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**Final Summary Overview**

# Investigator-Initiated Research: The Comprehensive School Safety Initiative Study of Police in Schools

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This summary is based on two longer reports, which are available from the first author:

Gottfredson, D.C., Crosse, S., Tang, Z., Bauer, E.L., Harmon, M.A., Hagen, C.A., & Greene, A.D. (2020). The Effects of School Resource Officers on School Crime and Responses to School Crime. Manuscript submitted for publication.

Gottfredson, D.C., Crosse, S., Bauer, E.L., Tang, Z., Harmon, M.A., Hagen, C.A., Greene, A.D. (2020). Are Effects of School Resource Officers Moderated by School and Student Characteristics? Manuscript submitted for publication.

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Concern over the safety of students, teachers, and administrators in our nation's schools continues to grow. In part, this is due to the widespread media coverage of mass school shootings, such as the events at Columbine High School in 1999, Sandy Hook Elementary School in 2012, and Stoneman Douglas High School in 2018. Many initiatives have been established in the recent past to enhance school safety and ensure fair, non-discriminatory, and effective discipline policies and practices (U.S. Department of Education, 2014). These initiatives often involve partnerships between schools and law enforcement agencies to increase school security, including stationing police officers in schools.

The practice of placing police officers in schools has sky-rocketed over the past 40 years. Data from the Safe School Study, a national study of school violence conducted in 1976 by Research Triangle Institute for the National Institute of Education (NIE), showed that only 1% of the nation's schools had police stationed in them (NIE, 1978). By 2016, 48% of schools had sworn law enforcement officers present in the school at least once per week, and the percentage was much higher (65%) for secondary schools (Musu-Gillette et al., 2018). School resource officers (SROs), who receive specialized training for their roles as school-based police officers, account for most of the law enforcement presence in schools. Forty-two percent of all public schools reported having an SRO present at least once a week during the 2015-16 school year. Secondary schools (58%) were more likely than primary schools (30%) to report having one or more SRO present at least once a week, and schools with larger enrollments were more likely to report the presence of SROs. For example, 84 percent of secondary schools with enrollments of 1,000 or more reported SRO presence (Musu-Gillette et al., 2018).

The widespread adoption of SROs has been driven primarily by increased federal funding (James & McCallion, 2013; Na & Gottfredson, 2013). The U.S. Department of Justice Office of Community Policing Services (COPS) initiated the "COPS in Schools" grant program, which awarded a total of $753 million to 3,000 grantees to hire 6,500 SROs between 1999 and 2005. Since 2005, law enforcement agencies could apply for grants to fund SROs under the broader COPS Hiring Program (CHP) (James & McCallion, 2013).

According to the National Association of School Resource Officers (NASRO), SROs have three main roles: educator, informal counselor, and law enforcer (Canady, James, & Nease, 2012). Reviews of the extent to which SRO duties reflect this model suggest that there is great variation across schools and districts in terms of the actual roles and responsibilities taken on by SROs (Nolan, 2018). Many districts do not have formal agreements about the roles of SROs, increasing the potential for conflict and confusion. In particular,

there has been much concern about lack of clear boundaries around what types of discipline matters should be handled by law enforcement officers and by school personnel. Questions have also been raised about the potential for role conflict when police officers engage in dual roles as counselor/teacher and law enforcer (Devlin & Gottfredson, 2018). Partly in response to these concerns, as well as concerns that intensification of school discipline disproportionately affects minority youth and youth with disabilities, many school districts have recently begun to place limits on SRO discretion (Hirschfield, 2018). However, decisions about whether and how to utilize SROs are most often made in the absence of rigorous empirical evidence.

Reviews of the early empirical literature on the effects of placing SROs in schools concluded that little is known about the effects of placing SROs in schools on the levels of crime, disorder, or responses to these outcomes experienced by the schools in which these officers are placed. Our review of more recent studies concluded that SRO presence increases the recording of drug crimes, crimes involving weapons, and serious violent crimes as well as the severity of responses to these crimes. Very little research has tested moderating effects of SRO placement. Despite increased rigor in the more recent studies compared with earlier research, these studies still leave significant questions about the effect of placing SROs on school crime and responses to school crime. More longitudinal studies are especially needed.

This study assesses the effects of SROs in schools using a longitudinal design. The research questions addressed in this study are: (1) What are the effects of SROs on school disciplinary offenses and disciplinary actions? and (2) Do the effects of SROs vary by student characteristics, SRO approach and dose, or community and school characteristics?

## Methods

We gathered and analyzed data on public middle and high schools in California[3] that increased SRO staffing levels at a specific time (treatment schools) and on a set of matched schools that did not increase

[3] We also planned to include Florida schools in the study. Although we initiated data collection activities with Florida schools, we ultimately excluded these schools for the following reason: Although the research team, following the research protocol, selected treatment schools that increased their SRO dosages and excluded any potential comparison schools that also increased their dosage at the same time as the treatment schools, the team later discovered that the Florida treatment and comparison schools were similar in their number of SRO hours per week after the intervention point. The research team observed no significant difference in SRO dose between treatment and comparison schools after the intervention point (39.3 vs. 36.7 SRO hours per week; 4.5 vs. 4.9 SRO hours per week per 100 students). Because the two sets of schools did not differ in SRO usage, any treatment vs. comparison differences on outcome measures could not be interpreted as due to the addition of SROs. The higher than anticipated level of SRO usage in the comparison schools is likely due to the widespread use of SROs in Florida schools generally—the research team was unable to locate comparison schools that were not already using SROs at high dosage levels.

SRO staffing levels at the same time as the treatment schools (comparison schools). The increases in SRO staffing levels at the treatment schools resulted from the award of CHP grants to local law enforcement agencies in 2013 or 2014. The grants were intended to support the placement of SROs in schools. We focused on schools in California because law enforcement agencies in that state received a large number of CHP grants, and the state had administrative data on schools that could support the analyses planned.

We used two approaches that primarily relied on monthly administrative data on outcomes for assessing the effects of increased SRO staffing levels or "intervention" on schools and students. First, we examined longer-term effects of increased SRO staffing levels at 11 and 20 months post-intervention using aggregated monthly data in a pre- and post-test comparison group design. The comparison group was selected through a matching procedure that created sets of schools that were comparable in terms of prior disciplinary actions and demographic characteristics. Although this design achieved matching on several important observed characteristics, it cannot by itself rule out the possibility that the schools that increased SRO staffing levels as a result of the CHP grants differed in unmeasured ways that might influence the study outcomes, preventing the unambiguous attribution of observed effects solely to the intervention.

To address that limitation, we used monthly data in an interrupted time series design (Cook, Shadish, & Wong, 2008; Shadish; Cook, & Campbell, 2002) with a comparison series to examine whether study outcomes changed in the 2 or 3 month period coinciding with the intervention. Comparative time series designs produce unbiased estimates of treatment effects and have been recognized as one of the most rigorous research designs, especially for studying the effects of broad policy changes (Gottfredson, Cook, Gardner, Gorman-Smith, Howe, Sandler, & Zafft, 2015).

**Sample**

Using administrative data from the Common Core of Data (CCD) and information gathered from CHP grantees in California, we applied the following criteria to identify the sample of treatment schools: (1) highest grade was greater than 6, (2) SRO staffing level was at least 8 hours per week, and (3) SRO hours increased as result of the 2013 or 2014 CHP funding. Thirty-three schools met these criteria.

We selected one or more comparison schools for each treatment school. To ensure the treatment and comparison schools were as equivalent as possible prior to an increase in SRO staffing level in the treatment schools, we used data from the CCD and the California Department of Education (CDE) to match

schools on the following characteristics: (1) instructional level; (2) metropolitan status; (3) percentage of students eligible for free or reduced-price lunch; (4) percentage of white non-Hispanic, black non-Hispanic[4], and Hispanic students; (5) school enrollment; (6) rate of suspensions and expulsions for the year preceding the increase in SRO staffing levels; and (7) geographic proximity (e.g., same part of the state, if not the same school district). Once the pool of matched comparison schools was selected, we confirmed with the schools and, as appropriate, local law enforcement agencies that each school neither received 2013 or 2014 CHP-funded SROs nor had an increase in SRO hours at the same time as its corresponding treatment school. Of the schools that matched the treatment schools, 72 schools met these criteria. Tests of statistical significance indicated no differences between conditions on the matching characteristics or on the pretreatment trajectories of total disciplinary incidents or exclusionary discipline practices. Likewise, no statistically significant differences were found between treatment and comparison schools on administrator reports of security or prevention practices prior to the intervention. Geographic proximity was more difficult to achieve because the increase in SRO staffing level tended to occur in small school districts with few schools available for matching; 18.2 % of the treatment schools had at least one matched comparison school in the same school district.

**Data Sources and Measures**

The study used both administrative data and self-report data. For the selected treatment and comparison schools, we drew upon monthly school-level administrative data from CDE on counts of disciplinary offenses and actions for the 2011-12 to 2016-17 school years[5]. We augmented the administrative data with self-report data from the local law enforcement agencies (CHP grantees for treatment schools) responsible for SROs in the selected schools at the intervention point, and from school administrators and SROs at those schools.

*Intervention Measure*. Treatment schools increased SRO staffing levels at a specific time, while comparison schools did not increase SRO staffing levels at the same time as treatment schools.

[4] Hereafter "white" and "black."

[5] We also proposed to examine SRO effects on student referrals to the juvenile justice system. This data element was not available from the CDE. We explored the possibility of using biannual U.S. Department of Education Office for Civil Rights (OCR) data on referrals to the justice system to augment the CDE data, but this avenue proved unworkable because (a) the biannual nature of the OCR data made it difficult to accurately align "pre" and "post" data points with the timing of SRO placement for many schools in the study, and (b) the 2015-2016 data tool we required to access the data was not made available soon enough to meet our timelines for data analysis.

Implementation Measures. To examine the extent to which effects were sensitive to how the intervention was implemented, we measured SRO approach (law enforcement vs. other activities), and SRO hours (high vs. low dose).

*Outcome Measures*. The outcome measures, based on administrative data, were monthly school-level counts of disciplinary offenses and disciplinary actions. Disciplinary offenses were recorded for each student and each offense involved in a disciplinary incident[6]. CDE established different reporting requirements for disciplinary offenses by students with and without special needs. For students with special needs (based on whether a student had an individualized education plan), offenses were recorded if they resulted in any disciplinary action; however, for students without special needs, offenses were recorded only if they resulted in the removal of a student from their regular instructional setting for one or more days. These differing definitions of reportable offenses for students with special needs versus students without special needs required that we report outcomes separately for these two groups of students. Offense measures included (1) total offenses, (2) more and less severe offenses (based on a hierarchy of offenses suggested in CDE's CALPADS Data Guide Version 9.3 dated 05/18/18), and (3) type of offense (weapon-related, drug-related, related to crime against persons, related to crime against property, and serious violent).

The disciplinary action measure consisted of exclusionary actions taken in response to offenses. An exclusionary action was removal of a student from regular instructional setting.

*Moderator Measures*. Moderator measures included student special education status (with special needs/without special needs) and race/ethnicity (white, black, and Hispanic), instructional level (middle vs. high), and metropolitan status (urban/suburban vs. town/rural).

*Covariate Measures*. We used CDE data on school enrollment to control for potential differences between treatment and comparison schools on this variable. Enrollment counts by race were controlled in the analyses of moderation by race.

[6] We also analyzed disciplinary incidents, which can involve multiple students and multiple offenses. The results from these analyses largely mirrored the results on offenses and so are not reported.

**Analysis**

We set the intervention point for all treatment schools to the month in which SRO staffing levels increased, and used the same intervention point for the corresponding comparison school(s). To examine the main effects of SROs, we conducted generalized estimating equation (GEE) analyses on the time (pre- vs. post-intervention) by condition (treatment vs. comparison) interactions for two sets of time periods: (1) 23 months pre- and 11 months post-intervention, and (2) 23 months pre- and 20 months post-intervention. GEE analyses were based on mean counts per school per month for each outcome aggregated to the relevant time period (e.g., 23 months pre- or 11 months post-intervention).

For outcomes with statistically significant time by condition interactions based on GEE analyses, we conducted auto-regressive integrated moving average (ARIMA) analyses. Both GEE and ARIMA analyses account for data interdependence among repeated measures of each outcome; ARIMA analyses further account for fluctuations in time series due to cyclical effects. The ARIMA analyses, which were conducted separately for treatment and comparison schools, entailed fitting statistical models to series of monthly data and testing whether observed immediate post-intervention outcomes (2 or 3 months post-intervention) differed from those projected in the modeled pre-intervention series. Tests were conducted for a statistically significant change in the slope parameter (e.g., change in the rate of decrease or increase in the number of disciplinary offenses) for the period immediately following the increase in SRO staffing levels in the treatment schools.

To examine the moderation of intervention effects, we conducted GEE analyses on the three-way interaction of time (pre- vs. post-intervention), condition (treatment vs. comparison), and moderator level (e.g., middle school vs. high school) on each disciplinary offense and action outcome for the two sets of time periods discussed earlier. For the three-way interactions that were statistically significant at $p<.10$, we examined the two-way interaction of time and condition for each level of a given moderator (e.g., high school) and each set of time periods[7].

[7] Due to different reporting requirements for disciplinary offenses and actions by special needs status, we were unable to examine moderation by special education status directly. We instead conducted separate 2-way interaction tests by special education status, and conducted all other tests of moderation separately by special education status. An exception is the analyses of moderation by race, which omitted students with special needs because a large number of schools had unknown enrollment counts for one or more races for students with special needs.

To examine the sensitivity of the intervention effects to how the intervention was implemented, we conducted GEE analyses on the time (pre- vs. post-intervention) by implementation variable level (e.g., law enforcement vs. other activities for SRO approach) interactions for each of the two sets of time periods. Unlike the other outcome analyses conducted, these analyses did not distinguish between treatment and comparison schools.

All analyses were conducted using SAS 9.4 (SAS Institute, Inc., Cary, NC) and controlled for school enrollment.

## Results

### The SRO Program

Based on the survey of school administrators, the treatment and comparison schools were equivalent on mean number of SRO hours per week pre-intervention (13.1 and 11.1 hours for treatment and comparison schools, respectively; NS), but differed on this measure post-intervention (20.4 and 10.9 hours for treatment and comparison schools, respectively; $p<.05$); the pre- to post-intervention change in SRO hours per week was significant for the treatment schools only ($p<.05$). Moreover, based on information provided during our initial calls to the law enforcement agencies responsible for SROs in the selected schools, treatment schools post-intervention had more SROs than comparison schools (1.0 vs. 0.4 SROs; $p<.01$), more SRO hours per week (27.0 vs. 12.3 hours; $p<.01$), and more hours per week per 100 students (3.7 vs. 1.1 hours per week per 100 students; $p<.01$).

The SRO survey, augmented with LEA interview responses for schools that were missing SRO surveys, provided information about SRO activities at their assigned treatment schools. On average, SROs in these schools spent about half their time (48%) in law enforcement and order maintenance activities. They spent 30% of their time on counseling and mentoring and another 20% of their time in teaching activities.

Interviews with LEAs provided information about the training provided to SROs and the availability of written documents describing the duties of SROs (e.g., manuals and memoranda of understanding [MOUs] with participating schools). Among CHP grantees, 80% of respondents stated that some training was required, and 67% stated that the SROs received 40 hours or more of specialized SRO training at some point in time. However, only 25% stated that any follow-up training was regularly provided. Only 20% of the law

enforcement agency (LEA) grantees stated that they had a written manual specifying SRO procedures. All of them, however, stated that a formal agreement governing the use of SROs existed between the LEA and the school district. Interestingly, less than 10% of these MOUs covered controversial topics such as the use of physical or chemical restraints on students and use of firearms. Only 13% covered making arrests on school grounds, and only a third of the agreements covered expectations for reporting criminal offenses to law enforcement or responding to student disciplinary infractions.

**Outcomes**

Consistent with prior research on SRO effects, this study found statistically significant intervention effects on weapon- and drug-related offenses across both follow-up time periods examined and on exclusionary discipline practices at 11 months post-intervention. For these time periods, the mean number of offenses increased for the treatment schools and decreased for the comparison schools, and the mean number of actions decreased substantially less for the treatment schools than for the comparison schools. The ARIMA analyses confirmed that larger increases in weapon- and drug-related offenses and exclusionary actions were evident for treatment than for comparison schools during the 2 or 3 months directly following the intervention. The observed effects were statistically significant for students without special needs but not for students with special needs.

These main effects masked large differences by school location. Metropolitan status moderated the intervention effects on nearly all of the disciplinary offenses and actions examined at 11 and 20 months post-intervention for students without special needs. These findings indicated the intervention effects were stronger for the urban/suburban schools than for the town/rural schools, and were always in the direction of a greater increase in offenses and actions following the SRO intervention for schools in urban/suburban locations as opposed to town/rural locations. A similar interaction by metropolitan status was found for students with special needs on drug-related offenses at 11 months post-intervention.

Results also varied by student race and ethnicity within school[8]. The mean number of offenses or actions tended to increase for black and Hispanic students at the treatment schools after the SRO intervention while they decreased for the same groups at the comparison schools as well as for white students at the treatment and comparison schools. This type of effect was found at one or more time point for both

[8] These interaction tests were conducted only for students without special needs.

black vs. white and Hispanic vs. white comparisons on more severe offenses, weapon-related offenses, and exclusionary actions. In addition, stronger effects were observed for black vs. white students on crimes against persons and serious violent crimes at 20 months post intervention, and stronger effects were observed for Hispanic vs. white students on drug-related offenses at 11 months post intervention.

Moderating effects for instructional level were also evident, but only for property-related offenses. The mean number of crime against property offenses increased for middle schools in the treatment condition 20 ($p<.10$) and 11 months after the SRO intervention relative to middle schools comparison schools, and decreased for high schools in both conditions.

SRO dosage and approach had no effect on the study outcomes. That is, none of the time by implementation variable level interactions were statistically significant at $p<.05$.

## Conclusions and Implications

The study design improved upon the designs used to study SRO effects in previous studies. It used a longitudinal matched comparison group to contrast change in schools with increased SRO staffing levels with change in schools that did not increase their SRO staffing levels at the same time-point. It augmented these analyses with comparison time series analyses designed to further rule out trends and confounding events as alternative explanations for the study results. The study was sufficiently powered to detect the main effects under investigation.

This study found no evidence to suggest that increasing the dosage of SROs via CHP grants to local law enforcement agencies reduces school crime. Instead, consistent with prior research, it found that the intervention increased measures of school crime – particularly for weapon and drug-related offenses. It also found clear evidence that increasing SRO staffing levels results in increased exclusion from school in response to disciplinary infractions. Increases in offenses and exclusionary reactions to offenses were most evident for students without special needs as opposed to students with special needs, schools in urban/suburban as opposed to town/rural locations, and for black and Hispanic as opposed to white students. Our study provides more and stronger evidence to support the idea that placing SROs in schools results in excluding students from school, and that this punishment falls disproportionately on minority students.

**Limitations**

The study has several limitations. First, the outcome measures used in this study might have been influenced by the placement of SROs in the school. This limitation creates ambiguity around the interpretation of the study findings. It is possible that SRO presence increases actual offending behavior. It is equally likely that SRO presences increases the detection and recording of crime. The study did not investigate the mechanisms responsible for the observed increase in crime outcomes. We did, however, interview school staff knowledgeable about school discipline practices in ten schools in which an increase in offending was observed following the placement of SROs. The interviewees articulated a number of mechanisms that might link SRO presence to an increase in the reporting and recording of school crime, suggesting that the increase in school crimes after SRO placement may be due at least in part to increased surveillance and harsher responses to student behavior as opposed to increases in the behaviors themselves. More definitive research is needed on the mechanisms through which SROs increase the level of school crime and the severity of responses to school crime.

A second limitation is that the study was limited to a non-random sample of schools in one state. Generalizability is therefore limited. It is encouraging, however, that the results from our sample replicate results from prior national studies. The CDE data used for most analyses covered only the more severe disciplinary infractions, and the types of offenses included in the database differed for students with and without special needs. Finally, the study is relatively underpowered for detecting moderator effects. That some moderator effects were observed suggests that low power was not a major concern, but we may have detected additional moderator effects with a larger number of schools.

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