and locations of these agencies.

To estimate the effects of SRO presence in schools on student outcomes, this study uses a fuzzy RD approach. Specifically, it uses the 2018 indicator for a full-time school law enforcement officer as the treatment variable and relies on the similarity between districts linked to agencies at the discontinuity in school-based policing CHP awards at the application score cutoff to identify the impact of SROs on student outcomes. Once we have matched each law enforcement agency in our data to the nearest school district, we set up the following fuzzy RD analysis:

$$(1) \quad SRO_{jk} = \alpha_0 + \alpha_1 \mathbb{I}(Score_k > 0) + \alpha_2 f(Score_k) + \alpha_3 X_{jk} + \mu_{jk} \text{ if } Score_k \in [-b, b]$$

$$(2) \quad Y_{jk} = \gamma_0 + \gamma_1 \widehat{SRO}_{jk} + \gamma_2 f(Score_k) + \gamma_3 X_{jk} + \rho_{jk} \text{ if } Score_k \in [-b, b]$$

The first stage regresses the 2018 measure of SRO presence in school  $j$  linked to law enforcement agency  $k$  ( $SRO_{jk}$ ) on a parametric function of the centered application score of agency  $k$  from the COPS hiring grant program ( $f(Score_k)$ ), an indicator equaling one if the agency scored above the binding cutoff, and zero otherwise ( $\mathbb{I}(Score_k > 0)$ ), and school and agency control variables ( $X_{jk}$ ). The second stage then regresses outcomes for students in school  $j$  linked to agency  $k$  on the predicted SRO variable from the first stage and the parametric function of the centered running variable. We consider multiple functional forms for the running variable, including linear, cubic, and quintic. We also restrict the analytical sample to various local bandwidths  $[-b, b]$  around zero.

The logic behind this estimation is, first, that schools residing near law enforcement agencies that receive COPS hiring grants for school-based police officers in 2015 to 2017 are more likely to gain SROs by 2018 than are schools residing near law enforcement agencies that do not receive such grants. We confirm this in a first stage equation (Table 2). Second, this approach assumes that schools residing near agencies that score *just below* the COPS grant

award threshold do not differ systematically from schools residing near agencies that score *just above* the COPS grant award threshold. If this is the case, then the  $\gamma_1$  parameter from the second stage should reflect the causal effect of SRO presence on student outcomes. To strengthen the internal validity of this design, we also control in  $X_{jk}$  for a series of lagged dependent variables from the 2014 school year. Therefore, we can interpret the effects of a full-time SRO as the effects on *within-school changes* over time in student offenses, disciplinary consequences, and academic measures.

The vector of control variables also includes indicators of school level (elementary / middle / high / other); indicators of school type (traditional / charter / magnet / special education / alternative); indicators of school location (urban / suburban / town / rural); logged student enrollment; pupil-to-teacher ratio; proportion of students by race and ethnicity (Black / Hispanic / other); proportion of students identified as Limited English Proficiency; proportion of students identified to receive services through the Individuals with Disabilities in Education Act; number of FTE staff by category (guidance counselors / security guards / nurses / psychologists / social workers); population size served by law enforcement agency (fewer than 150,000 / greater than 150,000); community policing score of the agency; fiscal need score of the agency; and crime score of the agency.<sup>4</sup>

In the first stage equation, we weight observations by the number of police officers requested in the CHP grant application divided by the number of students in the district. This weighting reflects the fact that student exposure to SROs will be greater in small districts that receive a relatively large number of police officers than in large districts that receive a relatively small number of police officers. In the second stage equation, we weight observations by the

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<sup>4</sup> These three scores are subscores of the CHP application score described above.

number of students in the school. We use standard errors computed from 1,000 bootstrapped samples and clustered by school district to reflect the variation in treatment at the agency-district level. Bootstrapped standard errors also reflect best practices for ensuring that first stage partial F-statistics are not overly inflated (Lal, Lockhart, Xu, & Zu, 2021).

## **Results**

### **Patterns in School Adoption of SROs**

Table 1 presents a simple comparison of characteristics between schools with a full-time SRO and schools without a full-time SRO among schools who apply for a CHP school-policing grant. We also present the average characteristics of all schools in the U.S. as collected by the CRDC. Schools who employ a full-time SRO have higher crime and discipline rates than those without a full-time SRO. This difference is significant for most of the outcomes analyzed. In general, student suspensions, expulsions, arrests and referrals, absences, and gun-related offenses are significantly higher in schools with full-time SROs. Higher discipline rates in schools with police are also found by Na and Gottfredson (2011). The finding of higher crime rates in schools with full-time SROs is consistent with other findings in the literature. For example, the Justice Policy Institute (2020) reports that schools with police report more crimes than schools without one. These crimes include gun-related offenses like attacks and threats with weapon, and other offenses. Similarly, Na and Gottfredson (2011) find that schools with police report considerably more offenses, for each of fourteen different crime types, than schools without police. When analyzing characteristics of schools with and without an SRO, Theriot (2009) finds that, in general, rates of arrests and charges are higher in the former.

In terms of school characteristics, Table 1 shows that secondary schools are more likely to have full-time SROs than elementary schools. Elementary schools make up only 11% of the schools with full-time SROs, but 57% of the schools without full-time SROs. Prior national reports and research studies confirm this differential pattern by school level (Na & Gottfredson, 2011; NCES, 2021). Interestingly, our sample of schools shows that on average the proportion of White students is higher in schools with full-time SROs. On the other hand, the proportion of minority students, including Black and Hispanic students, is lower in schools that have a full-time SRO than in those without one. This finding in student composition contrasts with other findings in the literature. For instance, Theriot (2009) reports that schools with an SRO have a significantly higher share of students from a racial or ethnic minority.

### **Effects of SROs on Student Outcomes**

We proceed to examine the impacts of SRO presence on three categories of student outcomes: (i) incidence of violent offenses in schools; (ii) incidence of students receiving disciplinary or law enforcement actions; and (iii) incidence of academic outcomes. To do so, we use the threshold effect of COPS hiring grants on SRO full-time presence at schools as a first stage. Our preferred estimate in row two, column one, of Table 2, which uses a bandwidth of 20 points and a linear control for the running variable, shows that being near a law enforcement agency that scored above the COPS hiring grant award threshold increases a school's likelihood of having any SRO by 18.0 percentage points ( $p < 0.01$ ). The corresponding F-statistic is 38.1, which allows for a good degree of certainty for hypothesis testing in the second stage (Lee, McCrary, Moreira, and Porter, 2020). The RD coefficient estimate is positive and statistically significant at the 99 percent level in 13 of the 15 alternative specifications with different bandwidths and polynomial orders, and ranges in magnitude between a 11.7 percentage point

increase and 19.5 percentage point increase in likelihood of a full-time SRO. Figure 2(b) illustrates this discontinuity in the likelihood of full-time SRO presence visually.

Moving to the second stage of the fuzzy RD, the first three columns of Table 3 present effects of full-time SRO presence on school shootings and the incidence of student offenses. Each cell of the table presents an impact estimate, its standard error in parentheses, and the average outcome for schools without a full-time SRO in brackets. We find little evidence of an effect of SROs on the likelihood of a shooting incident at the school, although the point estimate is positive and marginally significant in the full sample and secondary school sample. We find, at least for the full sample, a sizeable and positive effect of firearm offenses – suggesting that having an SRO increases the number of reported firearm offenses by .184 per 100 students (or a 400 percent increase from baseline). This result represents a common finding that SROs increase the detection of weapons offenses (Gottfredson et al. 2020). When we expand the analysis to include the bulk of (non-firearm related) school violence, primarily fights and threats, we find that the presence of an SRO leads to a reduction of 6 offenses per 100 students in a given year (or a 260 percent decrease from baseline). This result is similar in both primary and secondary schools and substantial – given that the mean rate of events in the whole sample is 3 per 100 students (albeit with a strong positive skew). In totality, these results suggest that police in schools might effectively reduce fighting and attacks at school.

However, the remainder of Table 3 makes it clear that this potential benefit comes at very high cost. Across all schools, the presence of an SRO increases the rate of in-school suspensions by 5.7 students (per 100), and the rate of out-of-school suspensions by 10.9 students (per 100). The effects for expulsion (.885) and referral for arrest (2.647) are smaller but still quite large in relative terms. These increases in student punishment are particularly dramatic in middle and



high schools, with observed increases per 100 students of 17.8 more students receiving out-of-school suspension, 1.7 more students receiving expulsion, and 4.8 more students receiving referral to law enforcement or school-based arrest. Although we find no effect on student grade retention, we do find large effects on chronic absenteeism, suggesting that the presence of a full-time SRO increases the rate of chronic absence by 12.2 students for every 100 students. Perhaps not surprisingly, these effects are more prominent (and always significant) in secondary schools as compared to primary schools. In either case, the effects are very large – often representing an increase of 100% or more over baseline levels.

In addition to estimating fuzzy RD (2SLS) models of the effects of full-time SROs on student outcomes, we also conduct sharp RD (reduced form) estimation of the effects of the school-based policing award cutoff. These results, plotted graphically in Appendix Figure A2, tell us the effects of a school-based policing award at a nearby agency on student outcomes, regardless of whether that award translated into the placement of a full-time SRO. The results from this analysis are generally consistent with the 2SLS results, although smaller in size. In particular, they suggest that school-based policing awards increase the likelihood a school shooting by 1 percentage point; increase gun-related offenses by .04 incidents per 100 students; decrease other violent offenses by .2 incidents per 100 students (not significant); increase ISS by 1.9 students suspended per 100; increase OSS by 3.6 students suspended per 100; increase expulsions by .2 students expelled per 100; increase police referrals and arrests by .5 students referred or arrested per 100; increase chronic absenteeism by 3.0 students chronically absent per 100; and increase grade retention by 0.6 students retained a grade level per 100. As intent-to-treat estimates, we regard these effects as likely lower bounds of the true effects of SROs on student outcomes.

## **Effects of SROs on Outcomes, by Student Characteristics**

Of course, concerns about police in schools extend beyond concerns over impacts on the average student. The school-to-prison pipeline narrative asserts that having police in schools may disproportionately affect students by race, gender, or other characteristics (Homer & Fisher, 2020). We test for heterogeneous effects formally by using CRDC's disaggregated data on disciplinary and law enforcement actions by student race, gender, limited English proficiency status, and disability status. Specifically, we replicate the same fuzzy RD method from above with dependent variables of the number of students per 100 with each characteristic type receiving a certain disciplinary action or adverse academic outcome. Unfortunately, the CRDC does not collect disaggregated data on student offenses.

These results are presented in Table 4. In column 1, we see that SRO presence increases the incidence of ISS most for Black students at 11.4 additional suspensions per 100 students, and male students at 7.4 additional suspensions per 100 students. The estimated effect on Black students is over double the effect size for White students. Columns 2 and 3 present effects of SROs on OSS and expulsion by student subgroup. The largest effects of SROs on student OSS occur for Black students at 26.2 per 100 students, students with disabilities at 23.0 per 100 students, and male students at 16.9 per 100 students. Once again, the increase in OSS incidence associated with full-time SRO presence for Black students is more than double the same increase in OSS for White students. The patterns of disproportional effects are similarly stark for expulsion, which represents the most serious school-assigned disciplinary consequence.

Referrals to police occurring as a result of SRO presence also appear to predominantly affect Black students with 5.3 additional referrals or arrests per 100 students, students with disabilities with 4.5 additional referrals or arrests per 100 students, and male students with 3.4

additional referrals or arrests per 100 students. The effect of a full-time SRO on Black student referrals and arrests is, similar to school disciplinary outcomes, over two times larger than the same effect on White students. Finally, in columns 5 and 6, we estimate differential effects of full-time SROs on chronic absenteeism and grade retention. Students with disabilities represent the most affected group for this outcome, experiencing an increase in chronic absenteeism of 13.4 students per 100 due to increased police presence. In the case of this outcome, SROs increase the chronic absenteeism rate of White students more so than Black or Hispanic students. Finally, there is only one group that experienced any significant change in grade retention rates following the introduction of a full-time SRO, and that was Black students who faced an increase of 3.5 grade retained students per 100.

The results of this analysis suggest that the introduction of SROs into schools intensifies the levels of punishment unevenly across different groups of students, and that Black students, male students, and students with disabilities generally bear the brunt of this punishment.

### **Robustness Tests**

In this section, we test the sensitivity of our analysis to alternative specifications, and check for potential threats to exogeneity. First, we estimate each of our main regressions from Table 3 substituting the linear control of the running variable with a quintic polynomial function and expanding the bandwidth from 20 to 40 points above and below the effective cutoff. This alternative specification corresponds to the first stage model in row 4, column 3 of Table 2. Appendix Table A1 presents corresponding second stage results. All of the estimated effects of SROs on student outcomes from our preferred models (Table 3) persist with this alternative bandwidth and polynomial function control. Specifically, full-time SROs increase the likelihood of a school shooting, increase firearm-related offenses, decrease other violent offenses, and

increase out-of-school suspensions, expulsions, referrals and arrests, and chronic absenteeism, in the full sample of schools. We lose some statistical power in the primary school and secondary school split samples, but the point estimates are still consistent with our general results.

Second, we attempt to account for the non-normal distribution of our outcome measures. These count variables have right-skewed distributions, with the majority of schools having low numbers of offenses and disciplinary events, and a smaller subset of schools having a very large number of offenses and disciplinary events. To account for these distributions, we winsorize all outcome measures by setting values above the 95<sup>th</sup> percentile value equal to the 95<sup>th</sup> percentile of that variable. The results from these transformed outcomes are presented in Appendix Table A2. Again, the main findings persist in this specification: other violent offenses go down, and out-of-school suspensions, expulsions, referrals and arrests, and chronic absenteeism all go up, upon introduction of a full-time SRO. Overall, outlier values do not appear to be driving our main findings.

Finally, we seek to confirm the internal validity of our RD design. That is, is it really the case that schools near agencies just to the left of the CHP award threshold are indistinguishable from schools near agencies just to the right of the CHP award threshold? To confirm that this discontinuity represents exogenous variation in SRO presence, we test the effect of being above school-based policing award cutoffs in 2015, 2016, and 2017, on lagged school characteristics and discipline and academic measures from 2014. Theoretically, the grant award cutoffs in 2015 to 2017 should have no effect on school measures in 2014 unless there is some endogeneity present in the cutoff. Column 1 in Table A3 presents the results from this reverse causality test. The school-based policing award cutoff has null effects on all twelve outcomes tested, although a marginally significant effect on students receiving expulsion ( $p < 0.1$ ). In addition to finding no

significant associations at the 95% level, the point estimates are also generally small in magnitude. This analysis helps to confirm that the award discontinuity is not associated with any baseline differences in school characteristics, student characteristics, or rates of discipline or academic outcomes.

We also repeat this same exercise using two-stage least squares estimation of the effects of a full-time SRO on 2014 variables (column 2 of Table A3), building off of the reduced form estimation. This time we still find null effects of a full-time SRO in 2018 on eleven of the twelve baseline variables from 2014. However, we do find one large and significant effect of a full-time SRO in 2018 on student chronic absenteeism in 2014 ( $p < 0.01$ ). Although this one violation of the reverse causality test could merely reflect random variation, we still urge some caution for interpretation of the main effects of SROs on student chronic absenteeism.

## **Discussion**

The results of this study present a difficult set of tradeoffs. On the one hand, SROs appear to meet some of their stated objectives. They appear to protect students from a non-trivial number of physical attacks and fights within schools – an effect that could generate a variety of long-term academic and psychological benefits to students through decreased exposure to violence (Burdick-Will, 2016) or through reductions to disruptions in the academic environment (Figlio, 2007). On the other hand, we find no evidence that SROs prevent school shootings or reduce more serious firearm-related offenses. In addition, having an SRO in the school also leads to undeniably harsher disciplinary punishments for students, and particularly for Black students, male students, and students with disabilities. This occurs despite the fact that SROs are typically not supposed to, and often do not intend to, become involved in minor disciplinary matters in

the school (Curran, Fisher, Viano, & Kupchik, 2019). The observed increase in suspensions, expulsions, and police referrals and arrests found in this study is especially worrying, given the potential for minor acts of misconduct in schools to translate into long-term involvement in the juvenile justice or adult criminal justice systems (Wald & Losen, 2003).

Some of the findings in this study parallel findings from prior research on SROs. For instance, we find positive effects of SROs on firearm-related offenses. Prior studies have documented that the funding of an SRO is often followed by an increase in reported and recorded crimes, particularly for drugs and weapons (Devlin & Gottfredson, 2016; Owens, 2017). This is likely because SROs add an additional layer of crime detection and crime reporting capacity to the school. Unfortunately, this reporting/recording phenomenon makes it difficult to ascertain with certainty whether SROs effectively make schools safer from the types of firearm crimes that SROs are often hired specifically to prevent. Nonetheless, even if SROs increase reporting of misbehavior, our study still finds that SROs reduce some forms of school-based violent offenses (similar to Sorensen, Shen, and Bushway, 2021).

Our robust finding that SROs intensify student suspensions, a result which replicates the results of much prior work (Gottfredson et al., 2020; Sorensen, Shen, & Bushway, 2021; Weisburst, 2019), provides the most compelling evidence yet that stationing police in schools could put at risk other efforts to improve equity in K-12 education. The suspension of students appears to directly harm both their academic achievement (Anderson, Ritter, & Zamarro, 2019; Hwang, 2018; Lacoë & Steinberg, 2019) and their longer-run outcomes such as educational attainment and criminal justice involvement (Bacher-Hicks, Billings, & Deming, 2019; Sorensen, Bushway, & Gifford, Forthcoming). Similarly, recent research has concluded that student absence from school reduces educational engagement and achievement (Aucejo &

Romano, 2016; Gottfried, 2014) and even decreases the likelihood of high school graduation and college enrollment (Liu, Lee, & Gershenson, 2021). Our large estimated effect of SROs on chronic absenteeism, which is the first time this link has been tested in the recent set of quantitative studies, provides another mechanism through which SROs can negatively affect students. The fact that our effect sizes were many times larger for Black students and students with disabilities forces us to conclude that the use of police in schools appears to seriously exacerbate existing opportunity gaps in education.

There are several limitations to this study. First, the CRDC data, which is a census covering every public school, appears to seriously underreport the presence of law enforcement officers at schools, based on comparison to a nationally representative survey also conducted by the U.S. Department of Education (NCES, 2021). The main difference in the measurements comes from the fact that the CRDC data which we use come from administrative data provided by school districts and the SSOCS comes from principals at the individual schools. Our estimates should be interpreted more narrowly therefore as a comparison between schools where the district *reports* law enforcement officer presence and schools where the district *reports* no law enforcement officer presence. Some of the schools in our population that are reported as having no law enforcement officer presence may in fact have an officer present at least part time. Without further information about the outcomes at schools where the district and principal disagree about the presence of an SRO, we cannot determine the direction or size of the bias.

Second, our fuzzy RD estimates provide information on the local average treatment effect of SROs based on the margin of schools that would not have hired an SRO absent the federal COPS grant funding mechanism. Like Owens (2017) and Weisburst (2019), this means that our estimates may not generalize to SROs hired or introduced to schools through other mechanisms.

Finally, the majority of outcome variables in this study are not normally distributed. They are mostly count variables with over-dispersion and zero-inflation, which could cause issues for statistical inference with OLS.

Nonetheless, this study presents – to our knowledge – the first school-level examination of sworn law enforcement officers across the full universe of public schools in the U.S. As districts across the country continue to consider (or re-consider) their investments in school-based policing, particularly in the context of new public health and mental health demands imposed on schools, our findings about the impacts of police officers on students from diverse communities should help to inform these decisions. It suggests that interventions should not just be judged on a single outcome, but comprehensively on many outcomes. It also suggests that the comprehensive impact of using resources for school police should be compared with the comprehensive impact of using resources in other ways to improve school safety and climate. For example, recent evaluations of implementations of restorative practices in schools have demonstrated the potential of a single intervention to both reduce suspensions and improve school climate (Augustine, et al. 2018).



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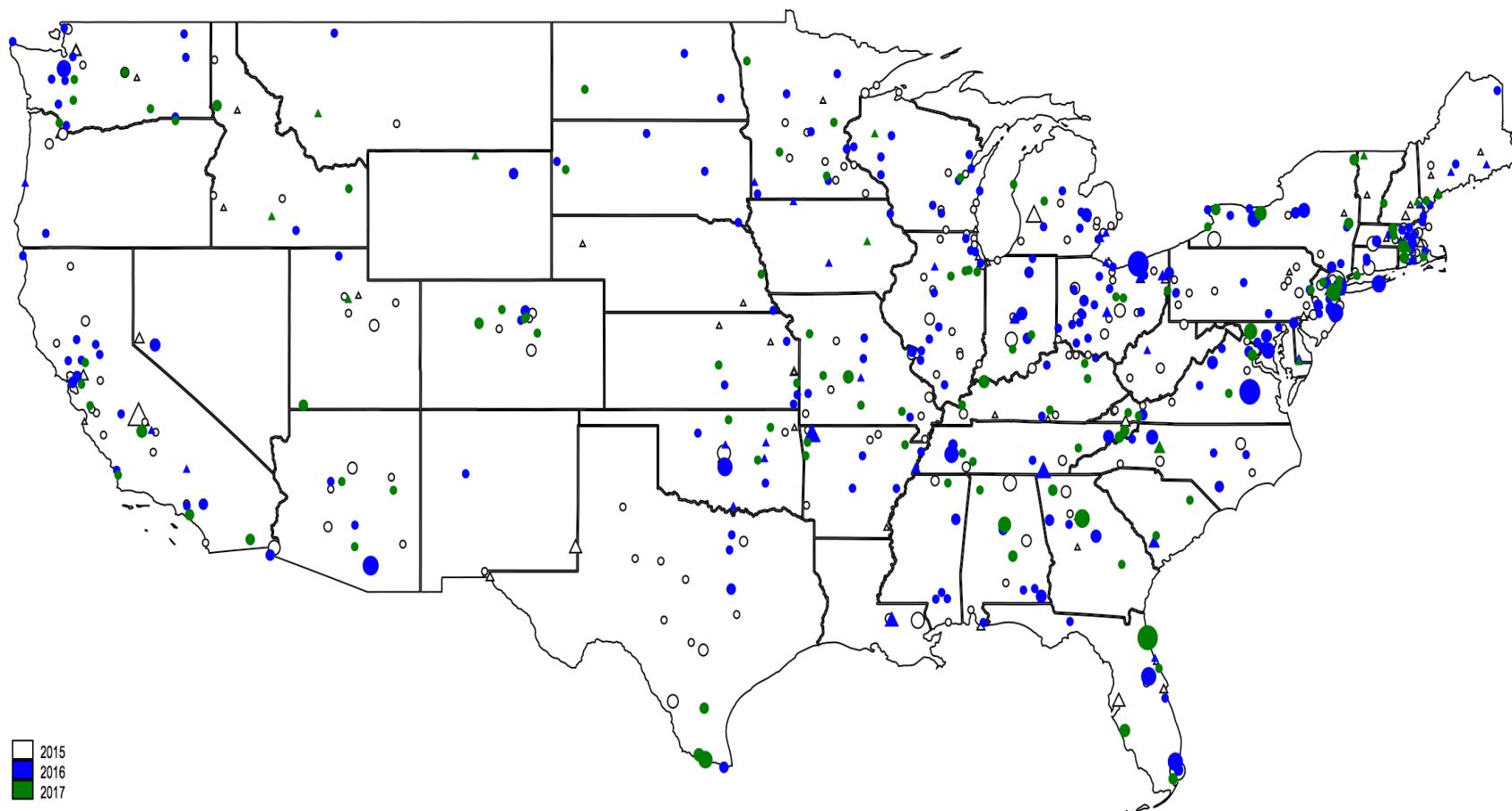
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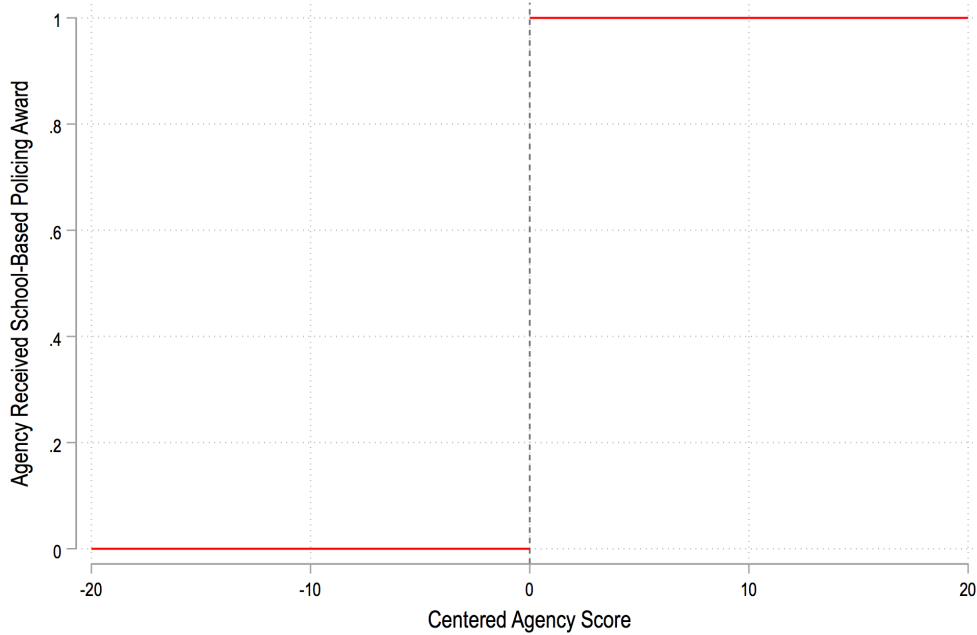
## Tables and Figures

**Figure 1. Locations of Agencies Applying for School-Based Policing Grants**

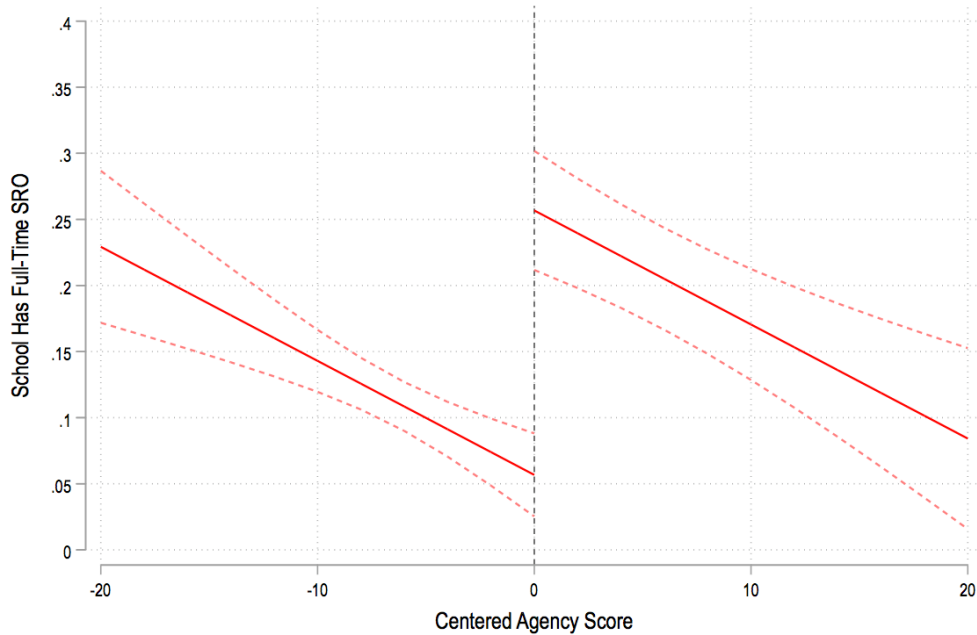


*Note.* This map plots the coordinates of each applicant for a school-based policing award through the COPS Hiring Program in the 2015, 2016, and 2017 grant cycles. For agencies applying in multiple years, we keep the year with the highest score. Circles represent agencies not receiving awards, and triangles represent agencies receiving awards. Each point is sized proportionally by the number of police officers requested in the application. Excluded in this map (though included in the analysis) are agencies in Hawaii and Alaska.

**Figure 2. Exposure to School-Based Police by Agency Application Score**  
**(a) Agency Receipt of School-Based Policing Award**



**(b) Full-Time SRO Presence in Nearby Schools**



*Note.* These graphs plot linear fits of SRO variables by centered agency application score: (a) restricts the dataset to one observation for each agency within 20 points of the effective threshold (314 applicants out of 567); (b) restricts to schools matched to agencies within 20 points of the effective threshold (5,247 schools out of 8,758). Dashed lines are 95% confidence intervals.



**Table 1. Descriptive Statistics**

	(1) Full CRDC Sample (84,726)	(2) Analytical Sample (n=8,758)	(3) No Full-Time SRO (n=7,659)	(4) Full-Time SRO (n=1,099)	(5) P-value of Difference (4) – (3)
<b>Outcomes</b>					
School shooting incident (per 100 students)	0.0044 (0.066)	0.0033 (0.058)	0.0032 (0.056)	0.0046 (0.067)	
Gun-related offenses (per 100 students)	0.045 (0.41)	0.033 (0.24)	0.031 (0.24)	0.051 (0.25)	**
Other offenses (per 100 students)	2.34 (6.72)	2.90 (7.35)	2.86 (7.30)	3.16 (7.66)	
Students receiving any ISS (per 100)	4.51 (7.87)	3.79 (7.13)	3.29 (6.67)	7.23 (9.06)	**
Students receiving any OSS (per 100)	4.78 (7.71)	4.69 (7.56)	4.25 (7.35)	7.72 (8.32)	**
Students receiving expulsion (per 100)	0.19 (1.14)	0.12 (0.81)	0.093 (0.74)	0.34 (1.16)	**
Students receiving arrest/referral (per 100)	0.48 (2.39)	0.49 (1.78)	0.39 (1.62)	1.16 (2.52)	**
Students chronically absent (per 100)	15.9 (15.1)	17.8 (15.7)	17.4 (15.7)	20.3 (15.0)	**
Students retained a grade (per 100)	1.65 (4.99)	1.98 (5.30)	1.95 (5.37)	2.22 (4.74)	
<b>School Policing</b>					
Full-time sworn law enforcement officer	0.15 (0.36)	0.13 (0.33)	0.00 (0.00)	1 (0)	NA
Part-time sworn law enforcement officer	0.11 (0.32)	0.090 (0.29)	0.10 (0.30)	0 (0)	**
<b>Agency Characteristics</b>					
School-based policing award	--	0.22 (0.42)	0.22 (0.42)	0.22 (0.42)	
Final score (centered)	--	-14.7 (19.4)	-14.4 (19.0)	-16.8 (21.7)	
Final score (original)	--	116.2 (17.9)	116.5 (17.6)	114.2 (19.8)	**
Fiscal need score	--	7.19 (2.31)	7.20 (2.30)	7.08 (2.39)	+
Crime score	--	24.7 (9.80)	24.8 (9.82)	23.8 (9.64)	**
Community policing score	--	71.3 (13.5)	71.3 (13.4)	70.9 (14.2)	
Large agency indicator	--	0.23 (0.42)	0.24 (0.43)	0.14 (0.34)	**
<b>School Characteristics</b>					
Elementary school	0.44 (0.50)	0.52 (0.50)	0.57 (0.49)	0.11 (0.31)	**
Middle school	0.16 (0.37)	0.17 (0.37)	0.15 (0.35)	0.31 (0.46)	**
High school	0.17 (0.38)	0.15 (0.36)	0.11 (0.31)	0.45 (0.50)	**
Other grade configuration	0.23 (0.42)	0.16 (0.37)	0.17 (0.37)	0.13 (0.34)	**
Traditional school	0.86 (0.34)	0.84 (0.37)	0.83 (0.38)	0.88 (0.32)	**

	(1) Full CRDC Sample (84,726)	(2) Analytical Sample (n=8,758)	(3) No Full-Time SRO (n=7,659)	(4) Full-Time SRO (n=1,099)	(5) P-value of Difference (4) – (3)
Special education school	0.012 (0.11)	0.010 (0.100)	0.011 (0.11)	0.0018 (0.043)	**
Charter school	0.064 (0.25)	0.041 (0.20)	0.046 (0.21)	0.0064 (0.080)	**
Magnet school	0.045 (0.21)	0.099 (0.30)	0.099 (0.30)	0.099 (0.30)	
Alternative school	0.015 (0.12)	0.015 (0.12)	0.016 (0.13)	0.011 (0.10)	
Urban location	0.26 (0.44)	0.31 (0.46)	0.30 (0.46)	0.35 (0.48)	**
Suburban location	0.36 (0.48)	0.53 (0.50)	0.54 (0.50)	0.41 (0.49)	**
Town location	0.13 (0.34)	0.082 (0.27)	0.078 (0.27)	0.11 (0.31)	**
Rural location	0.24 (0.43)	0.083 (0.28)	0.075 (0.26)	0.13 (0.34)	**
<b>Student and Staff Characteristics</b>					
Student enrollment	566.2 (445.6)	670.0 (480.2)	616.9 (426.2)	1038.0 (644.6)	**
Pupil-to-teacher ratio	16.6 (160.9)	16.2 (9.04)	16.3 (9.56)	15.7 (3.92)	*
Proportion of students White	0.52 (0.33)	0.43 (0.32)	0.42 (0.32)	0.53 (0.30)	**
Proportion of students Black	0.15 (0.23)	0.19 (0.26)	0.20 (0.26)	0.16 (0.21)	**
Proportion of students Hispanic	0.23 (0.27)	0.29 (0.28)	0.30 (0.29)	0.22 (0.25)	**
Proportion of students other race	0.097 (0.13)	0.091 (0.097)	0.091 (0.098)	0.093 (0.089)	
Proportion of students LEP	0.098 (0.14)	0.12 (0.15)	0.13 (0.16)	0.078 (0.11)	**
Proportion of students IDEA	0.14 (0.100)	0.14 (0.098)	0.14 (0.10)	0.13 (0.060)	**
FTE security guards	0.32 (1.10)	0.36 (1.10)	0.27 (0.92)	0.95 (1.80)	**
FTE guidance counselors	1.29 (1.51)	1.50 (1.67)	1.27 (1.41)	3.12 (2.33)	**
FTE nurses	0.59 (0.66)	0.51 (0.62)	0.46 (0.60)	0.86 (0.66)	**
FTE psychologists	0.35 (0.60)	0.32 (0.53)	0.31 (0.51)	0.45 (0.65)	**
FTE social workers	0.28 (0.65)	0.23 (0.54)	0.21 (0.49)	0.40 (0.75)	**

\*\* p<0.01, \* p<0.05, + p<0.1

*Note.* Each cell contains the variable mean and standard deviation. Column 5 indicates the significance level of a means comparison test between schools with a full-time SRO and schools without a full-time SRO in 2018.

**Table 2. Effects of School-Based Policing Award Cutoff on Full-Time SRO at School (First Stage)**

Bandwidth	N	(1) Linear	(2) Cubic	(3) Quintic
[-10, 10]	3,562	0.117** (0.035) F=11.46	0.024 (0.046) F=0.29	-0.078 (0.056) F=1.90
[-20, 20]	5,247	0.180** (0.029) F=38.13	0.170** (0.035) F=24.17	0.145** (0.043) F=11.51
[-30, 30]	7,202	0.139** (0.027) F=25.90	0.184** (0.030) F=37.39	0.195** (0.034) F=32.86
[-40, 40]	7,831	0.123** (0.023) F=29.00	0.175** (0.030) F=34.61	0.188** (0.032) F=33.77
[-50, 50]	8,278	0.124** (0.021) F=34.80	0.156** (0.029) F=28.50	0.182** (0.030) F=36.04

\*\* p<0.01, \* p<0.05, + p<0.1

*Note.* Each cell presents the estimated coefficient on the “above discontinuity” indicator from a different regression. The three columns show results from equations with linear, cubic, and quintic polynomial function controls of the running variable. The five rows show alternative bandwidths for restricting the sample. For reference, the MSE-optimal bandwidth for this discontinuity equals 18.9 points on each side of the cutoff according to the bias-corrected bandwidth selection approach of Calonico et al. (2017). Standard errors in parentheses are constructed from 1,000 bootstrapped samples, clustered by school district. All regressions include the full set of control variables including lagged outcomes, and are weighted by the number of officers requested in the CHP application per student in the school district. The shaded gray box in row two represents our preferred first stage equation (Tables 3 and 4); the shaded gray box in row four represents our alternative first stage equation (Appendix Table A2).

**Table 3. Effects of Full-Time SRO on Student Outcomes (2SLS)**

School Level	Offense Outcomes			Discipline Outcomes				Academic Outcomes	
	(1) School Shooting	(2) Firearm Offenses	(3) Other Offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
All Schools (n=5,247)	0.050+ (0.026) [0.0007]	0.184* (0.084) [0.046]	-6.609** (1.937) [2.539]	5.704* (2.331) [4.297]	10.888** (2.457) [5.472]	0.885** (0.235) [0.134]	2.647** (0.653) [0.286]	12.237** (4.284) [17.524]	0.849 (1.199) [1.389]
Primary (n=2,932)	0.009 (0.007) [0.0004]	0.161 (0.129) [0.024]	-6.444* (2.831) [1.639]	2.240 (1.599) [2.156]	4.950* (2.098) [3.112]	-0.065 (0.127) [0.023]	0.302* (0.142) [0.061]	7.028+ (3.998) [13.948]	0.952 (0.698) [1.080]
Secondary (n=2,315)	0.082+ (0.045) [0.0011]	0.201 (0.124) [0.075]	-6.418* (2.700) [3.722]	8.107* (3.969) [7.116]	17.840** (5.126) [8.577]	1.717** (0.552) [0.280]	4.835** (1.439) [0.582]	18.099* (7.751) [22.231]	1.818 (2.024) [1.796]
DV Mean	0.001	0.045	2.469	5.061	5.920	0.183	0.370	17.739	1.445
DV SD	0.031	0.261	6.459	8.881	7.889	0.969	1.580	13.946	3.177
Lagged DV	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

\*\* p<0.01, \* p<0.05, + p<0.1

*Note.* Each cell represents the effect of a full-time SRO on the outcome variable from a two-stage least squares regression. School shooting is measured as a binary indicator. Offense outcomes are measured as incidents per one hundred students, and discipline outcomes as number of students receiving the consequence per one hundred students. The first stage equation (row two, column one, of Table 2) regresses full-time SRO status on the discontinuity instrument within a bandwidth of 20 points above and below zero, and with a linear control for the running variable. Standard errors from 1,000 bootstrapped samples are provided in parentheses, clustered by school district. All regressions include the full set of control variables and are weighted by the number of students at the school. Weighted baseline values of each outcome for schools without full-time SROs are presented in brackets for each sample.

**Table 4. Effects of Full-Time SRO on Outcomes by Student Characteristics (2SLS)**

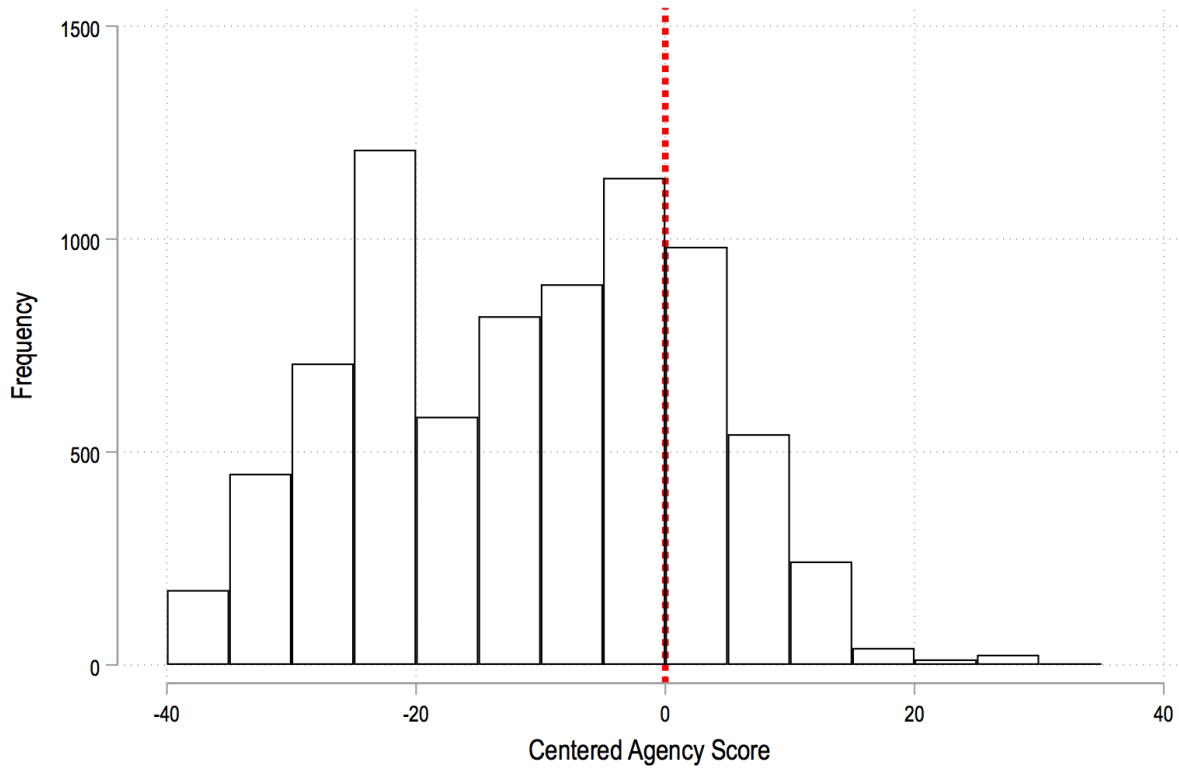
Student Characteristic	N	Discipline Outcomes				Academic Outcomes	
		(1) ISS	(2) OSS	(3) Expulsion	(4) Referral or Arrest	(5) Chronic Absence	(6) Grade Retention
<b>Gender</b>							
Male	5,241	7.362* (2.903) [5.822]	16.924** (3.496) [7.427]	1.247** (0.325) [0.205]	3.428** (0.839) [0.383]	12.435** (4.039) [17.946]	1.691 (1.349) [1.634]
Female	5,242	4.143* (1.899) [2.657]	4.744** (1.459) [3.331]	0.502** (0.143) [0.096]	1.871** (0.479) [0.184]	11.373** (3.957) [17.800]	0.507 (0.956) [1.210]
<b>Race/Ethnicity</b>							
White	5,176	3.390 (2.154) [3.500]	10.619** (2.799) [4.927]	0.640** (0.188) [0.108]	2.408** (0.633) [0.232]	12.379* (5.145) [18.355]	2.216 (1.453) [1.297]
Black	5,049	11.436* (4.499) [7.156]	26.235** (5.962) [9.136]	2.135** (0.704) [0.353]	5.322** (1.716) [0.561]	9.980 (6.340) [22.291]	3.511* (1.789) [1.753]
Hispanic	5,197	4.615+ (2.386) [3.329]	9.597** (2.326) [4.150]	0.423 (0.312) [0.128]	2.526** (0.680) [0.235]	6.527 (4.741) [20.899]	-0.179 (1.395) [1.787]
<b>Other Groups</b>							
Limited English	4,894	5.353 (3.585) [2.765]	10.888** (3.715) [3.538]	0.760** (0.262) [0.078]	0.688 (2.139) [0.342]	9.917 (6.467) [18.291]	-1.538 (2.425) [2.238]
Has Disability	5,211	4.160 (3.313) [6.553]	23.010** (5.022) [9.372]	0.953* (0.411) [0.226]	4.512** (1.333) [0.518]	13.364** (4.931) [24.590]	2.914 (2.012) [1.858]

\*\*p<0.01, \*p<0.05, +p<0.1

*Note.* Each cell represents the effect of having a full-time SRO on the outcome variable from a two-stage least squares regression. Outcomes are measured as student counts per one hundred students in that group. The first stage equation is the same as estimated in row two, column one, of Table 2; in this first stage, full-time SRO status is regressed on the discontinuity instrument within a bandwidth of 20 points above and below zero, with a linear control for the running variable. Standard errors from 1,000 bootstrapped samples are provided in parentheses, clustered by school district. All regressions include the full set of control variables and are weighted by the number of students at the school. Weighted baseline values of each outcome for schools without full-time SROs are presented in brackets for each group.

## Appendix A. Additional Figures and Tables

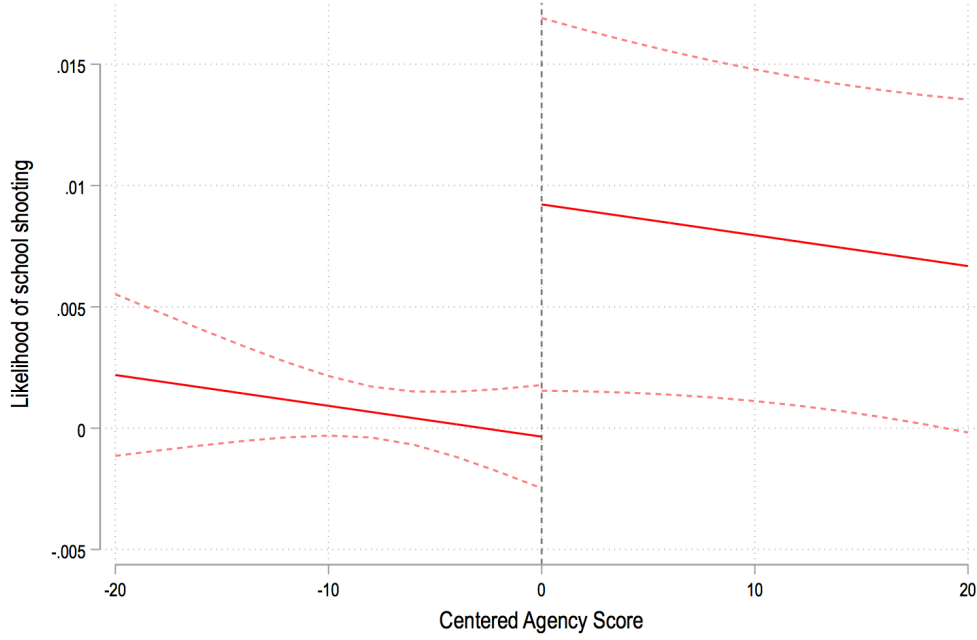
### Figure A1. Density Plot of Centered Agency Score



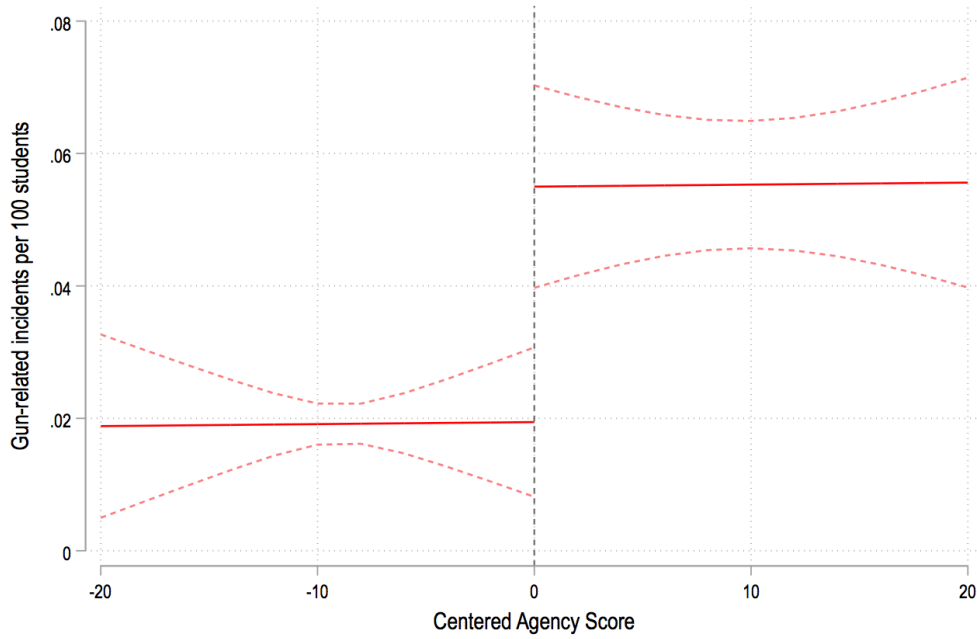
*Note.* This histogram restricts the sample to schools linked to school-based policing agency applicants with centered scores within 40 points of the effective threshold (7,831 schools out of 8,758 total).

**Figure A2. Reduced Form Effects of School-Based Policing Award Cutoff on Student Outcomes**

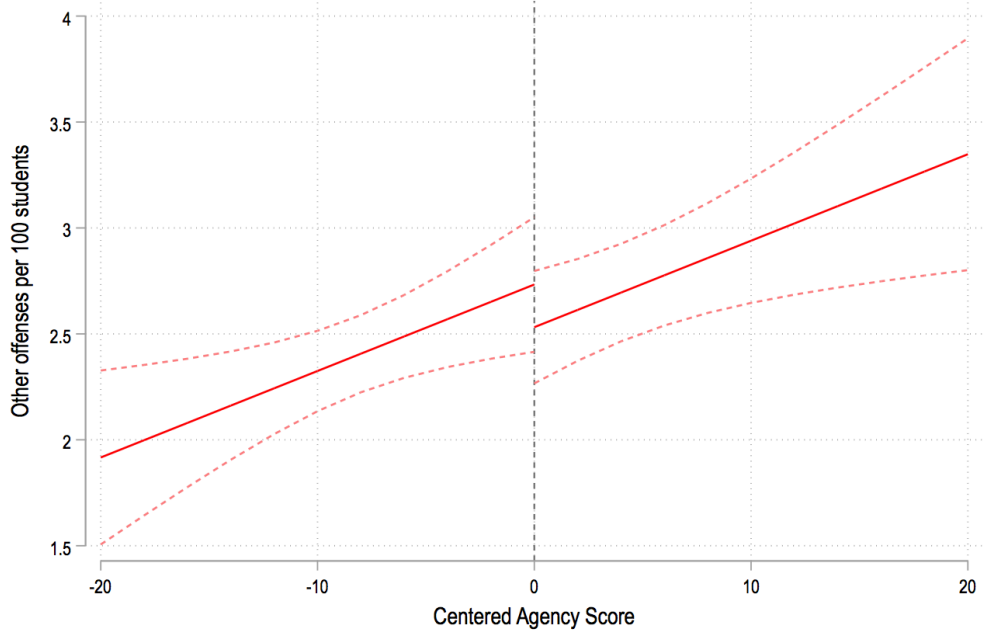
**(a) School Shooting**



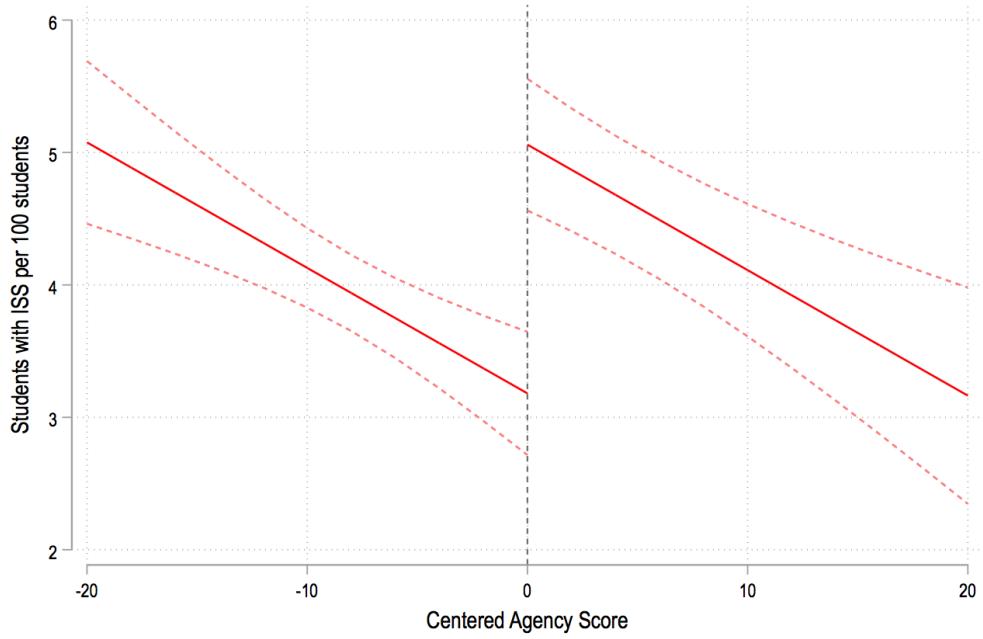
**(b) Gun-Related Offenses**



**(c) Other Offenses**

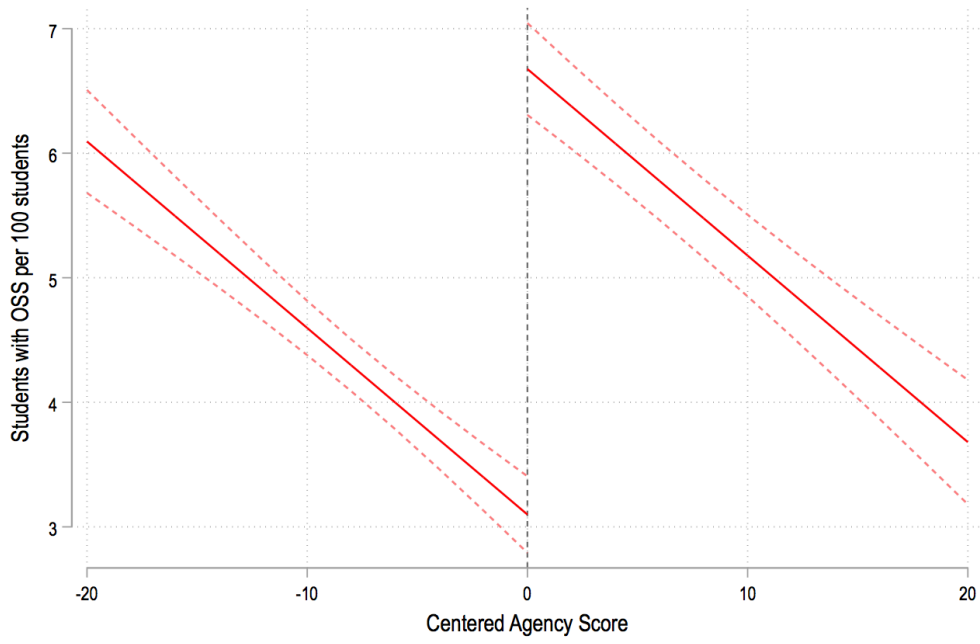


**(d) In-School Suspension**

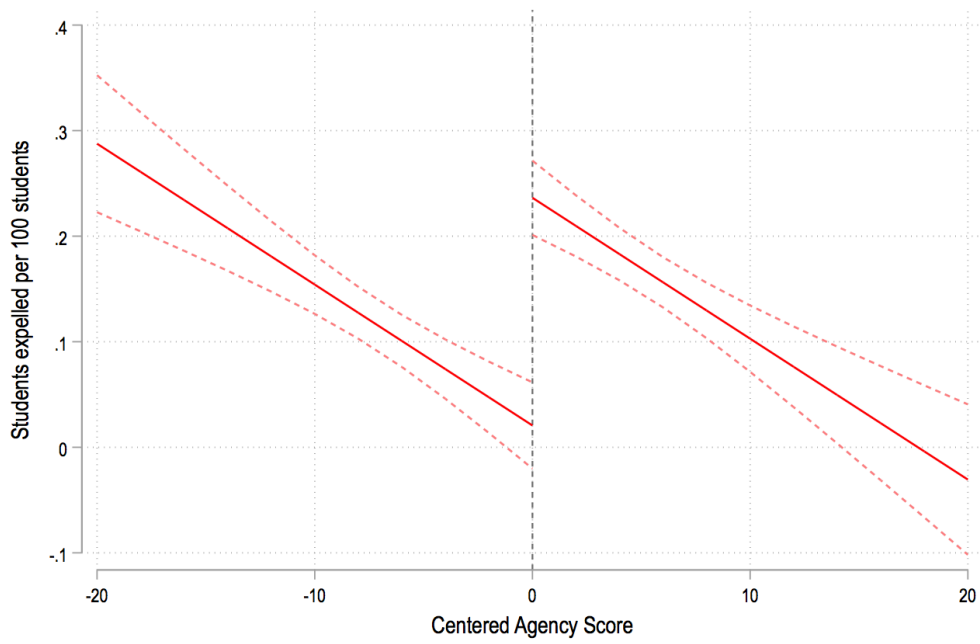




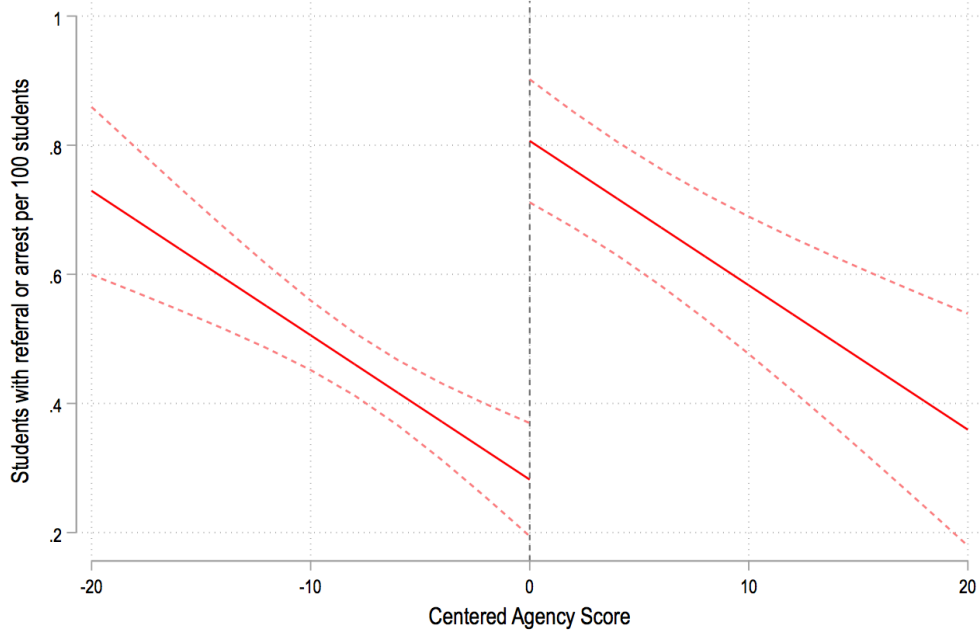
**(e) Out-of-School Suspension**



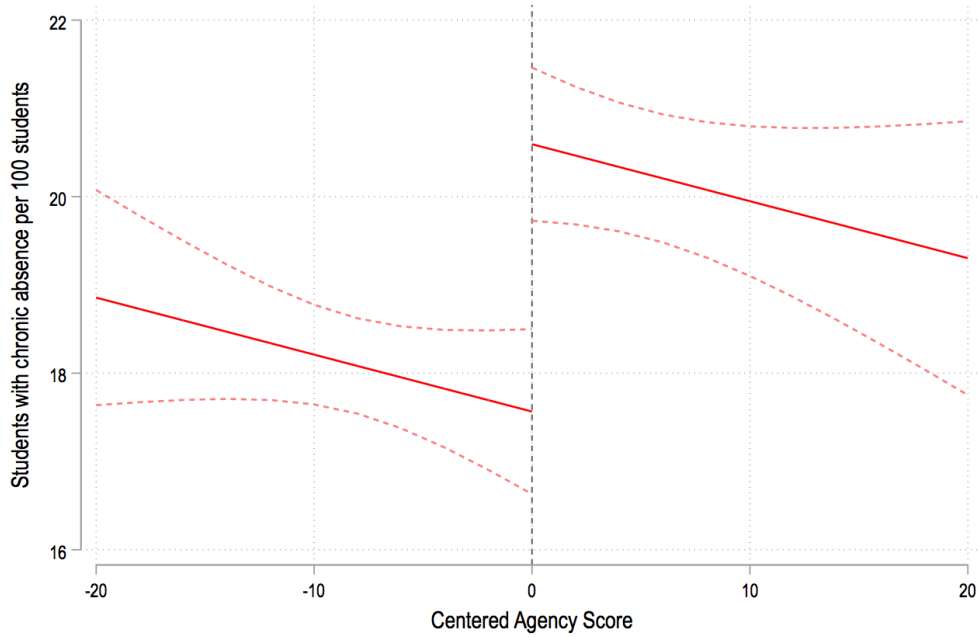
**(f) Expulsion**



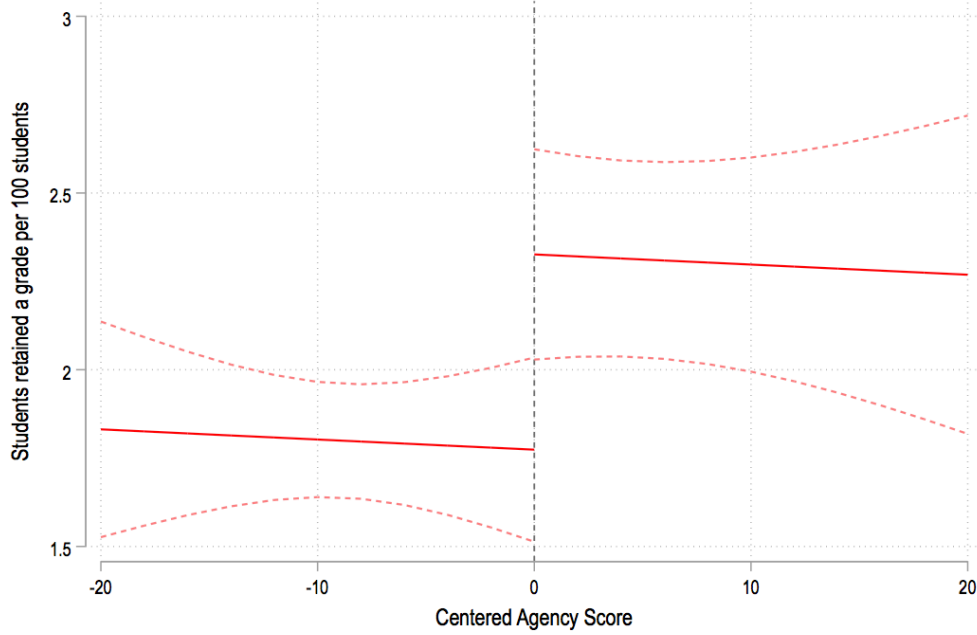
**(g) Police Referral and Arrest**



**(h) Chronic Absence**



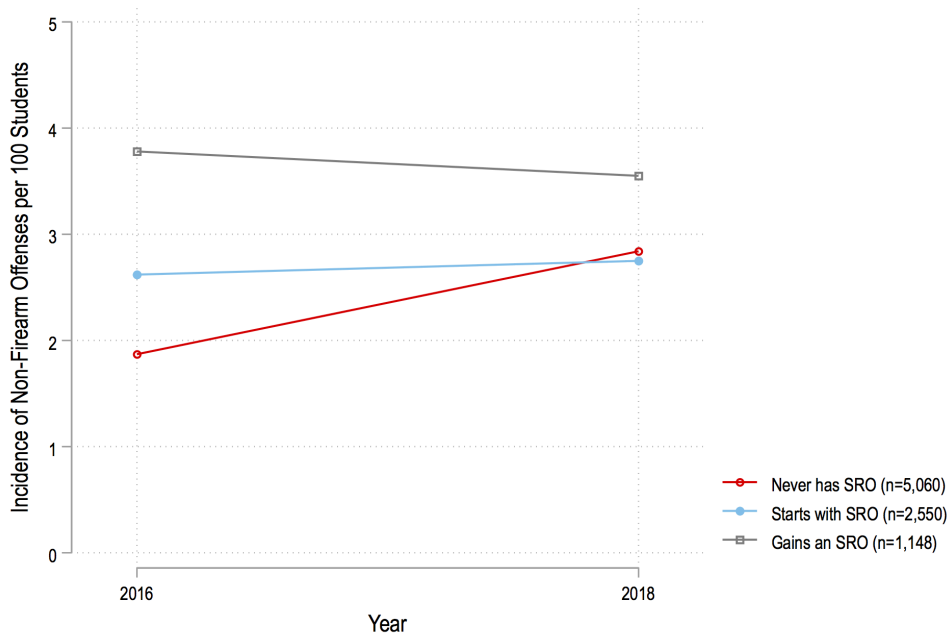
### (i) Grade Retention



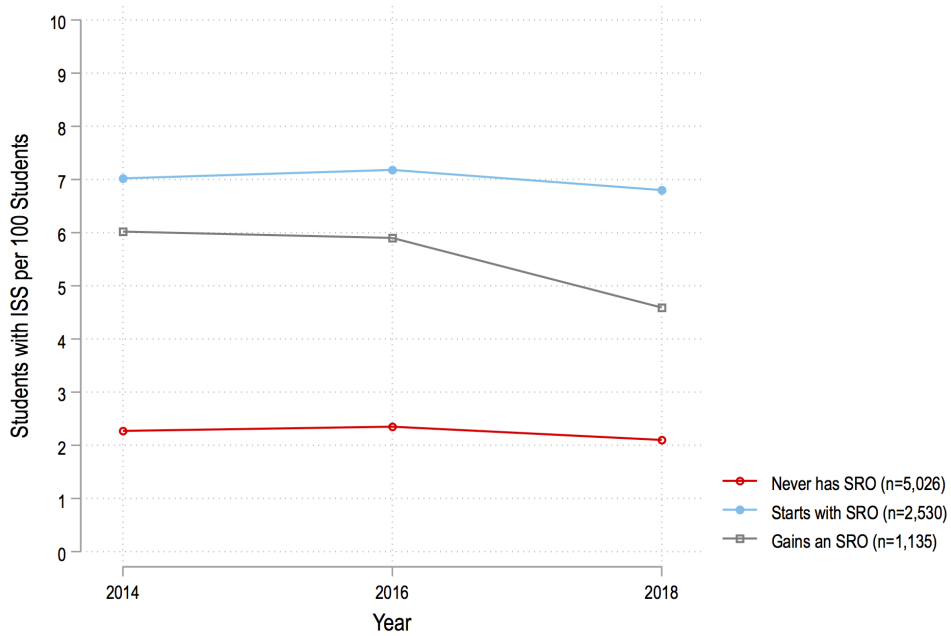
*Note.* Each graph above plots the marginal predictions of each outcome by centered agency score above and below the cutoff. These marginal predictions come from a linear regression within the [20,20] bandwidth, controlling for the full set of control variables, and weighted by student enrollment. 95 percent confidence intervals, represented by dashed lines, are constructed from standard errors from 1,000 bootstrapped samples, clustered by school district.

Figure A3. Descriptive Trends in Offense and Discipline Outcomes by SRO Status

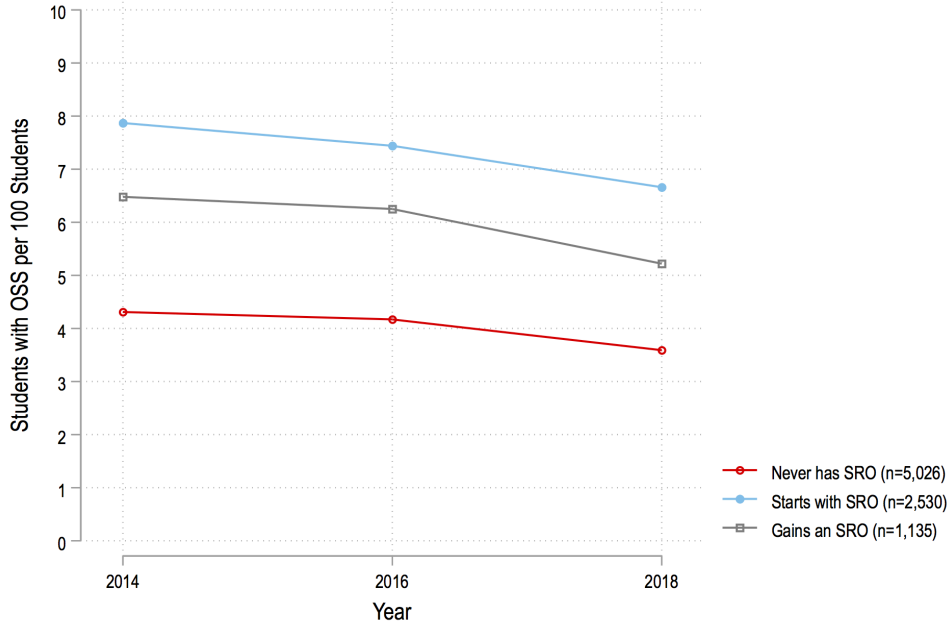
(a) Non-Firearm Offense



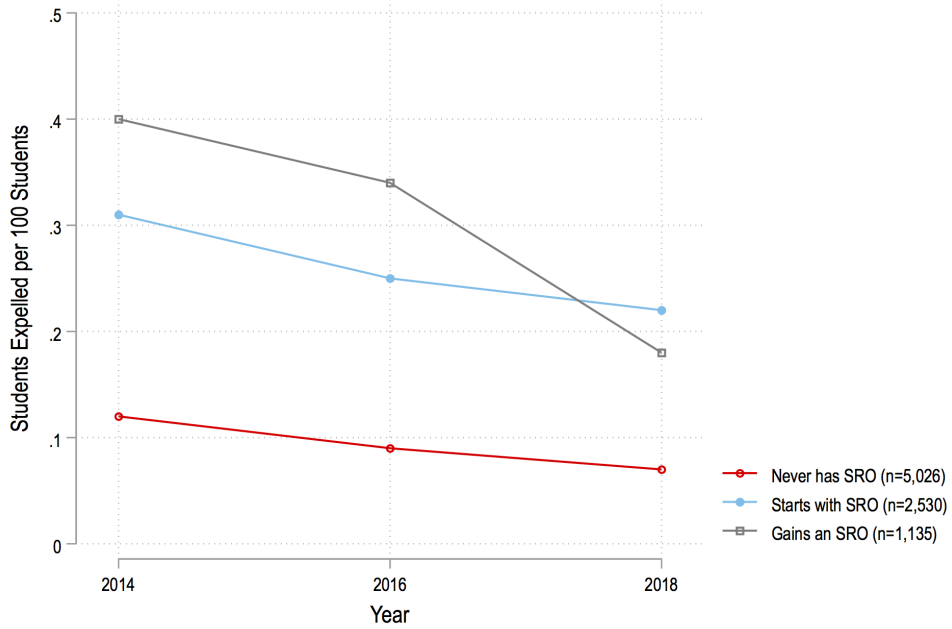
(b) In-School Suspension



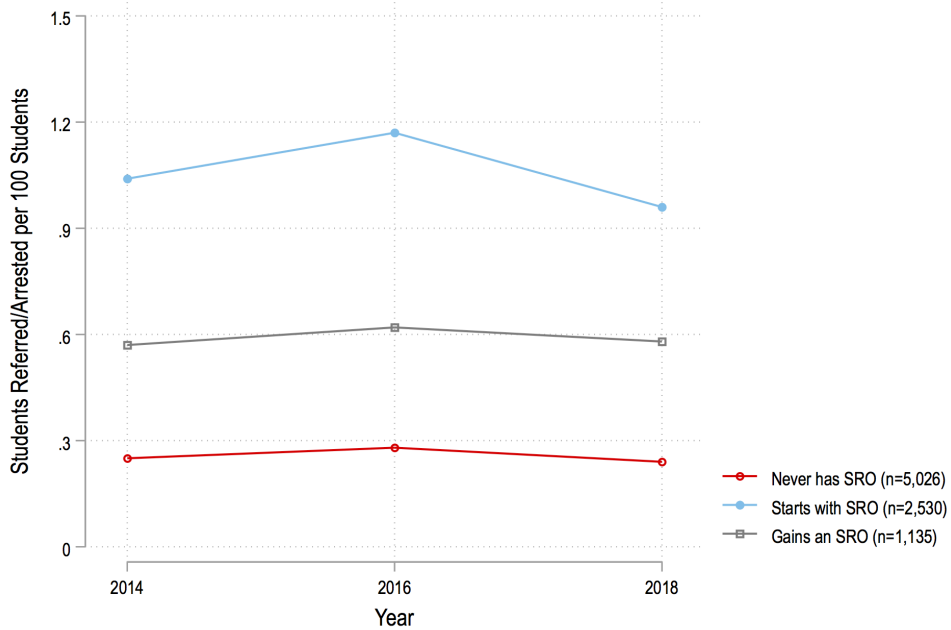
### (c) Out-of-School Suspension



### (d) Expulsion



(e) Police Referral or Arrest



*Note.* These graphs plot variable means by CRDC survey year for the analytical sample of schools (n=8,758). Non-firearm offense information is not available for 2014. Schools are labeled as “Never has SRO” if they did not have a full-time SRO in 2014 and did not gain one in a later wave; they are labeled as “Starts with SRO” if they already had a full-time SRO in 2014; and they are labeled as “Gains an SRO” if they gain a full-time SRO sometime between 2014 and 2018. School shootings and gun-related offenses are not common enough in each treatment group and year to make meaningful trend comparisons.

**Table A1. Alternative Specification: Effects of Full-Time SRO on Student Outcomes with Wider Bandwidth and Quintic Polynomial Function of Running Variable (2SLS)**

School Level	Offense Outcomes			Discipline Outcomes				Academic Outcomes	
	School Shooting	Firearm Offenses	Other Offenses	ISS	OSS	Expulsion	Referral or Arrest	Chronic Absence	Grade Retention
All Schools (n=7,831)	0.064* (0.025) [0.0007]	0.223* (0.105) [0.038]	-7.312** (2.069) [2.645]	4.278+ (2.286) [4.156]	11.908** (2.689) [5.228]	1.096** (0.257) [0.113]	3.078** (0.703) [0.430]	21.352** (5.661) [16.555]	2.399 (1.574) [1.408]
Primary (n=4,480)	0.014 (0.009) [0.0004]	0.169 (0.138) [0.020]	-6.856* (2.912) [1.947]	0.394 (1.449) [2.025]	4.716* (1.998) [3.023]	0.069 (0.076) [0.021]	0.567** (0.217) [0.136]	13.980** (5.390) [13.288]	2.801** (1.063) [1.085]
Secondary (n=3,351)	0.127 (0.124) [0.0012]	0.279 (0.220) [0.061]	-6.530 (4.152) [3.590]	4.486 (3.572) [7.038]	6.753+ (3.591) [8.209]	0.295 (0.320) [0.238]	4.758 (2.994) [0.828]	10.094 (10.493) [20.974]	2.775 (2.982) [1.844]
DV Mean	0.001	0.039	2.579	4.740	5.599	0.152	0.486	16.836	1.427
DV SD	0.029	0.258	7.179	8.266	8.018	0.836	1.866	13.854	3.851
Lagged DV	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

\*\* p<0.01, \* p<0.05, + p<0.1

*Note.* Each cell represents the effect of having a full-time SRO on the outcome variable from a two-stage least squares regression. School shooting is a binary indicator, offense outcomes are measured as incidents per one hundred students, and discipline outcomes as student counts per one hundred students. The first stage equation regresses full-time SRO status on the discontinuity instrument within a bandwidth of 40 points above and below zero, with a quintic function control for the running variable. Standard errors from 1,000 bootstrapped samples are provided in parentheses, clustered by school district. All regressions include the full set of control variables and are weighted by the number of students at the school. Weighted baseline values of each outcome for schools without full-time SROs are presented in brackets for each sample.

**Table A2. Alternative Specification: Effects of Full-Time SRO on Winsorized Student Outcome Measures (2SLS)**

School Level	Offense Outcomes			Discipline Outcomes				Academic Outcomes	
	School Shooting	Firearm Offenses	Other Offenses	ISS	OSS	Expulsion	Referral or Arrest	Chronic Absence	Grade Retention
All Schools (n=5,247)	0.051+ (0.027) [0.0007]	0.001 (0.019) [0.013]	-2.885* (1.170) [1.933]	2.925+ (1.709) [3.633]	10.264** (2.379) [4.868]	0.522** (0.126) [0.056]	1.887** (0.456) [0.185]	8.762* (3.654) [16.729]	-0.029 (0.665) [1.191]
Primary (n=2,932)	0.009 (0.007) [0.0004]	0.019 (0.019) [0.008]	-3.048+ (1.578) [1.424]	1.475 (1.331) [1.939]	5.617** (2.080) [2.957]	0.056+ (0.032) [0.017]	0.310* (0.146) [0.058]	7.208* (3.674) [13.745]	0.931 (0.635) [1.063]
Secondary (n=2,315)	0.080 (0.069) [0.0011]	-0.028 (0.034) [0.019]	-2.648 (1.922) [2.603]	4.008 (3.765) [5.864]	15.813* (7.840) [7.382]	0.955* (0.433) [0.107]	3.241* (1.594) [0.352]	11.429 (7.548) [20.657]	-0.407 (1.227) [1.361]
DV Mean	0.001	0.014	1.924	4.204	5.201	0.068	0.244	16.938	1.247
DV SD	0.031	0.047	3.503	5.974	5.572	0.172	0.607	11.065	1.876
Lagged DV	No	No	No	Yes	Yes	Yes	Yes	Yes	Yes

\*\* p<0.01, \* p<0.05, + p<0.1

*Note.* Each cell represents the effect of having a full-time SRO on the outcome variable from a two-stage least squares regression. School shooting is a binary indicator, offense outcomes are measured as incidents per one hundred students, and discipline outcomes as student counts per one hundred students. Outcomes are winsorized such that the top five percent of the upper tail are recoded to the value of the 95<sup>th</sup> percentile. The first stage equation regresses full-time SRO status on the discontinuity instrument within a bandwidth of 20 points above and below zero, with a linear function control for the running variable. Standard errors from 1,000 bootstrapped samples are provided in parentheses, clustered by school district. All regressions include the full set of control variables and are weighted by the number of students at the school. Weighted baseline values of each outcome for schools without full-time SROs are presented in brackets for each sample.



**Table A3. Reverse Causality Test: Effects of School-Based Policing on Baseline Covariates**

Outcome Measures in 2014	(1) Effect of SBP Award 2015-17 (Reduced Form)	(2) Effect of Full- Time SRO 2018 (2SLS)
<b>Student characteristics</b>		
Percent of students White	0.012 (0.008)	0.027 (0.021)
Percent of students Black	-0.006 (0.004)	-0.013 (0.017)
Percent of students Hispanic	-0.001 (0.006)	0.019 (0.019)
Percent of students LEP	-0.004 (0.009)	-0.011 (0.022)
Percent of students IDEA	0.003 (0.006)	0.020 (0.015)
Percent of students male	-0.007 (0.004)	-0.014 (0.013)
<b>Discipline and academic measures</b>		
Students with ISS	0.613 (1.291)	1.206 (3.140)
Students with OSS	1.015 (1.112)	-2.294 (2.710)
Students with expulsion	0.313+ (0.159)	0.427 (0.461)
Students with referral/arrest	-0.007 (0.216)	0.216 (0.653)
Students chronically absent	-2.071 (2.616)	-34.174** (9.722)
Students grade retained	0.218 (0.538)	3.502+ (1.974)

\*\*p<0.01, \*p<0.05, +p<0.1

*Note.* Column one shows estimated effects on lagged outcomes and covariates (in 2014) of the school being near an agency just above the school-based policing award cutoff during years 2015 to 2017 as compared to being near an agency just below the cutoff. Column two shows the corresponding 2SLS effects of a full-time SRO in 2018 on lagged outcomes and covariates. All models are estimated within the [-20, 20] bandwidth with a linear control for the running variable and full set of controls, weighted by the number of students. Standard errors are calculated from 1,000 bootstrapped samples, clustered by school district.

## Appendix B. Simulation of CHP Award Assignment and Cutoffs

The CHP does not have a single score threshold above which agencies receive awards and below which they do not receive awards. Instead, each year the COPS office coordinates a multi-step process to assign awards based primarily on applicant scores, but also in line with two statutory requirements. The first requirement is that each state or territory with an eligible agency that applies for a CHP grant must receive at least 0.5 percent of total allotted funds. The second requirement is that agencies serving populations of fewer than 150,000 (“small agencies”) receive equivalent funds to agencies serving populations of greater than 150,000 (“large agencies”).

Based on public documentation from the COPS website and discussions with COPS office personnel, we have been able to successfully replicate the process by which the COPS office assigns awards to applicant agencies. Below describes our step-by-step process for which we simulate award assignment separately in each grant year.

### State allocation

The first stage of award assignment occurs by state/territory according to the statutory requirement that each state or territory with an eligible agency applicant must receive 0.5% of total funds. In this first stage, we take the following steps:

1. We calculate the statutorily-allotted amount for each state/territory in which at least one eligible agency applies for CHP funding as the total allotted funds that grant year times 0.005.
2. For each of these states/territories, we then sort agencies in descending order based on their final score (fiscal need score + crime score + community policing score + bonus points). For state  $i$  in year  $t$ , we therefore have a sorted list of agency scores  $s_{1it}, \dots, s_{nit}$ .
3. We assign an award to the top scoring agency, and subtract the amount of money requested by the agency (=number of officers requested \* per-officer cost) from the state allotment. We remove the agency from the pool of potential recipients.
4. We repeat step 3 iteratively for agencies still in the pool of potential recipients until the entire state allotment has been used or until all eligible agencies in the state/territory have received an award. The final agency to receive an award in this way is designated as agency “ $q$ ” (with score  $s_{qit}$ ).

For each state  $i$  in each grant year  $t$ , we then define the state-allocation cutoff as follows:

$$C_{it}^{state} = \frac{1}{2}(s_{q,it} + s_{q+1,it})$$

In this formula,  $s_{q,it}$  represents the score of the lowest-scoring agency to receive an award through the state allocation process, and  $s_{q+1,it}$  represents the score of the highest-scoring agency to not receive an award through the state allocation process in state  $i$  and year  $t$ . The effective cutoff is therefore defined for each state and year as halfway between these two scores.

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For states in which every agency receives an award, we do not calculate a cutoff score. This is because there is no binding cutoff for agencies in states where the award is not competitive.

## National allocation

The second stage of award assignment occurs at the national level with the remaining funds. This second phase allocates money out of two separate pots: one for large agencies, and one for small agencies. We therefore take the following steps:

1. We calculate the amount of money that has already been spent during the state allocation on awards for small agencies, and the amount that has already been spent during state allocation on awards for large agencies.
2. We then determine how much money is still available for small agencies and how much money is still available for large agencies under the statutory requirement that exactly half of total funds must go to small agencies and exactly half must go to large agencies. Let us call these two remaining pots  $T_s$  for small agencies and  $T_l$  for large agencies.
3. Then we sort the remaining small agencies without awards in descending score order ( $s_{1st}, \dots, s_{nst}$ ) and the remaining large agencies without awards in descending score order ( $s_{1lt}, \dots, s_{nlt}$ ).
4. We assign an award to the top scoring agency in each agency size category, and subtract the amount of money requested by the agency (=number of officers requested \* per-officer cost) from the remaining small agency or large agency allotment. We remove the agency from the pool of potential recipients.
5. We repeat step 4 iteratively for agencies still in the pool of potential recipients until the entire small agency allotment and large agency allotment have been used. The final small agency to receive an award in this way is designated as agency “ $q$ ” (with score  $s_{qst}$ ) and the final large agency to receive an award in this way is similarly designated as agency “ $q$ ” (with score  $s_{qlt}$ )

We then define the two national allocation cutoffs for small and large agencies as follows:

$$C_t^{small} = \frac{1}{2}(s_{q,st} + s_{q+1,st})$$
$$C_t^{large} = \frac{1}{2}(s_{q,lt} + s_{q+1,lt})$$

In this formula,  $s_{q,st}$  represents the score of the lowest-scoring small agency to receive an award through the national allocation process, and  $s_{q+1,st}$  represents the score of the highest-scoring small agency to not receive an award through the national allocation process in year  $t$ . Similarly,  $s_{q,lt}$  represents the score of the lowest-scoring large agency to receive an award through the national allocation process, and  $s_{q+1,lt}$  represents the score of the highest-scoring large agency to not receive an award through the national allocation process. The effective cutoff is therefore defined for each year and agency size as halfway between these two scores.<sup>5</sup>

## Cutoff determination

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<sup>5</sup> It is possible that after the national allocation process there are still leftover funds. The COPS office says that at this point they will look for the next highest-scoring agency with requested funds amount lower than the remaining funds. We do not model this process directly.

Following the state and national award allocation simulation, we must determine the effective, or binding, cutoff for each individual agency. To do so, we simply use the following formulas:

$$C_{jit}^* = \min(C_{it}^{state}, C_t^{small}) \text{ if agency } j \text{ is small}$$

$$C_{jit}^* = \min(C_{it}^{state}, C_t^{large}) \text{ if agency } j \text{ is large}$$

In this way, each agency  $j$  in state  $i$  in grant year  $t$  is held to the minimum cutoff of either the state allocation cutoff or the national agency size cutoff, whichever is lower.

Finally, for each agency-year, we center its final score around its own binding cutoff, which is specific to state and agency size:

$$s_{jit}^* = s_{jit} - C_{jit}^*$$

The variable  $s_{jit}^*$  is the centered running variable used in our analysis, and the discontinuity for school-based policing awards therefore occurs exactly at zero.