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

Lucy C. Sorensen[1], Montserrat Avila-Acosta[1], John B. Engberg,[2] Shawn D. Bushway[1,2]

[1]University at Albany, State University of New York

[2]RAND Corporation

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Correspondence should be addressed to Lucy C. Sorensen, Milne Hall, Rockefeller College of Public Affairs and Policy, 135 Western Avenue, Albany, NY 12222. Email: lsorensen@albany.edu.

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 for federal school-based policing grants of linked police agencies. We find that SROs effectively reduce some forms of violence in schools, but do not prevent gun-related incidents. We also find that SROs intensify the use of suspension, expulsion, police referral, and arrest of students. These increases in disciplinary and police actions are consistently largest for Black students, male students, and students with disabilities.

Keywords: School resource officer, SRO, policing, school discipline, school safety

INTRODUCTION

According to federal data, about half of public schools in the U.S. had a school resource officer on school grounds at least once a week during the 2017/2018 school year (National Center for Education Statistics, 2021). The U.S. Department of Justice defines a school resource officer, or SRO, as a "career law enforcement officer assigned in a community policing capacity to a local educational agency" (U.S. Department of Justice, 2019). Debates over the use of SROs have heightened in recent years. School districts often view SROs as the first line of defense against school shootings and other acts of serious school violence (Canady et al., 2012; Congressional Research Service, 2018; Fisher et al., 2020a; Viano et al., 2021). However, growing public concerns about the role of SROs in the punishment and arrest of students, particularly Black students, for minor misconduct (e.g. Gleit, 2022; Hirschfield, 2008; Homer & Fisher, 2020; Nolan, 2011), have led over 50 districts serving over 1.7 million students to end or cut back their SRO programs since May of 2020 (Riser-Kositsky et al., 2022).

Despite these developments, quantitative research evidence on the effects of SROs on school environments and student outcomes is limited (Anderson, 2018; Gottfredson et al., 2020; Na & Gottfredson, 2011; Owens, 2017; Sorensen et al., 2021; Weisburst, 2019; Zhang, 2019). It is limited, in part, by the scarcity of publicly available administrative data on SRO personnel in schools. This study presents rigorous and broadly relevant quantitative results to inform future school district decisionmaking regarding partnerships with police. It uses recently released data from the 2013/2014 and 2017/2018 waves of the U.S. Department of Education's Civil Rights Data Collection (CRDC) to present new quantitative evidence about the impacts of SROs in public schools across the U.S. We use a fuzzy regression discontinuity (RD) approach[1] to estimate the

[1] Regression discontinuity analysis is a statistical method used in evaluating the impact of programs that have a cutoff point for eligibility. These analyses rely on the fact that entities scoring just above some arbitrary threshold are often

causal effects of new SRO placement on violent incidents at school as well as on student disciplinary outcomes, arrests and police referrals, chronic absence, and grade retention.

The results from our analysis indicate that SROs change school environments and student outcomes in important ways. Consistent with the cited objectives of SRO programs, we find that the increased presence of SROs reduces the incidence of some forms of school violence such as physical attacks without a weapon. Contrary to the objectives, however, the expansion of SROs leads to increased costs to students in the form of increased use of sanctions by both the school and law enforcement: SROs increase the incidence of out-of-school suspension, expulsion, police referral, and arrest. For many of these disciplinary consequences, the increased use of punishment appears larger for Black students than White students, larger for male students than female students, and larger for students with disabilities than students without disabilities.

We arrived at these findings by linking each law enforcement agency that applies for SRO funding to the set of schools they intend to serve based on new information obtained through a Freedom of Information request for portions of the school-based policing grant applications from the federal Community Oriented Policing Services (COPS) Hiring Program (CHP). The CHP, 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 school-based policing programs, selecting from a total of 791 eligible applicants. Our method uses the CHP grant application score as a running variable, and school presence of SROs as the treatment variable, in a fuzzy RD design to compare schools partnered with police agencies that scored just above and

in many senses no different from entities scoring just below the threshold, other than in their eligibility. A fuzzy regression discontinuity approach can help account for lack of compliance with eligibility rules, inconsistent application of eligibility rules, and other factors.

just below the effective grant award threshold for each year and agency type. This allows us to isolate exogenous variation in SRO placement in schools.

We first use this framework to test the efficacy of SROs in reducing gun violence and other forms of violence in schools—a major rationale for public investment in school policing (Coon & Travis, 2012; Madfis, 2016; Viano et al., 2021). SROs could reduce violence either through traditional law enforcement activities or through non-traditional activities like mentoring, informal counseling, and education (Broll & Howells, 2021; Hirschfield, 2009b; Kupchik, 2009; Mielke et al., 2021). Indeed, prior research has shown that SRO presence generally improves student, staff, and parent perceptions of school safety (Lapointe, 2016; Pentek & Eisenberg, 2018).[2] Estimating effects of SROs on the actual incidence of violence, however, is more challenging because researchers cannot observe actual offenses; they can only observe offenses that are reported by either the school or the police (Devlin & Gottfredson, 2018; Owens, 2017). If SRO presence changes the initial detection of violence or ultimate reporting of violence, this can obfuscate true SRO impacts on student behavior. Our study examines specifically the impact of SROs on violent offenses reported by schools, rather than police, but with an acknowledgement that multiple mechanisms could drive changes in reported violence.

We also use the fuzzy RD framework to examine the potential costs to students of using SROs in schools, with a specific focus on suspensions, law enforcement arrests, and other disciplinary and academic outcomes. The simple arrival of an armed police officer by definition changes the school social climate (Hirschfield, 2009b; Kupchik, 2009, 2010; Theriot, 2016), increases the surveillance reach of the police department (Beger, 2003; Connery, 2020; Owens, 2017; Tiller, 2014), and increases the chance that behavior that would have otherwise been dealt

[2] This evidence on perceptions is somewhat mixed, with Theriot and Orme (2016) finding null impacts of SROs on perceptions of school safety overall, while Black students and victimized students feel less safe with an SRO.

with by the school disciplinary system is referred to the juvenile justice system (Hirschfield, 2008; Kupchik, 2009; Sorensen et al., 2021; Theriot, 2009).[3] Research has documented that the behavior of SROs within schools can affect subsequent patterns of delinquency or sanctions reported at the school (Devlin & Gottfredson, 2018; Fisher & Devlin, 2020). Some SROs are regularly involved in standard school discipline, including for students who have engaged in minor delinquency rather than serious acts of violence (Curran et al., 2019; Fisher et al., 2020a; Kupchik, 2010).[4] Consequently, quantitative evidence documents that the arrival of an SRO corresponds to increases in standard disciplinary sanctions against students (Gottfredson et al., 2020; Sorensen et al., 2021; Weisburst, 2019).

These concerns are heightened because schools already assign disciplinary sanctions to Black students at a disproportionately higher rate than to White students in the U.S. (Losen & Martinez, 2020; Riddle & Sinclair, 2019). The location of police officers within school also sharpens concerns related to practices that connect students of color to the justice system (Kupchik, 2010; Skiba et al., 2014). Videos capturing acts of police violence against Black students in schools that went "viral" have further solidified this impression that SROs might specifically target and cause harm to Black students (Goldstein, 2020; Lee, 2015). Survey evidence does indicate that SROs in schools with more Black students get more involved with student discipline while SROs

[3] The U.S. has a separate system of justice for juveniles that does not treat the actions of juveniles as crimes, but rather delinquent acts. One important difference from the adult system, is that juveniles can enter the juvenile justice system through referrals from school officials, parents, and community members in addition to arrest. The available evidence on this practice suggests that the vast majority of students referred to the juvenile justice system for behavior at school do not enter through the actions of SROs (May et al., 2018).
[4] Best practices dictate that SROs should not be involved with regular school discipline (NASRO, 2021; President's Task Force, 2015; USDOE and USDOJ, nd; USDJ, 2019). SROs have no direct authority to deliver standard disciplinary sanctions like suspension or detention, which remain the sole domain of school administrators. However, SROs can increase suspensions by increasing the rate of detection and referrals for school-related delinquent acts or through the threat of arrest if direct action is not taken by the school administrators. The use of police leverage to activate sanctions that fall outside the realm of the justice system is a standard community policing tactic known as third-party policing (Buerger & Green Mazerolle, 1998).

in schools with more White students focus more on security risks from outside the school (Fisher et al., 2020b). Two recent papers find that suspensions correlated with the deployment of SROs disproportionately affect Black students (Sorensen et al., 2021; Weisburst, 2019). Recent experimental evidence suggests that this effect could be part of a larger phenomenon whereby all students at schools with more minoritized students are considered more blameworthy for misbehavior than at schools with fewer minoritized students (Owens, 2022). As a result of these patterns, we pay particular attention to the possibility that SRO placement affects students of different races/ethnicities, genders, and other characteristics differently.

Research on SRO Effects on Schools and Students

While there is a substantial body of qualitative evidence on SROs (e.g., Curran et al., 2019) there is a much smaller body of quantitative research on the impacts of SROs. The findings of the quantitative body of work are generally consistent with qualitative accounts about the effects of introducing police into schools. Our study is not the first to attempt to quantitatively assess the effects of police in schools on student outcomes (Anderson, 2018; Gottfredson et al., 2020; Na & Gottfredson, 2011; Owens, 2017; Sorensen et al., 2021; Weisburst, 2019; Zhang, 2019). However, this prior literature has been constrained by limited data on SROs and has therefore relied heavily on either small samples of schools or on district-level funding proxies for SRO presence. Our use of the U.S. Department of Education survey data and our causal design based on COPS funding protocols supplements the literature in novel ways.

There are two major challenges facing researchers quantitatively assessing 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 a small non-representative subsample of schools (N=470) in 2003/2004, 2005/2006, and 2007/2008, from the

School Survey on Crime and Safety (SSOCS), a representative cross-sectional survey of school administrators. Na and Gottfredson found that schools that added armed security officers recorded 29 percent more weapons and drug violations in the year they added the officer than schools that did not add armed security staff. This finding may indicate 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 did not test whether patterns of change before the arrival of the SRO were similar for schools that did or did not receive a police officer.

Two key papers addressed that 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 2003/2004, 2005/2006, and 2007/2008 waves of SSOCS, Owens (2017) showed that CIS grants were associated with reductions in recorded student misbehavior at schools in the same county as the grant-receiving law enforcement agency. Grants also led to small increases in the likelihood that school administrators in those counties report contacting police about the incidents that are recorded. Owens (2017) also used data from the National Incident Based Reporting System between 1997 and 2007 to identify delinquent events in schools officially reported to the police. She found that police jurisdictions that received CIS grants learned about more violent delinquency and weapons and drug violations in schools, and had more arrests of juveniles under age 15 for delinquent acts at school.

One limitation of Owen's work is that it lacked data on key school-related student outcomes like suspensions. Weisburst's paper in 2019 responds directly to this issue. 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 (ISS), out-of-school suspensions (OSS), and expulsions by school administrators using data on seventh through twelfth graders in Texas public schools between the 1998/1999 and 2007/2008 school years. She did not have data on student misbehavior unless it led to one of those three disciplinary actions. She also lacked data on the presence of SROs at the school or district level. She found that CIS funding for a district increased the number of middle school students who received disciplinary actions, particularly those associated with low-level offenses. She found that CIS funding increased suspensions and expulsions at a rate 50 percent greater for Black students than White students – a result 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 middle school students in a district with a CIS grant experienced a 2.5 percent reduction in high school graduation rates and a 4 percent decrease in college enrollment. She did not find a significant difference in these effects by race.

Although both papers study variation in funding for SROs, they are limited to looking at the impact of CIS law enforcement funding on a proximal school district. Although both use some school-level data, they do not link the grants to intentions to increase the number of SROs at a particular school or subset of schools within a particular district. 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 to a law enforcement agency, which may overlap with an entire district, more than one district, or only a portion of a district.

This muting is also observed in a third study, by Anderson (2018), which also looked at increased funding for SROs, though like Owens (2017) and Weisburst (2019) it lacked data on which schools within a district actually received an SRO. Anderson (2018) studied the impact of North Carolina (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 that switched on for the 2013/14 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. 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.

Two other recent papers, by Zhang (2019) and Gottfredson et al. (2020), did study the presence of an SRO at the school level. Zhang (2019) used between-school variation in the presence of police in West Virginia middle and high schools from 2014 to 2016 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 (2019) found that schools with a police officer for at least one year recorded roughly 35 percent more drug violations than schools without an officer. Gottfredson et al. (2020) found that increased SRO presence increased the number of drug- and weapon-related offense reports and the number of exclusionary disciplinary actions by school administrators relative to matched schools without increased SRO staffing. 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 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. The authors used incident-level administrative data and a difference-in-differences approach to examine the apparent impact of the addition or subtraction of an SRO in middle schools on differences in short-term student outcomes in school and in the juvenile justice system during the years 2005 to 2009, as well as long-term student outcomes in educational attainment and the adult criminal justice system. They found that the addition of an SRO is associated with a reduction in serious violent behavior on school grounds, but is not associated with a change in weapon, drug, or alcohol offenses, suggesting that the prior results showing a positive correlation might indeed have been a selection artifact. They also found that an increase in SRO presence is associated with a higher chance of a referral to law enforcement, particularly for Black and Hispanic students, and a higher chance of students receiving long-term out-of-school suspension, transfers to alternative schools, or expulsion, given a reported offense. Increased SRO presence also raises modestly the number of juvenile justice complaints against students. Despite the reliance on within-school variation instead of between-school differences, the Sorensen, Shen and Bushway (2021) analysis is not identified on exogenous variation in the presence of SROs. 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 many of these papers is their use of variation in SROs from 10-15 years ago. Debates about defunding the police have arisen since then and continue. These debates and the events that motivate them may be changing the effects of police presence on students. In addition, theft and violent victimization rates for students have declined over 80 percent from 1992 to 2018, both inside and outside of schools (NCES, 2021, Table 228.20). The potential positive effect of SROs may now be more limited given the much lower base rate of offenses, while the negative effects might still occur regularly.

The current study, which studies school-level variation in the presence of an SRO for a sample of U.S. schools from across the nation for the 2017/2018 school year represents a significant improvement over the Sorensen, Shen and Bushway (2021) study. It also represents an advancement over the approach of Owens (2017) and Weisburst (2019). Like these two latter studies, we use variation induced by the federal COPS funding. However, unlike these studies, we have a measure of whether each school has an SRO and we use the discontinuity at the application score cutoff for grant awards 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 by school in SROs. The current approach also allows us to look at the direct impact of SROs at the school level rather than the district level, which is a major advantage given that there is substantial variation in the use of school resource officers within districts.

METHODS

School Data

This study uses data from the 2013/2014 and 2017/2018 waves of the CRDC from all public schools in the U.S., excluding preschools and schools with fewer than 25 students. The final sample prior to linking the CRDC with law enforcement data includes 94,918 schools in the most recent survey wave.

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. 23):

> "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 [of] assault for school authorities)."[5]

Importantly, this survey differentiates the sworn law enforcement officer (or SRO) role 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. 23).

Outcomes of Interest

CRDC has rich information on a variety of student outcomes that may be affected by the 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, 2017; Sorensen et al., 2021; Zhang, 2019), school climate (Devlin et al., 2018), disciplinary or law enforcement actions (Fisher & Hennessy, 2016; Gottfredson et al., 2020; Sorensen et al., 2021; Weisburst, 2019), and academic outcomes (Weisburst, 2019). Accordingly, we examine the following dependent variables:

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[5] Although, technically, sworn law enforcement officers encompass a broader set of police officers involved in schools, we will use this term interchangeably with SRO.

- *Gun-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 violent 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.
- *ISS*: The number of students receiving any in-school suspension.
- *OSS*: The number of students receiving any out-of-school suspension.
- *Expulsion*: The number of students receiving expulsion.
- *Police referral or arrest*: The number of students referred to a law enforcement agency or official or receiving a school-based arrest.
- *Chronic absence*: The number of students chronically absent, defined as missing more than 15 school days.
- *Grade retention*: The number of students retained a grade level.

These count outcomes generally have right-skewed distributions with large numbers of zero values (see Appendix Figure A1[6]). To ensure that our estimates are not driven by outlier values, we therefore take the inverse hyperbolic sine of each dependent variable (Bellemare & Wichman, 2020).[7]

[6] All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher's website and use the search engine to locate the article at http://onlinelibrary.wiley.com.

[7] Two recent working papers (Chen & Roth, 2022; Mullahy & Norton, 2022) have highlighted issues with using an inverse hyperbolic sine transformation. They note that this transformation yields estimates that vary based on the original scaling of the dependent variable. (This is also the case with the "log plus one" transformation). We acknowledge this limitation but believe the estimates with the transformed variable are still more reliable than with the untransformed variables given the underlying distributions (Appendix Figure A1a-h). Nonetheless, we show in the results section that our main findings also hold using either raw count outcomes or log-transformed outcomes.

For the disciplinary and academic outcomes described above, we can also further disaggregate 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. We have lagged measures of all disciplinary and academic dependent variables from the 2014 wave of CRDC data. Unfortunately, the 2014 data do not contain measures of gun-related or other offenses, so we do not have lagged measures for these two outcomes. Our control variables are drawn from the CRDC and Common Core of Data from NCES and described in the empirical strategy section.

Law Enforcement Agency Data

In addition to data on schools, we also collected data on law enforcement agencies who applied for CHP grants. Through a Freedom of Information Act request to the U.S. Department of Justice COPS office, we received detailed information for all applicants to the 2015, 2016, and 2017 CHP grant cycles. These grant applications focus on multiple issues, 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 deployed SROs complete National Association of School Resource Officers 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. Appendix B describes in greater detail some of the qualitative themes present in agency applications for school-based policing grants, in terms of why and how agencies planned to expand their SRO programs.

CHP Award Selection Process

Our fuzzy RD approach requires focus on eligibility for CHP awards, so we cover that award process here in detail. 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. To select the recipients of these awards, the COPS office reviewed each application and calculated a score as a weighted sum of a community policing score (50 percent), a crime score (30 percent), a fiscal need score (20 percent), and miscellaneous bonus points[8]. They constructed the community policing score based on perceived current commitment to community policing and planned community policing activities; the crime score based on reported crime in the jurisdiction for the previous three years; and the fiscal need score based on changes in the law enforcement budget and local poverty and unemployment rates (U.S. Department of Justice, 2017). Higher levels of crime result in higher crime scores, and higher levels of fiscal need similarly result in higher fiscal need scores.

Assignment of awards based on these factors occurred over two stages, aligned with 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 of assignment, 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 COPS office assigns

[8] The purpose and specification of bonus points can vary from year to year, often corresponding to current preferences of U.S. 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.

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 can differ by year, by state, and by agency size. We replicate the award assignment process (see Appendix C) and 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 bracket, 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 conceptually how "close" the agency was to receiving an award. Appendix Figure A2a-b shows the distribution of these centered scores, first for all CHP applicants and then for school-based policing applicants within our preferred score bandwidth. Appendix Figure A3 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.

Matching Law Enforcement Agencies to Schools

To achieve an exact match between law enforcement agencies and schools, we obtained the full text of relevant sections of the police agencies' applications for the CHP grants for school-based policing through a FOIA request to the COPS office. Each application described the problem area and an explanation of the agency's need for federal assistance. We used a common instrument to code the applications of the 348 police agency applications that scored within 20 points of their effective cutoff. This allowed us to identify the exact school district or districts with which each law enforcement agency has a partnership. It also enabled us in many cases to achieve a more granular level of matching than the school district level, as many of the applications specified the exact school or schools where the requested SRO(s) would be placed if the grant were to be approved. In 162 (46.6 percent) of the 348 applications, the law enforcement agency specified the exact school or group of schools that would receive an SRO upon grant receipt. In 186 (53.4 percent) of the applications, the agency only specified a group of potential schools – such as a school district or a town – across which the SRO(s) would be placed. We used the NCES online public school search tool to manually identify and link each school or district identified in an agency application to CRDC schools.

Using information on the number of officers requested and the number of matched schools for each agency applicant, we calculated the expected number of additional FTE SROs at each school conditional on the matched agency receiving a CHP award. For schools where we knew with certainty that they would receive a full-time SRO, this value equals one. For schools that had a probabilistic chance of receiving a full-time or shared SRO if their matched agency received an award, we calculated the expected number of additional FTE SROs as follows for each school linked to agency *k:*

$$E(SRO|Score_k > 0) = \frac{OfficersRequested_k}{SchoolsMatched_k}$$

Because this expected increase in SROs conditional on agency grant receipt varies across schools and is often less than one, we would expect the average increase in FTE SROs from a CHP award at schools in our sample to lie in the range between zero and one. CHP grants last for three years and the placement of new SROs typically occurs shortly after grant receipt (Owens, 2017). Therefore, we should observe all officers funded by a 2015, 2016, or 2017 grant to be present in a school by the 2018 CRDC survey. Appendix Figure A4 visualizes the relative timing of grant receipts, measurement of SROs at schools, and measurements of current and lagged outcomes.[9]

[9] Since agencies that did not win awards in one grant cycle can reapply in later cycles, we kept only the highest scoring year for agencies that applied in multiple years. This means that we kept either the year in which the agency

Our final analytical dataset contains 3,443 schools linked to 299 agencies within a 20-point bandwidth of the cutoff.[10] This restricted dataset is comparable in most ways to the full universe of schools in the CRDC in 2018 (see Appendix Table A1). There are slight differences worth noting. Our analytical sample has a lower average number of FTE SROs present at schools (0.18 as compared to 0.23) and lower average counts of student offenses and disciplinary actions. This SRO gap may reflect that police agencies are more likely to apply for federal funding when they do not already have a fully-developed SRO program. It may also reflect that police agencies frequently propose to use federal funds to add SROs to elementary schools, since middle and high schools are more likely to already have SROs. Appendix Table A1 indeed shows an overrepresentation of elementary schools in our sample when compared to the full CRDC sample. This overrepresentation of elementary schools may also explain the slightly lower incidence of offenses and disciplinary actions in our sample. Although our analytical sample does not perfectly represent the full universe of public schools, it does generally represent the universe of schools currently seeking SROs, which tend to be more skewed toward the elementary level. Our analytical sample also contains a diverse set of schools in terms of size, type, student composition, and geographic region. Figure 1 illustrates this geographic diversity of the locations of grant applications and receipts for school-based policing investments across the U.S.

Within our analytical sample, we can compare schools linked to agencies that received school-based policing awards to schools linked to agencies that did not receive school-based policing awards in Table 1. At the time of CHP application, agencies that win awards have

successfully received a grant or the year in which the agency got closest to receiving a grant. Dropping duplicate agency applications and keeping only the top scoring year results in losing 177 (22.5%) of the 786 eligible, competitive school-based policing applications.

[10] Within a 15-point bandwidth, we have a total of 3,005 schools linked to 243 agencies. Of this group, 2,857 schools linked to 241 agencies can be matched to the 2014 survey to allow inclusion of lagged outcomes.

somewhat higher levels of crime, higher levels of fiscal need, and higher commitments to community policing than agencies that did not win awards. This is not surprising, given that each of these components feeds into the final COPS-calculated score for the agency. Post-award, we also see evidence that schools linked to agencies that won awards have greater SRO presence in 2018 than schools linked to agencies that did not win awards. We hypothesize this is due to the grants facilitating SRO hiring directly. Compared to schools linked to non-award-winning agencies, schools linked to award-winning agencies are more likely to be elementary schools, magnet schools, to be in urban or rural locales, and more are likely to be located in the South.

Post-award differences in school outcome measures across agencies with and without school-based policing awards are small but generally suggest schools linked to award-winning agencies had higher levels of gun-related offenses, higher levels of discipline, and lower levels of other violent offenses. These differences do not invalidate our empirical strategy because: (i) they could reflect post-award effects of SROs; and (ii) our method only requires that schools/agencies just to the left of their effective score threshold look like schools/agencies just to the right of their effective score threshold, not that the entire treatment group looks like the entire control group.

Empirical Strategy

The placement of SROs into schools does not occur at random. Because of this, any naïve differences in student outcomes observed across schools with and without SROs could merely reflect underlying differences in the types of schools that are more or less likely to receive SROs instead of reflecting causal impacts of the SROs. Our goal in this analysis was to identify a set of schools that received additional SROs and a comparison set of schools that did not receive additional SROs for reasons that are as good as random. RD designs allow for this exact type of comparison. In particular, they rely on the fact that entities scoring just above some arbitrary threshold are systematically no different from entities scoring just below the threshold, other than in their eligibility for treatment.[11] In our case, school assignment to treatment (additional SROs) is not perfectly predicted by the matched agency's CHP application score being above the cutoff, but we do expect the average number of SROs in schools to increase at this cutoff. This leads us to use a two-stage "fuzzy" RD approach. The first stage predicts the number of FTE SROs based on the matched agency's application score relative to the effective CHP award cutoff. The second stage then regresses school outcomes on this predicted number of SROs.

Mathematically, we specify these two stages of the analysis as follows:

(1) $$SRO_{jk} = \alpha_0 + \alpha_1 I(Score_k > 0) + \alpha_2 Score_k + \alpha_3 I(Score_k > 0) \times Score_k + \mu_{jk}$$

(2) $$Y_{jk} = \gamma_0 + \gamma_1 \widehat{SRO_{jk}} + \gamma_2 Score_k + \gamma_3 I(Score_k > 0) \times Score_k + \rho_{jk}$$

The first stage regresses the 2018 measure of SROs in school j linked to law enforcement agency k (SRO_{jk}) on the centered application score of agency k from the COPS hiring grant program ($Score_k$)[12], an indicator equaling one if the agency scored above the binding cutoff and zero otherwise ($I(Score_k > 0)$), and the interaction of these two variables ($I(Score_k > 0) \times Score_k$) to allow the slope on the running variable to change at the discontinuity. The second stage then regresses (the inverse hyperbolic sine of) outcomes for students in school j linked to agency k on the predicted SRO variable from the first stage, the centered running variable, and the interaction of the centered running variable and the above-discontinuity indicator. All regressions restrict the analytical sample to various local bandwidths around zero. We weight observations by student

[11] It is also important that there is no "manipulation" in the running variable. In our case, this means that neither agencies nor the COPS office should be able to intentionally manipulate scores to shift agencies to just above or just below the discontinuity. We conduct a formal RD manipulation test (Cattaneo et al., 2020) that produces a T-statistic of -0.127 and a p-value of 0.899, indicating no evidence of score manipulation around the discontinuity.
[12] We test the sensitivity of our results to the inclusion of a quadratic function of the running variable as well in Appendix Figure A5a-h.

enrollment and use standard errors computed from 1,000 bootstrapped samples, clustered by police agency to reflect the variation in treatment that occurs at the agency level.[13]

The logic behind this estimation approach is first, that schools matched to law enforcement agencies that receive CHP grants for school-based police officers in 2015 to 2017 are likely to have more SROs by 2018 than are schools matched to 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 linked to 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, except in ways that are captured by the running variable $Score_k$. Third, this approach requires the standard instrumental variables assumption that the CHP school-based policing awards only affect school outcomes through increases in SROs at the school.[14] If these conditions are met, then the γ_1 parameter from the second stage will reflect the local average treatment effect of an additional SRO on student outcomes.[15]

To strengthen the internal validity of this design, we also estimate the model described above with control variables. Most importantly, for each disciplinary and academic outcome we can control for the lagged dependent variable from the 2013/2014 school year. With this inclusion, we can interpret the effects of an increase in SROs as the effects on *within-school changes* over

[13] Bootstrapped standard errors reflect best practices for ensuring that first stage partial F-statistics are not overly inflated (Lal et al., 2021). We use the Stata command `bootstrap' with 1,000 resamples with replacement, clustered by agency.
[14] We believe the exclusion restriction is reasonable in this case, since the CHP grant is received by the law enforcement agency, which is linked to school districts primarily through its role in providing SROs. One possible violation to this assumption would be if the school district receives an income shock from receiving an SRO funded by the grant if the district would have otherwise paid for the SRO themselves. Another possible violation would occur if the school district is less likely to hire other personnel such as security guards under agency receipt of the grant. To check for these types of violations, we estimate the effects of the award discontinuity on other types of school personnel – security guards, teachers, counselors, social workers, psychologists, and guidance counselors. We find null effects for each of these outcomes, suggesting there were limited income or substitution effects happening for personnel hiring within school districts from these CHP grants. Results available upon request.
[15] This approach also allows for measurement error in the reporting of the number of SROs at the school, which otherwise would lead to attenuation bias in the treatment effect estimate.

time in student disciplinary consequences and academic measures. (Unfortunately, we do not have lagged measures from CRDC for the gun offense or other violent offense outcomes). Our control variables also include indicators of school level (elementary / middle / high / other); indicators of school location (urban / suburban / town / rural); indicators of region (South / Midwest / Northeast / West); logged student enrollment; pupil-to-teacher ratio; proportion of students by race and ethnicity (White, non-Hispanic / Black / Hispanic / other); proportion of students identified as Limited English Proficiency (LEP); proportion of students identified to receive services through the Individuals with Disabilities in Education Act (IDEA); 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.

This fuzzy RD approach consistently estimates the local average treatment effect (LATE) for compliers at the threshold (Bertanha & Imbens, 2020). In our case, the LATE represents the impact of a unit increase in FTE SROs on the outcome for all schools that change their number of SROs in response to winning an award. The LATE averages over all agencies, whether they request one SRO to cover multiple schools, one SRO to cover a single school, or multiple SROs to cover a single school.[16] The first stage coefficient on the discontinuity, α_1, therefore represents the average increase in the number of SROs per school that result from an award.

As described in a prior section, we used information from CHP application text fields to calculate for each school the expected number of additional SROs gained, conditional on the matched agency receiving a school-based policing grant ($E(SRO|Score_k > 0)$).[17] A value of one

[16] The "average" in LATE refers to an average over all "complier" schools – that is, all schools that receive awards and have a different number of SROs than would be predicted if they hadn't received awards.

[17] This measure is similar to a "compliance" measure, which is used in models that allow subjects to choose the "dose" of their treatment by being non-compliant with the assigned treatment. Compliance is assumed to be endogenous. In the present case, the subjects are choosing their dose when they request a particular number of SROs in their grant application, and larger doses are expected to lead to larger treatment effects.

on this measure would indicate that we anticipate the school will receive one full-time SRO upon agency award receipt. A value of less than one—0.5 for instance—could mean that the school has a 50 percent likelihood of receiving a full-time SRO or that the school has a 100 percent likelihood of sharing a single SRO with another school. We can use this measure to estimate an "interacted RD" model that interacts the above-discontinuity indicator with our compliance measure to strengthen the first stage prediction of school SRO presence (Caetano et al., 2021; Coussens & Spiess, 2021; Huntington-Klein, 2020). Coussens & Spiess (2021) show that an interacted model estimates a "super local average treatment effect" (SLATE), in our case giving more weight to schools for which the agency explicitly requests more SROs.[18] Intuitively, there is more information in observations for schools where the agency says with certainty that one or more SROs will be placed than in a school that we expect to share a new SRO with several other schools. We describe this method in more detail in Appendix D and present findings using both the fuzzy RD and the interacted fuzzy RD in the results section.

RESULTS

Patterns in School Adoption of SROs

Table 1 presents two comparisons. First, it compares characteristics of schools linked to agencies that did not receive CHP awards and schools linked to agencies that did receive awards (columns 1-3). Second, it compares characteristics of schools without an SRO and schools with an SRO in our analytical sample (columns 4-6). Focusing on the latter comparison, we observe that schools who employ an SRO have similar offense rates but significantly higher discipline rates and police

[18] Although Coussens and Spiess (2021) show that including an interaction is equivalent to weighting by the variable interacted with the instrument, we do not explicitly weight by the requested number of SROs. We continue to weight by the number of students in the school, thereby providing an estimate of an impact for the average student in a "compliant" school.

referral/arrest rates than those without an SRO. In terms of school characteristics, secondary schools are more likely to have SRO presence on campus than elementary schools. Elementary schools make up only 28 percent of the schools with an SRO, but 59 percent of the schools without an SRO. Interestingly, our sample of schools shows that on average in schools that have an SRO the proportion of White students is higher, and the proportion of Black and Hispanic students is lower, than in those without one. This finding in student composition contrasts with some earlier findings in the literature (e.g., Theriot, 2009) but mirrors patterns of school policing documented in newer studies (Gleit, 2022). These patterns of student race/ethnicity may in part reflect that SROs are more prevalent in rural and town schools in our data than in urban or suburban schools.

Effects of Grant Awards on SRO Placement in Schools

To validate our approach for estimating effects of increased SRO presence on student outcomes, we first show that the CHP grant thresholds strongly increase the number of FTE SROs at matched schools as a first stage. Row two, column one, of Table 2, which uses our preferred bandwidth of 15 points, shows that being linked to a law enforcement agency that scored above the effective CHP grant award threshold increases the average school's number of FTE SROs by 0.247 ($p<0.01$). This estimate of less than one is consistent with our inclusion of many schools in the analytic sample for which a shared SRO was requested. The average number of requested SROs per school in our sample is 0.151, suggesting close to a one-to-one correspondence between agency proposed officer placements and actual officer placements in schools. The F-statistic in this first stage is 21.4, which allows for a good degree of certainty for hypothesis testing in the second stage (Lee et al., 2022). This coefficient estimate remains consistent at 0.251 ($p<0.01$, $F=20.3$) with the inclusion of all covariates (see column 2).

Figure 2 illustrates this discontinuity in SRO presence graphically for the fuzzy RD. We show both the graph of the estimated function given by equation 1, as well as average values of the number of SROs in 2-point bins of the centered score. The observed gap in the line segments at the vertical discontinuity line is a graphical representation of the 0.247 value of the coefficient estimate presented the first column of Table 2 for the 15-point bandwidth.[19]

Column 3 of Table 2 shows that when we interact the discontinuity indicator with the expected increase in FTE SROs conditional on grant receipt (calculated as described in the data section), we observe an associated increase of 0.94 FTE SROs in schools per requested SRO ($p<0.01$, $F=10.9$). The proximity of this estimate to one suggests that our calculations about where CHP applicants intended to place SROs from their application text are fairly accurate—schools linked to award winners have the number of additional SROs that they requested.

We also estimate both the fuzzy RD and interacted RD at smaller (10-point) and larger (20-point) bandwidths, with and without control measures, and find significant effects of the discontinuity on school SROs at the 99 percent level for all specifications except the interacted RD with a 10-point bandwidth.[20]

Effects of SROs on Student Outcomes

We proceed to examine the second-stage impacts of SROs 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 adverse academic outcomes.

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[19] The downward slope of the line on the left hand of the discontinuity in Figure 2 merits some speculation. This implies that the CHP applicant score may be negatively associated with SRO presence if it weren't for the effect of the award itself. To the extent this is the case, we think it is likely because fiscal need is one element of the application score. Agencies with higher fiscal need may be less likely to have preexisting SRO programs, whereas agencies with lower fiscal need may be more likely to have preexisting SRO programs. This would explain the observed pattern. The existence of a linear trend in the treatment variable with respect to the running variable does not by itself bias results in an RD design, however.

[20] This latter result reflects power/small sample problems caused by the smaller bandwidth and the interaction term.

Incidence of violent offenses in schools

The first two columns of Table 3 present effects of a unit increase in SRO presence on the incidence of student offenses. School firearm offenses occur quite rarely, and so it is difficult to identify substantive rate changes in this outcome. Nonetheless, we find a positive effect of SROs on firearm offenses—suggesting that having an SRO increases the number of reported firearm offenses by between 0.04 and 0.06 per 100 students depending on the specification (or more than a 150 percent increase from the average). This result supports a common finding that SROs increase the detection of weapons offenses (Gottfredson et al., 2020). When we examine the impact of SROs on the bulk of (non-firearm related) school violence, primarily fights and threats, we find in model specifications including covariates (Panel 2) that the presence of an SRO leads to a reduction of between 0.5 and 0.6 offenses per 100 students (or an approximately 30 percent decline from the average). Model specifications without covariates (Panel 1) show null effects of SROs on other violent offenses. In totality, these results suggest that police in schools might modestly reduce fighting and attacks at school and substantially increase gun-related incidents.

Incidence of students receiving disciplinary or law enforcement actions

However, the remainder of Table 3 makes it clear that any potential benefits in violence reduction or gun detection come at very high costs to students. SRO presence has inconsistent but small effects on the prevalence of in-school suspension, but it does increase the rate of out-of-school suspension by between 1.4 and 3.0 students suspended (per 100). This represents somewhere between a 35 and 80 percent increase from average out-of-school suspension rates. We interpret this to mean at least in part that increased detection by the SRO leads principals to take disciplinary actions that they would not have otherwise taken. It is also possible that the presence of an SRO leads the principal to suspend a student when they would have otherwise used a different punishment. We have no way of distinguishing between these two possibilities.[21]

The effects for expulsion (between 0.04 and 0.15) and referral for arrest (between 0.04 and 0.21) are smaller in absolute magnitude but still quite large in relative terms and statistically significant in most specifications. Across models, these changes represent an increase of between 25 and 90 percent of the average expulsion rate and an increase of between 10 and 50 percent of the average police referral and school-based arrest rate.

Incidence of adverse academic outcomes

We find no effect on student grade retention, and inconsistent effects on chronic absenteeism. In particular, we find negative effects of SROs on chronic absenteeism before we add covariates, but positive effects of SROs on chronic absenteeism after we add covariates and the lagged dependent variable. Because of this switch in the direction of the effect across two models, and the lack of significance at the 95 percent level for the other two models, we cannot make any firm conclusions about the effects of SROs on chronic absence of students.

Reduced form effects of school-based policing awards

In addition to estimating two-stage least-squares (2SLS) (treatment-on-the-treated) models of the effects of full-time SROs on student outcomes, we also conduct reduced form (intent-to-treat) estimation of the effects of the school-based policing award cutoff. These results, presented numerically in Appendix Table A2 and plotted graphically in Appendix Figure A6a-h, tell us the effects of a school-based policing award at a linked agency on student outcomes, regardless of whether that award translated into a reported increase in SRO presence. The results from this

[21] Generally, researchers believe that the presence of an SRO would lead the principal to be harsher, rather than more lenient. However, it is at least possible that the principal could be convinced to suspend the student rather than send them to the juvenile justice system. We are not able to test this idea directly in this paper due to the absence of data on juvenile justice referrals. Sorensen et al. (2021) do not find strong evidence for the "lessening" hypothesis.

analysis are generally consistent with the 2SLS results, although smaller in size. They suggest that school-based policing awards have no effect on gun-related offenses; decrease other violent offenses by 0.14 incidents per 100 students; increase in-school suspension by 0.19 students suspended per 100; increase out-of-school suspension by 0.57 students suspended per 100; increase expulsions by 0.04 students expelled per 100; increase police referrals and arrests by 0.05 students referred or arrested per 100; increase absenteeism by 0.53 students chronically absent per 100; and increase grade retention by 0.09 students retained per 100. As intent-to-treat estimates, we regard these effects as likely lower bounds of the true effects of SROs on student outcomes. We also disaggregate by school level in the reduced form estimates, finding that violence reduction happens predominantly in elementary schools whereas discipline and arrest increases happen across all settings. The increases in out-of-school suspensions, expulsions, and police referrals and arrests, are all largest in high schools.

Effects of SROs on Outcomes, by Student Characteristics

As discussed below, the results of our analysis suggest that the introduction of SROs into schools intensifies 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. These findings come in the context of concerns about police in schools extending beyond impacts on the average student. The school-to-prison pipeline narrative asserts that having police in schools likely disproportionately affects 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, LEP status, and disability status. Specifically, we replicate the fuzzy RD method (including full covariates and lagged outcomes) with transformed dependent variables of the count per 100 students 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 and visualized in Figure 3a-c for select outcomes. For the purpose of comparison, we will focus on effect sizes in terms of number of additional incidents ("count") rather than in terms of semi-elasticities, since different student groups have different baseline rates of disciplinary outcomes. Because there are fewer schools with sufficient representation of students from each subgroup, some of these subgroup effects are noisily estimated, leading some differences across groups to not be statistically significant (see Table 4 for formal tests by gender, race/ethnicity, Limited English status, and disability status). Nonetheless, the point estimates of SRO impacts often vary dramatically in consistent ways across student subgroups, and so we describe and focus on these differences in point estimates.

In column 1, we see that increased SRO presence has null effects on the incidence of in-school suspension for most student subgroups, apart from a 29.3 percent increase for male students. The largest effects of SROs on student out-of-school suspension occur for Black students at 5.20 additional suspensions per 100 students, students with disabilities at 5.84 additional suspensions per 100 students, and male students at 3.85 additional suspensions per 100 students. The additional number of Black students suspended due to an additional SRO is nearly double the additional number of White students suspended due to an additional SRO. For expulsion, the most serious school-assigned disciplinary consequence, the increase in number of students expelled resulting from an additional SRO is over three times the size for Black students (0.56 additional expulsions per 100 students) than the corresponding increase for White students (0.17 additional expulsions per 100 students). Disproportionate effects are similarly stark for students with disabilities, who receive 0.35 additional expulsions per 100 students under an additional SRO.

Arrests and referrals to police occurring because of increased SRO presence also predominantly affect Black students with 0.76 additional referrals or arrests per 100 students. The effect of an increase in one FTE SRO on Black student referrals and arrests is over double the size of the same effect on White student referrals and arrests. The increase in referrals and arrests for students with disabilities (0.50 per 100 students) is similarly more than double the increase for students without disabilities (0.18 per 100 students). Finally, in columns 5 and 6, we estimate differential effects of FTE SROs on chronic absenteeism and grade retention. Effects on chronic absenteeism appear largest for female students, White students, and students with disabilities. Notably, for students with disabilities, each additional SRO leads to 5.12 additional students chronically absent per 100, but we encourage caution in interpreting these results given the inconsistency in absence effects across model specifications. Black students and students with disabilities even show increased rates of grade retention after gaining an SRO, with an increase of 2.2 Black students and 1.6 students with disabilities retained per 100.

Robustness Tests

In this section, we test the sensitivity of our analysis to alternative specifications and check for potential threats to exogeneity. Appendix Figure A5a-h plots the point estimates and 95 percent confidence intervals of all specification checks on a single figure for each outcome, using both the fuzzy and interacted fuzzy RD approaches.

First, we re-estimate all models without weighting by student enrollment. Results all qualitatively remain the same.[22] Second, we test the sensitivity of our findings to alternative bandwidths around the CHP award threshold of 10 points and 20 points. Although the estimates with these alternative bandwidths tend to be noisier, they generate point estimates that are in the

[22] The full results from the first stage and second stage without the student enrollment weighting are also provided in Appendix Tables A3 and A4.

same direction and with approximately the same magnitude as our main findings for all outcomes. Third, we add a quadratic control of the running variable to each regression to account for potential nonlinear trends and again uncover very similar findings for all outcomes.[23] Fourth, we include as a control the vector of all lagged dependent variables for all disciplinary and academic outcomes instead of merely controlling for the single lagged dependent variable in each regression separately. Again, our results remain unchanged with this specification, except for the effect on chronic absence, which becomes null.

Not shown in Appendix Figure A5a-h, we additionally test the sensitivity of our findings to alternative transformations of the outcome measures. First, we use the raw count measures instead of the measures transformed using the inverse hyperbolic sine and re-estimate (Appendix Table A5). Results remain the same although with effect sizes much larger in magnitude, suggesting that outliers might play an outsized role in driving these estimates. Next, we take the natural log of the count measures (technically, the natural log of the counts plus one) and re-estimate again. These results are shown in Appendix Table A6 and generally mirror the results found with the inverse hyperbolic sine transformation. The one exception is for other violent offenses, where we observe either null effects or an increase in violence due to the additional SRO. We have further investigated this discrepancy and determined through a series of linear probability models that SROs increase reported violence at the extensive margin but decrease reported violence at the intensive margin (see Appendix Figure A7). This dichotomy, in addition to explaining why we find different effects depending on the scale and transformation of the dependent variable (Chen & Roth, 2022; Mullahy & Norton, 2022), also may help to explain why

[23] We have experimented with adding higher order polynomial function controls for the running variable, and the point estimates in the second stage remain very similar, but we lose power in the first stage, and therefore these estimates are quite noisy.

some prior studies have found that SROs increase reported offenses (Devlin & Gottfredson, 2016; Owens, 2017) while others have found that SROs have no effect on or decrease reported offenses (Anderson, 2018; Sorensen et al., 2021).

Finally, we also estimate the sensitivity of our findings to three different alternative treatment variables, again illustrated in Appendix Figure A5a-h. The first is an indicator of any (>0 FTE) SRO presence at the school. The effects of any SRO presence are very similar in direction, magnitude, and significance to the effects of a one unit increase in FTE SROs (our preferred measure). The second is a count of the number of FTE SROs per 500 students. Again, the effects of this count of SROs scaled by student enrollment on various student outcomes parallels nearly exactly our estimated effects from the baseline approach. The third alternative treatment measure is the natural log of the count of SROs plus one. Again, despite the rescaling of the treatment variable, we observe the same general pattern of findings.

We also seek to confirm the internal validity of our RD design. That is, are the targeted schools for agencies just to the left of the CHP award threshold indistinguishable, on average, from targeted schools for 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.[24] 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. Appendix Figure A8 presents the results from this reverse causality test. The school-based policing award cutoff has null effects on all thirteen outcomes tested, although a marginally

[24] We also check as to whether the award discontinuity has an effect on student enrollment in 2018, which would suggest that students may either enter or leave schools selectively based on whether there is an SRO present. We find null effects that are negative in magnitude. Results available upon request.

significant effect on chronic absence in 2014 ($p<0.1$). In addition to finding no significant associations at the 95 percent level, the point estimates are 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.

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 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 reduced disruption in the academic environment (Figlio, 2007). On the other hand, we find no evidence that SROs reduce more serious gun-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 even though SROs are typically not trained to, and often do not intend to, become involved in minor disciplinary matters in the school (Curran et al., 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 echo findings from prior research on SROs. For instance, we find that SROs increase reports of firearm-related offenses. Prior studies have documented that the funding of an SRO is often followed by an increase in reported and recorded delinquency, particularly for drugs and weapons (Devlin & Gottfredson, 2018; Owens, 2017). This is likely because SROs add an additional layer of detection and 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 shooting incidents 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 et al., 2021).

Our finding that SRO investments increase school-based arrests and referrals to law enforcement both confirms patterns found in prior research (Homer & Fisher, 2020; Owens, 2017; Sorensen et al., 2021) and supports the theory that police stationed in schools change the punishments associated with student misconduct (Hirschfield, 2008; Kupchik, 2009; Theriot, 2009). Another finding replicates prior work (Gottfredson et al., 2020; Sorensen et al., 2021; Weisburst, 2019) that found SROs intensify student suspensions and expulsions, not because the police directly suspend students (they do not have that power), but because the introduction of an SRO causes school administration to increase the use of suspensions and expulsion. Together, these findings on arrests and discipline provide the most compelling evidence yet that stationing police in schools could put at risk other efforts to improve equity in K-12 education. Suspension of students appears to directly harm both their academic achievement (Anderson et al., 2019; Hwang, 2018; Lacoe & Steinberg, 2019) and their longer-run outcomes such as educational attainment and criminal justice involvement (Bacher-Hicks et al., 2019). The arrest of students also hinders later educational engagement and attainment (Hirschfield, 2009a; Mark et al., 2022). The fact that our effect sizes on exclusionary discipline and police referrals and arrests were descriptively larger for Black students and students with disabilities leads us to conclude that the use of police in schools might 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 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 we use come from administrative data provided by school districts and the SSOCS comes from principals at the individual schools. Although underreporting of SROs would not bias our estimates (under the assumption that agency CHP grants do not affect CRDC reporting practices of both SRO presence and outcome measures at targeted school districts), it means that our estimates are only generalizable to localities that do report SRO presence. 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. Because the CHP requires that agencies provide National Association of School Resource Officers training to any deployed SROs, our estimates also may not generalize to SROs with less comprehensive training.

Finally, we have no way to disentangle the causal pathways through which SROs influence student offenses and disciplinary outcomes. We do not know, for instance, whether the increase in gun-related incidents reflects changes in detection and reporting activities at the school or reflects criminogenic effects of SROs due to the escalating interactions with students (Higgins et al., 2022; Nolan, 2011). Given the low incidence of gun-related offenses, this outcome is also most susceptible to influence from outlier values. The observed reduction in other forms of school violence also appears to reflect multiple underlying forces – resulting in an increase of reported violence at the extensive margin and a decrease at the intensive margin.[25] In terms of disciplinary response, we also cannot know whether the increase in suspensions and expulsions comes from increased detection of student misconduct by SROs, or from increased pressure on school administrators to punish student misconduct, or from some other mechanism. Offenses and consequences are intrinsically linked with one another. It could be that intensified disciplinary consequences under SROs subsequently drive reductions in student offenses through deterrence. It could also be that the SRO effects on out-of-school suspension, expulsion, and arrests, would actually be larger if it were not for the simultaneous decrease in violent offenses that could have otherwise led to exclusionary discipline (Sorensen et al., 2021). Future research with incident-level data on both student behavior and school responses could delve into the specific changes in internal school processes that occur when an SRO is introduced to the school environment.

Despite these limitations, this study presents an important new school-level examination of sworn law enforcement officers across a diverse sample 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. This study 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

[25] Both offense outcomes lack the lagged variable control from the 2014 CRDC wave and therefore could be more susceptible to potential sources of endogeneity than the disciplinary or academic outcomes. We think it is unlikely that this is a serious issue, however, given that the effects on gun-related offenses and other violent offenses do not change with inclusion of a vector of all available lagged disciplinary and academic outcomes (Appendix Figure A5a-h).

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 (Adukia et al., 2022; Augustine, et al. 2018).

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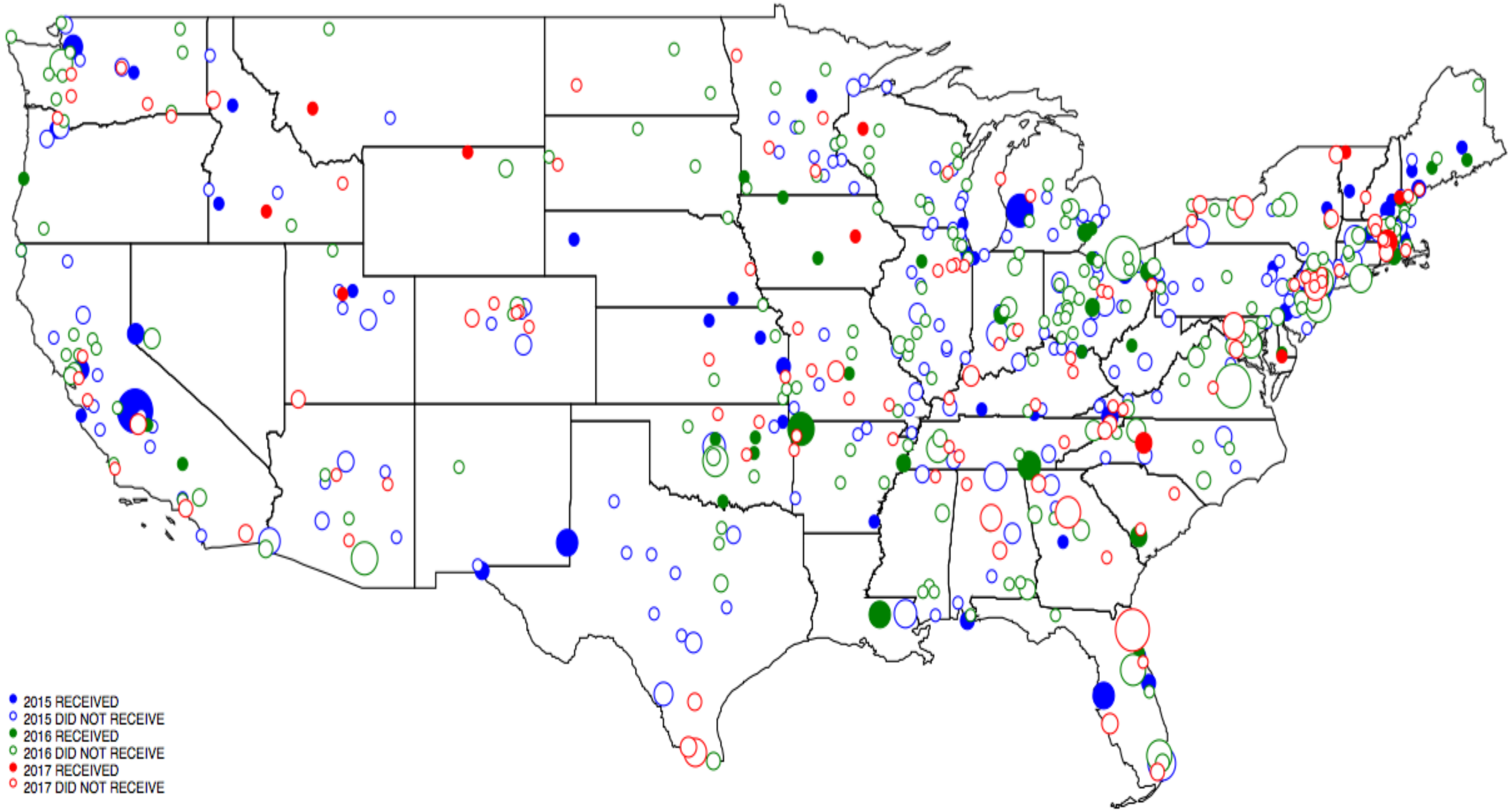
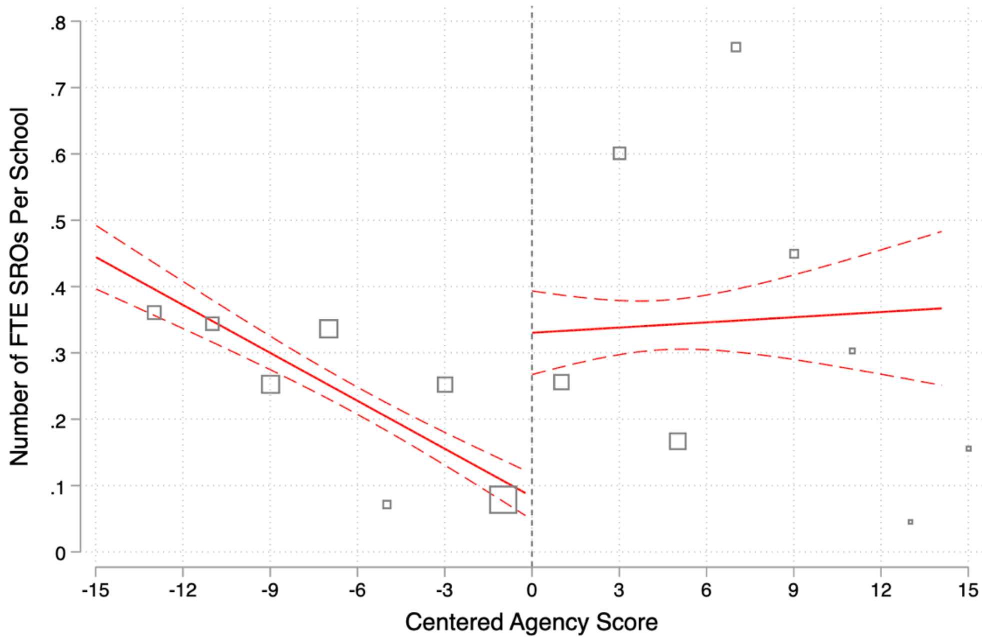
TABLES AND FIGURES



Notes: This map plots the coordinates of each applicant for a school-based policing award through the COPS Hiring Program in the 2015 (blue), 2016 (green), and 2017 (red) grant cycles. Filled-in circles represent applicants that receive awards, and not-filled-in circles represent applicants that did not receive awards. Each marker is sized proportionately by the number of police officers requested. Hawaii and Alaska are not included since there are no school-based policing applicants from these states.

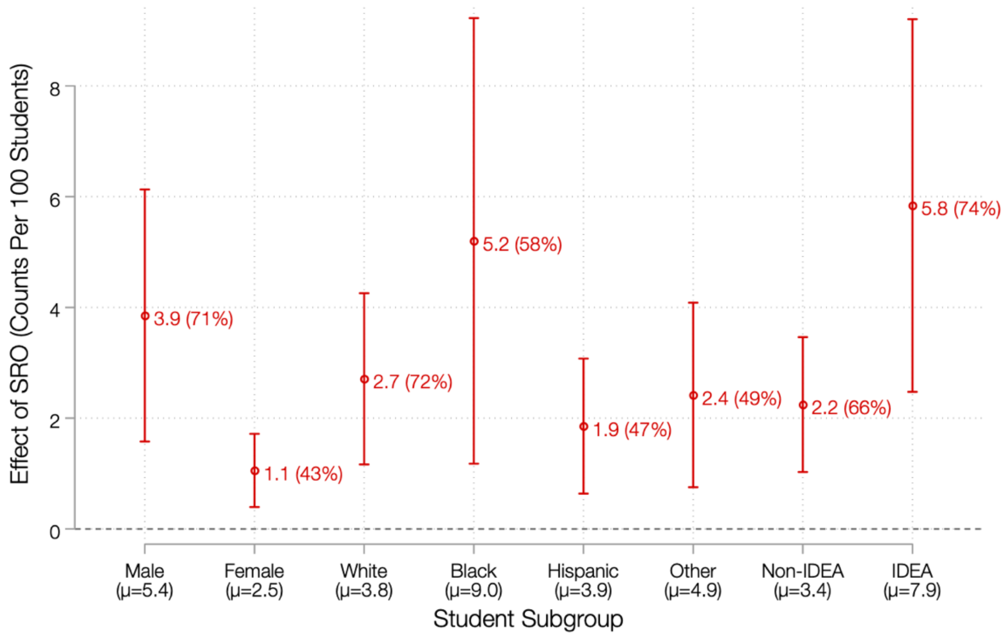
Figure 1. Locations of Agencies Applying for School-Based Policing Grants.



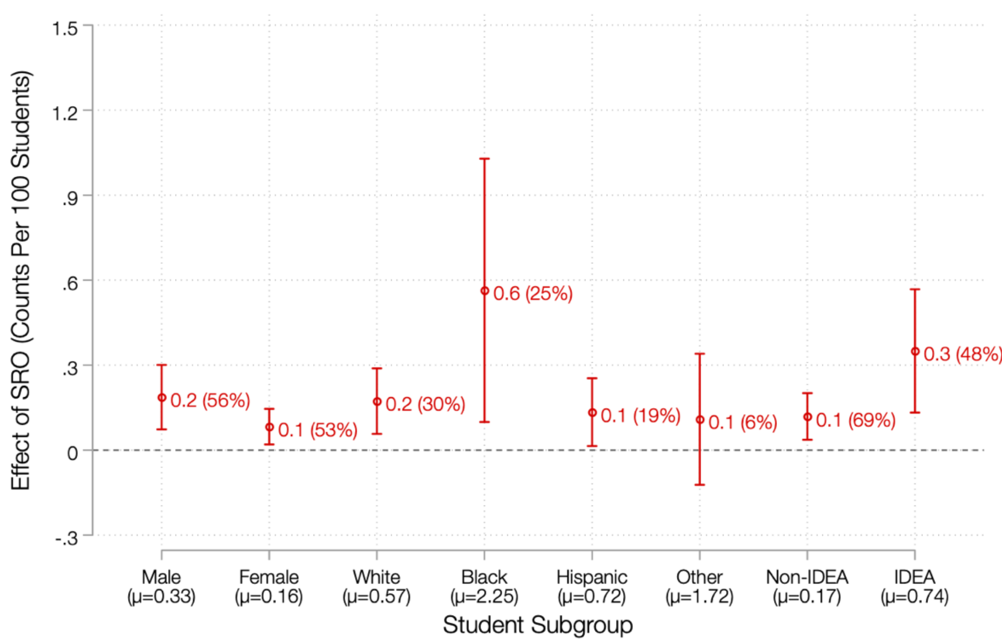
Notes: The graph shows binned means, linear plots, and 95% confidence intervals for school SRO FTEs by centered agency application score. These graphs restrict the dataset to schools linked to agency applicants within 15 points of the effective threshold. Bins are constructed in 2-point increments, weighted by total enrollment of matched schools to agencies within the bin.

Figure 2. Number of FTE SROs in Matched Schools by Agency Application Score.

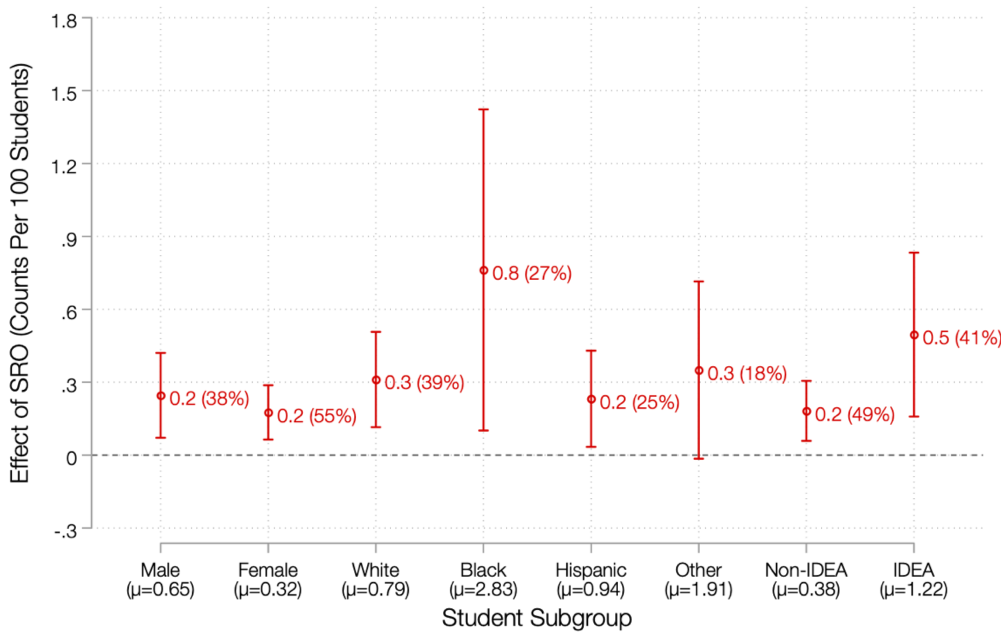
(a) Out-of-School Suspension



(b) Expulsion



(c) Police Referral and Arrest



Notes: These graphs present estimated effects of an additional FTE SRO on the number of students with a particular characteristic receiving the given consequence per 100 students with that characteristic, with 95% confidence intervals from bootstrapped standard errors (1,000 resamples), clustered by agency. Estimates are from the fuzzy RD specification with all control variables: school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students LEP, and the lagged dependent variable.

Figure 3 (a)–(c). Effects of a FTE SROs on Select Outcomes, by Student Characteristics.

Table 1. Descriptive statistics for analytical sample by agency SBP award status and school SRO status.

	(1) No SBP Award (n=2,136)	(2) SBP Award (n=869)	(3) Std Difference SBP – No SBP	(4) No SRO (n=2,366)	(5) Any SRO (n=639)	(6) Std Difference SRO – No SRO
Outcomes (per 100 students)[a]						
Gun-related offenses	0.020 (0.132)	0.078 (0.561)	0.058**	0.036 (0.357)	0.039 (0.135)	0.003
Other offenses	2.676 (7.512)	1.708 (3.824)	-0.967**	2.413 (7.019)	2.332 (5.197)	-0.081
Students with any ISS	2.749 (5.163)	3.334 (6.791)	0.584*	2.258 (4.927)	5.364 (7.394)	3.107**
Students with any OSS	4.082 (6.774)	4.857 (6.439)	0.775**	3.672 (6.393)	6.655 (7.214)	2.983**
Students with expulsion	0.121 (0.794)	0.206 (0.938)	0.085*	0.080 (0.490)	0.389 (1.532)	0.309**
Students with arrest/referral	0.316 (1.278)	0.355 (1.130)	0.039	0.207 (0.834)	0.774 (2.092)	0.568**
Students chronically absent	17.491 (12.626)	17.916 (12.768)	0.425	17.389 (12.710)	18.434 (12.483)	1.045+
Students retained a grade	2.286 (5.099)	2.022 (2.905)	-0.264+	2.241 (4.530)	2.094 (4.741)	-0.147
School Policing[a]						
FTE sworn police officers	0.135 (0.356)	0.228 (0.454)	0.093**	0.000 (0.000)	0.761 (0.505)	0.761**
Any sworn police officer	0.193 (0.395)	0.261 (0.440)	0.068**	0.000 (0.000)	1.000 (0.000)	1.000
Agency Characteristics[b]						
School-based policing award	0.000 (0.000)	1.000 (0.000)	1.000	0.271 (0.445)	0.355 (0.479)	0.084**
E(SRO|score>0)	0.150 (0.226)	0.157 (0.230)	0.006	0.124 (0.205)	0.257 (0.271)	0.133**
Final score (centered)	-6.023 (4.356)	4.350 (3.278)	10.373**	-2.784 (5.862)	-3.910 (7.344)	-1.127**
Final score (original)	123.68 (8.303)	132.20 (6.254)	8.519**	125.89 (8.101)	127.10 (10.477)	1.207**
Fiscal need score	7.584 (2.153)	7.649 (2.069)	0.064	7.635 (2.114)	7.485 (2.181)	-0.150
Crime score	25.776 (8.248)	29.786 (7.889)	4.010**	26.585 (8.477)	28.231 (7.708)	1.645**
Community policing score	77.616 (10.251)	82.435 (6.530)	4.819**	79.315 (9.808)	77.878 (8.600)	-1.437**
Large agency indicator	0.379 (0.485)	0.310 (0.463)	-0.069**	0.411 (0.492)	0.166 (0.372)	-0.245**
School Characteristics[a]						
Elementary school	0.478 (0.500)	0.638 (0.481)	0.160**	0.589 (0.492)	0.283 (0.451)	-0.306**
Middle school	0.162 (0.369)	0.142 (0.349)	-0.021	0.134 (0.341)	0.238 (0.426)	0.103**
High school	0.173 (0.378)	0.137 (0.344)	-0.036*	0.112 (0.315)	0.349 (0.477)	0.237**
Other grade configuration	0.187 (0.390)	0.084 (0.278)	-0.103**	0.165 (0.371)	0.130 (0.336)	-0.035*
Alternative school	0.023 (0.150)	0.018 (0.135)	-0.005	0.023 (0.151)	0.016 (0.124)	-0.008
Charter school	0.072 (0.259)	0.037 (0.188)	-0.035**	0.077 (0.267)	0.006 (0.079)	-0.071**
Magnet school	0.378 (0.486)	0.655 (0.480)	0.277**	0.434 (0.497)	0.410 (0.498)	-0.023
Special education school	0.009 (0.094)	0.006 (0.076)	-0.003	0.010 (0.100)	0.000 (0.000)	-0.010**

	(1) No SBP Award (n=2,136)	(2) SBP Award (n=869)	(3) Std Difference SBP – No SBP	(4) No SRO (n=2,366)	(5) Any SRO (n=639)	(6) Std Difference SRO – No SRO
Urban location	0.257 (0.437)	0.376 (0.485)	0.119**	0.317 (0.465)	0.199 (0.399)	-0.118**
Suburban location	0.539 (0.499)	0.384 (0.487)	-0.155**	0.512 (0.500)	0.429 (0.495)	-0.083**
Town location	0.085 (0.279)	0.083 (0.276)	-0.002	0.067 (0.250)	0.149 (0.356)	0.082**
Rural location	0.118 (0.323)	0.157 (0.364)	0.038**	0.104 (0.305)	0.224 (0.417)	0.120**
South region	0.484 (0.500)	0.570 (0.495)	0.086**	0.555 (0.497)	0.338 (0.473)	-0.216**
Midwest region	0.141 (0.348)	0.112 (0.315)	-0.029*	0.117 (0.322)	0.189 (0.392)	0.072**
Northeast region	0.149 (0.357)	0.060 (0.237)	-0.090**	0.102 (0.303)	0.203 (0.403)	0.102**
West region	0.226 (0.418)	0.259 (0.438)	0.033+	0.227 (0.419)	0.269 (0.444)	0.043*
Student enrollment	629.91 (466.51)	602.81 (407.33)	-27.104	571.70 (382.64)	808.59 (606.42)	236.885**
Number of teachers per pupil	0.068 (0.028)	0.068 (0.022)	-0.000	0.068 (0.027)	0.067 (0.024)	-0.001
Proportion of students White	37.272 (32.785)	40.942 (30.466)	3.670**	34.157 (30.983)	53.799 (31.781)	19.643**
Proportion of students Black	19.156 (25.818)	16.665 (23.433)	-2.490*	19.898 (26.121)	13.020 (20.423)	-6.879**
Proportion of students Hispanic	34.694 (31.769)	33.804 (30.081)	-0.890	37.240 (31.671)	24.056 (27.460)	-13.183**
Proportion of students other	8.878 (10.539)	8.589 (8.418)	-0.289	8.706 (10.171)	9.125 (9.196)	0.419
Proportion of students LEP	12.502 (14.702)	13.398 (15.871)	0.896	14.090 (15.523)	7.839 (11.949)	-6.252**
Proportion of students IDEA	13.630 (10.700)	14.519 (8.839)	0.889*	14.002 (11.133)	13.461 (5.521)	-0.540+
FTE security guards	0.390 (1.213)	0.343 (1.169)	-0.047	0.259 (0.859)	0.808 (1.953)	0.549**
FTE psychologists	0.315 (0.608)	0.312 (0.456)	-0.004	0.283 (0.545)	0.430 (0.635)	0.147**
FTE social workers	0.192 (0.848)	0.261 (0.475)	0.069**	0.188 (0.457)	0.301 (1.390)	0.113*
FTE nurses	0.388 (0.535)	0.605 (0.616)	0.216**	0.370 (0.524)	0.750 (0.622)	0.380**

** p<0.01; * p<0.05; + p<0.1.

[a] Measured in 2018 after receipt (or no receipt) of the CHP award

[b] Measured in the year of application for the CHP award

Notes: Each cell in columns 1,2, 4, and 5 contains the unweighted variable mean and standard deviation. The SBP categories refer to schools linked to agencies that do and do not receive school-based policing awards. The SRO categories refer to schools with and without any reported sworn law enforcement officers in 2018. Columns 3 and 5 present t-tests of the standardized difference in variable means between the noted two groups of schools. n indicates the number of schools linked to agencies in each column. Statistics and tests are unweighted.

Table 2. Effects of school-based policing award cutoff on FTE SROs (first stage).

	Fuzzy RD		Interacted RD	
Bandwidth	(1)	(2)	(3)	(4)
[-20, 20]	0.218**	0.193**	0.914**	0.653*
	(0.073)	(0.064)	(0.314)	(0.284)
	F=8.8	F=9.2	F=8.5	F=5.3
	n=3,433	n=3,433	n=3,433	n=3,433
[-15, 15]	0.247**	0.251**	0.935**	0.827**
	(0.053)	(0.056)	(0.283)	(0.269)
	F=21.4	F=20.3	F=10.9	F=9.5
	n=3,005	n=3,005	n=3,005	n=3,005
[-10, 10]	0.182**	0.170**	0.331	0.259
	(0.057)	(0.058)	(0.232)	(0.256)
	F=10.1	F=8.5	F=2.0	F=1.0
	n=2,490	n=2,490	n=2,490	n=2,490
Covariates	No	Yes	No	Yes

** p<0.01; * p<0.05; + p<0.1.

Notes: All fuzzy RD models present the estimated coefficient on the discontinuity and control for centered agency score and centered agency score interacted with the discontinuity. All interacted RD models show the linear combination of the estimated coefficient on the discontinuity and the estimated coefficient on the discontinuity interacted with E(SRO|D=1) from models controlling for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. n is the number of schools in the analysis. Standard errors in parentheses are constructed from 1,000 bootstrapped samples, clustered by agency. Columns 2 and 4 include the full set of control variables, including school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students LEP.

Table 3. Effects of FTE SROs on student outcomes (2SLS).

	Offense Outcomes		Discipline Outcomes				Academic Outcomes	
	(2) Gun offenses	(3) Other offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
Panel 1 Basic Controls (n=3,005)								
Fuzzy RD								
Semi-elasticity	1.554*	0.013	0.323*	0.775**	0.437*	0.110	-0.165*	0.005
(SE)	(0.693)	(0.089)	(0.141)	(0.211)	(0.190)	(0.133)	(0.079)	(0.096)
Count	0.047	0.025	1.027	3.027	0.070	0.044	-2.902	0.009
Interacted RD								
Semi-elasticity	1.576*	-0.064	0.098	0.453*	0.266	0.196	-0.115+	-0.030
(SE)	(0.634)	(0.123)	(0.170)	(0.207)	(0.174)	(0.132)	(0.062)	(0.085)
Count	0.048	-0.124	0.312	1.769	0.043	0.079	-2.023	-0.056
Panel 2 Full Controls (n=2,857)								
Fuzzy RD								
Semi-elasticity	1.949*	-0.295*	0.243+	0.618**	0.897**	0.516**	0.112*	0.191+
(SE)	(0.790)	(0.115)	(0.124)	(0.190)	(0.274)	(0.168)	(0.050)	(0.104)
Count	0.059	-0.571	0.773	2.414	0.143	0.208	1.970	0.360
Interacted RD								
Semi-elasticity	1.995**	-0.314*	0.082	0.378+	0.542+	0.497**	0.078	0.106
(SE)	(0.771)	(0.124)	(0.151)	(0.209)	(0.314)	(0.191)	(0.057)	(0.131)
Count	0.060	-0.607	0.261	1.476	0.087	0.201	1.372	0.200
Mean	0.030	1.935	3.180	3.906	0.160	0.404	17.587	1.882

**p<0.01; *p<0.05; +p<0.1.

Notes: Outcomes are counts per 100 students, transformed by the inverse hyperbolic sine. The semi-elasticity is calculated as in Bellemare and Wichman (2020). Standard errors are from 1,000 bootstrapped resamples, clustered by agency. "Count" provides the estimated effect in incidents per 100 students. n is the number of schools in the analysis. Bandwidth is restricted to 15 points on both sides of the discontinuity. All fuzzy RD models use the discontinuity as an instrumental variable and control for centered agency score and centered agency score interacted with the discontinuity. All interacted RD models use the discontinuity and E(SRO|D=1) interacted with the discontinuity as instrumental variables and control for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. For the full controls estimates, models also include school level indicators, school locale indicators, region indicators, agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity, percent of students with disabilities, percent of students Limited English Proficiency, and the lagged dependent variable when available (all outcomes except for offense outcomes).

Table 4. Effects of full-time SRO on outcomes by student characteristics (2SLS).

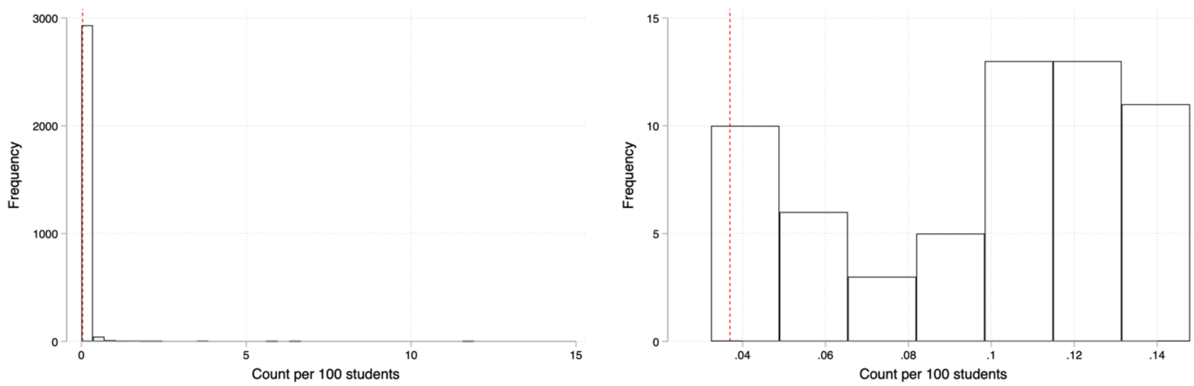
Student Characteristic		Discipline Outcomes				Academic Outcomes	
	Statistic	(1) ISS	(2) OSS	(3) Expulsion	(4) Ref/Arrest	(5) Absence	(6) Retention
Gender							
Male	Count	1.301*	3.854**	0.187**	0.246**	1.973*	0.483+
(n=2,850)	(SE)	(0.611)	(1.161)	(0.058)	(0.089)	(0.945)	(0.256)
	Elasticity	0.293	0.711	0.565	0.377	0.107	0.211
	Mean	4.439	5.421	0.330	0.653	18.384	2.283
Female	Count	0.349[a]	1.057**[a]	0.083**[a]	0.176**	2.336*	0.262+
(n=2,851)	(SE)	(0.218)	(0.337)	(0.032)	(0.057)	(0.987)	(0.157)
	Elasticity	0.172	0.430	0.527	0.553	0.125	0.158
	Mean	2.025	2.457	0.158	0.319	18.734	1.653
Race/Ethnicity							
White	Count	0.663	2.711**	0.173**	0.311**	3.692*	1.235**
(n=2,825)	(SE)	(0.428)	(0.789)	(0.059)	(0.100)	(1.531)	(0.464)
	Elasticity	0.209	0.718	0.302	0.394	0.187	0.365
	Mean	3.165	3.778	0.573	0.789	19.715	3.387
Black	Count	2.212	5.201*[b]	0.564*[b]	0.762*	-2.167[b]	2.234*
(n=2,731)	(SE)	(1.359)	(2.052)	(0.237)	(0.337)	(2.368)	(0.907)
	Elasticity	0.303	0.578	0.251	0.270	-0.095	0.367
	Mean	7.306	8.997	2.248	2.826	22.921	6.083
Hispanic	Count	0.699	1.857**[b]	0.134*	0.232*	0.564[b]	1.310*
(n=2,810)	(SE)	(0.483)	(0.622)	(0.061)	(0.101)	(1.349)	(0.643)
	Elasticity	0.199	0.472	0.186	0.248	0.026	0.282
	Mean	3.508	3.937	0.720	0.935	21.481	4.641
Other race	Count	0.887	2.419**	0.109	0.350+	3.883+	1.596*
(n=2,749)	(SE)	(0.618)	(0.850)	(0.118)	(0.186)	(2.311)	(0.631)
	Elasticity	0.208	0.491	0.064	0.183	0.205	0.385
	Mean	4.273	4.929	1.718	1.911	18.951	4.148
Other Groups							
Limited English	Count	1.106	2.353+	0.503	0.493	1.914	0.886*
(n=2,528)	(SE)	(1.198)	(1.337)	(0.329)	(0.393)	(2.394)	(0.431)
	Elasticity	0.158	0.317	0.115	0.105	0.083	0.192
	Mean	7.010	7.432	4.390	4.692	23.034	4.603
No Limited English	Count	1.144*	2.848**	0.152**	0.219**	0.752	0.641**
(n=2,856)	(SE)	(0.488)	(0.779)	(0.047)	(0.069)	(0.893)	(0.241)
	Elasticity	0.355	0.717	0.926	0.527	0.040	0.288
	Mean	3.222	3.975	0.164	0.417	18.678	2.225
Has disability	Count	1.644+	5.840**[d]	0.350**[d]	0.496**[d]	5.117**[d]	1.614*[d]
(n=2,777)	(SE)	(0.866)	(1.716)	(0.111)	(0.172)	(1.838)	(0.639)
	Elasticity	0.286	0.738	0.475	0.406	0.192	0.412
	Mean	5.755	7.912	0.736	1.221	26.715	3.919
No disability	Count	1.105*	2.246**	0.119**	0.182**	0.688	0.426*
(n=2,857)	(SE)	(0.430)	(0.621)	(0.042)	(0.063)	(0.894)	(0.174)
	Elasticity	0.381	0.660	0.694	0.486	0.039	0.248
	Mean	2.898	3.405	0.172	0.375	17.461	1.719

**p<0.01; *p<0.05; +p<0.1, [a] different from male (p<0.1), [b] different from white (p<0.1), [c] different from no Limited English (p<0.1), [d] different from no disability (p<0.1).

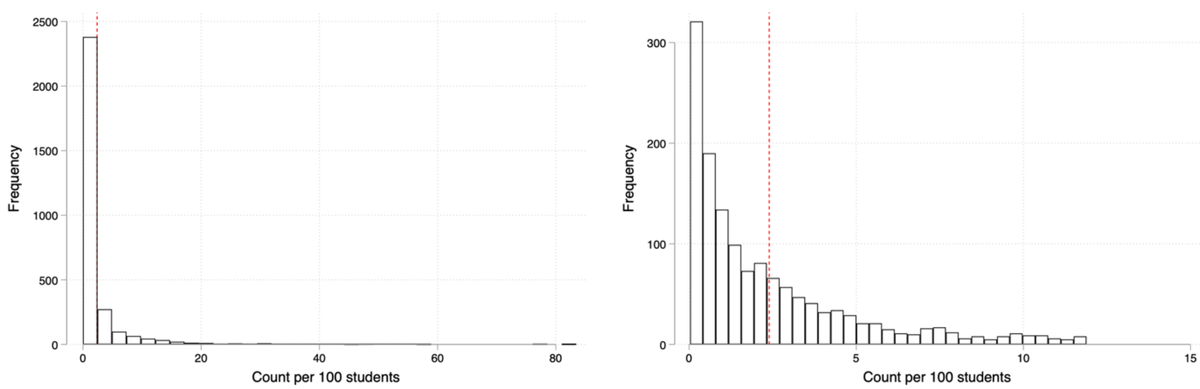
Notes: Bootstrapped standard errors from 1,000 resamples in parentheses, clustered by agency. All estimates use the discontinuity as an instrumental variable with a bandwidth of 15 points on either side of the discontinuity. n is the number of schools in the analysis. Controls include: centered agency score, centered agency score interacted with the discontinuity, school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students Limited English Proficiency, and the lagged dependent variable.

APPENDIX A. ADDITIONAL FIGURES AND TABLES

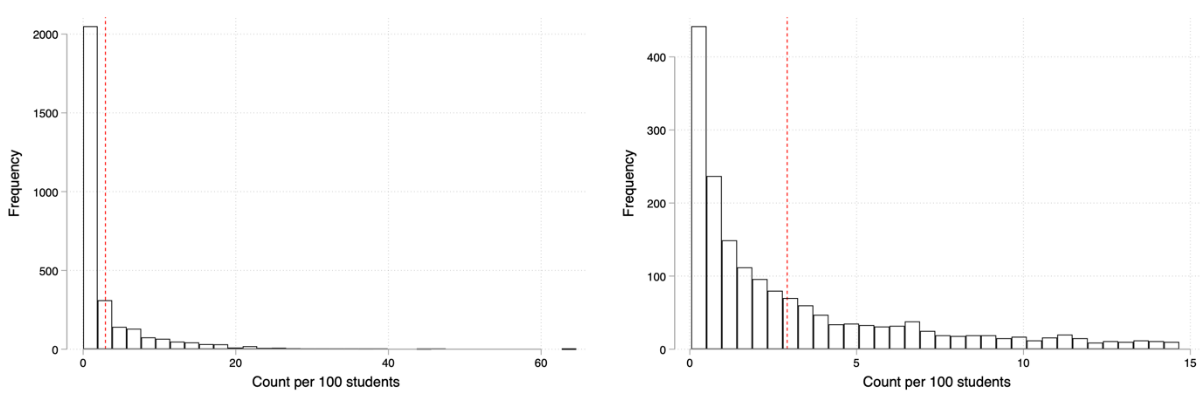
(a) Gun-Related Offense



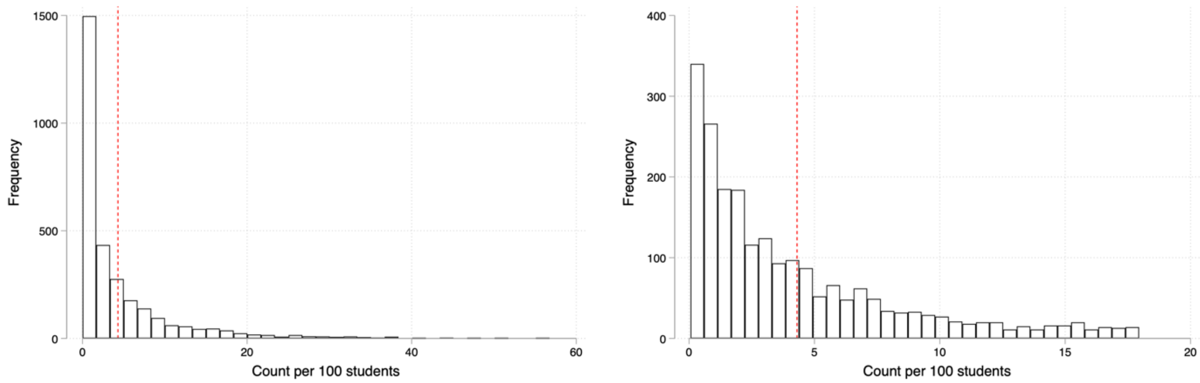
(b) Other Offense



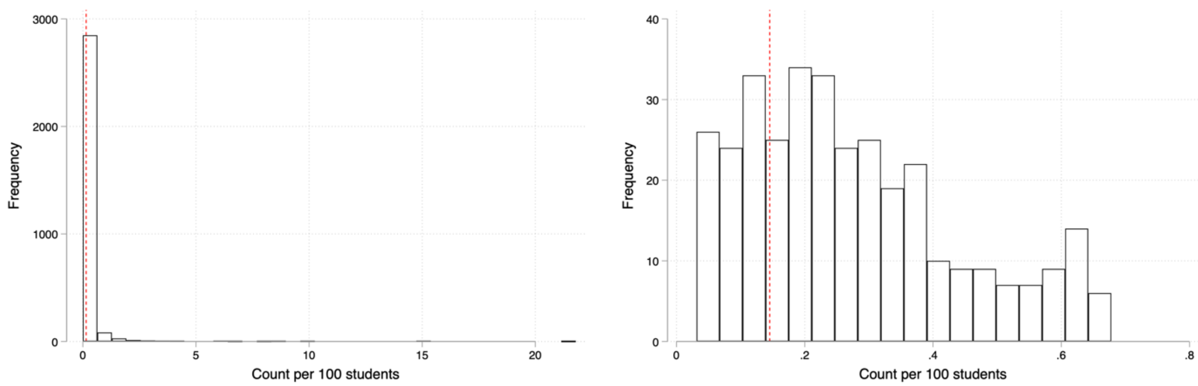
(c) In-School Suspension



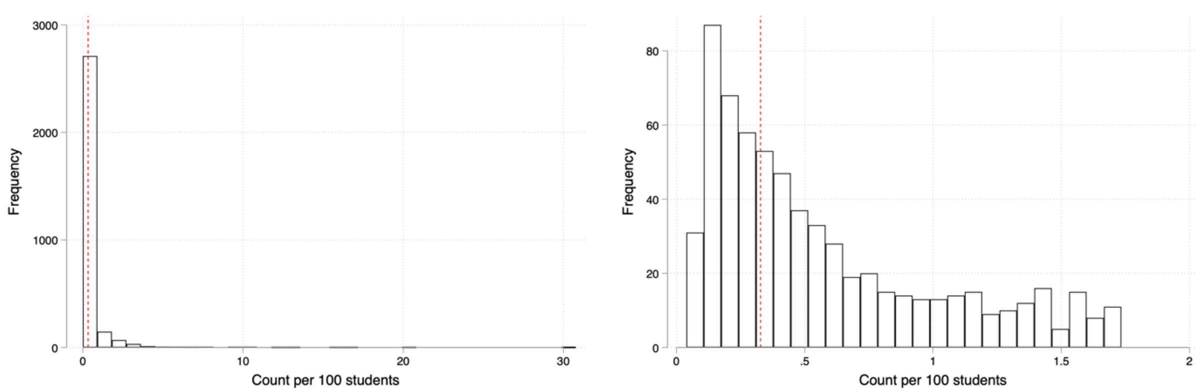
(d) Out-of-School Suspension



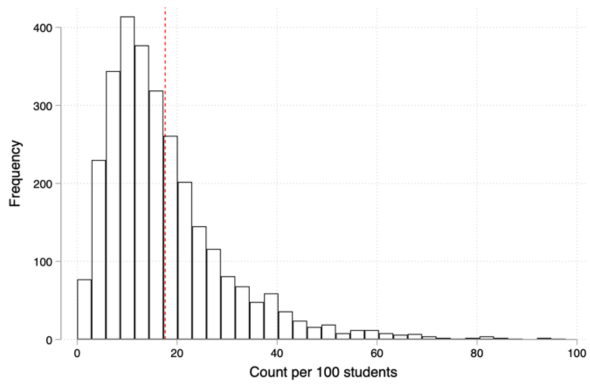
(e) Expulsion

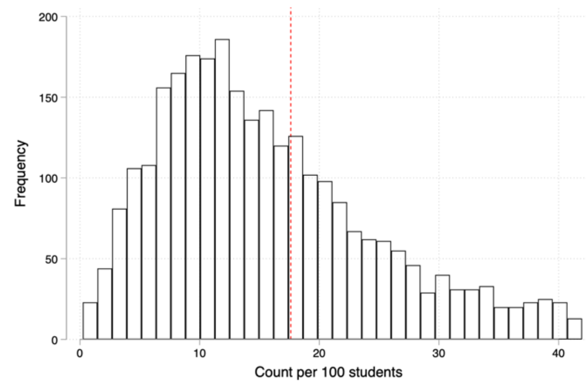


(f) Police Referral or Arrest

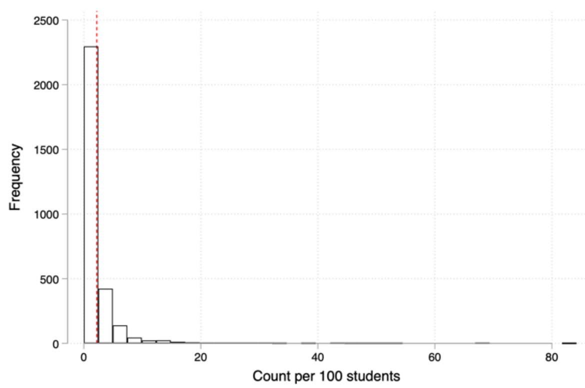


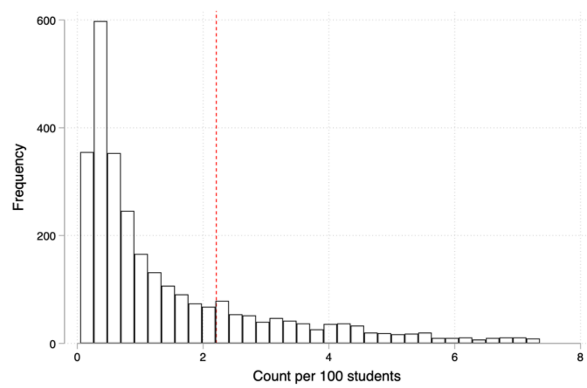
(g) Chronic Absence





(h) Grade Retention

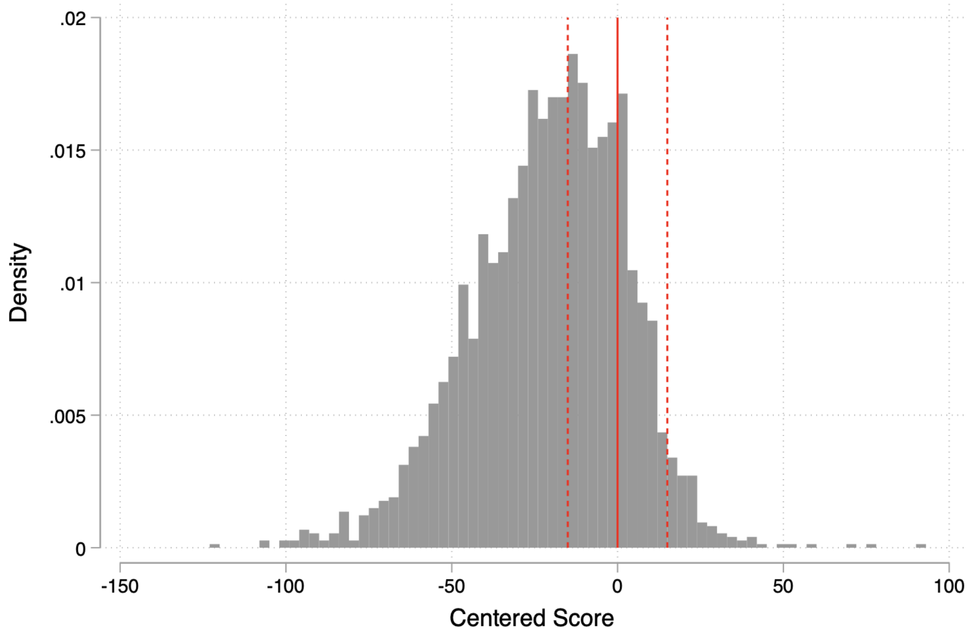




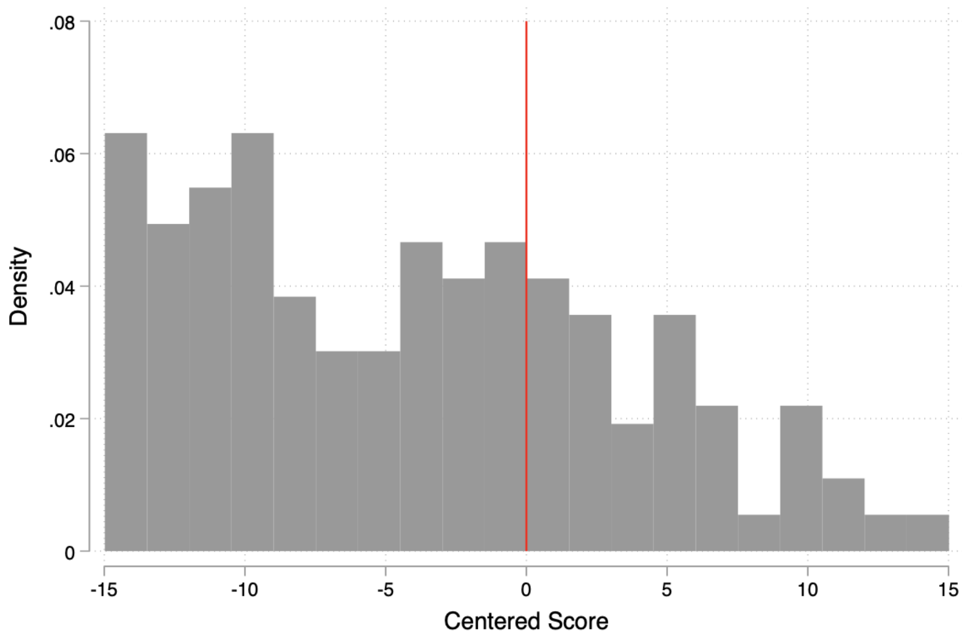
Notes: For each outcome, the left figure plots a frequency histogram within the 15-point bandwidth of schools linked to police agencies. The right figure plots a frequency histogram excluding zero values and excluding values above the 95th percentile of the outcome. The red dotted line represents the variable mean.

Figure A1 (a)-(h). Histograms of Outcome Measures.

(a) All Applicants

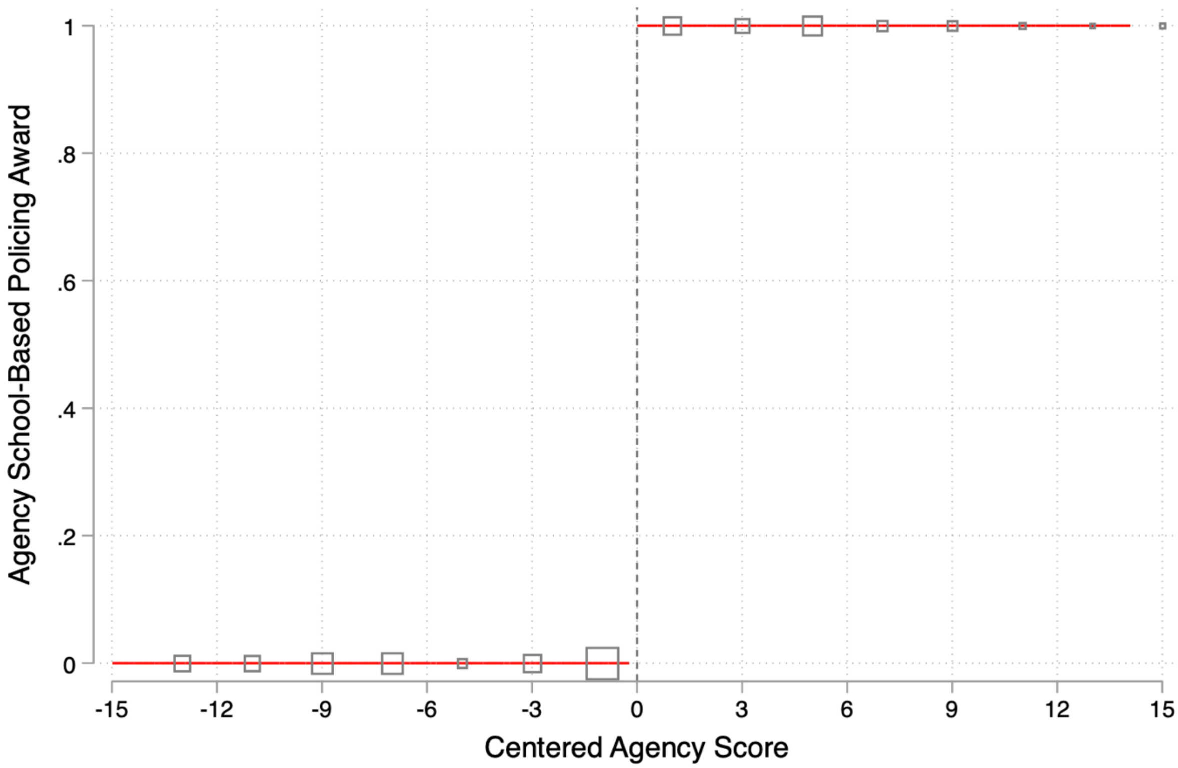


(b) School-Based Policing Applicants in Preferred Bandwidth



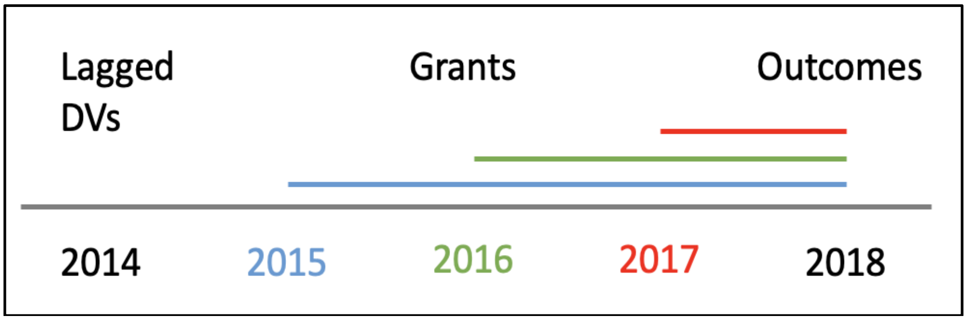
Notes: Histogram (a) includes applicants for all categories of CHP grants (2,452 applicants), and (b) restricts to school-based policing applicants within 15 points of the bandwidth (243 applicants). The corresponding RD manipulation test (Cattaneo, Jansson, & Ma, 2020) produces a T-statistic of -0.1274 and a p-value of 0.8986, indicating there is no evidence of score manipulation around the discontinuity.

Figure A2 (a)-(b). Density Plot of Agencies by Centered Application Score.



Notes: The graph shows binned means and linear plots for an indicator that equals one if the agency received a school-based policing award by centered agency application score. These graphs restrict the dataset to one observation for each applicant within 15 points of the effective threshold (243 applicants). Bins are constructed in 2-point increments, weighted by total enrollment of matched schools to agencies within the bin.

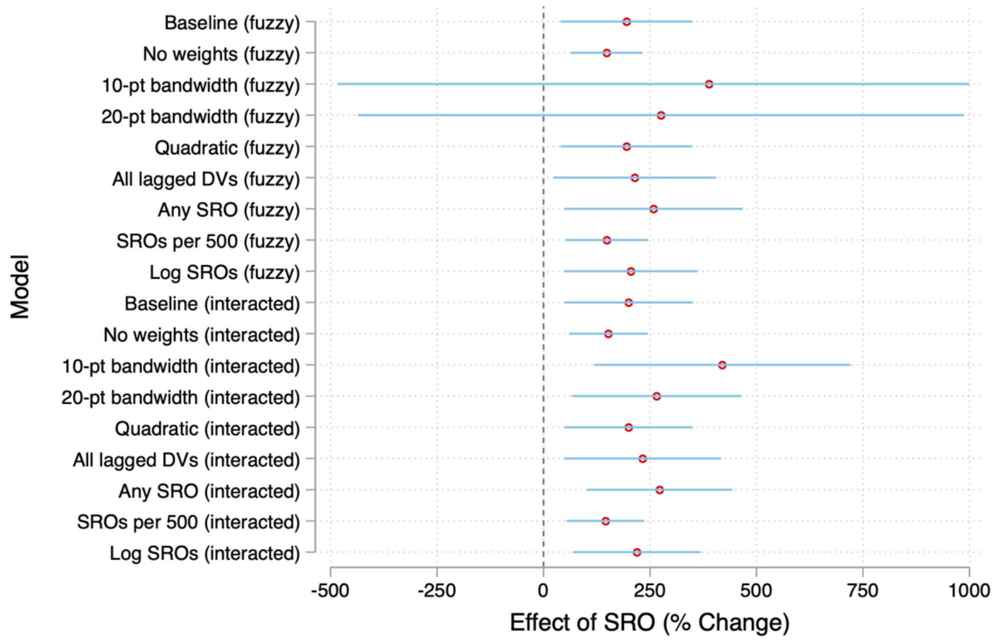
Figure A3. Effect of Discontinuity on Agency Receipt of School-Based Policing Award.



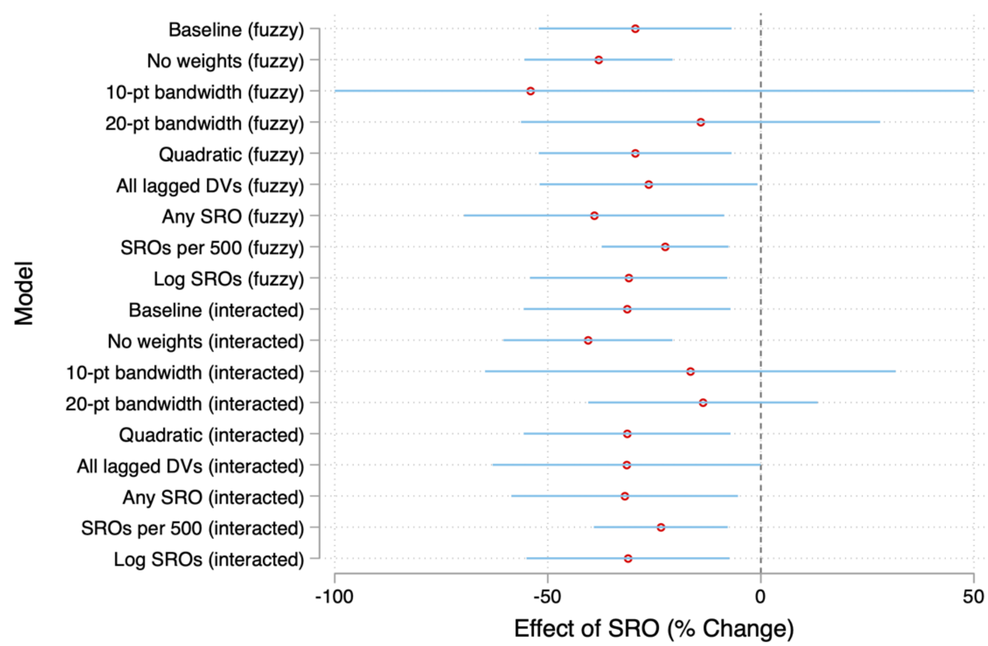
Notes: All CHP grants last three years, and funds are provided in the first fiscal year of the grant cycle. The school-based officers funded by CHP grants in 2015 (blue line), 2016 (green line), and 2017 (red line) grant cycles therefore all should be present in their assigned school during the 2017-18 school year and observed in the 2018 CRDC data. Our measure of SRO presence and outcome variables are taken from the 2018 CRDC data and lagged outcome variables are taken from the 2014 CRDC data.

Figure A4. Timeline of CHP Grant Awards and CRDC Measures.

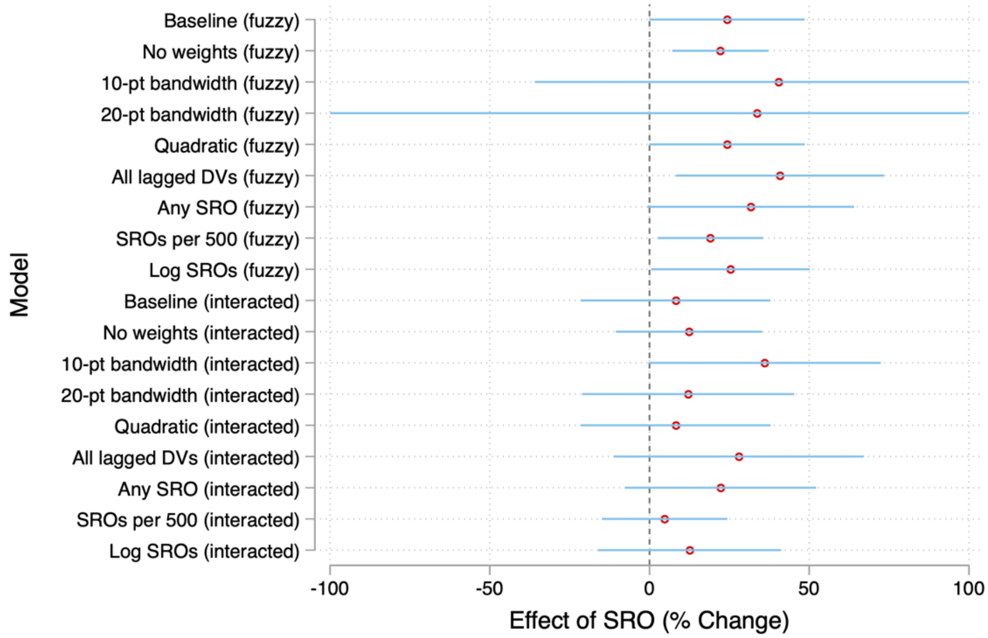
(a) Gun-Related Offense



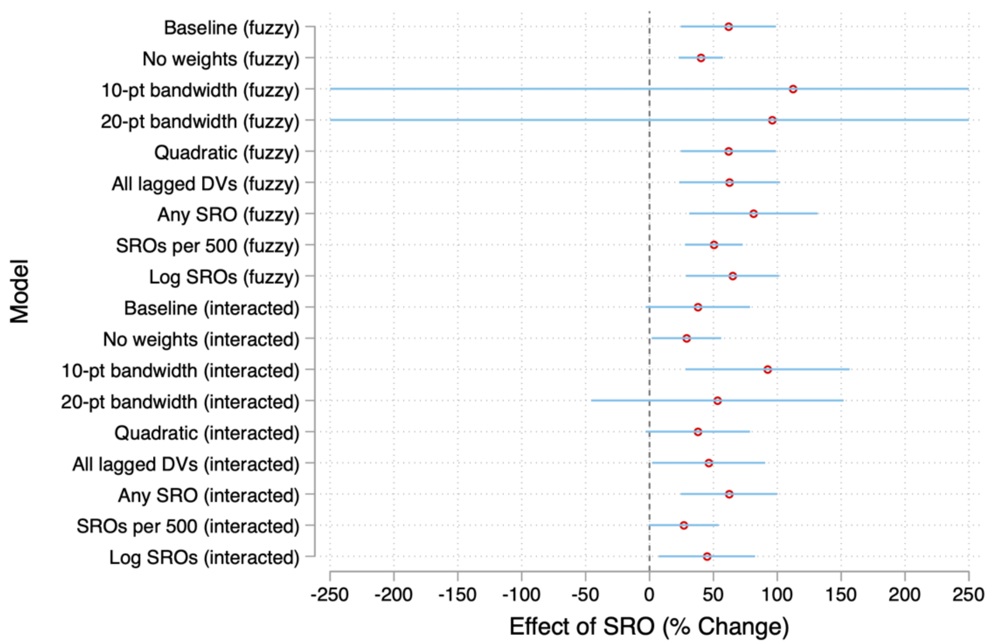
(b) Other Offense



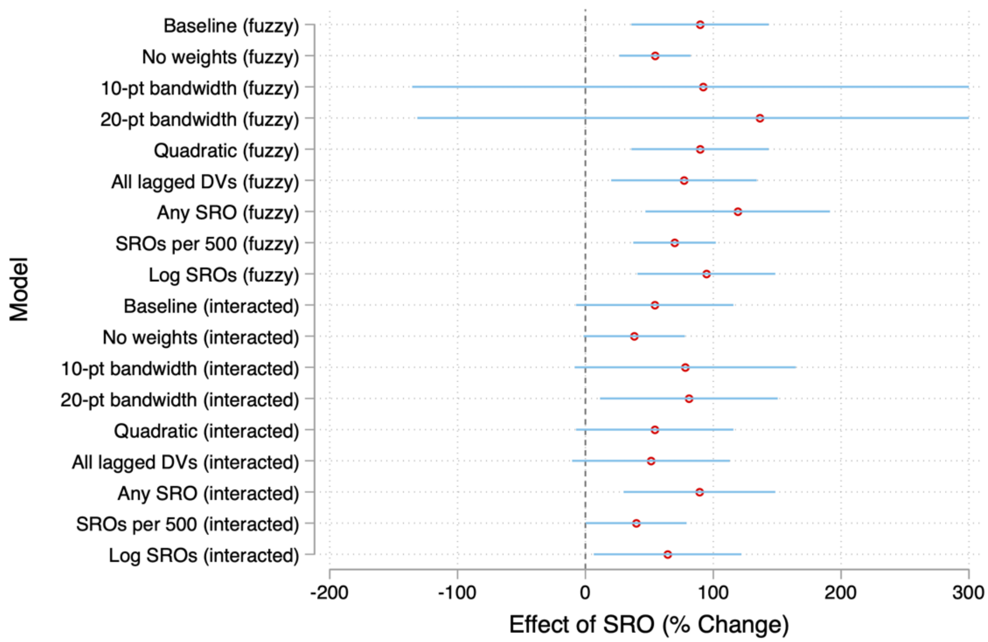
(c) In-School Suspension



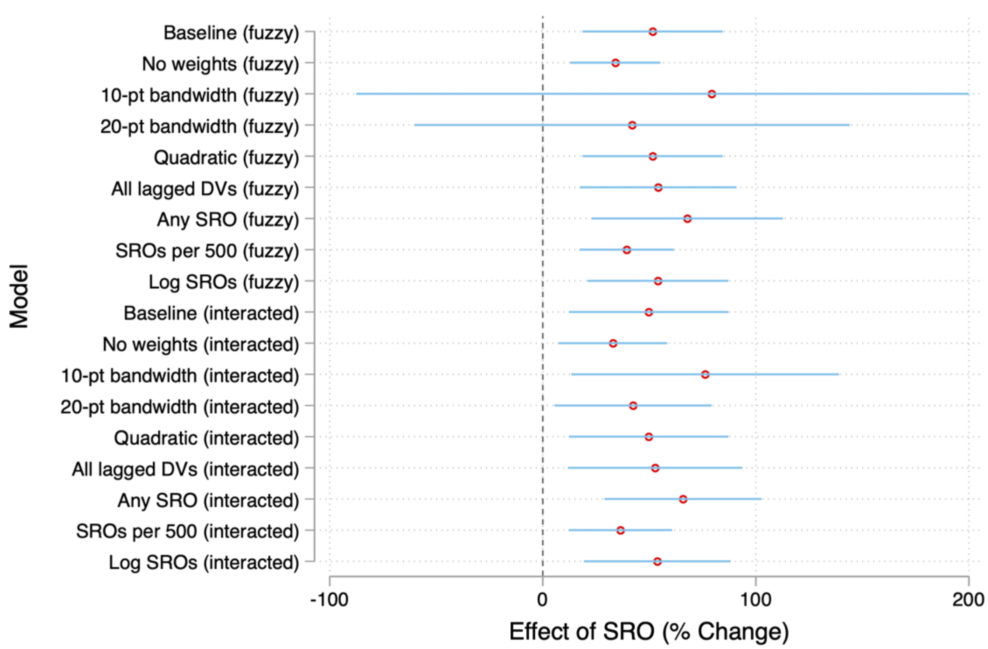
(d) Out-of-School Suspension



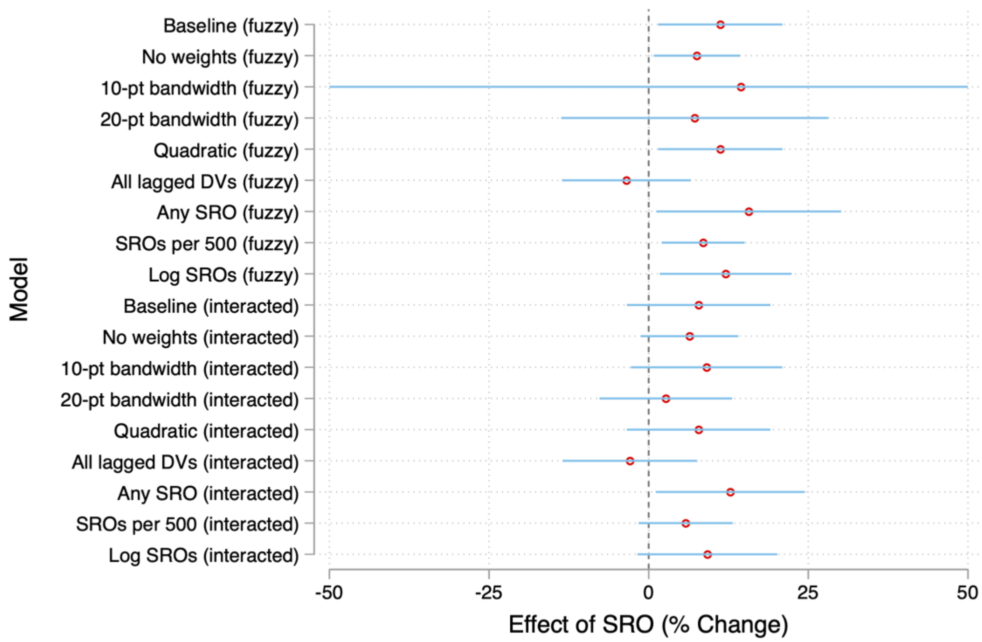
(e) Expulsion



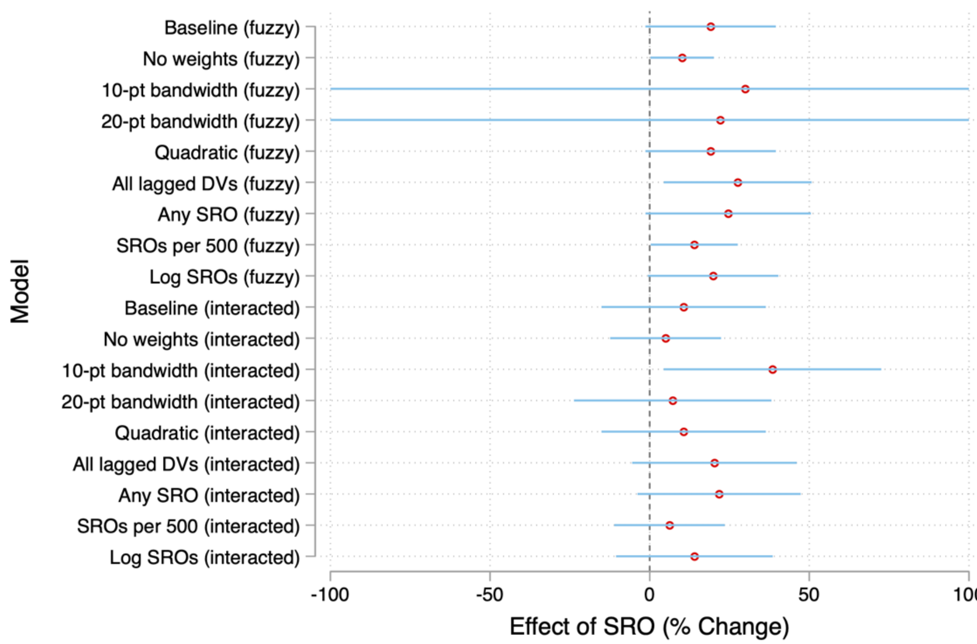
(f) Police Referral and Arrest



(g) Chronic Absence



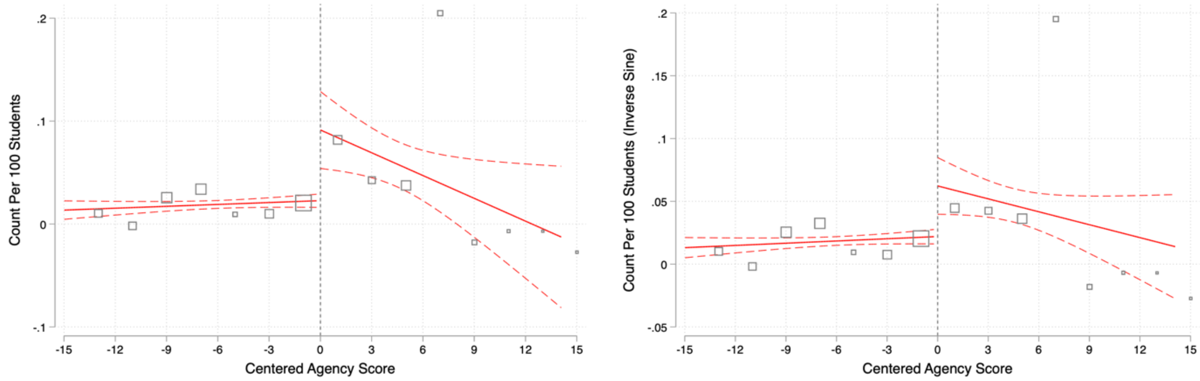
(h) Grade Retention



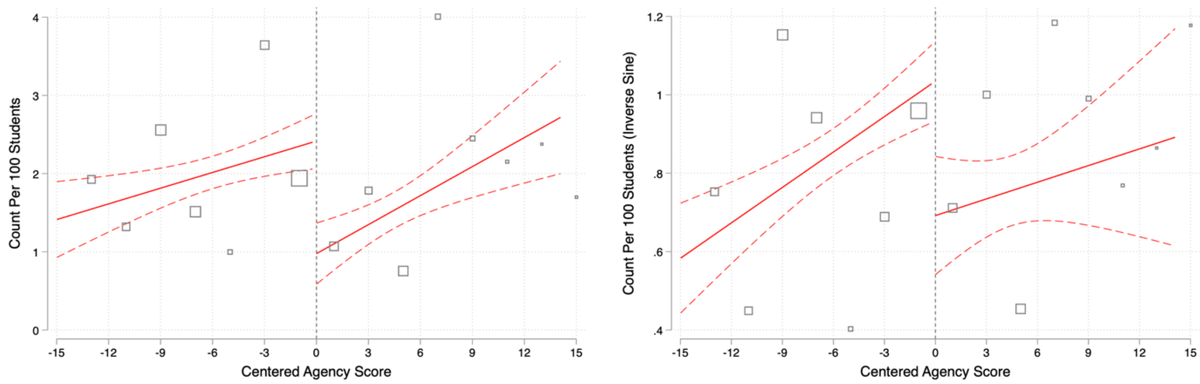
Notes: These figures present fuzzy and interacted fuzzy RD estimates from alternative specifications, including excluding analytical weights; changing the bandwidth restriction; adding a quadratic control of agency score; controlling for a vector of all lagged dependent variables; and replacing FTE SROs with an any SRO indicator, number of SROs per 500 students, or ln(FTE SROs+1). 95% confidence intervals constructed based on bootstrapped standard errors from 1,000 resamples, clustered by agency. Full control variables (including lagged dependent variables for the discipline and academic outcomes) included in all models.

Figure A5 (a)-(h). Sensitivity of SRO Effects to Alternative Model Specifications.

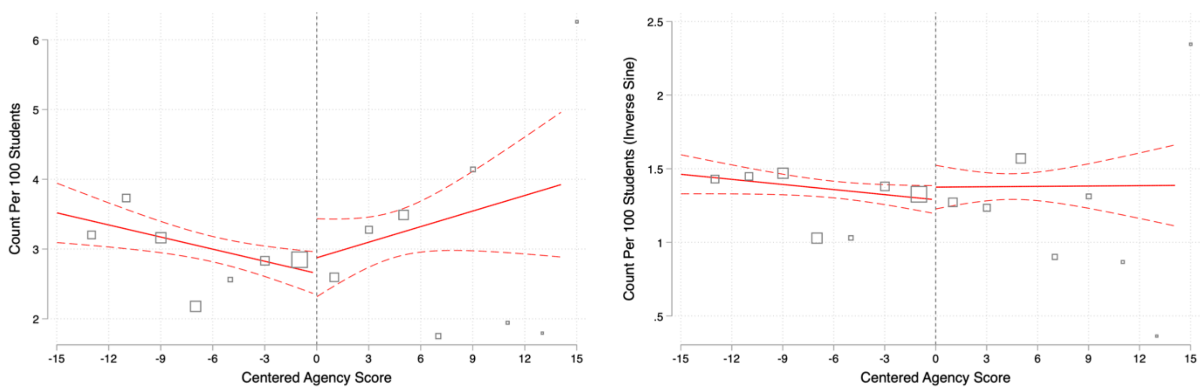
(a) Gun-Related Offenses



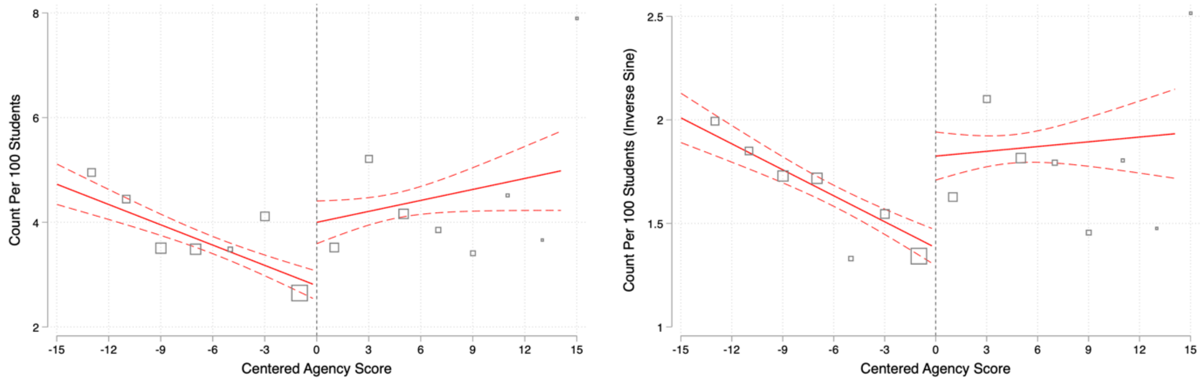
(b) Other Offenses



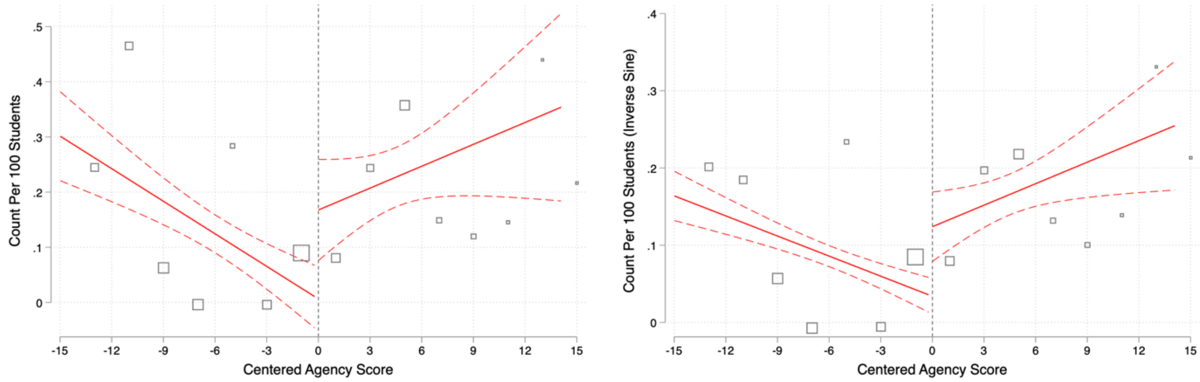
(c) In-School Suspension



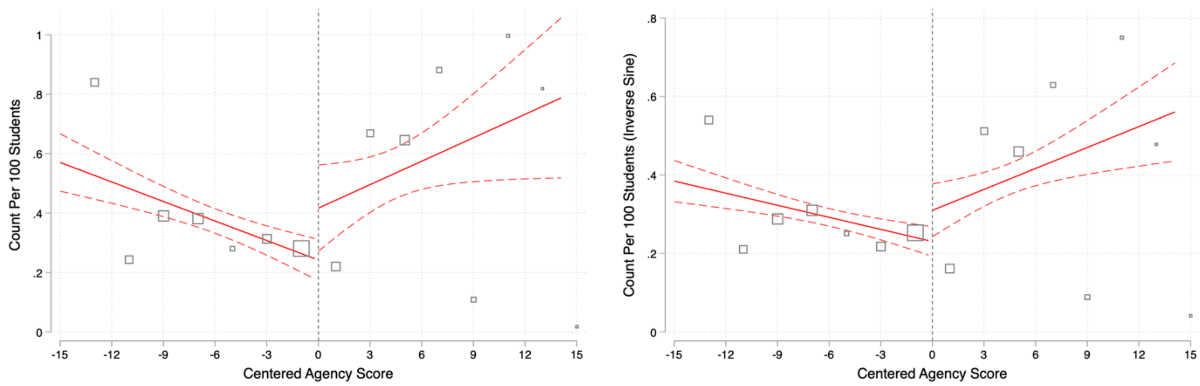
(d) Out-of-School Suspension



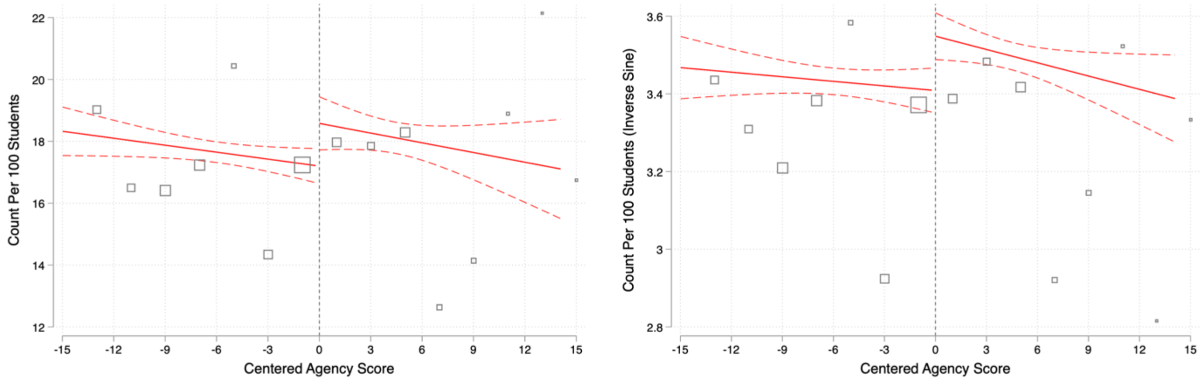
(e) Expulsion



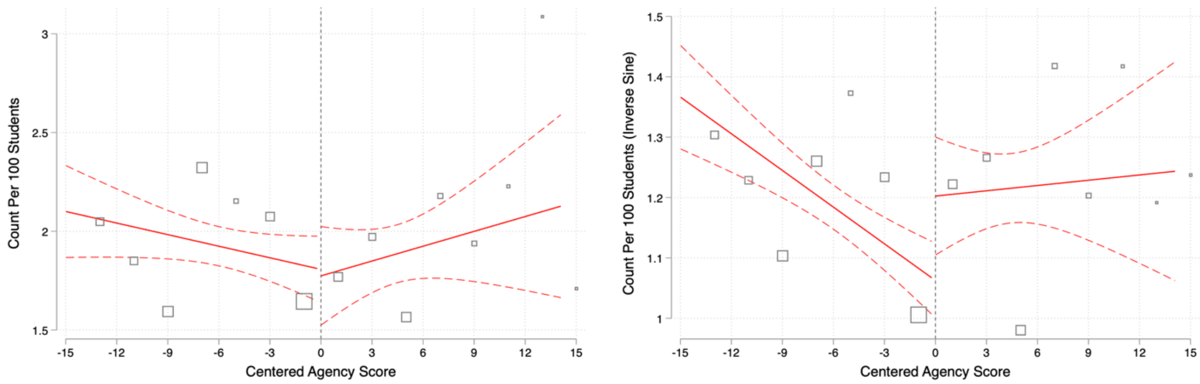
(f) Police Referral and Arrest



(g) Chronic Absence

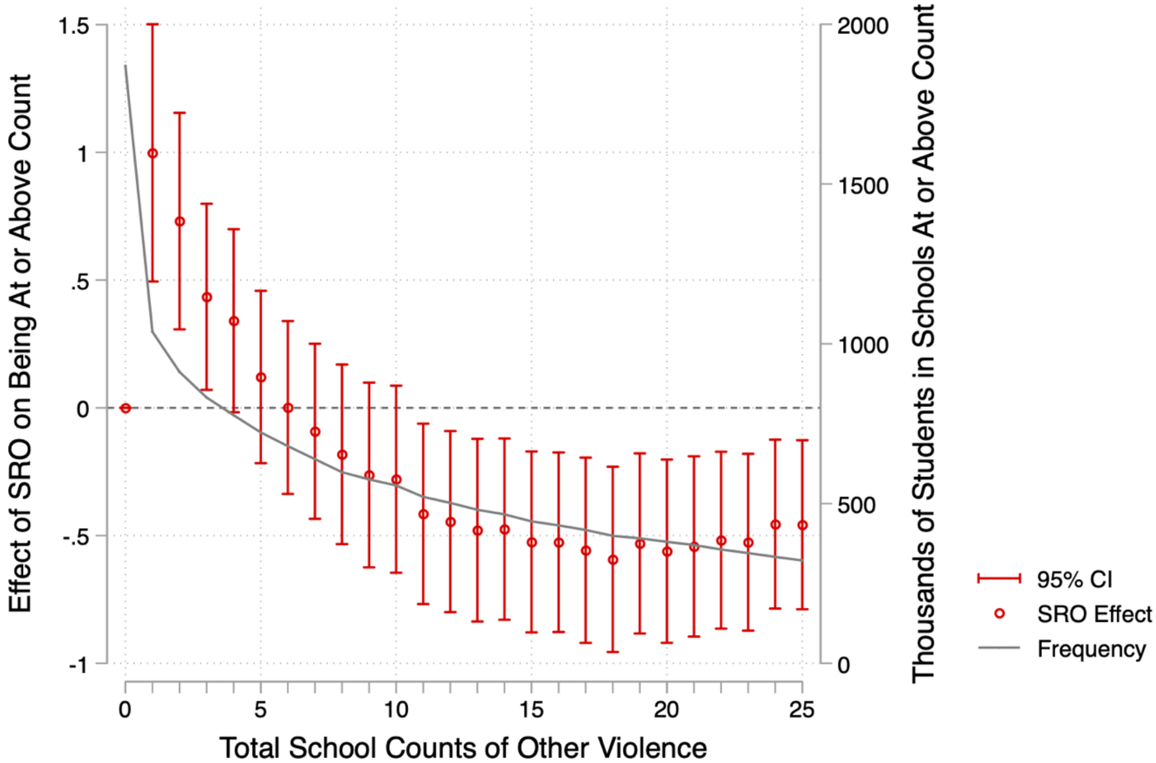


(h) Grade Retention



Notes: These graphs show binned means and linear plots of residualized outcome variables by centered agency application score below and above the school-based policing award cutoff. The left graph shows the untransformed outcome, and the right graph shows the inverse hyperbolic sine transformed outcome. These graphs restrict the dataset to schools within 15 points of the effective threshold. Binned means are constructed in 2-point increments, weighted by total enrollment within the bin, and dashed lines represent 95% confidence intervals of the linear plot.

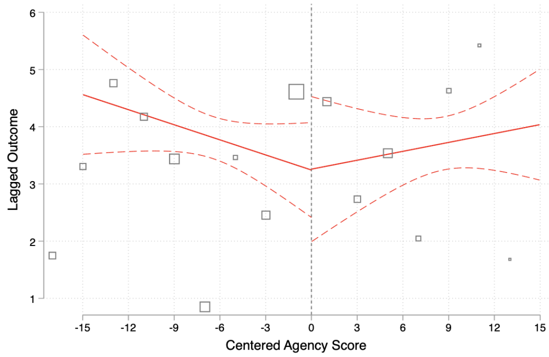
Figure A6 (a)-(h). Effects of School-Based Policing Award Cutoff on Student Outcomes (Reduced Form).



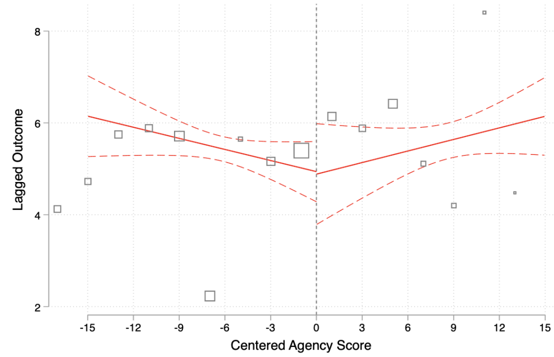
Notes: This figure plots on the left y-axis the estimated effects of an additional FTE SRO on an indicator of whether the school had equal to or greater than the number of reported violent incidents specified on the x-axis, using our preferred fuzzy RD approach with covariates. The right y-axis and grey connected line show the number of students in schools at each level of reported violence.

Figure A7. Effects of a FTE SRO on Reported Other Violence at the Extensive and Intensive Margins.

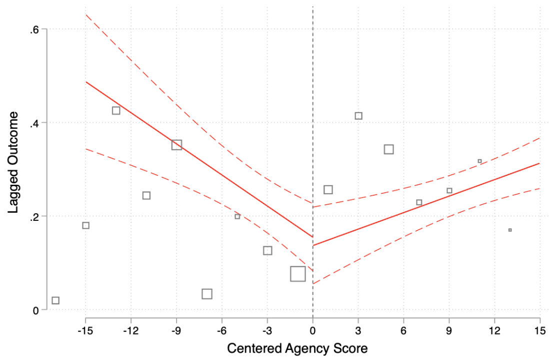
In-School Suspension



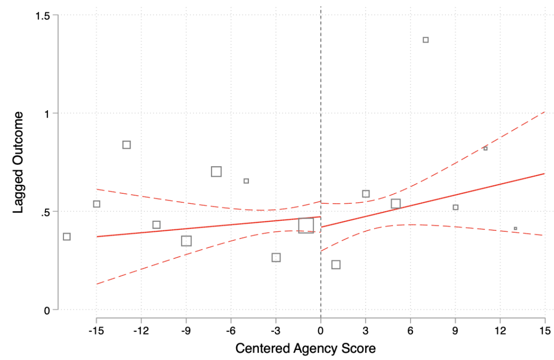
Out-of-School Suspension



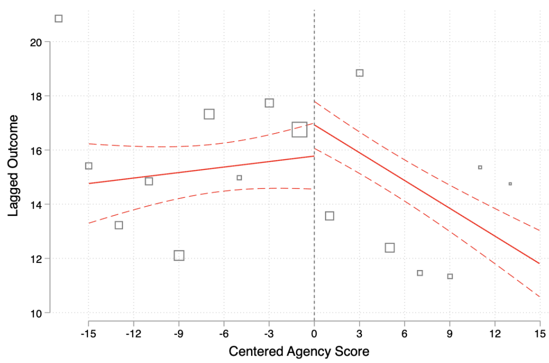
Expulsion



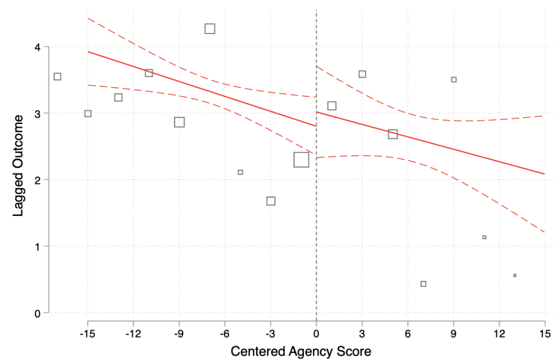
Police Referrals and Arrest



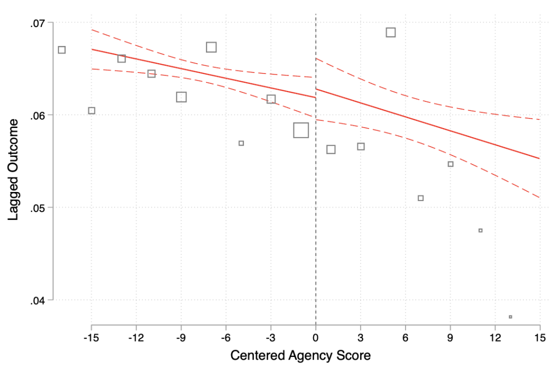
Chronic Absence



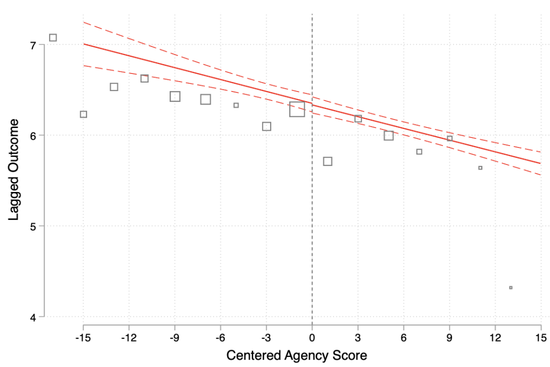
Grade Retention



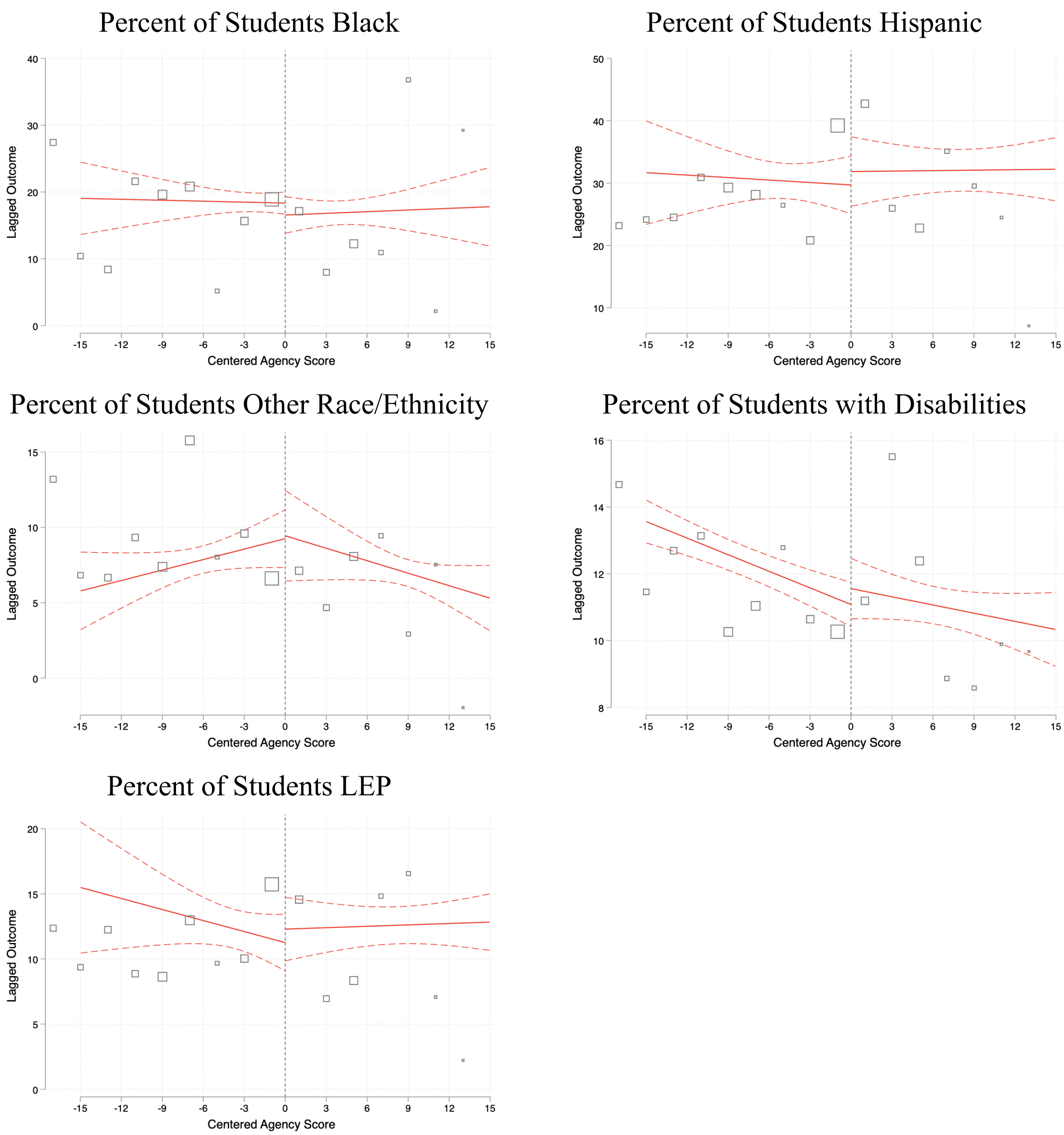
Teachers Per Pupil



Log Student Enrollment



t



Notes: These graphs show binned means and linear plots of baseline variables by centered agency application score below and above the school-based policing award cutoff, restricting to schools within a 15 point bandwidth. Binned means are constructed in 2-point increments, weighted by total enrollment, and dashed lines represent 95% confidence intervals of the linear plot.

Figure A8. Reverse Causality Test: Effects of 2015-2017 School-Based Policing Award Cutoffs on Baseline Measures in 2014.

Table A1. Comparison of full CRDC survey and analytical sample in 2018.

	Full CRDC (n=94,918)		Analytical Sample (n=3,433)	
	Mean	SD	Mean	SD
Outcomes (per 100 students)				
Gun-related offenses	0.047	(0.513)	0.034	(0.303)
Other offenses	2.544	(10.471)	2.417	(6.636)
Students with any ISS	4.398	(8.824)	3.014	(6.022)
Students with any OSS	5.163	(10.929)	4.470	(7.114)
Students with expulsion	0.265	(3.813)	0.156	(0.915)
Students with arrest/referral	0.575	(3.657)	0.354	(1.864)
Students chronically absent	15.787	(13.564)	17.635	(13.215)
Students retained a grade	3.325	(11.425)	2.200	(4.496)
School policing				
FTE sworn police officers	0.233	(1.843)	0.181	(0.444)
Any sworn police officer	0.245	(0.430)	0.222	(0.415)
School characteristics				
Elementary school	0.439	(0.496)	0.520	(0.500)
Middle school	0.155	(0.362)	0.157	(0.364)
High school	0.177	(0.381)	0.165	(0.371)
Other grade configuration	0.230	(0.421)	0.159	(0.366)
Alternative school	0.035	(0.184)	0.023	(0.150)
Charter school	0.074	(0.262)	0.056	(0.229)
Magnet school	0.493	(0.500)	0.446	(0.498)
Special education school	0.023	(0.148)	0.008	(0.087)
Urban location	0.269	(0.444)	0.301	(0.459)
Suburban location	0.323	(0.468)	0.463	(0.499)
Town location	0.133	(0.340)	0.104	(0.305)
Rural location	0.274	(0.446)	0.131	(0.338)
South region	0.349	(0.477)	0.502	(0.500)
Midwest region	0.261	(0.439)	0.138	(0.345)
Northeast region	0.140	(0.347)	0.134	(0.341)
West region	0.240	(0.427)	0.225	(0.418)
Student enrollment	522.636	(448.339)	612.087	(450.823)
Number of teachers per pupil	0.069	(0.027)	0.069	(0.027)
Proportion of students White	52.220	(32.904)	38.649	(32.679)
Proportion of students Black	14.944	(23.437)	19.654	(27.199)
Proportion of students Hispanic	23.132	(26.716)	33.068	(31.564)
Proportion of students other	9.704	(13.025)	8.629	(9.934)
Proportion of students LEP	9.525	(14.693)	12.477	(15.339)
Proportion of students IDEA	15.288	(14.238)	13.984	(10.105)
FTE security guards	2.627	(383.706)	0.395	(1.243)
FTE psychologists	0.544	(61.834)	0.336	(0.578)
FTE social workers	0.276	(0.917)	0.267	(0.805)
FTE nurses	0.573	(1.188)	0.460	(0.573)

Notes: The CRDC sample shown here is from the 2018 data collection and has all the same sample restrictions as our main analysis (e.g. the student enrollment minimum) except for requiring a match to an agency applicant for a school-based policing grant in 2015-2017.

Table A2. Reduced form effects of school-based policing awards, by school level.

Specification	(2) Gun Offenses	(3) Other Offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
All Schools (n=2,857)								
Semi-elasticity	0.489	-0.074**	0.060*	0.147**	0.227**	0.128**	0.030**	0.045*
(SE)	(0.136)	(0.022)	(0.024)	(0.022)	(0.045)	(0.035)	(0.011)	(0.020)
Count	0.015	-0.143	0.192	0.573	0.036	0.052	0.525	0.085
Mean	0.030	1.935	3.180	3.906	0.160	0.404	17.56	1.882
Elementary Schools (n=1,522)								
Semi-elasticity	0.233**	-0.036**	0.014*	0.021**	0.025	0.049**	0.001	0.005
(SE)	(0.070)	(0.008)	(0.006)	(0.006)	(0.037)	(0.014)	(0.003)	(0.003)
Count	0.002	-0.044	0.019	0.041	0.001	0.004	0.016	0.009
Mean	0.008	1.238	1.328	1.921	0.020	0.085	13.61	1.595
Middle Schools (n=448)								
Semi-elasticity	0.701**	0.088	0.108+	0.241**	0.299**	0.008	0.004	0.034
(SE)	(0.223)	(0.061)	(0.066)	(0.058)	(0.081)	(0.050)	(0.022)	(0.041)
Count	0.040	0.248	0.625	1.447	0.081	0.005	0.071	0.019
Mean	0.057	2.825	5.764	5.993	0.270	0.653	16.57	0.551
High Schools (n=454)								
Semi-elasticity	1.256+	0.073	0.066	0.343**	0.628**	0.448**	0.091	0.068
(SE)	(0.696)	(0.135)	(0.180)	(0.133)	(0.173)	(0.167)	(0.064)	(0.133)
Count	0.058	0.107	0.332	1.756	0.219	0.380	2.250	0.209
Mean	0.046	1.478	5.057	5.125	0.348	0.847	24.66	3.052

**p<0.01; *p<0.05; +p<0.1.

Notes: Bootstrapped standard errors from 1,000 resamples in parentheses, clustered by law enforcement agency. All models use the discontinuity as an instrumental variable and control for centered agency score and centered agency score interacted with the discontinuity. Bandwidth is restricted to 15 points on both side of the discontinuity. For the full controls estimates, models also include the following controls: school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students Limited English Proficiency, and the lagged dependent variable when available (all outcomes except for gun-related offenses and other offenses).

Table A3. Unweighted effects of award cutoff on FTE SROs (first stage).

	Fuzzy RD		Interacted RD	
Bandwidth	(1)	(2)	(3)	(4)
[-20, 20]	0.148**	0.136**	0.587**	0.445*
	(0.037)	(0.034)	(0.183)	(0.176)
	F=15.6	F=15.5	F=10.3	F=6.4
	n=3,433	n=3,433	n=3,433	n=3,433
[-15, 15]	0.189**	0.200**	0.631**	0.570**
	(0.027)	(0.033)	(0.166)	(0.172)
	F=49.1	F=37.5	F=14.4	F=11.0
	n=3,005	n=3,005	n=3,005	n=3,005
[-10, 10]	0.128**	0.118**	0.274	0.201
	(0.030)	(0.034)	(0.187)	(0.203)
	F=18.2	F=11.9	F=2.1	F=1.0
	n=2,490	n=2,490	n=2,490	n=2,490
Covariates	No	Yes	No	Yes

** p<0.01; * p<0.05; + p<0..1

Notes: These first stage regressions are not weighted by student enrollment. All fuzzy RD models present the estimated coefficient on the discontinuity and control for centered agency score and centered agency score interacted with the discontinuity. All interacted RD models show the linear combination of the estimated coefficient on the discontinuity and the estimated coefficient on the discontinuity interacted with E(SRO|D=1) from models controlling for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. n is the number of schools in the analysis. Standard errors in parentheses are constructed from 1,000 bootstrapped samples, clustered by agency. Columns 2 and 4 include the full set of control variables, including school level indicators, school locale indicators, region indicators, agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity, percent of students with disabilities under IDEA, percent of students LEP.

Table A4. Unweighted effects of FTE SROs on student outcomes (2SLS).

	Offense Outcomes		Discipline Outcomes				Academic Outcomes	
	(2) Gun offenses	(3) Other offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
Panel 1 Basic Controls (n=3,005)								
Fuzzy RD								
Semi-elasticity	1.190**	-0.188*	0.354**	0.398**	0.181	0.085	-0.119*	-0.162**
(SE)	(0.413)	(0.075)	(0.084)	(0.084)	(0.116)	(0.096)	(0.047)	(0.063)
Count	0.036	-0.364	1.126	1.554	0.029	0.034	-2.093	-0.305
Interacted RD								
Semi-elasticity	1.156**	-0.251**	0.189	0.236+	0.113	0.082	-0.100*	-0.126+
(SE)	(0.426)	(0.097)	(0.137)	(0.132)	(0.131)	(0.108)	(0.043)	(0.067)
Count	0.035	-0.486	0.601	0.922	0.018	0.033	-1.759	-0.237
Panel 2 Full Controls (n=2,857)								
Fuzzy RD								
Semi-elasticity	1.482**	-0.381**	0.221**	0.402**	0.545**	0.341**	0.075*	0.102*
(SE)	(0.431)	(0.089)	(0.076)	(0.088)	(0.142)	(0.108)	(0.034)	(0.051)
Count	0.045	-0.737	0.703	1.570	0.087	0.138	1.319	0.192
Interacted RD								
Semi-elasticity	1.519**	-0.406**	0.124	0.290*	0.381+	0.329*	0.064	0.050
(SE)	(0.468)	(0.101)	(0.117)	(0.139)	(0.202)	(0.130)	(0.039)	(0.089)
Count	0.046	-0.785	0.394	1.133	0.061	0.133	1.126	0.094
Mean	0.030	1.935	3.180	3.906	0.160	0.404	17.587	1.882

**p<0.01; *p<0.05; +p<0.1.

Notes: Regressions not weighted by student enrollment. Outcomes are per 100 students, transformed by the inverse hyperbolic sine. Semi-elasticity is calculated as in Bellemare and Wichman (2020). Standard errors are from 1,000 bootstrapped resamples, clustered by agency. “Count” provides the estimated effect in incidents per 100 students. n is the number of schools in the analysis. Bandwidth is restricted to 15 points on both sides of the discontinuity. Fuzzy RD models use the discontinuity as an instrument and control for centered agency score and centered agency score interacted with the discontinuity. Interacted RD models use the discontinuity and E(SRO|D=1) interacted with the discontinuity as instruments and control for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. For full controls estimates, models also include school level indicators, school locale indicators, region indicators, agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity, percent of students with disabilities, percent of students Limited English Proficiency, and the lagged dependent variable when available (all outcomes except for offense outcomes).

Table A5. Effects of FTE SROs on raw counts of student outcomes (2SLS).

	Offense Outcomes		Discipline Outcomes				Academic Outcomes	
	(2) Gun offenses	(3) Other offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
Panel 1 Basic Controls (n=3,005)								
Fuzzy RD	0.305*	-5.584**	0.879	5.658*	0.575*	0.522	-12.411*	-1.599
	(0.151)	(1.868)	(2.145)	(2.332)	(0.224)	(0.348)	(5.254)	(1.198)
Interacted RD	0.280*	-4.523**	-1.467	3.770+	0.372*	0.424	-8.495+	-1.135
	(0.133)	(1.687)	(2.645)	(2.290)	(0.184)	(0.445)	(4.496)	(0.961)
Panel 2 Full Controls (n=2,857)								
Fuzzy RD	0.364*	-8.035**	1.192	5.494**	0.987**	1.550**	8.703**	-0.976
	(0.166)	(2.361)	(1.943)	(2.054)	(0.309)	(0.498)	(3.360)	(1.180)
Interacted RD	0.349*	-6.965**	-0.493	4.096+	0.650+	1.181+	6.348+	-1.185
	(0.155)	(2.178)	(2.288)	(2.224)	(0.333)	(0.639)	(3.785)	(1.453)

**p<0.01; *p<0.05; +p<0.1.

Notes: Outcome measures are expressed as the raw counts of the outcome per 100 students. Bootstrapped standard errors from 1,000 resamples in parentheses, clustered by law enforcement agency. All fuzzy RD models use the discontinuity as an instrumental variable and control for centered agency score and centered agency score interacted with the discontinuity. All interacted RD models use the discontinuity and E(SRO|D=1) interacted with the discontinuity as instrumental variables and control for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. Bandwidth is restricted to 15 points on both side of the discontinuity. n is the number of schools in the analysis. For the full controls estimates, models also include the following controls: school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students Limited English Proficiency, and the lagged dependent variable when available (all outcomes except for gun-related offenses and other offenses).

Table A6. Effects of FTE SROs on logged student outcomes (2SLS).

	Offense Outcomes		Discipline Outcomes				Academic Outcomes	
	(2) Gun offenses	(3) Other offenses	(4) ISS	(5) OSS	(6) Expulsion	(7) Referral or Arrest	(8) Chronic Absence	(9) Grade Retention
Panel 1 Basic Controls (n=3,005)								
Fuzzy RD	0.319	2.148**	2.556**	5.890**	0.481+	0.102	-0.633*	0.205
	(0.195)	(0.691)	(0.856)	(1.589)	(0.275)	(0.435)	(0.305)	(0.427)
Interacted RD	0.417*	0.964	1.113	3.416*	0.366	0.481	-0.440+	-0.065
	(0.194)	(0.974)	(1.010)	(1.514)	(0.263)	(0.391)	(0.241)	(0.386)
Panel 2 Full Controls (n=2,857)								
Fuzzy RD	0.447+	0.019	1.813*	4.285**	1.265**	1.456**	0.436*	0.945*
	(0.240)	(0.554)	(0.755)	(1.232)	(0.395)	(0.507)	(0.190)	(0.463)
Interacted RD	0.532*	-0.547	0.860	2.554+	0.812+	1.456**	0.305	0.527
	(0.248)	(0.765)	(0.911)	(1.407)	(0.462)	(0.554)	(0.219)	(0.559)

**p<0.01; *p<0.05; +p<0.1.

Notes: Outcome measures are expressed as the natural log of the outcome count plus one. Bootstrapped standard errors from 1,000 resamples in parentheses, clustered by law enforcement agency. All fuzzy RD models use the discontinuity as an instrumental variable and control for centered agency score and centered agency score interacted with the discontinuity. All interacted RD models use the discontinuity and E(SRO|D=1) interacted with the discontinuity as instrumental variables and control for centered agency score, E(SRO|D=1), the discontinuity interacted with centered agency score, E(SRO|D) interacted with centered agency score, and E(SRO|D=1) interacted with centered agency score and the discontinuity. Bandwidth is restricted to 15 points on both side of the discontinuity. n is the number of schools in the analysis. For the full controls estimates, models also include the following controls: school level indicators (elementary/middle/high/other), school locale indicators (urban/suburb/town/rural), region indicators (South, Midwest, Northeast, West) agency size, agency community policing score, agency crime score, agency fiscal need score, log student enrollment, number of teachers per pupil, percent of students by race/ethnicity (Black, Hispanic, other), percent of students with disabilities under IDEA, percent of students Limited English Proficiency, and the lagged dependent variable when available (all outcomes except for gun-related offenses and other offenses).

APPENDIX B. QUALITATIVE THEMES IN SCHOOL-BASED POLICING GRANT APPLICATION TEXT

In addition to identifying the schools to which the SROs would be assigned, the CHP applications also provided written justifications for the requested funds in two required fields that described the problem area and detailed the need for federal assistance. One of the biggest categories of winning requests were requests for funds that would enable the agency to maintain an officer who has been, or will be cut shortly due to budget shortfalls, or restaff a position that had been lost due to budget cuts. The impact of the Great Recession on municipal and school budgets was a dominant theme. For example, one agency stated that "we will use these funds to continue to employ a school resource (officer) which is scheduled to be laid off in October 2016 due to budget cuts."

There were also a number of applications from agencies that already had active SRO programs and were seeking to expand them, either by adding additional officers to schools that already had officers, or to an additional school, often elementary or middle schools. These applications tended to be very positive about their current program. For example, one agency stated that it needed to "hire two officers to be deployed into our two elementary schools. Our department has had great success with officers in the ….XXXX high school and the …..XXXX middle school."

The final category of successful applications were from agencies trying to hire their first ever School Resource Officer. Some of these agencies saw this as a way to deal with the often-growing number of calls for service from the schools that would otherwise need to be handled reactively by officers on patrol. One application stated, that "it is our hopes that many of these incidents can be resolved in a timely manner and not have to impact our current patrol needs." The word "proactive" was used 25 times in the Problem Area descriptions for the 74 applications that were awarded funds. Other times, the request for a new SRO program was accompanied by more general language about the need to create a community policing presence in this school, often with a laundry list of activities that included the prevention of alcohol and drug use as well as gang activity, with brand name programs such as D.A.R.E and G.R.E.A.T. Six different applications appealed to President Obama's final report on 21st Century Policing (2015), which describes the need for community policing efforts within schools.

Relatively few applications focused on the need for a police presence to prevent school shootings, and no application used the need to protect schools against the possibility of school shootings as the sole justification for the SRO. Typical of applications that mentioned school shootings was an application from a rural agency with longer than standard response times which stated that "a large number of students and staff could be killed or injured in the length of time needed for just the patrol response." The agency then immediately moved onto to describe the problems the schools were facing from drugs and alcohol use.

APPENDIX C. REPLICATION 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 replicate 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 multiplied by 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 $s_{1it}, \ldots, s_{nit}$.

3. We assign an award to the top scoring agency, and subtract the amount of money requested by the agency 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:

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

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.

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.[26]

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:

[26] There were two states not competitive in 2015 (Vermont and Wyoming), three states in 2016 (Alaska, West Virginia, and Wyoming), and two states in 2017 (Nevada and Wyoming).

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 $(s_{1st}, \ldots, s_{nst})$ and the remaining large agencies without awards in descending score order $(s_{1lt}, \ldots, s_{nlt})$.
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 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:

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

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

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.[27]

Cutoff determination

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

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

$C_{jit}^{*} = min\left(C_{it}^{state}, C_{t}^{large}\right)$ if agency j 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.

[27] 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.

APPENDIX D. DETAILS ON INTERACTED RD METHOD

The first and second stage equations for the interacted RD method for school j linked to agency k are as follows:

$$(1)\qquad SRO_{jk} = \alpha_0 + \alpha_1 I(Score_k > 0) + \alpha_2 E(SRO|Score_k > 0) \times I(Score_k > 0)$$
$$+\alpha_3\, Score_k + \alpha_4 E(SRO|Score_k > 0)$$
$$+\alpha_5 I(Score_k > 0) \times\; Score_k + \alpha_6\, Score_k \times E(SRO|Score_k > 0)$$
$$+\alpha_7\, Score_k \times E(SRO|Score_k > 0) \times I(Score_k > 0)$$
$$+\mu_{jk}\; if\; Score_k \in [-b, b]$$

$$(2)\qquad Y_{jk} = \gamma_0 + \gamma_1 \widehat{SRO_{jk}}$$
$$+\gamma_3 Score_k + \gamma_4 E(SRO|Score_k > 0)$$
$$+\gamma_5 I(Score_k > 0) \times\; Score_k + \gamma_6\, Score_k \times E(SRO|Score_k > 0)$$
$$+\gamma_7\, Score_k \times E(SRO|Score_k > 0) \times I(Score_k > 0)$$
$$+\rho_{jk}\; if\; Score_k \in [-b, b]$$

The interacted model is over-identified, with both terms in the first line of (1) instrumenting for the predicted number of SROs. It incorporates information about heterogeneous first stage treatment effects at the discontinuity – an award will have a bigger impact at schools where it will lead to a larger increase in SROs, which results from the agency requesting a larger number of SROs for the school.

The interacted model differs from the fuzzy model in that it uses information from each application about the number of SROs that the agency has requested to place with each school, should it win the award. We use $E(SRO|Score_k > 0)$ to represent the requested number. We anticipate that α_1, the coefficient on the discontinuity, will be zero because this reflects the extrapolation to an award winner that requests NO additional SROs. We anticipate that α_2, the coefficient on the interaction of the discontinuity with the promised number of SROs, will be one. That is, we expect the number of SROs at schools that win awards to exceed the number at comparable schools that do not win awards by an amount equal to the requested number of SROs. If award winners deviate from their requested numbers, the estimates of these two coefficients will provide information about this deviation.

In some cases, the requests in the applications are very specific and the value of $E(SRO|Score_k > 0)$ reflects that specificity. For example, if an agency states that they will place two additional SROs in Maple High if they win the award and does not mention any other schools in the application, then the value of $E(SRO|Score_k > 0)$ for Maple High is 2. All other schools in Maple High's district are omitted from the analysis.

In other cases, the requests in the applications are vague, although we assume that they provide an unbiased expectation for each school. For example, an agency might state that they will hire an additional SRO for high schools in Lincoln District. If Lincoln District has three high schools, we set $E(SRO|Score_k > 0)$ to 1/3 for each high school in Lincoln District and include all three high schools in the analysis. This is an unbiased estimate of the increase in SROs for these high schools, whether the additional SRO splits their time among the three high schools or spends all their time in one of the three high schools.

Of course, the requests in the application are not exogenous. Therefore, like the RD running variable, we enter the request as a covariate, allowing the slope to differ on each side of the discontinuity. Only the jump at the discontinuity is used to instrument for the increase in the number of SROs per school resulting from an award.

The interacted RD is motivated by the desire to get a better predictor of the treatment and is related to the work by Coussens and Spiess (2021). Coussens and Spiess show that the optimal instrument is one that provides the best predictor of the endogenous variable. In the present case, the endogenous variable of interest is the average number of additional SROs brought about by the awards at the score discontinuity. Coussens and Spiess also show that interacting the exogenous instrumental variable, which in our case is the $I(Score_k > 0)$ indicator, with possibly endogenous covariates such as the requested number of SROs yields a consistent estimator of the treatment effect and improves precision over an IV estimator without the interaction. Furthermore, they show that the interacted estimator is equivalent to weighting the local average treatment effect by the covariate used in the interaction. In their case, they interact with a measure of compliance to the exogenous treatment. In our case, we interact by the requested number of SROs. Therefore, we obtain a "super local average treatment effect" (SLATE), which gives greater weight to schools for which more SROs are requested.[28]

APPENDIX D REFERENCES

Caetano, C., Caetano, G., & Escanciano, J. C. (2021). Regression Discontinuity Design with Multivalued Treatments. *arXiv preprint arXiv:2007.00185*.

Coussens, S., & Spiess, J. (2021). Improving Inference from Simple Instruments through Compliance Estimation. *arXiv preprint arXiv:2108.03726*.

[28] This work is also related to Caetano, Caetano & Escanciano (2021), which examines interactions in the context of RD models.