Patrolling Public Schools: The Impact of Funding for School Police on Student Discipline and Long-term Education Outcomes

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Abstract

As police officers have become increasingly common in U.S. public schools, their role in school discipline has often expanded. While there is growing public debate about the consequences of police presence in schools, there is scant evidence of the impact of police on student discipline and academic outcomes. This paper provides the first quasi-experimental estimate of funding for school police on student outcomes, leveraging variation in federal Community Oriented Policing Services (COPS) grants. Exploiting detailed data on over 2.5 million students in Texas, I find that federal grants for police in schools increase middle school discipline rates by six percent. Further, I find that low-income students and Black and Hispanic students experience the largest increases in discipline. I also find that exposure to a three-year federal grant for school police is associated with a 2.5 percent decrease in high school graduation rates and a four percent decrease in college enrollment rates.

JEL Classification: H76, J24, I28, K42

Keywords: School Discipline, School Resource Officers, Police, Community Oriented Policing Services (COPS)

The views in this study are the result of independent research and do not necessarily reflect the views of the Texas Education Research Center, the Texas Education Agency, the Texas Higher Education Coordinating Board or the Texas Workforce Commission. This project was supported by a National Academy of Education Spencer Dissertation Fellowship grant.

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1 INTRODUCTION

Police are an active presence in U.S. public schools. In 2014, 43 percent of all public schools had security staff at school at least once a week, affecting over 70 percent of students across the country (Zhang et al., 2016). While estimates vary, government surveys suggest that there are at least 20,000 police officers working in schools (James and McCallion, 2013). In the wake of recent high-profile school shooting incidents, there are new plans to escalate police presence in public schools at the local and national level (e.g. DOJ, 2018; Perano and Ellis, 2018).

School Resource Officers (SROs) serve a number of roles, including protecting campuses from outside threats and educating students about safety and the law. However, SROs can also fill another role: They can engage in daily school discipline issues and administer punishments for student behavior. Despite the fact that reported crimes against students have decreased in recent decades, suspensions and expulsions have become more commonplace, increasing by nearly 200 percent between 2000 and 2014, and affecting Hispanic and Black students at 1.5 to over three times the rate of White students (DeVoe et al., 2003; OCR, 2014; Zhang et al., 2016).

As police have become a fixture in public schools, policy-makers, educators, and researchers are debating the merits of this approach to school discipline. Proponents of school police advocate that SROs are critical to establishing safe school environments and serve as educators, counselors, and positive role models for students (Canady et al., 2012).[1] Critics of school police argue that SROs can instead create a heavy-handed disciplinary culture

[1] These roles are referred to as the Triad model of SRO responsibility, as educators, informal counselors, and law enforcement officers. This model advises against SRO involvement in routine school discipline matters (Canady et al., 2012).

that adversely affects learning and may further disadvantage poor and minority students in low-performing schools (Balko, 2018).

Using data on over 2.5 million public-school students in Texas, I find that federal funding for school police increases disciplinary rates for middle school students by six percent but does not change high school disciplinary rates. I also examine second-order effects on long-term educational outcomes and find suggestive evidence that exposure to a three-year federal grant for school police decreases high school graduation rates by approximately 2.5 percent and college enrollment rates by four percent.

I find that the impact of police funding differs across student race and socioeconomic status. While all student race and income groups display significant increases in disciplinary actions, the effects are largest for low-income students and Black and Hispanic students. These results are consistent with work that finds that school disciplinary policies have a disparate impact on poor students and minority students. The results imply that the effects of expansions in school police may be most pronounced for marginalized student groups.

2 LITERATURE REVIEW

School discipline policy has the potential to have real impacts on academic success and educational attainment. Safety is a prerequisite to learning, and policies that increase school safety and deter dangerous or disruptive behavior may have a positive effect on student academic success. A small number of survey studies find that students have relatively positive perceptions of SROs and believe they increase school safety (e.g. Raymond, 2010; Brown and Benedict, 2005). Likewise, bullying and aggressive behavior can inflict serious psychological harm on student victims and SROs may be capable of reducing in-school bullying (Ttofi and Farrington, 2011; Wilson and Lipsey, 2007).

Alternatively, disciplinary actions may stigmatize disciplined students and decrease their attachment to school, negatively affecting their performance (Steinberg and Lacoe, 2017; Wald and Losen, 2003; Mendez, 2003). Studies in economics have found that juvenile arrests and juvenile detention decrease the probability of completing high school and increase the probability of future arrests (Aizer and Doyle, 2015; Kirk and Sampson, 2011; Hjalmarsson, 2008). The economic literature on juvenile behavioral responses to criminal sanctions has also found that juveniles may be less deterred by changes in punishment severity (Lee and McCrary, 2009) and may be more negatively impacted by the experience of sanctions (Aizer and Doyle, 2015; Bayer et al., 2009). By extension, school discipline, citations, arrests, or referrals to juvenile detention may lead to future involvement with the criminal justice system, in a process often termed the "school-to-prison pipeline" (Wald and Losen, 2003). Further, if students obtain a criminal record for offenses in school, they may face future barriers to employment (Pager et al., 2009).[2]

Through these channels, school police can positively or negatively impact the educational attainment of the students that they interact with, potentially affecting their human capital development, labor market attachment, and earnings later in life (Hanushek and Welch, 2006). This is the first study to estimate the impact of funding for school police on student disciplinary and education outcomes using quasi-experimental quantitative methods.

There is a large qualitative and ethnographic literature that documents the growth of harsh school sanctions policies and their disparate impact on low-income minority students

[2] Juveniles with criminal records can also face restricted eligibility for federal grants and loans for college, increasing the barriers to enrollment (Lovenheim and Owens, 2014).

(e.g. Nolan, 2011; Kupchik, 2010; Devine, 1996). This work has found that administrators' and teachers' roles in school discipline and classroom management are increasingly outsourced to SROs, and that SROs not only utilize their ability to arrest students for criminal offenses, but frequently participate in school discipline matters such as code of conduct violations (Kupchik, 2010).

However, literature reviews and meta-analysis studies note the lack of quantitative empirical evidence evaluating the impact of school police (Steinberg and Lacoe, 2017; James and McCallion, 2013; Fisher and Hennessy, 2016; Addington, 2009; Brown, 2006; Finn and McDevitt, 2005). Studies in this space have often been limited by small samples or consider simple observational pre-post or cross-sectional comparisons between schools.[3]

A recent paper by Owens (2017) is a notable exception; it examines the impact of changes in police hiring on arrests in and out of school for students of different ages using national data at the police department and county-level. Similar to the current study, Owens (2017) estimates her model using quasi-experimental variation in federal Community Oriented Police Services (COPS) grant funding for school police. She finds that expansions in school police increase property and violent arrests for children younger than high school age on school grounds and increase drug arrests for high school aged juveniles off of school grounds. Owens (2017) finding of increases in in-school arrest rates for younger students is consistent with the finding in this study that funding for school police increases middle school disciplinary actions. I build upon Owens' work by exploring the academic ramifications of school police using detailed student-level data. I am able to examine the impact of grants for school police

[3]An example of descriptive research in this area is Na and Gottfredson (2011), which uses a survey of 470 schools and a difference-in-differences design, and finds that schools that increase policing report an increase in non-serious violent crimes.

on student disciplinary actions, high school graduation and college enrollment, as well as how these effects vary for students in different demographic groups.

Studying the impact of school police presence on students has proved difficult for a number of reasons. First, appropriate data is hard to obtain. While schools follow a mandate to track aggregate disciplinary outcomes, detailed student-level data sets are not widely available. More importantly, information on the number of police employed in particular school districts is not uniformly tracked because SROs are typically employed by a third-party police department rather than directly by a school district. Beyond data constraints, the assignment of police officers to particular schools and districts is designed by school administrators, city officials, and law enforcement leaders and is non-random. School districts with higher rates of students in poverty, higher minority populations, higher levels of disciplinary actions, and lower graduation rates typically have a larger police presence (Kupchik and Ward, 2014). Given these selected characteristics, cross-sectional comparisons between school districts that have police and those that do not will be biased. However, even when researchers examine changes in police presence in a particular school district, the timing of investments in police may also be a function of changes in discipline and student behavior Owens (2017).[4] If school districts choose to hire police when they experience an increase in negative student behaviors, then not only is discipline a function of policing but policing is also a function of discipline. In this setting, simple longitudinal or panel data analysis will be biased by simultaneity.

I use information on federal COPS grants to fund police in public schools to address these measurement obstacles. I measure the impact of these grants on a range of student

[4]Literature in economics and criminology has shown the importance of accounting for simultaneity in police presence and crime rates (Nagin, 2013). A growing body of economics research using quasi-experimental methods that finds that increasing police presence reduces crime rates in the general population (e.g. DeAngelo and Hansen, 2014; Draca et al., 2011; Lin, 2009; Klick and Tabarrok, 2005; Di Tella and Schargrodsky, 2004).

outcomes, using variation across years *within* school districts, rather than cross-sectional variation across school districts. Within a given district, I compare disciplinary outcomes for students enrolled in years when the district receives federal grant funding to students enrolled in years without this funding. I also adapt this model to consider secondary effects on high school graduation and college enrollment, by examining the impact of differences in exposure to grants across student cohorts within school districts.

Critically, I also account for non-random school district decisions to seek funding for police in particular years by including grant application timing as a direct control in the model.[5] This strategy complements and builds on Owens (2017), whose paper uses variation in the size of grant awards for school police but does not control for school district grant application decisions.

3 INSTITUTIONAL CONTEXT

3.1 Federal COPS Grants for School Police

The COPS office at the Department of Justice (DOJ) was originally established to fund the hiring of new police officers as part of the Violent Crime Control Act of 1994. In 1998, the COPS office extended its grant programs to include funding for schools, with the launch of pilot programs to fund SRO hiring and partnerships between police departments, schools, and other community organizations.

Political interest school police escalated after the high-profile Columbine school shoot-

[5]The empirical approach in this study is closely related to work on the impact of COPS hiring grants for traditional police departments on municipal crime rates (Weisburst, 2017). Likewise, this paper is also related to the larger literature on the impact of COPS grants on crime (Evans and Owens, 2007; GAO, 2005; Zhao et al., 2002).

ing in 1999. Prior to 1999, school police were primarily confined to large urban districts (Addington, 2009; Brown, 2006).[6] Early school police programs were linked to policies of "zero-tolerance" toward student misconduct in the 1980s and 1990s.[7] Over the past two decades, new SRO programs have been founded while pre-existing SRO programs have grown; today, there are at least 20,000 SROs nationwide and over 70 percent of students attend schools with security staff on campus at least once a week (Zhang et al., 2016; James and McCallion, 2013). Policy-makers have invested in school police with the aims of both preventing tragic school shootings and generally improving safety in public schools.

Since Columbine, political support for COPS school grants has fluctuated. Federal appropriations for COPS school grants declined in the mid-2000s as part of a broader reduction in COPS funding by the Bush Administration, which had concerns about the overall effectiveness of the grants (Evans and Owens, 2007). During the Obama Administration, officials became increasingly concerned about the active role many SROs play in disciplining students, the large disparity in school discipline by student race, and the fact that student interactions with SROs may have repercussions for student involvement in the criminal justice system later in life. Given these concerns, COPS funding levels for school police declined in 2009. In 2014 and 2016, the Departments of Education and Justice released new guidance and resources for SRO programs, defining a narrow role for school police that excludes involvement in routine discipline and highlights the importance of disciplinary systems that do

[6] A sampling of the earliest records of police operating in public schools are Los Angeles, CA (1948), Indianapolis, CA (1939), and Flint, MI (early 1950s). The National Association of School Resource Officers (NASRO) was established in 1991, also prior to the expansion of school police that was spurred by the Columbine shooting (Brown, 2006).

[7] "Zero-tolerance" policies refer to laws or school policies that require predetermined consequences for specific student offenses, without considering mitigating circumstances or context for an offense incident. Recent work by Curran (2016) examines state "zero-tolerance" statutes and finds that these polices modestly increase overall suspension rates and have larger impacts on Black students.

not discriminate against groups of students (Steinberg and Lacoe, 2017; EOP, 2016). In a reversal of this approach, the Trump Administration has considered revoking this guidance and has introduced plans to increase funding for COPS school grants (DOJ, 2018; Camera, 2017).

During the sample period of this paper, 1999 to 2008, there were three broad groups of federal grants available for use in schools. This paper focuses on the impact of the largest program, COPS in Schools (CIS), which provided up to $125,000 in hiring funds per SRO over a period of three years. Approximately three-quarters of all COPS funding for school security has been granted through CIS. The COPS office has also funded additional grants for school security that are broader and more flexible in scope, Secure Our Schools (SOS) grants, school-based partnerships (SBP), the Safe Schools Initiative (SSI), as well as other program grants for school district police departments. I focus on the CIS program because of its size and articulated goal to increase police officer presence in public schools. While CIS grants are the primary focus of the paper, I include controls for other COPS school grant programs in the empirical model.[8]

The application process for CIS grants is narrative-based. Each applicant is asked to describe safety problems facing their school district and their proposed approach to remedying these problems, denote any community partnerships that support the grant proposal, and state their request for assistance. Review of these applications is based on the subjective judgements of individuals at the COPS office. It is likely that grant awardees were not randomly selected among school districts in each grant solicitation period. Because of this,

[8]SOS grants funded security technology, security assessments, and training for school police. SBP focused on building community partnerships with law enforcement agencies for particular security projects. SSI provided flexible funding for school/community safety, though no SSI grants have been distributed in Texas.

the research design in this paper does not rely on cross-sectional variation in grant receipt across school districts, but rather focuses on within school district variation, comparing years with federal CIS funding to years that are not funded for the same school district. The model also controls for school district decisions to apply for CIS funding, which vary over time, and could be a function of changing approaches to student discipline in school districts.

3.2 School Resource Officers and School Discipline in Texas

The setting for this study is the state of Texas. With 5.2 million students enrolled, Texas public schools have over 10 percent of the U.S. student population and represent the second largest state school system after California (NEA, 2016). The student body in Texas is diverse, with Black and Hispanic students representing over half of the student population (see Table 1). Though this paper is restricted to a single state, the size and diversity of the setting make the findings informative for other contexts.

In the sample used in this study, 49 percent of Texas public-school students were suspended or expelled between the 7th and 12th grade. Recent reports by the organization Texas Appleseed have found that over 275,000 misdemeanor tickets are issued to students for truancy and other misconduct each year, and minority students are disproportionately disciplined relative to White students (Fabelo et al., 2011; Fowler et al., 2010; Fowler, 2010). In recent years, these reports have prompted new Texas legislation limiting issuance of citations for misbehavior in school and mandating increased training for SROs (Texas, 2013, 2015). It is difficult to know if school discipline patterns in Texas are representative of the rest of the country, because of a lack of comprehensive data in other states.

Texas has embraced the use of SROs in schools. Larger public-school districts often have designated police departments that only operate in their school districts. A typical police patrol ratio in a large school district is two officers per high school, one officer per middle school, and rotating patrol in elementary schools. In addition to school patrol, several school districts in Texas have specialized police units, including K-9 teams, gang suppression units, crisis response teams, traffic safety, and incident reporting hotlines.[9] The size and budget of these police departments varies; in 2007, Houston ISD Police employed 289 staff at a cost of \$55 per student, while Edgewood ISD employed 31 staff at a cost of \$145 per student, and San Angelo ISD had a staff of 44 at a cost of \$16 per student (Fowler et al., 2010).[10]

Figure 1 (a) shows that the majority of COPS school grants in Texas have been distributed through the CIS program, with the objective of hiring of SROs in schools. Approximately seven percent of total federal CIS funds were distributed to Texas in this period. The majority of grants were distributed between 2000 and 2004, with funding peaking at $\approx$ \$15 million dollars in 2001.[11] The bottom panel of Figure 1 shows that COPS grants for school police have been consistently competitive, with grant applications outstripping grant acceptances in each year of the program.

4 EMPIRICAL MODEL AND DATA SOURCES

The empirical model uses panel data to measure the impact of receiving a CIS grant within school districts over time. I include school district fixed effects to account for unobserved

[9] These characterizations of school district police departments come from web searches of police departments in Texas.

[10] Texas Appleseed included this information for a handful of districts in their 2010 report (Fowler et al., 2010). Data for Houston ISD and San Angelo ISD is from the 2006 to 2007 school year, while data for Edgewood ISD is from the 2007 to 2008 school year.

[11] Throughout this paper, years refer to the academic calendar indexed by the spring semester. For example, grant statistics for 2000 cover the 1999 to 2000 academic year.

differences across school districts that are constant over time. In addition, I control for the non-random timing of school district decisions to apply for grants, which may be a function of changes in disciplinary culture and student behavior within a district.

The empirical model is as follows:

$$
\begin{aligned}
Discipline_{igdt} &= \beta_{1m} Accept_{dt} * MiddleSchool_{gt} + \beta_{1h} Accept_{dt} * HighSchool_{gt} \\
&\quad + \beta_{2m} Apply_{dt} * MiddleSchool_{gt} + \beta_{2h} Apply_{dt} * HighSchool_{gt} \\
&\quad + b_m OtherGrants_{dt} + b_h OtherGrants_{dt} \\
&\quad + \pi X_{igdt} + \delta_t + \gamma_g + \phi_d + \varepsilon_{igdt} \\
LongtermOutcome_{idt+k} &= \alpha_1 AcceptExposure_{dt} + \alpha_2 ApplyExposure_{dt} \\
&\quad + a OtherGrants_{dt} + \tilde{\pi} X_{idt} + \tilde{\delta}_t + \tilde{\phi}_d + \nu_{idt}
\end{aligned}
$$

where, i indexes students, g indexes grade, d indexes school district, and t indexes year. X_{igdt} is a vector of covariates that includes district-grade enrollment and student race (Black, Hispanic, White, or other race), gender, and whether the individual is classified as limited English proficiency (LEP), Special Education, gifted and talented, or economically disadvantaged. δ_t are year fixed effects, which capture aggregate time trends in student outcomes for all school districts. γ_g are grade-level fixed effects, which capture average differences in disciplinary actions across grades.

The primary outcome in the analysis is whether a student received a disciplinary action in a given year. This model uses student-year data for students in the 7th to 12th grade between 1999 and 2008. In the long-term outcome model, I focus on student cohorts enrolled in the 7th grade from 1999 to 2006 and measure high school graduation and college enrollment within 8 years (by age 20). This approach allows me to measure high school graduation for a broad base of 7th grade students and avoid issues of student attrition that may occur in later grades. The student-level data was obtained through the Texas Education Research Center

(ERC), a research platform that combines databases on kindergarten through 12th grade public-school students from the Texas Education Agency (TEA) and post-secondary students in Texas higher education institutions from the Texas Higher Education Coordinating Board (THECB).

The critical variables in the short-term model are $Accept_{dt}$ and $Apply_{dt}$, which refer to CIS grants. These variables are constructed to match the 3 year duration of CIS grant projects. The variable $Apply_{dt}$ is an indicator variable for whether a school district applied for funding in year t, $t-1$, or $t-2$, allowing this variable to be set to 1 for the period in which funding would be distributed if an application was accepted. Likewise, the variable $Accept_{dt}$ is an indicator for the duration of the grant project period if an application was accepted.[12] For example, a school district police department that applies for and receives a CIS grant in 2000 would have $Apply_{dt}$ and $Accept_{dt}$ set to 1 during the period 2000 to 2002; while if the grant application is denied, the $Apply_{dt}$ variable would be set to 1 and the $Accept_{dt}$ variable would be set to 0 for this period.[13] The variables $OtherGrants_{dt}$ control for other school grant programs administered from the COPS office, such as SOS grants. These controls are comparably defined to the $Accept_{dt}$ and $Apply_{dt}$ variables.

The grant application and acceptance data used in this paper was obtained through a Freedom of Information Act (FOIA) request to the COPS DOJ Office. I selected grants based on whether the program type was focused on school police or if the applicant had their

[12] In practice, school districts do not directly apply for COPS funding. Grantees are commonly municipal police departments, independent school district police departments, or other entities. In some cases, grant applications corresponded to a geographic area that covered more than one school district. In these instances, I manually matched grants to school districts using maps and web sources.

[13] The start time of a grant is indexed to the current academic year if a grant project (or application) starts between September and March, and is indexed to the following academic year if a project starts in April through August. Throughout this project, academic years are denoted as the year of the spring semester.

primary jurisdiction within public schools (e.g. school district police departments).

I consider grant variables separately depending on whether the student is in middle school (7th and 8th grade) or high school (9th through 12th grade), entering these variables as interactions with grade type. I add this structure because in most districts students are physically separated in different school buildings across these grades and SROs likely have different capabilities and approaches to interacting with students in middle school and high school. New SRO programs typically begin operating in high schools and then expand to middle schools (and elementary schools) as they grow in size and scope. This pattern of growth means that high schools are more likely to already have an SRO presence before they receive a grant treatment. In addition to differences in treatment across middle and high schools, students have developmental differences across these grades as well, which may impact the way that they respond to increased SRO presence.

For the long-term outcome model, the estimation approach considers future outcomes for cohorts of 7th graders. Here, the grant variables are defined in terms of years of exposure, as the number of years within a grant application or acceptance period. Exposure is calculated as a rate within the six years an "on-time" student would take to graduate high school between the 7th and 12th grades. For example, consider a grant that is transferred to a school district in the year 2000. A student beginning 7th grade in 2000 would be exposed to three accepted grant years and have a value of $\frac{1}{2}$ for $AcceptExposure_{dt}$ and $ApplyExposure_{dt}$. The exposure values depend on the year that a student enters the 7th grade; in the above case, a student in the 2001 7th grade cohort would be exposed to two years of a grant and have a calculated exposure of $\frac{1}{3}$ between the 7th and 12th grades. $OtherGrants_{dt}$ controls are also defined in terms of exposure in the long-term model.

In this framework, $Apply_{dt}$ illustrates changes in student outcomes when school districts want to increase their police presence but do not receive grant funding. These variables capture the effect of security initiatives a district could adopt on their own during years when they are interested in federal funding for SROs. $Accept_{dt}$ represents the impact of changes in grant funding for school police conditional on the choice to apply for a grant. School districts can alternate between grant acceptance states over time, switching between having an accepted grant, a rejected grant, or no application.

The last important feature of the model are school district fixed effects, ϕ_d, which control for unobserved differences across school districts that are constant over time.[14] These controls may reflect differences in district funding structures, approaches to discipline, and cultures, each of which are determinants of student outcomes but are unobservable in the data. After including school district fixed effects, the model uses variation across acceptance years, rejected years, and years with no application *within* the same school district. The specification is comparable to a difference-in-differences model with an additional control for unobservable characteristics associated with the timing of application decisions (Appendix Figure A1 depicts this comparison).[15] *The resulting identifying assumption is that conditional on the decision to apply for a federal grant for school police, the timing of grant proposal acceptance is not a function of changes in student outcomes within a school district.*[16]

[14]Students are assigned to the school district they are enrolled in during the 7th grade, rendering the output of the model "intent-to-treat" estimates. This assignment procedure assumes that students do not alter their school district in response to school police presence prior to entering the 7th grade, an assumption that is reasonable given that levels of student discipline are low in kindergarten through 6th grades.

[15]All appendices referenced in the text 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://www3.interscience.wiley.com/cgibin/jhome/34787

[16]While grant variables are defined as exposure rates in the long-term model, a similar identifying assumption holds because the timing of acceptances within districts determines the total number of years of grant exposure for school district cohorts.

The likelihood that a particular district wins a grant in one application year versus another application year is a function of the availability of federal funds. The number of possible grants that can be funded in each year varies with federal interest in the grant programs, and this is a key driver of the probability that a grant application is accepted for a particular district in a specific year (Figure 1, (c)).

A limitation of the model is that I am unable to observe the actual employment levels of police in school districts. School districts do not directly employ police, instead they are contracted through a third-party police department. Because I do not observe police employment, the empirical approach does not use COPS grant variation as an instrument for police presence; instead, I estimate the reduced form, or total, impact of receiving a CIS grant on student outcomes.[17] In the results section, I provide evidence that grants result in increases in police presence by estimating impacts of the grants on school district security spending. I also estimate increases in police hiring using a sub-sample of districts using data from the Federal Bureau of Investigation (FBI).

A second concern is that grant funding may be used for other purposes, or affect other aspects of a recipient's spending that might also impact student outcomes. This project focuses on CIS grants that are intended to be used to expand SRO hiring in public schools. However, it is possible that some of the funds could be used for other security or other school purposes, and this is a limitation of the study. This concern is mitigated by two factors. First, the organizations that actually apply for COPS funding are third-party police departments, and this organizational separation may make it more difficult for grant funds to be spent on

[17] Weisburst (2017) estimates a similar model using COPS grant acceptances as an instrument for police employment in municipal districts and to determine the impact of police force expansions on local crime rates. This two-stage model is possible because municipal police departments report employment to the FBI each year.

school initiatives that are not related to security. Second, while the focus of the model is the effect of grants designated for police hiring, the specification also includes controls for other school grants administered by the COPS office. The application controls for other grant types partially account for interest in investing in alternate security aims, such as security equipment and technology.

5 RESULTS

5.1 Summary Statistics

Table 1 provides a summary of the student data used in this project. The left panel describes the short-term sample, covering over 16 million student-years between 1999 and 2008 for students in the 7th through 12th grades. The right panel summarizes the long-term sample of 2.5 million students in cohorts entering the 7th grade between 1999 and 2006.[18] The demographic characteristics across these two samples are similar.

The student sample is diverse; student observations are $\approx$ 40 percent White, 40 percent Hispanic, 15 percent Black, and five percent other race, of which over 90 percent are Asian. Half of the sample is categorized as economically disadvantaged, or low-income, a designation that is derived from whether a student receives a free or reduced price lunch at school.[19]

26 percent of students receive disciplinary actions each year. Over the long term, 70 percent of 7th grade cohorts graduate from a public high school in Texas, and 47 percent

[18]The number of districts differs in the two samples due to district consolidation and reorganization in the period.

[19]This variable can also indicate low-income status using other definitions. These are annual income at or below the federal poverty line; eligibility for Aid to Families with Dependent Children or other public assistance programs (includes WIC program participants); and eligibility for benefits under the Food Stamp Act of 1977 or the Health and Humans Services (HHS) Poverty Guidelines.

enroll in college within 8 years of enrolling in the 7th grade.

CIS grants affect a large portion of student-years in the data, with 42 percent of observations corresponding to a grant application year and 25 percent of observations corresponding to a grant acceptance year (Table 1). These statistics imply a student-weighted grant acceptance rate of 60 percent.

Over the time frame of the study, there were 771 CIS grant applications and 335 acceptances (Table 2).[20] Awarded grants designated funding for three SROs per school district on average, with total funds of $324,000 per school district (weighted by student-years). 80 percent of students attended school districts that applied for a grant during the sample period, and 70 percent of students attended school districts that received a grant in the sample period.[21]

5.2 Baseline Results

A necessary condition for interpreting the impact of CIS grants as a consequence of changes to school security is that the grants significantly altered security resources. Table 3 measures the relationship between CIS grants and security spending recorded by school districts, with column (4) corresponding to the fully specified model. Panel (A) shows that the ratio of security spending to total school district spending increases by approximately seven percent when a school district has a CIS grant, from an average of 0.2 percent of total spending.[22]

[20] These counts are calculated to reflect counts of new grant-years by school districts. The summary statistics in the table are weighted by student population, but the grant observations are at the school district level.

[21] When grant characteristics are not weighted by student observations, 36 percent of districts applied for a grant during the sample period, 22 percent of districts were ever accepted for a grant, and 28 percent of districts were ever rejected for a grant. These numbers are lower than the weighted characteristics because larger school districts were more likely to apply for and receive COPS funding.

[22] Throughout this paper, percentage effects are calculated relative to the outcome mean for the entire sample period, rather than a "pre-treatment" period because districts may be treated multiple times within

While not significant, Panel (B) shows that this corresponds to a point estimate increase of $183,000 in security spending per year. This increase in spending is within range of one third of the average three-year grant award of $324,000, suggesting that CIS grant funds were used for school security rather than another purpose. While most districts regularly report having some security expense, Panel (C) shows that school districts are one percent more likely to report this expenditure when they are receiving a CIS grant, an outcome that could be related to grant compliance.

These positive estimates show that school districts devote more resources to security when covered by a federal grant. In practice, grants are typically administered to third-party police departments rather than directly to schools and school district security budgets may incompletely capture changes in grant spending. This institutional dynamic is likely to add measurement error to the security spending outcome variables and may decrease precision in these models.

Unfortunately, I cannot observe the impact of grant funding on SRO employment using the Texas ERC data. However, I am able to examine police hiring impacts using data on a sub-sample of Texas police departments from the FBI, in an analysis that is comparable to Owens (2017). Appendix Table A1 shows that CIS grant acceptance results in up to a six percent increase in the number of total police per 10,000 residents, and a 40 to 90 percent increase in the number of SROs per 10,000 residents. While these estimates are derived from small samples, they provide evidence that CIS grants are associated with increases in police hiring.[23]

Table 4 shows the impact of grants for school police on student discipline. Panel (A)

the sample period.

[23]For details on the estimation, refer to Appendix Table A1.

does not find a significant impact of grant funding on discipline when the effect is aggregated across grades. However, grant receipt strongly increases disciplinary actions among middle school students when the treatment is interacted with school type in Panel (B). Grant receipt increases disciplinary actions among middle school students by six percent per year and does not change rates of disciplinary actions for high school students. Table 5 shows that this increase is driven by disciplinary actions for low-level offenses or conduct code violations, rather than serious offenses. Appendix Table A2 provides evidence that out-of-school suspensions are the most common sanction for these low level offenses.

The middle school discipline effect could be related to expansion of SROs from high schools to middle schools with the assistance of grant funding. Though I cannot observe how school districts allocate SROs across school types, the middle school treatment effect is consistent with Owens (2017), who finds that CIS grants result in increases in arrests of students 14 or younger on school grounds.

Students enrolled in schools with CIS grants also have lower high school graduation and college enrollment rates. Police presence may create an adversarial school culture and alter the experience of attending school. Likewise, additional disciplinary actions could stigmatize disciplined students and reduce student confidence. Through these channels, school police have the potential to reduce student attachment to school and student educational aspirations. These channels could impact the likelihood of graduating high school or enrolling in college.[24]

[24]Throughout this project, I consider ultimate college enrollment outcomes for 7th grade cohorts that are not conditional on high school graduation. I do this to estimate the primary policy relevant effect of changes in college enrollment for all students in attendance. This means that part of the college enrollment effect is driven by students who do not complete high school and therefore cannot enroll in college. In fact, when the sample is restricted to students that ultimately graduate from high school, the percentage change in college enrollment is approximately half of the effect in the unconditional sample. These results are omitted due to space constraints but are available on request.

Table 6 shows that the effect of exposure to one three-year CIS grant is associated with a decline in the probability of graduating high school by 2.5 percent or 1.7 percentage points.[25] There is also a negative association between grant receipt and student college enrollment rates, driven by a decline in two-year college enrollment. Exposure to one three years CIS grant reduces the likelihood of ultimately enrolling in college by four percent and the likelihood of enrolling in a four-year college by seven percent.

The estimates imply that a 10 percent increase in a school district's security expense ratio is associated with a 8.6 percent increase in middle school discipline per year, and 3.6 percent decline in high school graduation (for students exposed to a three year grant).

Each column in Tables 3, 4, 5 and 6 successively adds controls, with column (4) corresponding to the fully specified model. Adding student covariates to the model in columns (2) and (4) does not substantively alter the estimates (across these tables). Adding application controls to columns (3) and (4) in these tables accounts for time varying unobserved characteristics associated with school district interest in grant funding. The application controls are not always significant, but they do alter the "Accept" coefficient magnitude. These coefficients imply that when districts choose to apply for funding they are less likely to have a security budget and have higher high school graduation and college enrollment rates. The preferred specification adjusts for changes in student outcomes that are associated with school district decisions to apply for funding.

[25]The coefficient in the table corresponds to full grant exposure for six possible years between 7th and 12th grade (or the equivalent of two grants). A single grant is two-year long and corresponds to half of the regression coefficient.

5.3 Robustness Tests of the Baseline Model

In this section, I conduct several robustness tests of the baseline model. First, I test the validity of the identification assumption, that conditional on grant application decisions, the timing of CIS grant acceptance is not a function of changes in student outcomes within a school district. Figure 2 interacts the accept variables with year indicators before and after treatment, to examine how treatment is related to changes in disciplinary outcomes over time.[26]

These graphs show that the timing of CIS grant acceptance is unrelated to pre-treatment changes in student disciplinary actions for middle school and high school students. In the post-treatment years, disciplinary actions increase for middle school students but are unchanged for high school students.

In contrast to the discipline outcome which varies by year, high school graduation and college enrollment are cumulative outcomes that are observed only once per student. In Figure 3, I consider how the impacts on long-term outcomes change with increased exposure to grants. The figures show that students with more years of exposure to grants are increasingly less likely to graduate from high school or enroll in college.

Next, I conduct a series of placebo tests that artificially vary the timing of treatment to provide evidence that the results are not spurious. The purpose of this exercise is to

[26] Due to computational constraints, these graphs use data collapsed to the district-grade-year level that is weighted by the number of students within these cells. The specification used for this event study is adapted from the primary model to accommodate the fact that districts may have multiple grant treatments at different points in time. These graphs are created by duplicating the data for each possible treatment year and stacking these data sets to form a "pseudo panel." For example, the 2003 sub-panel considers new applications in 2003 and plots acceptance coefficients in the years prior to and following 2003, while also separately controlling for potential concurrent treatments in years other than 2003 (such as a new grant in 2001). In each year panel, the designated treatment year is considered over time, and treatments in adjacent years are included as additional model controls.

benchmark the actual model result to regressions in which we do not expect to find a grant treatment effect. In each test, I randomly assign "Accept" treatments to districts that have applied for a grant in a given year, maintaining the overall acceptance rate for that year in the sample. I replicate this procedure 1,000 times and plot the distribution of estimates in Appendix Figure A2. The placebo distributions validate the actual estimates: The model estimate for the impact of grant receipt is outside of the 95 percent confidence interval for the increase in middle school discipline.

I extend this analysis to a test of the high school graduation and college enrollment effects in Appendix Figure A3. Here, I randomize the fraction of years a student is exposed to grant funding for each 7th grade district-cohort year such that this fraction is less than the "Apply Exposure" fraction and the overall acceptance exposure matches the rate in the data. Using this test, I find that the model estimates for high school graduation and college enrollment are also outside the 95 percent confidence interval in the placebo distributions.

In Appendix Tables A3 and A4, I display a series of additional robustness checks. One concern with the baseline model is that school districts that apply for or are accepted for grants may be markedly different from districts that do not apply for grants. In the baseline model, I include school district fixed effects to account for differences across these district types that are constant over time. However, the time trends across districts with different grant participation may differ. To test the importance of this concern, I first restrict the sample to school districts that ever applied for a grant (specification 2), were ever accepted for a grant (specification 3), or were both rejected and accepted for a grant (specification 4) to allow the time trend and covariate coefficients to be estimated within group. In specification (5), I include the full sample but interact year effects with four grant history groups, namely those that never applied, were only accepted, were only rejected, or were both accepted and rejected. Lastly in (6), I allow year effects to be separately estimated for districts that applied for grants in different years. The estimates are comparable across each of these specifications.

A second concern is that school districts that are located in different parts of the state or serve student populations of different sizes may have different trends in discipline and long-term academic outcomes. I separately estimate year effects for 20 geographic regions in Texas in specification (7), and 9 student enrollment size groups in specification (8). Again, the estimates in these specifications are similar to the results of the baseline model.

School campuses within school districts may vary substantially in terms of student characteristics, disciplinary policy, and other resources. In this paper, I focus on differences in outcomes within school districts because I observe policy changes at the school district level. In specification (9), I substitute school district fixed effects with school campus fixed effects to address heterogeneity across schools within districts. This change does not substantively alter the results.

A key component of this paper is considering how funding for police impacts students in different race and income groups. Students in these different demographic groups likely have different trends in discipline, high school graduation and college enrollment. Likewise, school districts may have different approaches to discipline or have varying educational performance across these student groups. In specifications (10) and (11), I explicitly allow the year fixed effects and district fixed effects to vary by student race and income groups. Including these additional controls does not alter the total coefficient on grant receipt. In the Treatment Heterogeneity Section (Section 5.4) below, I utilize flexible models that allow year and district fixed effects to vary by student race and income (comparable to specification 11).

A potential mechanism for the high school findings could be that students who are most likely to be disciplined drop out of school when funding for police officers increases. I test the importance of student attrition in explaining the findings by restricting the sample to students who remain in school through the 10th grade or higher in specification (12). I find consistent estimates for discipline in these samples, suggesting that student attrition is a minor contributor to the findings.

In several of the analyses in this paper, I use data that is collapsed to the district-level and weighted by student enrollment cells. As a sense check of this procedure, I include estimates using collapsed data weighted by the number of students in each cell in (13).[27]

I include additional grade level robustness checks for the discipline outcome in Appendix Table A3. Specifications (14) and (15) show that the effects are robust to including grade by year fixed effects and grade by district fixed effects. Lastly, the analysis in this project is restricted to students in the 7th through 12th grade because students in this age range have higher rates of discipline than younger students. I estimate the impact of CIS grants for school police on students in the 1st through 6th grades in specification (16). I find no effect of the grants on students in these grades, likely because both discipline rates and police presence are low for these students.

5.4 Treatment Heterogeneity

In Tables 7 and 8, I consider treatment effects for different student demographic groups, split by race and socioeconomic status, using the economic disadvantage indicator. I con-

[27] Proportional weights equal to hundreds of students are applied in the collapsed sample. The sample sizes in the weighted collapsed data set and the unweighted baseline differ due to rounding.

sider differences by race because of the large disciplinary gaps that have been documented by researchers, policymakers, and advocates. Similarly, I consider student poverty because poorer students also experience higher rates of discipline and are less likely to graduate from high school.[28] In each of the models in this section, I include flexible controls that allow time trends and district fixed effects to vary by student race and income groups.[29] Each model interacts the main treatment effects with each student race and income group.

The demographic pattern of grant treatment effects on discipline is striking. Nearly all groups of middle school students experience significant increases in discipline, but the effects are strongest for *low-income* students and *Black* and *Hispanic* students. These effects correspond to a five percent increase in discipline for low-income White students and an seven percent increase in discipline for low-income Black and Hispanic students. For students that are not low-income, Black students experience a ten percent increase in discipline, followed by a six percent increase for Hispanic students and a four percent increase for White students. The estimates imply that when school districts expand resources for school police, low-income and minority students are disciplined more intensively.

The post-estimation tests at the bottom of Table 7 show that the treatment effects for low-income students are not statistically different from one another across race. Further, the treatment effects for both Black and Hispanic students are not statistically different from one another across income groups (within race). As in the aggregated model, none of the groups

[28]I have also considered models that additionally interact the treatment effect with student gender. I do not find significant differences in treatment effects on discipline across gender and within race and income groups. In the long-term analysis there is some evidence that high school graduation effects are stronger for men and college enrollment effects are stronger for women. The results are omitted due to space constraints but available on request.

[29]These controls are not included in the baseline models above. The baseline models are robust to inclusion of these controls (see specifications 10 and 11 of Tables A3 and A4 above.

experiences significant changes in high school discipline.

The group mean column shows that there are also large baseline gaps in discipline by race and socioeconomic class. Relative to White students that are not low-income, low-income Black students are up to three times as likely to have a disciplinary action, while low-income Hispanic and White students are two times as likely to have a disciplinary action. Likewise, Black and Hispanic students that are not low-income are two times and one and a half times more likely to have a disciplinary action.

Table 8 considers how treatment effects for high school graduation and college enrollment differ by student demographic group. The analysis shows significant decreases in high school graduation for Hispanic and White students and significant decreases in college enrollment for low-income Black, Hispanic and White students. However, I cannot reject the null hypothesis that all treatment effects are equal for either outcome. These models do not provide strong evidence that the declines in high school graduation or college enrollment are concentrated in a particular student group. While somewhat less precise, the effects are broadly consistent with estimated declines in middle school discipline that are experienced by all student groups in Table 7. As in the discipline outcome, the group mean column in this table shows that low-income students and minority students are less likely to graduate high school (0.7 to 0.9 times) and enroll in college (0.5-0.9 times).

It is not surprising that funding for police in public schools differentially impacts students with different demographic characteristics. Prior work has shown that school districts with higher enrollment of non-White students are more likely to have "zero-tolerance" mandatory expulsion policies for certain offenses (Curran, 2017), that much of the racial disparity in discipline exists across schools (Anderson and Ritter, 2017), and that school districts with higher enrollment of Black students utilize more punitive discipline responses (Welch and Payne, 2010, 2012). These studies are consistent with the findings in this project. The observed patterns support *a priori* concerns that SROs disproportionately disadvantage low-income students and Black and Hispanic students. Overall, the demographic analysis implies that a student's experience with school discipline at an early age has potential ramifications for high school graduation and college enrollment. Negative school discipline experiences could shape the way that students are perceived by teachers, school administrators, and peers, and may also affect a student's confidence and attachment to school.

6 CONCLUSION

The widespread use of police officers in public schools is a relatively recent development. While school police programs have gained popularity as a policy to protect students against rare but tragic school shooting events, in practice, these officers are often actively involved in the enforcement of school discipline.

When school police officers are involved in the daily lives of students, they have the capability to alter student behavior, disciplinary consequences, attachment to school, and educational attainment. Though the potential consequences of school police interventions are large, there have been few evaluations of their efficacy.

This study provides the first estimate of the impact of funding for school police on student discipline and educational attainment using quasi-experimental methods. Using variation in federal COPS grants for school police, I measure the effect of receiving an increase in funding on students, conditional on school district decisions to apply for this funding. This strategy addresses biases related to both the non-random assignment of police to particular school districts and the non-random timing of investments in police within school districts.

Using detailed data on over 2.5 million public-school students in the state of Texas, I find that grants for school police increase disciplinary actions for middle school students. Over the long-term, exposure to federal funding for school police is associated with small but significant declines in high school graduation rates and college enrollment.

The results vary across student demographic groups. I find that expansions in grant funding have the largest effects on low-income students and Black and Hispanic students. This finding is consistent with prior work that finds that these marginalized student groups are most vulnerable to school discipline sanctions. This disparate policy impact is concerning and has implications for potential reforms to school policing and school discipline.

The large sample in the study, covering all students in public school in a populous and diverse state, means the results are likely informative for other contexts. While the analysis is limited by the fact that I cannot directly observe police employment in schools, the grant transfers I examine approximate practical policies. Policymakers are often limited in their capacity to monitor the implementation of regulations or subsidies; instead they are more likely to administer funds for articulated goals, similar to the grant program that is the focus of this study. This paper finds a negative average impact of grant transfers for school police on student outcomes.

On the whole, the results suggest that SROs have the potential to negatively affect students, through both increasing student discipline involvement and reducing student educational attainment. The literature on economic returns to schooling has shown that attending an additional year of high school can raise individual earnings by approximately 10 percent per year (Oreopoulos, 2006). Drawing on these findings, I conduct a back-of-the-envelope calculation of costs of this policy. I consider only costs from the decline in high school graduation and assume this decline results from only one less year of schooling for each student that did not graduate. To be conservative, I assume baseline annual earnings of affected students of $20,000 with a five percent earnings reduction per year, a discount rate of 20 percent, and a working career of 30 years.[30] The resulting loss in earnings is $105 million dollars for affected students, leading to an aggregate policy cost of $162 million including the value of grant transfers. This calculation is illustrative: It does not include emotional or psychological costs of school discipline, the value of increased safety or perceptions of safety, benefits for subgroups of students who may be positively affected, costs of more than a single year decrease in schooling, or costs related to reductions in college enrollment. Despite these limitations, this exercise highlights the fact that the results in this study raise serious questions about the value of future investments in school police.

More research is needed to understand how the utilization of public-school police compares to alternative approaches to school discipline, including positive behavioral interventions and supports and changes to disciplinary codes (Steinberg and Lacoe, 2017). Future work should evaluate best practices in school discipline as well as the cost-effectiveness of different disciplinary approaches.

[30] These values are purposefully conservative and likely provide an underestimate of costs. Median earnings for individuals with less than a high school diploma ranged from $22,000 to $27,000 between 1995 and 2015, in 2015 dollars (NCES, 2016). Additionally, working careers often exceed 40 years, and discount rates are often assumed to be less than 20 percent in net-present-value calculations. This calculation is based on the treatment exposure rates in the sample, the estimated effect sizes for high school graduation. I estimate costs from students affected in the long-term sample, or the 8 cohorts of students enrolled in 7th grade from 1999 to 2006.

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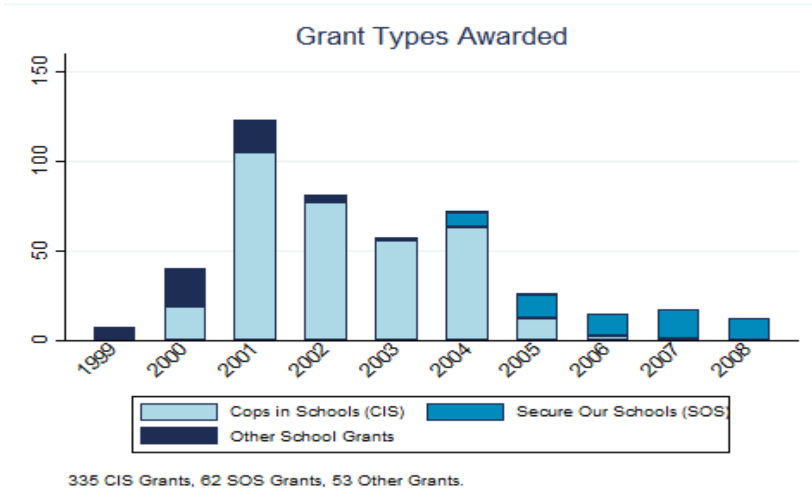
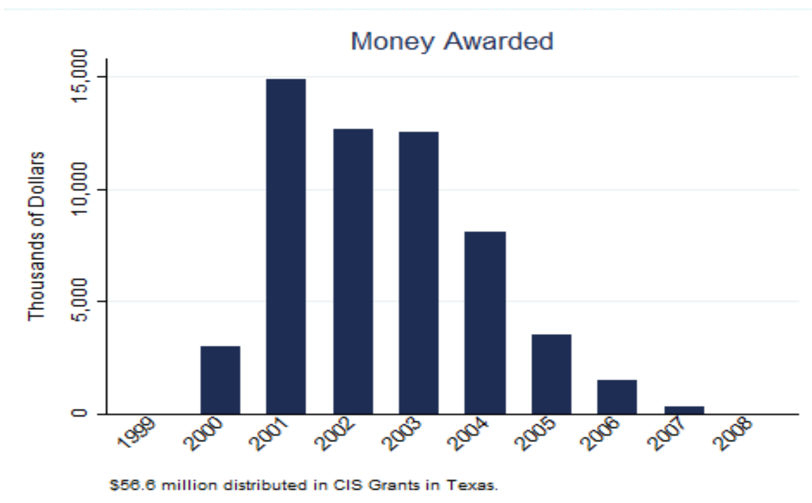
Tables and Figures

Figure 1: COPS in Schools (CIS) grants for schools in Texas

(a)



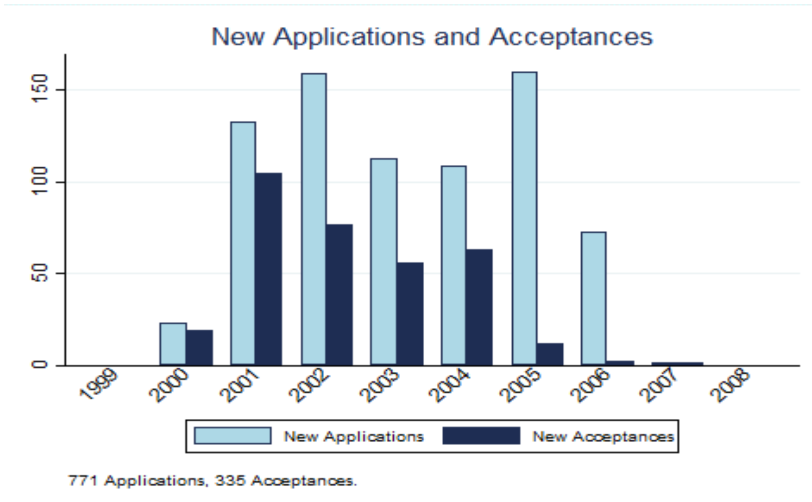
(b)



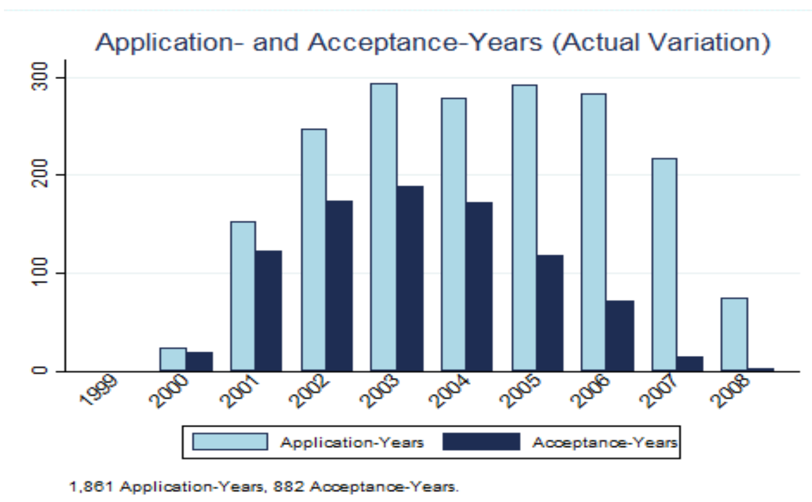
The graphs above show variation in COPS grants to agencies in Texas over time. The top figure (a) tracks grants of multiple types, while the bottom figure (b) tabulates funds awarded only for COPS in Schools (CIS) grants, which are the focus of this study. Grant tabulations are conducted at the school district level, rather than the police department grant level, to match the variation used in analysis. This means that grants awarded to multiple districts are counted more than once. Coded years in these graphs correspond to the spring of an academic year; for example, the 2000 grant tally covers the 1999 to 2000 academic year.

Figure 1: CIS grants for schools in Texas

(c)



(d)



The graphs above show variation in COPS grants to agencies in Texas over time. The top figure (c) tracks new applications and acceptances of COPS in Schools (CIS) grants, while the bottom figure (d) tabulates duration years of an accepted CIS grant or intended CIS application. An "Acceptance-Year" refers to a year in the three year awarded grant period. An "Application-Year" refers to a year when either a grant was awarded or a grant would have been awarded, if rejected. Grant tabulations are conducted at the school district level, rather than the police department grant level, to match the variation used in analysis. This means that grants awarded to multiple districts are counted more than once. Coded years in these graphs correspond to the spring of an academic year; for example, the 2000 grant tally covers the 1999 to 2000 academic year.

Table 1: Summary statistics, short-term and long-term student samples

Short-term Sample	Mean	***Long-term Sample***	Mean
Number of Districts	1,186	Number of Districts	1,165
Number of Students	4,365,009	Number of Students	2,506,849
Number of Student-Years	16,285,552		
Demographic Controls		Demographic Controls	
Number of Students in Grade	2,383	Number of Students in Grade	2,780
% Male	0.512	% Male	0.511
% White	0.433	% White	0.418
% Black	0.143	% Black	0.146
% Hispanic	0.395	% Hispanic	0.406
% Limited English Proficiency	0.060	% Limited English Proficiency	0.065
% Special Education	0.131	% Special Education	0.133
% Gifted	0.115	% Gifted	0.109
% Economically Disadvantaged	0.474	% Economically Disadvantaged	0.500
Grant Variables		Grant Variables	
Acceptance-Year, CIS Grant	0.249	Acceptance-Exposure, CIS Grant	0.245
Application-Year, CIS Grant	0.417	Application-Exposure, CIS Grant	0.418
Acceptance-Year, Other School Grant	0.066	Acceptance-Exposure, Other School Grant	0.050
Application-Year, Other School Grant	0.090	Application-Exposure, Other School Grant	0.068
Short-term Outcome		Long-term Outcome	
Any Disciplinary Action	0.259	Graduate High School	0.699
Any Disciplinary Action - Middle School	0.279	Enroll in College within 2 years after High School	0.472
Any Disciplinary Action - High School	0.247		

This table summarizes the sample used in the analysis. The left panel summarizes the sample of short-term student outcomes, of which the primary focus is disciplinary actions. This sample includes student-year observations for students in the 7th through 12th grade from the 1998 to 1999 school year to the 2007 to 2008 school year. The right panel summarizes the long-term student sample. This sample includes student observations for 8 cohorts of students enrolled in the 7th grade from the 1998 to 1999 school year to the 2005 to 2006 school year.

Table 2: Summary statistics, CIS grants

COPS in Schools (CIS) Grants	Acceptance		Application	
	Mean	S.D.	Mean	S.D.
N: Number of Grants	335		771	
School District Police Department	0.323	(0.468)	0.321	(0.467)
More than One School District Per Grant	0.556	(0.497)	0.501	(0.500)
Eligible Officers per District	2.927	(3.717)		
Total Award per District ($)	323,996.3	(416,772.8)		
Total Award per Student ($)	14.44	(33.82)		

School District COPS in Schools (CIS) Grant History	School District	
	Mean	S.D.
N: Number of Districts	1,186	
Ever Applied	0.796	(0.403)
Ever Accepted	0.697	(0.460)
Ever Rejected	0.561	(0.496)
Both Accepted and Rejected	0.461	(0.498)
Number of Applications	1.980	(1.586)
Number of Acceptances	1.103	(0.983)
Number of Rejections	0.877	(1.028)

This table summarizes characteristics of COPS in Schools (CIS) grant awards, weighted 7th through 12th grade student enrollment. Grant tabulations are conducted at the school district level, rather than the police department grant level, to match the variation used in analysis. The acceptance column summarizes the 335 accepted CIS grants, while the application column includes all CIS grant applications. The number of intended officer hires and award size are only available for accepted grants. The bottom panel summarizes CIS grant application histories at the school district level for all school districts in the analysis sample.

Table 3: Impact of CIS grants on school district budgets

	(1)	(2)	(3)	(4)
A. Security Expense Ratio				
Accept	0.000164*	0.000164*	0.000139+	0.000139+
	(0.00007)	(0.00007)	(0.00008)	(0.00008)
Apply			0.00003	0.00003
			(0.00005)	(0.00005)
Observations	16,189,127	16,189,127	16,189,127	16,189,127
Y Mean	0.00206	0.00206	0.00206	0.00206
B. Total Security Expense				
Accept	389,938	393,960	174,141	182,617
	(431,929)	(430,811)	(456,886)	(456,938)
Apply			346,837*	339,428*
			(169,688)	(172,756)
Observations	16,189,506	16,189,506	16,189,506	16,189,506
Y Mean	2,428,000	2,428,000	2,428,000	2,428,000
C. Has Security Fund				
Accept	0.0028	0.0026	0.0121*	0.0117*
	(0.0035)	(0.0034)	(0.0052)	(0.0051)
Apply			-0.0141*	-0.0138*
			(0.0055)	(0.0055)
Observations	16,189,506	16,189,506	16,189,506	16,189,506
Y Mean	0.945	0.945	0.945	0.945
Year FE	X	X	X	X
Grade FE	X	X	X	X
District FE	X	X	X	X
Student-Level Covariates		X		X
Apply Controls			X	X

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$
This table shows the impact of COPS in Schools (CIS) grant awards on school district budget variables. Robust standard errors are clustered at the school district level in all regressions. Column (4) is the preferred specification used in this paper. "Security Expense Ratio" is the ratio of school district spending on security functions to total district spending. "Total Security Expense" is the total dollar amount listed for security in school district budgets. "Has Security Fund" is an indicator for whether a district had any spending on security. These models have the limitation that grants are typically administered to third-party police departments, rather than directly to school districts. Because of this institutional feature, changes in school security spending may not be fully recorded in the school district budget. Outcomes above vary at the district-year-level but are estimated in student-year-level regressions to weight results by student population characteristics; the district clusters in these regressions account for outcome duplication. Additional grant controls for other COPS school grants ("Accept Other Grant" in columns (1) to (4) and "Apply Other Grant" in columns (2) and (4)) are included but not displayed. Year FE are indicators for years from 1999 to 2008, Grade FE are indicators for grade-level (7th to 12th grade), and District FE are indicators for school district. Covariates include district grade enrollment, student gender and race, and student status as Limited English Proficiency (LEP), economically disadvantaged, gifted or special education.

Table 4: Impact of CIS grants on student discipline

	(1)	(2)	(3)	(4)
A. Discipline (all grades)				
Accept	0.0037	0.0043	0.0026	0.0044
	(0.0039)	(0.0036)	(0.0048)	(0.0043)
Apply			0.0019	0.0002
			(0.0028)	(0.0027)
Y Mean	0.259	0.259	0.259	0.259
B. Discipline				
Accept*Middle School	0.0141**	0.0141**	0.0148*	0.0164**
	(0.0050)	(0.0048)	(0.0058)	(0.0055)
Apply*Middle School			-0.0002	-0.0026
			(0.0040)	(0.0041)
Accept*High School	-0.0028	-0.0018	-0.0050	-0.0033
	(0.0043)	(0.0039)	(0.0052)	(0.0048)
Apply*High School			0.0032	-0.0026
			(0.0032)	(0.0041)
Y Mean: Middle School	0.279	0.279	0.279	0.279
Y Mean: High School	0.247	0.247	0.247	0.247
Accept: Middle School=High School				
F-Test	11.28	11.51	14.05	12.94
P-Value	0.0008	0.0007	0.0002	0.0003
Observations	16,285,552	16,285,552	16,285,552	16,285,552
Year FE	X	X	X	X
Grade FE	X	X	X	X
District FE	X	X	X	X
Student-Level Covariates		X		X
Apply Controls			X	X

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1
This table shows the impact of COPS in Schools (CIS) grant awards on whether a student receives a disciplinary action. Robust standard errors are clustered at the school district level in all regressions. Column (4) is the preferred specification used in this paper. Additional grant controls for other COPS school grants ("Accept Other Grant" in columns (1) to (4) and "Apply Other Grant" in column (4)) are included but not displayed. Year FE are indicators for years from 1999 to 2008, Grade FE are indicators for grade-level (7th to 12th grade), and District FE are indicators for school district. Covariates include district grade enrollment, student gender and race, and student status as Limited English Proficiency (LEP), economically disadvantaged, gifted or special education.

Table 5: Impact of CIS grants on offense severity

	(1)	(2)	(3)	(4)
A. Conduct Code Violation				
Accept*Middle School	0.0179***	0.0181***	0.0194**	0.0208***
	(0.0048)	(0.0047)	(0.0060)	(0.0058)
Apply*Middle School			-0.0020	-0.0037
			(0.0050)	(0.0050)
Accept*High School	-0.0050	-0.0043	-0.0002	0.0012
	(0.0039)	(0.0039)	(0.0049)	(0.0046)
Apply*High School			-0.0067	-0.0077+
			(0.0043)	(0.0041)
Y Mean: Middle School	0.245	0.245	0.245	0.245
Y Mean: High School	0.215	0.215	0.215	0.215
Accept: Middle School=High School				
F-Test	20.92	22.25	12.61	12.19
P-Value	0.0000	0.0000	0.0004	0.0005
B. Serious Offense				
Accept*Middle School	0.0024	0.0023	-0.0009	-0.0006
	(0.0021)	(0.0021)	(0.0029)	(0.0029)
Apply*Middle School			0.0043*	0.0037+
			(0.0021)	(0.0020)
Accept*High School	0.0001	0.0004	0.0005	0.0010
	(0.0010)	(0.0010)	(0.0011)	(0.0010)
Apply*High School			-0.0005	-0.0008
			(0.0010)	(0.0010)
Y Mean: Middle School	0.0338	0.0338	0.0338	0.0338
Y Mean: High School	0.0302	0.0302	0.0302	0.0302
Accept: Middle School=High School				
F-Test	1.712	1.049	0.338	0.460
P-Value	0.191	0.306	0.561	0.498
Observations	16,285,552	16,285,552	16,285,552	16,285,552
Year FE	X	X	X	X
Grade FE	X	X	X	X
District FE	X	X	X	X
Student-Level Covariates		X		X
Apply Controls			X	X

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1
This table shows the impact of COPS in Schools (CIS) grant awards on whether a student is disciplined for a low-level code of conduct violation or a serious offense. Robust standard errors are clustered at the school district level in all regressions. Column (4) is the preferred specification used in this paper. "Conduct Code Violations" are violations of school rules that result in disciplinary actions. "Serious Offenses" include felony offenses, or any offense for weapons, substance abuse, sexual conduct, or violence. Additional grant controls for other COPS school grants ("Accept Other Grant" in columns (1) to (4) and "Apply Other Grant" in column (4)) are included but not displayed. Year FE are indicators for years from 1999 to 2008, Grade FE are indicators for grade-level (7th to 12th grade), and District FE are indicators for school district. Covariates include district grade enrollment, student gender and race, and student status as Limited English Proficiency (LEP), economically disadvantaged, gifted or special education.

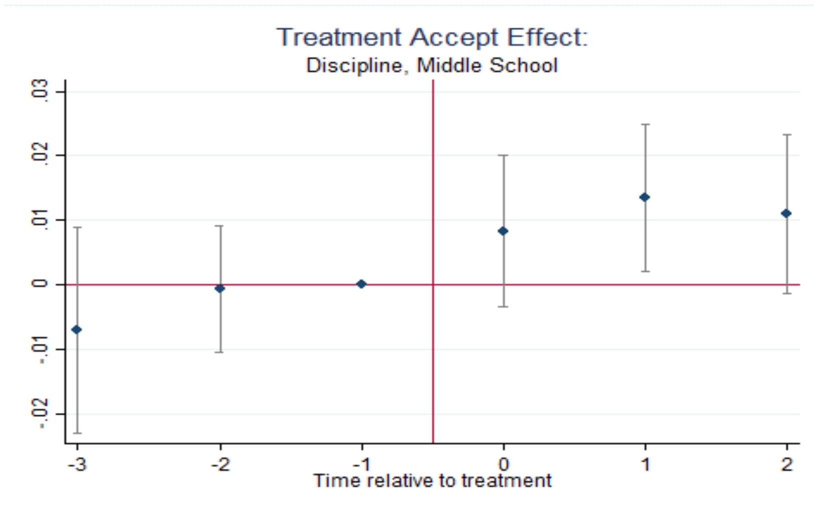
Table 6: Impact of CIS grants on long-term academic outcomes

	(1)	(2)	(3)	(4)
A. High School Graduation				
Accept Exposure	-0.0107	-0.0175*	-0.0217+	-0.0344***
	(0.0119)	(0.0079)	(0.0124)	(0.0094)
Apply Exposure			0.0180	0.0266*
			(0.0111)	(0.0108)
Y Mean	0.699	0.699	0.699	0.699
B. College Enrollment				
Accept Exposure	-0.0149	-0.0232*	-0.0201	-0.0378***
	(0.0128)	(0.00923)	(0.0160)	(0.0111)
Apply Exposure			0.00860	0.0231*
			(0.0120)	(0.0108)
Y Mean	0.472	0.472	0.472	0.472
C. 2-Year College Enrollment				
Accept Exposure	-0.0242+	-0.0303**	-0.0411**	-0.0538***
	(0.0134)	(0.00988)	(0.0155)	(0.0117)
Apply Exposure			0.0273*	0.0368***
			(0.0111)	(0.0104)
Y Mean	0.377	0.377	0.377	0.377
D. 4-Year College Enrollment				
Accept Exposure	0.00192	-0.000739	0.0120	0.00285
	(0.00695)	(0.00564)	(0.00812)	(0.00633)
Apply Exposure			-0.0169+	-0.00582
			(0.00937)	(0.0102)
Y Mean	0.212	0.212	0.212	0.212
Observations	2,506,849	2,506,849	2,506,849	2,506,849
Year FE	X	X	X	X
District FE	X	X	X	X
Student-Level Covariates		X		X
Apply Controls			X	X

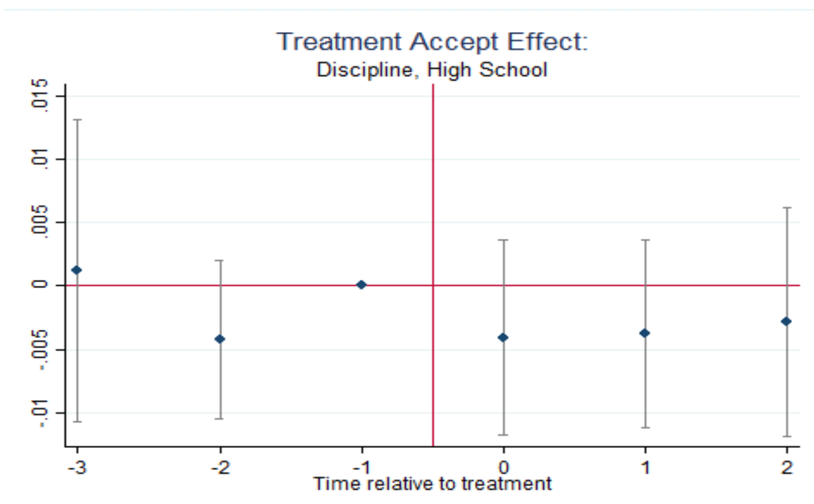
*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$
This table shows the impact of COPS in Schools (CIS) grant awards on whether a student graduates from high school or enrolls in college, within 8 years of enrolling in the 7th grade. Robust standard errors are clustered at the school district level in all regressions. Column (4) is the preferred specification used in this paper. The models above estimate cumulative outcomes for 7th grade student cohorts at the student level, and therefore exclude grade level effects. The "Accept Exposure" variable is the fraction of years between 7th and 12th grade for which a student was in a district that received grant funding. "Apply Exposure" is similarly defined. Each coefficient corresponds to a full six year treatment effect; the effect of a single three year grant corresponds to half of each point estimate. Additional grant controls for other COPS school grants ("Accept Exposure, Other Grant" in columns (1) to (4) and "Apply Exposure, Other Grant" in column (4)) are included but not displayed. Year FE are indicators for years from 1999 to 2008, District FE are indicators for school district. Covariates include district grade enrollment, student gender and race, and student status as Limited English Proficiency (LEP), economically disadvantaged, gifted or special education.

Figure 2: CIS grant effects on discipline over time

(a)



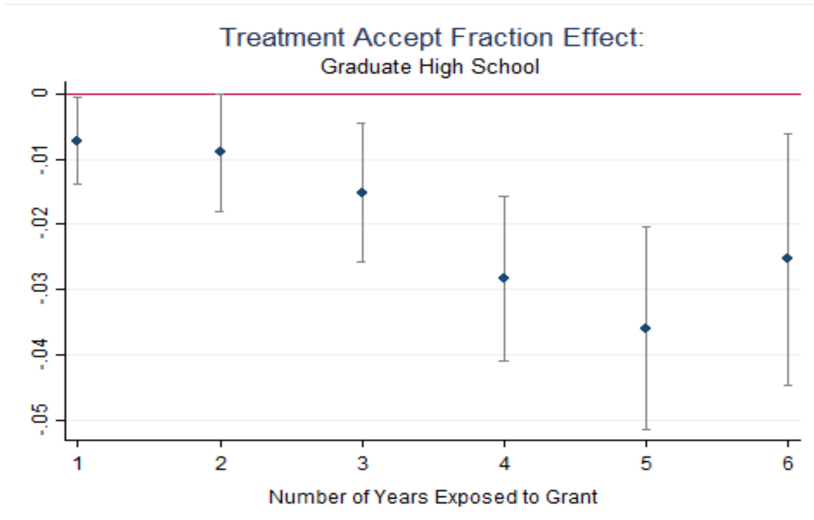
(b)



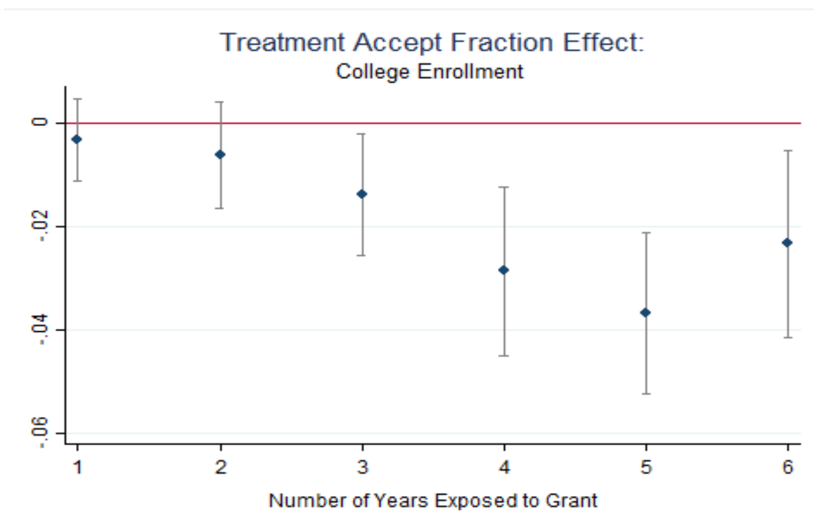
The graphs above show separate coefficient estimates from the same regression. Bars around coefficients represent a 95 percent confidence interval for each estimate with standard errors clustered at the school district level. Because a school district may receive multiple grants in different years, these graphs are created by duplicating the data for each possible treatment year and stacking these data sets to form a "pseudo panel." For example, the 2003 sub-panel considers new applications in 2003 and plots acceptance coefficients in the years prior to and following 2003, while also separately controlling for potential concurrent treatments in years other than 2003 (such as a new grant received in 2001). In each year panel, the designated treatment year is considered over time, and treatments in adjacent years are included as model controls. Pre- and post-treatment coefficients are separately estimated for the Accept and Apply variables, with the year prior to treatment (-1) omitted. Year "0" is the first treatment year of a grant. Due to computational constraints, these graphs were produced using data collapsed to the school district-grade-year level and weighted by the number of students within cells. The regressions correspond to the fully specified model in column (4) of Table 4. I include treatments in 2000 to 2007, so that each centered treatment year has at least one year of observed pre- and post-treatment data.

Figure 3: CIS grant effects on long-term outcomes by years of exposure

(a)



(b)



The graphs above show coefficient estimates for high school graduation and college enrollment regressions. Bars around coefficients represent a 95 percent confidence interval for each estimate with standard errors clustered at the school district level. Each estimate shows the effect of students exposed to a grant acceptance for a particular number of years between the 7th and 12th grade (with six years as maximum exposure). Each grant lasts for three years. Apply Exposure effects are also separately entered in these regressions by number of years a school district intended to receive a grant.

Table 7: CIS Grant Effects on Discipline, by Student Income and Race

	Discipline					
	Accept* Middle School	S.E.	Accept* High School	S.E.	Mean Middle School	Mean High School
A. Effects by Economic Disadvantage and Race						
Economically Disadvantaged						
Black	0.0363***	(0.0103)	0.0033	(0.0146)	0.477	0.402
Hispanic	0.0245*	(0.0096)	-0.0114	(0.0075)	0.340	0.307
White	0.0181**	(0.0066)	-0.0026	(0.0056)	0.352	0.307
Other Race	0.0252**	(0.0078)	0.0107	(0.0069)	0.186	0.169
Not Economically Disadvantaged						
Black	0.0348***	(0.0076)	0.0040	(0.0099)	0.334	0.288
Hispanic	0.0154*	(0.0073)	-0.0024	(0.0087)	0.245	0.238
White	0.0069+	(0.0039)	0.0051	(0.0043)	0.159	0.159
Other Race	0.0107*	(0.0044)	0.0042	(0.0044)	0.099	0.101
Observations	16,285,405					
All Accept Coefficients Equal	*Middle School*		*High School*			
F-Test	3.098		1.355			
P-Value	0.003		0.221			
Accept Race Coefficients Equal:						
Economically Disadvantaged						
F-Test	1.309		1.998			
P-Value	0.270		0.113			
Not Economically Disadvantaged						
F-Test	5.257		0.442			
P-Value	0.001		0.723			
Accept Income Coefficients Equal:						
Black						
F-Test	0.031		0.009			
P-Value	0.861		0.925			
Hispanic						
F-Test	1.797		1.914			
P-Value	0.180		0.167			
White						
F-Test	4.821		2.597			
P-Value	0.028		0.107			

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$

This table shows treatment effects interacted with student economic disadvantage and race. Robust standard errors are clustered at the school district level in all regressions. Treatment effects are separated by demographic group, with no omitted treatment category, for ease of comparison. The group mean shows the mean of the outcome for students in a particular group. Economic disadvantage is an indicator for whether a student qualifies for free or reduced price lunch. These regressions also include indicators for each student race by income group, year and grade fixed effects, district by grade enrollment counts and controls for student gender and status as Limited English Proficiency (LEP), gifted and talented, or special education. The models also include year and district fixed effects that are interacted with student race and income groups. These indicators allow the trends in discipline to vary across student demographic groups over time and allow each school district to have systematic differences in student discipline across student demographic groups.

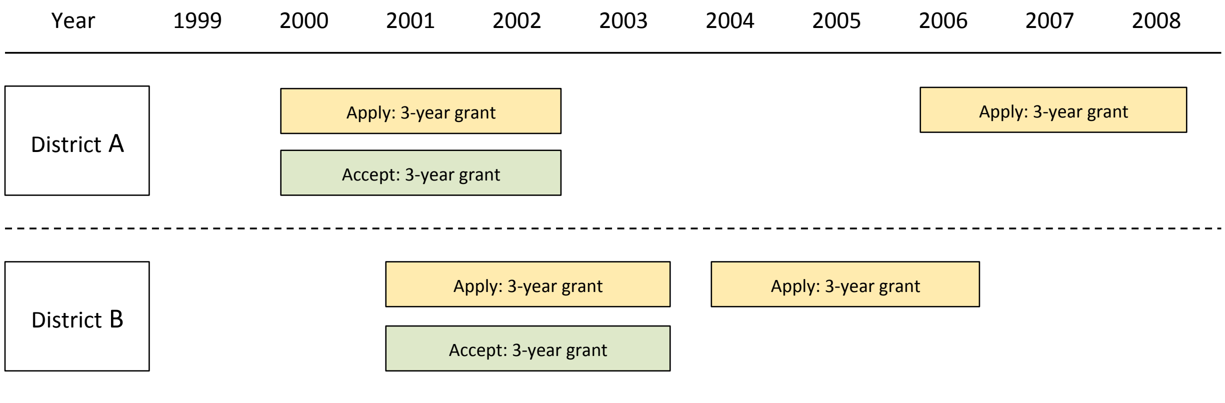
Table 8: CIS Grant Effects on Long-Term Academic Outcomes by Student Income and Race

	High School Graduation			***College Enrollment***		
	Accept Exposure	S.E.	Group Mean	Accept Exposure	S.E.	Group Mean
A. Effects by Economic Disadvantage and Race						
Economically Disadvantaged						
Black	0.0063	(0.0228)	0.593	-0.0386*	(0.0176)	0.368
Hispanic	-0.0327*	(0.0135)	0.624	-0.0397***	(0.0103)	0.334
White	-0.0387*	(0.0157)	0.579	-0.0340**	(0.0130)	0.301
Other Race	0.0003	(0.0268)	0.719	0.0014	(0.0325)	0.556
Not Economically Disadvantaged						
Black	-0.0262	(0.0177)	0.739	-0.0204	(0.0244)	0.560
Hispanic	-0.0271+	(0.0144)	0.751	-0.0185	(0.0142)	0.540
White	-0.0289**	(0.0093)	0.801	-0.0196	(0.0121)	0.627
Other Race	-0.0029	(0.0239)	0.808	0.0188	(0.0245)	0.649
Observations	2,506,154			2,506,154		
All Accept Coefficients Equal						
F-Test	0.855			1.109		
P-Value	0.542			0.355		
Accept Race Coefficients Equal:						
Economically Disadvantaged						
F-Test	1.459			0.558		
P-Value	0.224			0.643		
Not Economically Disadvantaged						
F-Test	0.439			0.816		
P-Value	0.725			0.485		
Accept Income Coefficients Equal:						
Black						
F-Test	2.096			0.706		
P-Value	0.148			0.401		
Hispanic						
F-Test	0.151			1.947		
P-Value	0.697			0.163		
White						
F-Test	0.508			0.766		
P-Value	0.476			0.382		

*** p<0.001, ** p<0.01, * p<0.05, + p<0.1
This table shows treatment effects interacted with student economic disadvantage and race. Robust standard errors are clustered at the school district level in all regressions. Models are estimated for cohorts of 7th graders, with observations at the student level. Treatment effects are separated by demographic group, with no omitted treatment category, for ease of comparison. The group mean shows the mean of the outcome for students in a particular student group. Economic disadvantage is an indicator for whether a student qualifies for free or reduced price lunch. These regressions also include indicators for each student race by income group, year fixed effects, district cohort enrollment counts and controls for student gender and status as Limited English Proficiency (LEP), gifted and talented, or special education. The models also include year and district fixed effects that are interacted with student race and income groups. These indicators allow the trends in discipline to vary across student demographic groups over time and allow each school district to have systematic differences in student discipline across student demographic groups.

A1 Appendix Tables and Figures

Figure A1: Depiction of Model Identification, Comparison of Two Hypothetical Districts



$$\begin{aligned} ShortTermOutcome_{igdt} \;=\;& \beta_{1m} Accept_{dt} * MiddleSchool_{gt} + \beta_{2m} Apply_{dt} * MiddleSchool_{gt} \\ & + \beta_{1h} Accept_{dt} * HighSchool_{gt} + \beta_{2h} Apply_{dt} * HighSchool_{gt} \\ & + \pi X_{igdt} + \delta_t + \gamma_g + \phi_d + \varepsilon_{igdt} \end{aligned}$$

$\boldsymbol{B_{2m,2h}}$ **:** This is the application control. It nets out the effect of district application choices on outcomes. “Apply” measures the time-varying unobservable effect of a district that is interested in funding. The coefficient compares “Apply” years to “No Apply” years. This coefficient also descriptively shows how outcomes change when districts are rejected for grants. In this example, the application control is identified from the average difference in outcomes from 2006-2008 to 1999 and 2003-2005 within District A, and the average difference from 2004-2006 to 1999-2000 and 2007-2008 within District B.

$\boldsymbol{B_{1m,1h}}$ **:** This is the conditional acceptance coefficient of interest. It compares “Accept” years to “No Apply” years, within a district. This coefficient captures the average difference in outcomes between the period 2000-2002 relative to 1999 and 2003-2005 within District A, and the difference from 2000-2003 to 1999-2000 and 2007-2008 within District B, after subtracting the estimated “Apply” coefficient.

Table A1: CIS Grant Impacts on Police Hiring in Texas, Evidence from FBI Data

Outcome=Police per 10,000 Residents	(1) Police Officer Rate	(2) Police Officer Rate	(3) Police Officer Rate	(4) SRO Rate	(5) SRO Rate	(6) SRO Rate
Accept, COPS in Schools (CIS)	1.269***	1.109*	0.0274	0.627	1.653**	0.262+
	(0.363)	(0.461)	(0.196)	(0.417)	(0.578)	(0.137)
Apply, COPS in Schools (CIS)		0.239	0.188		-1.101*	-0.112
		(0.331)	(0.150)		(0.480)	(0.113)
Year FE	X	X	X	X	X	X
Agency FE	X	X	X	X	X	X
Covariates	X	X	X	X	X	X
Application Controls		X	X		X	X
Weighted by Number of Students			X			X
Observations	8,062	8,062	380,886,900	468	468	644,570
Number of Agencies	737	737	737	136	136	136
Mean: Resident Population	47,244	47,244	960,983	137,730	137,730	871,265
Mean: Student Population	5,153	5,153	77,587	12,550	12,550	42,819
Mean: Y	23.06	23.06	9.722	4.043	4.043	0.294
Mean: Accept, CIS	0.0542	0.0542	0.0723	0.0598	0.0598	0.0869
% Change in Y from Accept, CIS	5.5%	4.8%	0.3%	15.5%	40.9%	89.1%

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$
This table uses data from the Uniform Crime Reports (UCR) published by the Federal Bureau of Investigation (FBI) from 1997 to 2008. The analysis adapts the framework of Owens (2017) to the model and sample in this paper. Robust standard errors clustered at the police agency/department or municipality/Census Place are in parentheses.The dependent variable in all columns is number of police officers per 10,000 residents in a municipality/Census Place. Columns (1) to (3) use police employment information recorded annually from the FBI Law Enforcement Officers Killed in Action (LEOKA) survey. Columns (4) to (6) examine rates of SRO officers calculated using the Law Enforcement Management and Administrative Statistics (LEMAS) survey for a smaller sample of police departments in 1997, 1999, 2000, 2003, and 2007. The sample includes municipal, county, and special district police departments indexed to the Census Place where each department is headquartered. The panel is unbalanced because police departments do not always report data to UCR in all years. To account for reporting errors, I clean the data by calculating the ratio of police officer counts to average police officers across years for each department. I then exclude observations where this ratio is above the 99th percentile or below the 1st percentile, calculating these rankings within 8 resident population groups.

Each specification includes additional controls for other school grants from the COPS office, COPS hiring grants for traditional police hiring, and other types of COPS police grants (including technology grants, small town grants, and targeted crime grants). Columns (2), (3), (5), and (6) include comparable "Apply" variables for each type of COPS grant. All models include Census Place population and additional covariates at the Census Place by year level, calculated by weighting county data by the population proportion of a place in relevant counties. County demographic controls include the proportion of Black, Hispanic, and White residents, the proportion of total enrolled public-school students to total residents and these rates for Black, Hispanic, and White students, the proportion of high school graduates to total residents, the pupil to teacher ratio, and the local school district revenue dollars per resident. Data on county population and school characteristics are taken from the Census and the National Center for Education Statistics (NCES). Columns (3) and (6) are weighted by public-school student enrollment, with observation totals comparable to student-year observations.

Table A2: Impact of CIS Grants for Police on Suspensions

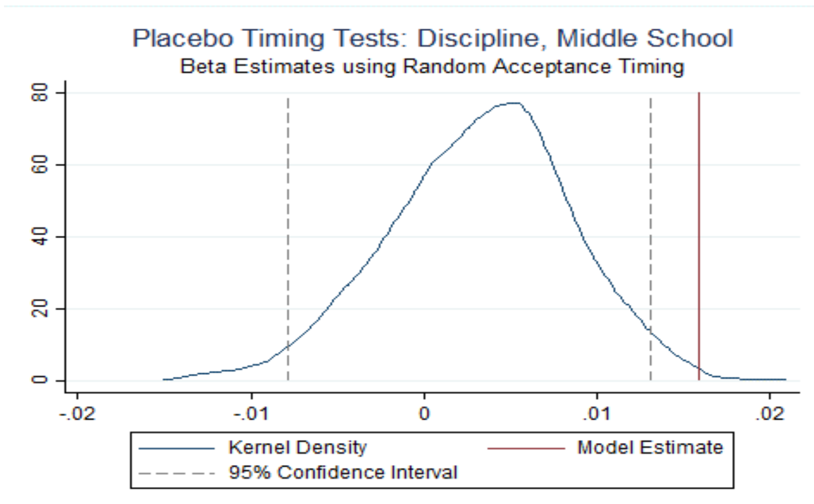
	(1)	(2)	(3)	(4)
A. Suspension (In-School)				
Accept*Middle School	0.0072	0.0077	0.0059	0.0069
	(0.0047)	(0.0048)	(0.0060)	(0.0058)
Apply*Middle School			0.0030	0.0020
			(0.0045)	(0.0044)
Accept*High School	-0.0055	-0.0054	-0.0109*	-0.0103*
	(0.0039)	(0.0038)	(0.0050)	(0.0048)
Apply*High School			0.00779*	0.00691*
			(0.0035)	(0.0035)
Y Mean: Middle School	0.238	0.238	0.238	0.238
Y Mean: High School	0.203	0.203	0.203	0.203
Accept: Middle School=High School				
F-Test	8.215	8.495	7.568	7.564
P-Value	0.0042	0.0036	0.0060	0.0061
B. Suspension (Out-of-School)				
Accept*Middle School	0.0111**	0.0107**	0.0076+	0.0088*
	(0.0040)	(0.0038)	(0.0045)	(0.0044)
Apply*Middle School			0.0049	0.0026
			(0.0049)	(0.0044)
Accept*High School	-0.00504+	-0.0037	-0.0041	-0.0023
	(0.0030)	(0.0026)	(0.0031)	(0.0028)
Apply*High School			-0.0012	-0.0020
			(0.0025)	(0.0021)
Y Mean: Middle School	0.114	0.114	0.114	0.114
Y Mean: High School	0.086	0.086	0.086	0.086
Accept: Middle School=High School				
F-Test	14.26	14.32	13.17	11.40
P-Value	0.0002	0.0002	0.0003	0.0008
Observations	16,285,552	16,285,552	16,285,552	16,285,552
Year FE	X	X	X	X
Grade FE	X	X	X	X
District FE	X	X	X	X
Student-Level Covariates		X		X
Apply Controls			X	X

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$

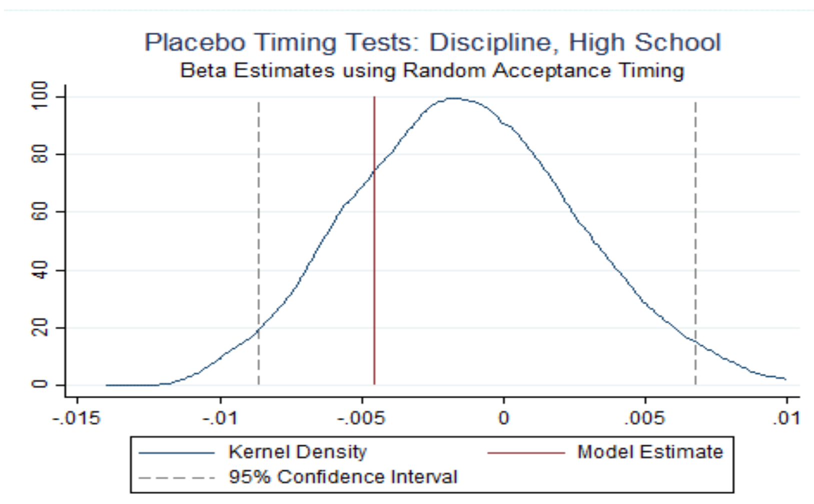
This table shows the impact of COPS in Schools (CIS) grant awards on whether a student receives an in-school or out-of-school suspension. Robust standard errors are clustered at the school district level in all regressions. Column (4) is the preferred specification used in this paper. "In-school" suspensions entail alternative education programs on a school campus, while "out-of-school" suspensions require students to be removed from school on a short-term basis. Additional grant controls for other COPS school grants ("Accept Other Grant" in columns (1) to (4) and "Apply Other Grant" in column (4)) are included but not displayed. Year FE are indicators for years from 1999 to 2008, Grade FE are indicators for grade-level (7th to 12th grade), and District FE are indicators for school district. Covariates include district grade enrollment, student gender and race, and student status as Limited English Proficiency (LEP), economically disadvantaged, gifted or special education.

Figure A2: Placebo Timing Tests, Discipline: Model Replications with Random Acceptance Timing

(a)



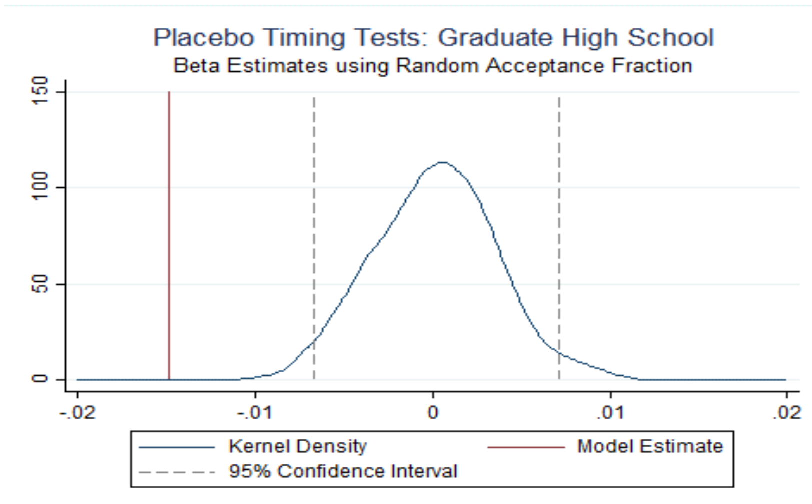
(b)



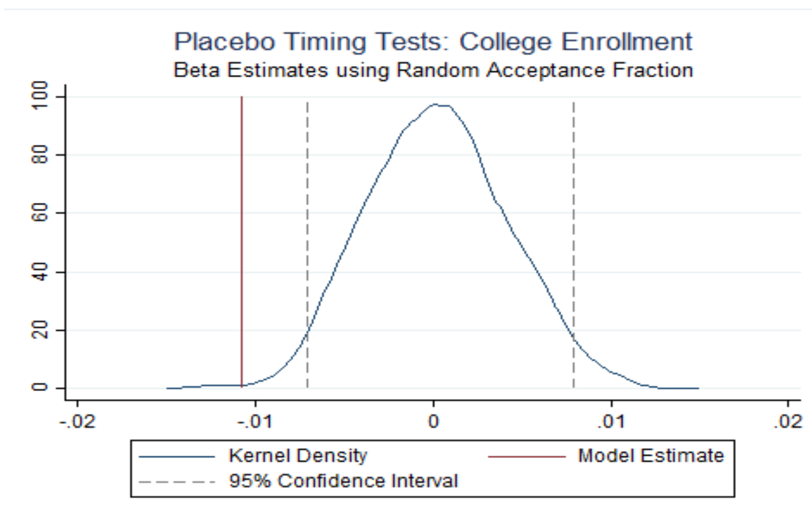
The graphs above show the results of replicating the model 1,000 times with randomized "placebo" grant acceptance timing. In each replication, acceptance treatment is randomly assigned to districts that applied for a grant in a given year, maintaining the overall acceptance rate for that year in the sample. This procedure randomizes grant acceptance conditional on district decisions to apply for grants. The actual model estimate is shown in red, while the gray dotted lines reflect the 95 percent confidence interval of the replications. Due to computational constraints, the estimates are produced using regressions collapsed to the district-grade-year level and weighted by the number of students in these cells. Both graphs are produced from the same set of regressions and plot "Accept" interacted with middle or high school grades.

Figure A3: Placebo Timing Tests, Long-Term Academic Outcomes: Model Replications with Random Acceptance Timing

(a)



(b)



The graph above shows the results of replicating the long-term sample model specification 1,000 times with randomized "placebo" acceptances. In the long-term sample, students are observed only once and grant variation is measured using "Accept Exposure." I randomly vary the level of exposure for students in placebo tests, equivalent to varying the intensity (or timing) of exposure. The replications randomly assign an acceptance exposure for each district that is less than or equal to the district's application exposure for a given cohort. In each replication, the total acceptance exposure rate is fixed to match the rate in the underlying data. The actual model estimate is shown in red, while the gray dotted lines reflect the 95 percent confidence interval of the replications. Due to computational constraints, the estimates are produced using regressions collapsed to the district-grade-year level and weighted by the number of students in these cells.

Table A3: Robustness Specifications: Discipline

		Discipline				
		Accept* Middle School	S.E.	Accept* High School	S.E.	Observations
Specification						
1	Base Model	0.0164**	(0.0055)	-0.0032	(0.0048)	16,285,552
2	Ever Applied for a CIS Grant	0.0183**	(0.0060)	-0.0016	(0.0053)	12,305,486
3	Ever Accepted for a CIS Grant	0.0193**	(0.0068)	0.0020	(0.0059)	10,468,614
4	Both Rejected and Accepted for a CIS Grant	0.0106	(0.0065)	-0.0077+	(0.0046)	7,023,544
5	With (No Apply, Accepted, Rejected, Both)*Year FE	0.0146**	(0.0055)	-0.0051	(0.0041)	16,285,552
6	With Year of Application Cohort FE	0.0172**	(0.0057)	-0.0024	(0.0047)	16,285,552
7	With Region*Year FE	0.0157**	(0.0052)	-0.0041	(0.0046)	16,280,316
8	With Student Enrollment Size Group*Year FE	0.0182**	(0.0059)	-0.0015	(0.0053)	16,285,552
9	With School FE instead of District FE	0.0130**	(0.0047)	-0.0021	(0.0047)	16,285,030
10	With Race*Income*Year FE	0.0149**	(0.0053)	-0.0058	(0.0042)	16,285,552
11	With Race*Income*(Year and District FE)	0.0160**	(0.0054)	-0.0050	(0.0044)	16,285,405
12	Enrolled through the 10th grade or higher	0.0129*	(0.0054)	-0.0012	(0.0044)	12,182,963
13	Collapsed Regression by District (Weighted)	0.0158**	(0.0050)	-0.0047	(0.0043)	14,833,772
14	With Grade*Year FE	0.0166*	(0.0070)	0.0035	(0.0053)	16,285,552
15	With Grade*(Year FE and District FE)	0.0125*	(0.00618)	0.0041	(0.0053)	16,284,882
16	Students in 1st-6th Grade	0.0009	(0.0015)	-	-	18,482,073

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$
The table above displays robustness specification tests of the findings. Robust standard errors are clustered at the school district level in all regressions. Specification (1) corresponds to the preferred model, or column (4) in Table 4. (2) restricts the sample to school districts that applied for a CIS grant during the sample period, (3) restricts the sample to districts that received a CIS grant during the sample period and (4) restricts the sample to districts that were both rejected and accepted for a CIS grant. In (5), the Year FE are interacted with 4 categories of district "grant history" in the sample period: districts that never applied for a CIS grant, districts that only had CIS grant acceptances, districts that only had CIS grant rejections, and districts that were both accepted and rejected for CIS grants. In (6), the model includes fixed effects for the year that a district first applied for a CIS grant, or "application cohort" fixed effects. Specification (7) adds region by year fixed effects, allowing the time trends to vary across 20 geographic regions specified by TEA in the data. Specification (8) adds separate year fixed effects for 9 different student enrollment population groups. In specification (9), the school district fixed effects are replaced with school campus fixed effects. Specifications (10) and (11) successively add student race and income group by year fixed effects and student race and income group by district fixed effects. Specification (12) is estimated on the sub-sample of students that remain enrolled in Texas schools through the 10th grade or higher. Model (13) shows the result of regressions that are collapsed at the school district level and weighted by student enrollment, respectively. Models (14) and (15) successively add grade by year fixed effects and grade by district fixed effects. Specification (16) shows results on student disciplinary actions for younger students in the 1st through 6th grades.

Table A4: Robustness Specifications: Long-Term Academic Outcomes

		High School Graduation			***College Enrollment***		
		Accept Exposure	S.E.	Observations	Accept Exposure	S.E.	Observations
Specification							
1	Base Model	-0.0344***	(0.0094)	2,506,849	-0.0378***	(0.0111)	2,506,849
2	Ever Applied for a CIS Grant	-0.0276**	(0.0097)	1,902,749	-0.0294**	(0.0109)	1,902,749
3	Ever Accepted for a CIS Grant	-0.0392***	(0.0117)	1,625,058	-0.0387**	(0.0131)	1,625,058
4	Both Rejected and Accepted for a CIS Grant	-0.0672***	(0.0147)	1,091,097	-0.0580***	(0.0155)	1,091,097
5	With (No Apply, Accepted, Rejected, Both)*Year FE	-0.0382**	(0.0132)	2,506,849	-0.0381**	(0.0132)	2,506,849
6	With Year of Application Cohort FE	-0.0344***	(0.0092)	2,506,849	-0.0341***	(0.0098)	2,506,849
7	With Region*Year FE	-0.0282**	(0.0088)	2,506,849	-0.0251**	(0.0081)	2,506,849
8	With Student Enrollment Size Group*Year FE	-0.0253**	(0.0096)	2,506,849	-0.0365***	(0.0106)	2,506,849
9	With School FE instead of District FE	-0.0348***	(0.0096)	2,506,630	-0.0282**	(0.0087)	2,506,849
10	With Race*Income*Year FE	-0.0252**	(0.0088)	2,506,154	-0.0275**	(0.0084)	2,506,154
11	With Race*Income*(Year and District FE)	-0.0256**	(0.0089)	2,506,154	-0.0362**	(0.0111)	2,506,630
12	Enrolled through the 10th grade or higher	-0.0239**	(0.0089)	1,755,159	-0.0185+	(0.0112)	1,755,159
13	Collapsed Regression by District (Weighted)	-0.0356***	(0.0100)	2,506,498	-0.0392***	(0.0116)	2,506,498

*** $p<0.001$, ** $p<0.01$, * $p<0.05$, + $p<0.1$
The table above displays robustness specification tests of the findings. Robust standard errors are clustered at the school district level in all regressions. Specification (1) corresponds to the preferred model, or column (4) in Table 4. (2) restricts the sample to school districts that applied for a CIS grant during the sample period, (3) restricts the sample to districts that received a CIS grant during the sample period and (4) restricts the sample to districts that were both rejected and accepted for a CIS grant. In (5), the Year FE are interacted with four categories of district "grant history" in the sample period: districts that never applied for a CIS grant, districts that only had CIS grant acceptances, districts that only had CIS grant rejections, and districts that were both accepted and rejected for CIS grants. In (6), the model includes fixed effects for the year that a district first applied for a CIS grant, or "application cohort" fixed effects. Specification (7) adds region by year fixed effects, allowing the time trends to vary across 20 geographic regions specified by TEA in the data. Specification (8) adds separate year fixed effects for 9 different student enrollment population groups. In specification (9), the school district fixed effects are replaced with school campus fixed effects. Specifications (10) and (11) successively add student race and income group by year fixed effects and student race and income group by district fixed effects. Specification (12) is estimated on the sub-sample of students that remain enrolled in Texas schools through the 10th grade or higher. Model (13) shows the result of regressions that are collapsed at the school district level and weighted by student enrollment, respectively.