School Finance Reforms and Juvenile Crime

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Several states initiated school finance reforms during the post-1990s, commonly named the "adequacy" era, with the primary purpose of providing adequate funding for low-income school districts. This article uses the space–time variation in court-ordered reforms in this period as shocks to school spending and investigates its effects on juvenile arrest rates and risky behaviors. Using a 2SLS-DDD approach and a wide range of data sets, I find that exposure to reform reduces the juvenile arrest rates, increases the likelihood of high school graduation, increases the time spent on educational activities, and reduces risky behaviors at schools. A 10% rise in real per-pupil spending is associated with 7.4 fewer arrests per 1,000 in the population aged 15–19. This rise is equivalent to a reduction of roughly 90,806 arrests annually. It also implies a minimum of 20% return in school spending due to the avoided costs of deterred crimes. (*JEL*: H72, I22, I24, K42)

1. Introduction

Crime has been one of the most controversial and debated issues in the U.S. policy environment. The number of inmates was roughly 700 per 100,000 persons in 2014, which gives the United States the highest rate of

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inmates per capita among OECD countries[1] (UNODC, 2019). The annual average per-capita cost of state-correction-institution inmates was $33,274 in 2015. Among states above the 75th percentile, per inmate cost was, on average, $59,454 (Mai and Subramanian, 2017). The social and political concerns inspired by these figures have generated a large literature that analyzes the causes and consequences of crime and often proposing possible channels to reduce crime. There is now convincing evidence that education has a crime-reducing effect. Lochner and Moretti (2004) find that roughly 94,000 fewer crimes would have occurred if high school graduation rates had increased by 1% in 1990. They use changes in compulsory schooling laws as a plausibly exogenous shock to education and show that the marginal social benefit of male high school graduation due to the reduction in crime rates was about $1,170–$2,100 in 1990. They estimate that the private return of high school graduation amounts to $8,040. Therefore, the social gain is between 14% and 26% of the private return to high school graduation. Other studies examine the education–crime relationship using other exogenous sources of variation for education, such as the minimum dropout age (Anderson, 2014), school starting age (Landersø, Nielsen, and Simonsen, 2016), schooling time (Cabus and De Witte, 2011), expansion of upper secondary schools (Åslund, Grönqvist, Hall, and Vlachos, 2018), public school choice lotteries (Deming, 2011), and more. Since educational levels among prison inmates in the United States are relatively low,[2] these results provide an attractive alternative to traditional criminal justice policies aimed to reduce crime. The estimated relationships these studies document are reasonably comparable and even more effective at reducing crime than conventional methods such as the militarization of the police force Bove and Gavrilova (2017).

Historically, the primary source of public school funding in the United States has been local property taxes. The close tie between spending on

1. The second country is Estonia with approximately 290 inmates per 100k population, less than half of the United States per capita inmates. The last thirteen countries, sorted based on inmates per capita, have inmates per capita less than 100, almost one-seventh of that of the United States.

2. Only about 4.4% of inmates at the correctional institutions had any college degree in 2015. In 1997, about 70% of US inmates had less than a high school degree (Heckman and LaFontaine, 2010).

public education and local property values can enforce and facilitate the inequalities in educational attainments as residents of poorer areas, who are likely lower educated, face more insufficient funding for public education. Scholars have recognized this structural problem in the early part of the twentieth century as a financial source of inequality in education (Del Plaine, 1920; Edwards, 1946). One possible solution was for states to increase their share in financing public schools and loosening the tie between local wealth and public education funding. However, it was not until the 1970s that the citizens and state authorities turned their attention to inequality in school funding. Their push for a structural change and demand for equalization resulted in a series of court-ordered and legislative reforms in the way public schools were funded. The subsequent mandates that emerged from court orders and state legislations are referred to in the literature as state-level School Finance Reforms (SFR). These reforms are intended to raise funds for schools and reduce the educational gap across school districts within states. Two main types of these reforms include the "equity SFR" during the 1970s and 1980s that targeted the resource inequalities across districts and the "adequacy SFR" during the 1990s with the primary purpose of providing sufficient funding for lower-income districts to boost educational opportunities regardless of equity concerns. Jackson, Johnson, and Persico (2016) provide evidence of the effectiveness of reforms during the "equity" period. They exploit the timing of the passage of court-mandated reforms and the type of funding formula as exogenous shocks to school spending and explore the effects of reforms on education and labor market outcomes. They show that a 10% increase in average real per-pupil spending during K-12 years of education is associated with 7% higher wages and a 3.2 percentage points reduction in the likelihood of adult poverty. Although there is an established literature on the relationship between school spending and education, most studies focus on pre-1990 SFRs or individual reforms. Surprisingly, few studies explore the effect of post-1990 SFRs on juvenile outcomes, including juvenile crimes.

This article aims to fill this gap in the literature by investigating the effect of differential increases in school spending during the post-1990s, also known as the *adequacy era*, on juvenile crime rates. The main target of these reforms was to provide sufficient funding for public schools while mitigating the gap in school spending of poor and rich districts. Since the timing and location of these court-ordered reforms were arguably orthogonal to state characteristics, the space–time variations in reforms provide plausibly exogenous shocks to school spending. These reforms were successful in increasing real per-pupil expenditures and lowering educational funding disparities within a state.

I find that a 10% increase in school spending on K-12 education, i.e., a $1,183 (in 2017 dollars) rise from the mean, reduces the total arrest rates of individuals aged 15–19 by 7.4 arrests per 1,000 populations of individuals in that age group. The effects are statistically and economically significant for most major crime categories, including property, violent, and simple crimes. How might school spending affect juvenile crime rates? First, I explain this relationship using a simple life-cycle framework of heterogeneous agents. Agents first choose between education and leisure; later, they decide between criminal activities and the legal labor market. School spending can change the relative wages of high- versus low-educated individuals and, through this channel, reduce their propensity to engage in crime. Second, I show that cohorts exposed to SFRs spend more time on education, have a higher probability of high school graduation, are more likely to enroll in college, and reveal less risky behaviors in school. Therefore, the reforms appear to increase both the quality and quantity of time invested in human capital as an alternative to street capital.

Evaluating potential externalities of school spending has important policy implications for educational policy design. If an optimum level of school spending is computed based on the cost–benefit principle, where the additional social cost of the funding, i.e., its tax burden, is balanced by the extra benefit it provides to society, then not only the direct benefits in the form of educational outcomes but the benefits beyond these immediate effects and even beyond the realm of education must be taken into account. Not taking these positive externalities into account implies that the spending will be below the socially optimal point. Examining the link between school spending and crime is also informative for policies designed to reduce crime. It offers a crime-prevention strategy as an alternative to traditional criminal justice measures such as an increase in sentencing severity.

This article contributes to the literature on education and crime in two ways. First, to the best of my knowledge, this is the first study to link school spending to juvenile crime.[3] Second, juvenile crime is very costly to society. In 2017, costs associated with juvenile crimes were estimated to be over \$170 billion, roughly 1% of U.S. GDP (FBI, 2017; Miller, Cohen, Swedler, Ali, and Hendrie, 2021).[4] The results of this article suggest a potential channel to fight crime via the reallocation of resources to strategies that indirectly prevent crime. Compared to other educational promotion policies, such as raising the minimum dropout age, the advantage of this channel is that it does not displace the problems to schools; instead, it reduces a juvenile's propensity to commit a crime.

The rest of the article is organized as follows: in Section 2.1, I review the literature on education and crime and studies that assess the short-term and long-term effects of school finance reforms. Section 2.2 provides a short history of school finance reforms in the United States. In Section 3, a simple theoretical model is developed to show the economic link between school spending and juvenile crime rates. Sections 4.1 through 4.3 provide empirical methods, data sources, and summary statistics. The main results are reported and discussed in Section 4.4. Section 5 discusses several endogeneity concerns. Section 6 empirically investigates the mechanisms that link school spending to juvenile crime, checks other outcome variables, and examines possible alternate mediating channels. Finally, conclude with a discussion on the implication of the results in Section 7.

2. Background

2.1. A Brief Literature Review

School finance reforms during the post-1990s arguably increased educational spending and alleviated inequality in school spending in affected states. Increases in state funding for public schools may affect students'

3. Bailey, Goodman-Bacon, Miller, Ludwig, Johnson, and Jackson (2019) show that head start spending and spending on K-12 education improve educational outcomes and earnings during adulthood. They also show that educational spending reduces the probability of being incarcerated during ages 20–50. In contrast, I study different outcomes, different cohorts, different shocks, different data, and different age groups.

4. This number is calculated by combining the age-specific offense reports of FBI (2017) for the United States and the direct and intangible cost estimations (Miller, Cohen, Swedler, Ali, and Hendrie, 2021).

test scores and academic achievement. Several studies explore these potential effects (Card and Payne, 2002; Dee, 2005; Chaudhary, 2009; Glenn, 2009; Springer, Liu, and Guthrie, 2009; Neymotin, 2010; Roy, 2011; Sherlock, 2011). For instance, Lafortune, Rothstein, and Schanzenbach (2018) show that school finance reforms were successful in increasing absolute and relative spending in school districts with lower initial spending. The reforms increased school resources in several ways. They raised real per-pupil spending, teachers' salaries, the teacher–pupil ratio, and total capital outlay. Through these channels, the reforms improved the test scores of students in low-income school districts. Their estimation of the link between academic performance and school resources are economically significant. The test score gap between low-income districts and the state average closes by 0.01 standard deviation each year in the years following the reforms.

Relevant to the current study, some papers focus on relatively longer-run outcomes such as educational attainment, college enrollment, and income mobility. Hyman (2017) exploits the 1994 Michigan school finance reform and the variation across districts based on its funding formula as the shock to school spending. He finds that a $1,000 increase in per-pupil spending during secondary school increases the probability of postsecondary enrollment and postsecondary degree receipt by 3.9 and 2.5 percentage points.

Candelaria and Shores (2019) examine the relationship between school spending and graduation rates. They use court-ordered SFRs between 1989 and 2010 to account for the endogeneity issues in spending and find that a 10% rise in per-pupil spending is associated with 5.06 percentage points increase in graduation rate among high-poverty districts.

Using data of 13 reforms across 20 states over the years 1980–2004, Biasi (2019) re-establishes the fact that SFRs reduced the spending gap across districts. Using a simulated instrument based on funding formulas, she documents that the increased per-pupil spending has a positive and sizeable effect on intergenerational mobility. The income rank of children with parents at the bottom quantiles of income increases as the expenses become more equalized. However, she finds little evidence that equalization affected students' college enrollment.

The main mediatory channel between school spending and crime is education. A growing body of literature in economics, education, and criminology examines the crime-reducing effect of education. The empirical evidence is sometimes inconclusive. Some cross-sectional standard regressions find no effect of education on crime. A recent wave of studies attempts to solve the endogeneity problem of education by introducing instrumental variables.[5] This literature documents a sizable crime-reducing effect of education.[6]

Grogger (1998) shows that variation in real wages can significantly explain crime rates. He posits that falling wages during the 1970s and 1980s were responsible for rising youth crime in this period. However, after controlling for the real wage rate, years of schooling do not significantly affect crime. Since human capital is one crucial determinant of wage and earnings, one can interpret the findings as to the indirect effect of education on crime through labor market wages.

Fella and Gallipoli (2014) provide a theoretical life-cycle model where individuals choose between education and crime. They show that education decreases crime by increasing the opportunity cost of crime. They calibrate the model with U.S. data and predict the social benefits of two policy-driven interventions: increasing prison sentences and subsidizing high school education. Both policies reduce crime. However, for the same crime reduction, a high school subsidy has higher efficiency and social gain, while this externality is absent in the case of increasing the prison sentence.

Anderson (2014) constructs a causal path between education and contemporaneous juvenile arrest rates using the space–time variation of mandatory minimum dropout age as an exogenous shock to education. He estimates that male individuals aged 16–18 who are exposed to a minimum dropout age of 18 have, on average, 10.27 fewer arrests per 1,000 of their age group population. This effect is negative for all categories of crime. Exposure to a minimum dropout age of 18 among 16- to 18-year-old male cohorts is

5. Refer to Lance (2011) for a review.

6. See, for example, Aizer and Doyle Jr. (2015), Akee, Halliday, and Kwak (2014), Åslund, Grönqvist, Hall, and Vlachos (2018), Bahrs and Schumann (2019), Beatton, Kidd, Machin, and Sarkar (2018), Bell, Costa, and Machin (2016), Bennett (2018), Brugård and Falch (2013), Buonanno and Leonida (2009), Campaniello, Gray, and Mastrobuoni (2016), Cano-Urbina and Lochner (2019), Chalfin and Deza (2017), Deming (2011), Dennison (2019), Dills and Hernández-Julián (2011), Groot and van den Brink (2010), Hjalmarsson, Holmlund, and Lindquist (2015), Machin Marie, and Vujiae (2011), Mancino Navarro, and Rivers (2016), and McAdams (2016).

associated with 3.2 fewer incidences of property crime, implying roughly 17,000 fewer property crimes in 2008.

Cook and Kang (2016) investigate the effect of dropping out of high school on crime rates among public school students in North Carolina. Their regression discontinuity design takes advantage of the change in the grade level of children born before and after the state-mandated cut-off age. They find that students born after the cut-off date and so eligible for a delayed entry outperformed those born just before the cut-off date. They have higher test scores in reading and math and were less likely to be detected as juvenile delinquents. Counterintuitively, the delayed entry students have higher rates of high school dropouts. They are also more likely to commit a felony offense during adulthood. They interpret these findings as a causal path from high school dropout to crime.

Cullen, Jacob, and Levitt (2006) use a lottery-based experiment to find the effect of school quality on students' test scores, academic achievement, behavior at school, as well as arrest by police. Although lottery winners choose schools that are better in a wide array of measures, their academic outcomes do not vary systematically compared to lottery losers. Interestingly, winning a lottery is associated with a lower likelihood of an arrest by the police. The results imply that schooling quality had a crime-reducing effect. Their results are in line with the findings of Deming (2011), who uses public school choice lotteries in a local school district to investigate whether an improvement in school quality could reduce crime. He finds that attending a first-choice school can reduce crime by as much as 50% among high-risk youth and diminish social costs associated with arrested crimes by roughly $30,000.

2.2. Background on School Finance Reform

It was not until the mid-19th century and through the "Common School Movement" that public education arose in the U.S. education system as a right for every citizen to earn free education regardless of their wealth, heritage, or class. However, until the second half of the 20th century, the primary source of public school funding came from local property tax. By 1920, the share of local tax in total school expenditures was as high as 83.4% versus 16.5%, the percentage of state funding (Corcoran and Evans, 2008). Voters' perspectives towards education and their eagerness to vote for parties that support property taxation and education promotion varied widely across districts. Lower-income districts that paid lower taxes, or voted for a more tax-based system, also had lower educational spending per pupil. The resulting dispersion in spending generated inequality in the quality of public education across different districts within a state that was primarily rooted in the school finance structure.

California was the first state to announce that such disparities in spending contradict the state's constitution. Starting from the California *Serrano I ruling* (1971), a series of state-mandated or court-ordered school finance reforms were initiated across U.S. states. The purpose of these reforms was to fill the spending gap across school districts and narrow down the inequality in public education expenditures within the state. A strand of literature focuses on the effect of these reforms on the levels and variations of spending and student outcomes at either the national-level or individual states (Figlio, Husted, and Kenny, 2004; Picus, Odden, and Fermanich, 2004; Toutkoushian and Michael, 2008; van Beurden, 2011).

Generally, there have been two categories of reforms across U.S. states. The first wave of SFRs started during the 1970s and 1980s. These so-called *equity* SFRs targeted the spending disparities across school districts. The second wave was initiated in Kentucky in 1990 when a supreme court declared insufficient and inequitable school funding across Kentucky's public schools. The legislative response was to pass the Kentucky Educational Reform Act (KERA). The reform successfully reduced the spending gap by increasing the expenditures in low-income districts. However, it did not have a significant effect on students' achievement gap (Clark, 2003; Welsh, Petrosko, and Taylor, 2006).

The primary goal of SFRs during the 1990s and 2000s was to allocate sufficient funding for low-income districts and schools in disadvantaged areas whether or not it resulted in more equitable spending. Hence, the second wave of reforms is referred to as the *adequacy era* reforms. Lafortune, Rothstein, and Schanzenbach (2018) show that court-ordered SFRs between the years 1990 and 2011 were successful not only in reducing inequality in per-pupil spending but also in reducing the gap in students' achievement. They identify sixty-four school finance reforms, among which court orders commenced thirty-nine. Using an event study, they distinguish different

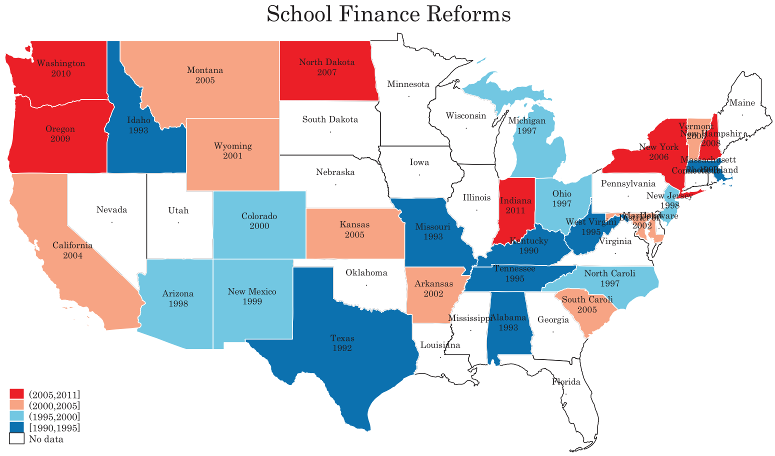

Figure 1. School Finance Reform among U.S. States, 1990–2014
Notes: The data and the timing of the most effective SFR come from Lafortune, Rothstein, and Schanzenbach (2018).

reforms based on their impact on school spending that could produce a major change.

A summary of the states with the most impactful SFR over the *adequacy era* is depicted in Figure 1. There are thirty states with a court-ordered SFR. Sixteen states had their major SFR during the 1990s. The last of these SFRs were House Bill at Indiana, which established student teaching stipend among minorities and disadvantaged fields and transferred up to \$150 million to state tuition reserve funds on a biannual basis.

3. A Simple Conceptual Framework

A theoretical framework can help us understand the economic forces that link school spending with juvenile crime. I use a two-period model in which agents are heterogeneous in their innate ability. The measure of ability, θ, is uniformly distributed on the interval $(\underline{\theta}, 1]$. The population is normalized to 1. In the first period, agents decide to spend time on educational activity or leisure. Education can take a value from the set $\{L, H\}$. Agents can attain educational level H or stay at educational level L and obtain utility from leisure. Utility from leisure is represented by l. The disutility of studying is an increasing function of the ability-independent cost of studying, d, such as the distance to commute to school, tuition, etc. It is also a decreasing function of agents' ability. I assume that the disutility is a linear function of the two factors and is given by $d - \log\theta$. In the second period, the labor supply of all agents is inelastic and normalized to one, so $L^s = 1$. The wages in the labor market can vary based on the interaction of agents' education, innate ability, and the quality of their education. The wage of educated persons is given by $w^H\theta q(S)$, where $q(S)$ is the quality of education. By assumption, $q'(\cdot) > 0$, which implies that the increase in per-pupil school spending, S, can increase the quality of education. Since low-educated agents did not attend schools, this term does not appear in their wages. Hence, their wage equation lacks the schooling quality function and is given by the term $w^L\theta$. Agents can also engage in criminal activities and gain utility while suffering from a perturbation component. If we assume an additively separable utility function, the return on criminal activity can be represented by φ, which has a uniform distribution on the interval $[-\bar{\varphi}, \bar{\varphi}]$. In case of committing a crime, agents confront the possibility of apprehension, with probability q, and enjoy prison consumption $\bar{c}$. There are no capital markets, and agents consume all their income in the second period. The production technology is $Y = AH_L^{\propto}H_H^{1-\propto}$, where H_L and H_H are the stock of low-educated and high-educated agents, respectively. Besides, the discount factor is β.

In the second period, low-educated agents face a utility maximization problem as follows:

$$U_2^L(\theta) = \text{Max}\left\{\log\left(w^L\theta\right), \left[(1-q)\left[\varphi + \log\left(w^L\theta\right)\right] + q\log\bar{c}\right]\right\}. \quad (1)$$

In the same manner, high-educated agents solve:

$$U_2^H(\theta) = \text{Max}\left\{\log\left(w^H\theta q(S)\right), \left[(1-q)\left[\varphi + \log\left(w^H\theta q(S)\right)\right] + q\log\bar{c}\right]\right\}. \quad (2)$$

In these formulations, consumption is logarithmic. If agents commit a crime, they are arrested with probability q and consume only prison consumption. They can enjoy criminal activity return and their labor wage only if they are not arrested. In case of not committing a crime, they consume their wages without uncertainty. Whether or not they engage in criminal activity, the utility of high-educated agents is an increasing function of schooling quality. The decision of both types of agents to commit crime depends on the return to criminal activities.

School spending can increase the quality of educated people and so increase the wage premium. Through this channel, it diminishes the share of uneducated people. As a result, the wage of uneducated people becomes relatively lower, while the relative wage of educated agents increases. This fact reduces the opportunity cost of crime for unskilled agents. Therefore, school spending boosts the share of educated people, among which the opportunity cost of crime is higher, and, through this channel, reduces criminal activities. The following remark summarizes these facts.

Remark 1 School spending is negatively correlated with the share of uneducated agents, i.e., $\frac{\partial \theta^*}{\partial S} < 0$. The crime rate is positively associated with the share of uneducated agents, i.e., $\frac{\partial C}{\partial \theta^*} > 0$. The school spending discourages the share of criminal agents, i.e., $\frac{\partial C}{\partial S} < 0$.

An increase in school spending decreases the share of uneducated individuals. The reduction in the percentage of unskilled individuals is associated with lower crime rates. Therefore, the response of the equilibrium measure of crime to school spending can be computed from the product of $\frac{\partial \theta^*}{\partial S}$ and $\frac{\partial C}{\partial \theta^*}$.

This simple framework can illustrate how an exogenous shock to school spending discourages crime by changing relative wages and reducing the share of uneducated people. Appendix A provides proof of the remark.

In this setting, the increase in spending improves the quality of education.[7] The better educational quality signals a higher wage premium for the next period. The expected higher wages encourage agents to increase the quantity of education. In the next period, the higher opportunity cost of educated agents discourages them from engaging in crime. Therefore, the framework offers a channel through which spending on K-12 education can

7. Any effect on the quantity of education, like increase in school days, passes through the quality function q (.)

affect crime rates. Section 6.1 explores these potential channels empirically and confirms the model predictions.

4. Data, Econometric Method, and Results

4.1. Empirical Strategy

The main challenge in establishing a causal link between school spending and individual outcomes is that the spending is at the discretion of the school districts and that there are unobservable factors that differentiate the low-funded districts from the high-funded districts. One can control for the fixed differences between the districts and the sweeping changes across districts by including the fixed effects of space and time, but the time-varying differences pose a threat to the causal links.[8] To overcome this issue, I take advantage of the plausibly exogenous variation in the timing of court mandates that initiated SFRs in 30 states from 1990 to 2015. The existence of other legislative reforms, several reviewing steps, and the overall length of the bureaucratic and judicial process provides a semirandomness in the *timing* of the court orders. In addition, the conscientious discretion of a judge may also establish a court order that leads to an SFR in a specific state. Meanwhile, a judge in another state may interpret relatively the same constitutional wording differently and avoid a reform mandate. Such differences in the judges' opinions regarding the constitutional basis of school funding are unlikely to be correlated with states' characteristics that drive the juvenile crime rates and provide a semirandom variation in the *location* of the reforms.

The plausible randomness in the *timing* and *location* of reforms makes the changes in spending orthogonal to the juvenile crime rates. Therefore, I expect that conditional on fixed effects, covariates, and trends, the timing of SFRs is not correlated with unobservable determinants of juvenile crime. The exogeneity of the space-time variation of reforms satisfies the exclusion restriction and generates a causal path between school spending and the outcome variables of interest.

8. Since individual outcomes are at the county level, I aggregate the spending into the county level. However, the same concern over endogeneity can be true for the county aggregates and OLS estimates could be biased for the same reasons.

The underlying assumption behind the empirical strategy is that the criminal behavior of birth cohorts that were exposed to reforms follows the same path and is determined by the same influences as cohorts that were not exposed except for the fact that they experienced a sharp rise in school spending. The empirical strategy compares the outcomes of birth cohorts that were affected by the SFR-induced raise in school spending, either fully or partially, to those birth cohorts that were not (first difference), in low-income to high-income counties (second difference), and over the years (third difference). The second difference reflects the fact that counties within states had different dosages in capturing the SFR-induced spending based on their income rank within the state. I operationalize the quantitative relationship between per-pupil school spending and county crime rates by using 2SLS-DDD regressions of the following forms:[9]

$$\log\left(\overline{\mathrm{PPE}}_{5-17}\right)_{bcst} = \alpha_1 \mathrm{Exp}_{bs} + \alpha_2 \mathrm{Exp}_{bs} \times \mathrm{Dosage}_c + \zeta_c \times t + \gamma_b + \vartheta_{st} + \varepsilon_{bcst} \quad (3)$$

$$\mathrm{ArrestRate}_{bcst} = \delta \log(\widehat{\overline{\mathrm{PPE}}_{5-17}})_{bcst} + \eta_c \times t + \pi_b + \sigma_{st} + \xi_{bcst}, \quad (4)$$

where b indexes the birth cohort, c the county, s the state, and t the year.

The parameters γ and π are matrices of birth cohort fixed effects for the first- and second-stage equations, respectively. To account for county-specific secular changes in the outcomes, I include an interaction between county fixed effects (represented by ζ and η in the first and second stage, respectively) and a linear time trend (t). To absorb other state-level policies that could take place in different years for different states, I also include state-year fixed effects, represented by ϑ and σ in the first- and second-stage regressions, respectively. Finally, ε and ξ are disturbance terms of the two equations. I cluster the standard errors at the county level to account for the serial correlations in the error terms. I also cluster standard errors at the state-year level to account for spatial correlations.[10]

9. This is a modification of the empirical strategy used by Jackson, Johnson, and Persico (2016).

10. In addition to two-way clustering at county and state-year level, I show the robustness of the estimated coefficients to clustering at county and state level in Appendix D. Clustering at the county level reduces the standard errors and makes almost all coefficients significant. However, clustering the standard errors at the state level inflates

The variable Exp is a constructed measure of exposure of birth cohorts to the reforms, as follows: for individuals in nonreform states, it takes a value of zero. Individuals in reform states who turned 17 and older by the passage of court-ordered SFR are also unexposed ($\exp = 0$). Individuals who were 5 years old or younger at the time of the SFR experienced increased funding during all the K-12 schooling years. Therefore, Exp takes a value of 12. I denote them as fully exposed cohorts. Individuals who were between 5 and 17 years old at the time of court-ordered SFR are considered partially exposed cohorts. Their measure of exposure is the number of years remaining from the passage of SFR to the end of K-12 education when they turn age 17.

The primary purpose of the reforms was to increase not only the absolute spending but also the relative spending of school districts in the lower tail of income. Since the crime data, as explained in Section 4.2, is at the birth-cohort-year-county level, I aggregate the per-pupil school spending (weighted by district-level enrollment)[11] at the county level to be able to track the crime outcomes of each cohort in each county.[12,13] If some school districts are considered low-income in one county while others are not, these districts experience a sharper increase in their resources. In this case, the district-to-county aggregation will bias the estimations downward. However, if, on average, low-income school districts are located in low-income counties, then there is a variation in SFR-caused funding among different counties within a state. This variation is based on different weights of

the standard errors though the regressions have enough power that the effects remain significant at conventional levels.

11. In Appendix L, I show the results where the aggregation is unweighted. The effects are quite comparable to the main results of the article.

12. Appendix G shows the first stage effects at the district level. The coefficients of district-level analysis are somewhat larger than county-aggregate first-stage regressions. This fact suggests that aggregating to the county level may attenuate the overall results. This appendix also shows the first-stage coefficients for two sub-sample of counties based on their quantile in district-per-county distribution.

13. In about 48% of cases, a school district lies within a county boundary. Also, roughly 28% of districts have overlap with a neighboring county. On average, a school district boundary passes 1.8 county boundaries. We take the average of spending across districts within a county. For instance, if county X is associated with district A (matches county X perfectly), district B (lies in county X and also a neighboring county), and district C (lies on X and two other neighboring counties), I calculate the average spending (as well as the number of pupils) in A, B, and C and assign the value to county X.

counties for SFR funding allocation. Lafortune, Rothstein, and Schanzenbach (2018) show that the reforms granted additional funding for all districts within a reform state to provide adequate funding and maintain sufficient educational quality. However, low-income districts were allowed higher weights, while high-income districts were given lower weights. Therefore, the nature of the post-1990s SFR poses a different dosage of resource allocation across districts within reform states. To exploit this source of variation, I interact the variable *Exposure* with a variable that captures the relative position of the county within affected states. In so doing, the variable *dosage* is constructed based on county rank in within-state 1990 income distribution.[14] Finally, δ captures the effect of exogenous changes in per-pupil school spending on crime measures.

In some analyses in later parts of the article, individuals are observed at the state level (no county identifier). Thus, there is no within-state variation. Without the *dosage* variable, the reform-induced spending is captured by *exposure* alone. Specifically, the equations of the following 2SLS-DD form are estimated[15]:

$$\log\left(\overline{\text{PPE}}_{5-17}\right)_{bst} = \alpha_1 \text{Exp}_{bs} + \zeta_s \times t + \gamma_b + \Lambda_t + \varepsilon_{bst} \tag{5}$$

$$y_{bcst} = \delta \log(\widehat{\overline{\text{PPE}}_{5-17}})_{bst} + \eta_s \times t + \rho_b + \varphi_t + \xi_{bst} \tag{6}$$

Birth cohort fixed effects are represented by γ and ρ in the first- and second-stage regressions, respectively. Year fixed effects are included in Λ and φ in the first and second stages, respectively. State fixed effects are included in ζ and η, for the first and second stage, accordingly, and are interacted with a time trend. To show the robustness of the main results to the state aggregation, Appendix E explores the first-stage effects and the 2SLS estimates using the UCR data and primary crime outcomes.

14. This is calculated as the quartiles of within-state income distribution in 1990. Moreover, The 1990-income is based on median personal income per capita at the county-level.

15. This system of equations compares the outcomes of birth cohorts that were affected by the reform to those that were not (first difference) over the years (second difference).

4.2. Data Sources

This article uses several data sources. The arrest data come from the FBI's Uniform Crime Reporting Program (UCR) extracted from Kaplan (2018b). The data are aggregated at the county level by the age of criminals over the years 2000–18. Arrest rate is defined as the total number of incidences at each age-gender-county-year group per 1,000 county-level population of individuals aged 15–19. The minimum age that UCR reports the arrest data is 15.[16] Therefore, the sample is restricted to only individuals aged above 15. Moreover, individuals above age 19 start leaving their households and probably their county or state. To avoid this migration issue, I restrict the sample to individuals below (including) age 19. The robustness checks show that the latter age restriction does not change the significance of the estimations.

The UCR data are gathered by self-reported arrest data from roughly 23,000 agencies at the local, county, and state levels. One problem with the UCR is that not all agencies report arrests at all months of the year. However, Kaplan (2018b) follows an interpolation procedure to make the arrest reports 12-months equivalent. Like any self-reported data, UCR could contain measurement errors because different agencies use different methodologies in their report or differ by their data collection strategies. Moreover, since the arrest data do not include offenders who succeeded in avoiding being apprehended by the police, it under-reports the accurate measure of crime. These measurement errors bias the estimation if they are correlated with the likelihood of the passage of SFRs and school spending. For example, under-reporting is higher in low-income counties where usually crime rates are higher. The increase in school spending in reform states among these counties is also higher (due to the equalizing nature of reforms). This correlation could potentially bias the coefficients. However, there are reasons that this fact is less concerning. First, since the Difference-in-Difference-in-Differences (DDD) nature of the model compares reform and nonreform states, this under-reporting should be systematically different in these two

16. It also reports arrest data for individuals below age 15 in one category as *under-15*. Since I assign different values of per-pupil spending to birth cohorts based on their age, it is not possible to distinguish different cohorts in the aggregated category. Therefore, they are excluded from the sample.

groups of states to bias the results. Second, the DDD compares cohorts in the same counties who were exposed to the reform in reform states to not only similar cohorts in nonreform states but also to the unexposed cohorts in the same county. For the measurement error to be correlated with the variables, the data collection techniques and the under-reporting must change within the same counties for different birth cohorts over the years. Since I also include state-year fixed effects, if such changes in data collection and reporting are due to some state characteristics that could vary over the years, as a state-level new policy in fighting against crime or introducing new technology to all agencies to help data collection, then state-year fixed effects will absorb these errors. Moreover, county fixed effects capture any reporting convention or data collection technique that is time invariant at the county level.

Another concern with the UCR is whether arrest rates are a valid proxy for crime rates or not. One problem is that not every crime that occurred is reported to the police, and the second is that not every crime reported necessarily leads to an arrest. For example, Kilpatrick, Resnick, Ruggiero, Conoscenti, and McCauley (2007) find that only 12% of college students that are a victim of sexual assaults report it to law enforcement. Langton, Berzofsky, Krebs, and Smiley-McDonald (2012) also show that about half of the violent crimes, roughly 3.4 million victimizations annually, were not reported to the police between the years 2006–10.

As long as the under-reporting problem is time invariant, county fixed effects absorb its cross-county variation. However, this under-reporting could cause a measurement error. Since I compare different birth cohorts within a county and similar counties in other states, the measurement error can bias the estimates if the under-reporting is correlated with the likelihood of the passage of SFRs. A better approach would be to use UCR offense data. However, the offense data do not recognize the age of offenders specifically. Instead, it aggregates offense data into two categories of adults and juveniles. Since the empirical model requires distinguishing different birth cohorts (exposed, partially exposed, and unexposed), UCR arrest data are more suitable. Fortunately, there is a high correlation between arrests and offenses reported. Using county-level offense and arrest data over the years 1974–2016 (data source: Kaplan (2019)), I find the following correlations: 91% for all crimes; 88% for violent crimes (murder, manslaughter, robbery, and assault); 92% for property crimes (burglary and theft). In a full fixed effect regression that also controls for county population, one actual crime committed leads to 0.36 more arrests at the county level (se = 0.014). One additional actual violent crime committed is associated with 0.54 more arrests categorized as violent crimes (se = 0.11). Furthermore, other research also points to the fact that there is a high correlation between arrest rates and crime rates (Lochner and Moretti, 2004).

The district-level finance data are extracted from the National Center for Education Statistics annual census of school districts. It covers the years 1990–2015 in yearly frequency.[17] I use the SFR event database from Lafortune, Rothstein, and Schanzenbach (2018).[18] I deflate the school spending data by June CPI to make them into 2017 constant dollars. Then, each birth cohort in UCR arrest data is assigned county-level real per-pupil spending during the years of K-12 education. Birth cohorts above age five at the beginning of the period are excluded because they could have been exposed to the previous reforms that are not included in the educational finance data. Birth cohorts below age 17 at the end of the period are also excluded. These restrictions will leave the final sample with individuals born between 1985 and 1999.

The data on population by age and race are extracted from SEER (2019). Job flow data (job destruction rate) are built from the Quarterly Workforce Indicator database. Quarterly wage data are extracted from the Quarterly Census of Employment and Wages. The number of police employees and police officers per capita are extracted from FBI (2018). State-level expenditure on law enforcement is taken from Kaplan (2018a).

In the sensitivity analysis, I also use data from other sources, including Current Population Survey 2000–17 (source: Flood, King, Rodgers, Ruggles, and Warren (2018)), American Time Use Survey 2003–17 (source: Hofferth, Flood, and Sobek (2018)), and Youth Risk Behavior Surveillance System 2001–15 (source: CDC (2017)).

17. This period covers the main reforms during the *Adequacy Era*. However, to check whether the crime reducing effect of SFR-induced spending are specific to this period or not, I re-evaluate the main analysis during the so-called *Equity Era* as well (1970–90). The results are reported and discussed in Appendix C.

18. Since their database covers the years 1990–2011, one may be concerned with whether or not the post-2012 reforms generate a confounding effect in the estimations. In Appendix J, I replicate the main results excluding post-2012 observations. The results are quite similar to the main findings of the article.

Table 1. Descriptive Statistics of County Characteristics, 2000–18

	Reform States		Nonreform States	
	Mean	SD	Mean	SD
Log(Real PPE, 5–17) (2017 constant dollars)	9.357	0.272	9.323	0.205
Level, per-pupil spending, 5–17 ($1,000) (2017 constant dollars)	12.087	4.197	11.438	2.478
No. of reporting agencies	7.248	8.264	6.678	9.931
Police officers (per 1,000 population)	18.186	5.12	16.971	5.047
Police employees (per 1,000 population)	23.966	7.658	22.287	6.7
Covariates:	90.47	13.709	85.838	17.371
% Whites	89.97	13.708	85.037	17.374
% Blacks	8.901	12.599	13.706	17.013
% Others	2.681	5.194	2.955	5.955
% Hispanics	11.626	14.981	6.635	5.768
% Job destruction rate	7.294	2.771	7.235	2.39
Log total quarterly wage	20.892	1.706	20.758	1.628
Average weekly wage	736.915	169.66	720.8	149.649
Observations	31,878		20,713	

Notes: Data sources are explained in Section 4.2.

4.3. Summary Statistics

Table 1 provides summary statistics of county characteristics across SFR-passed states and nonreform states. The average per-pupil spending in reform states is roughly $12,087 in 2017 dollars, while for all other states is slightly lower and amounts to $11,438. Resources spent on policing are also higher in reform states. The number of agencies that report arrest data is about 7.8% higher in reform states. Besides, police officers and employees are 7.1% and 6.8% higher in reform states. The county-level share of whites is larger in reform states. Nonreform states have a higher share of blacks and a lower share of Hispanics. The economic characteristics in both states are, however, quite similar.

Table 2 reports brief statistics of arrest rates between the years 2000–18 by reform and nonreform states by gender. Arrest statistics are categorized based on different crimes: drug crimes (selling and possession of all types of illegal drugs), violent crimes (manslaughter, robbery, weapon, murder, rape, aggravated assault, and simple assault), sexual crimes (prostitution, rape, and other sexual crimes), financial crimes (gamble and embezzlement), property crimes (larceny, burglary, motor vehicle theft, other types of theft, arson), index crimes (robbery, all types of theft, all types of assault, burglary,

Table 2. Summary Statistics of Juvenile Arrest Rates Aged 15–19, 2000–18

	Reform States		Nonreform States	
	Mean	SD	Mean	SD
Male arrest rates:				
All crimes	85.291	78.788	78.751	85.365
Property crimes	15.61	15.568	14.553	18.27
Violent crimes	12.228	12.792	10.084	12.498
Index crimes	17.579	17.525	16.001	19.798
Simple crimes	22.027	27.266	19.348	28.5
Drug crimes	14.945	19.671	12.602	16.908
Financial crimes	0.138	0.708	0.162	0.726
Sexual crimes	1.711	3.098	1.755	3.209
Observations	32,527		21,620	
Female arrest rates:				
All crimes	30.586	32.163	31.069	40.215
Property Crimes	6.428	8.159	6.138	11.243
Violent Crimes	4.846	5.878	3.928	5.634
Index Crimes	6.704	8.45	6.3	11.227
Simple Crimes	7.263	10.464	7.32	13.08
Drug Crimes	3.858	6.434	3.319	6.482
Financial Crimes	0.096	0.471	0.103	0.558
Sexual Crimes	0.478	1.566	0.482	1.538
Observations	32,528		21,620	

Notes: County-level arrest data comes from the FBI's Uniform Crime Reporting Program. It covers the years 2000–18 for individuals aged 15–19. Arrest rates are arrests per 1,000 population of the respective gender-age group.

arson, murder, rape), and all other crimes (labeled as simple crimes). Arrest rates in this table are reported per 1,000 population of age-gender group where age refers to 15- to 19-year-olds. Male arrest rates are higher in almost all categories across counties in reform states than nonreform states. The female arrest rate for all crimes is slightly larger in nonreform states but reveals a mixed pattern across different categories.

Figure 2 illustrates a qualitative map of U.S. counties based on their rank of per-pupil spending and arrest rates during the years 2000–18. Counties located in the west, east, mid-west, and northwest states have higher arrest rates and higher spending per student. However, any interpretation of these static correlations is misleading.

Four panels of Figure 3 depict the time series of juvenile arrest rates and per-pupil spending for four crime categories. For comparison purposes, all measures are computed relative to initial birth cohorts' values so that they equal one for individuals born in 1985. Per-pupil spending (shown on the

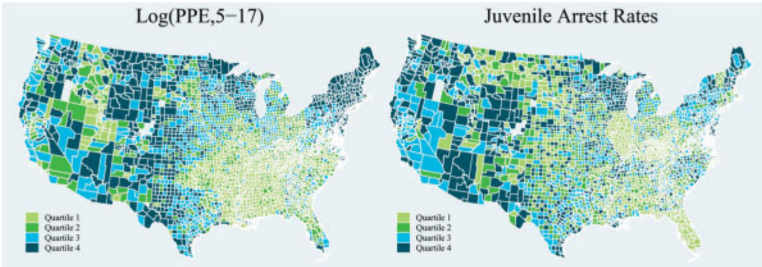

Figure 2. County National Rank in Juvenile Crime and School Spending 2000–18
Notes: Counties are shown based on their national rank in total arrest rates of individuals aged 15–19 (right panel) and the average county per-pupil school spending (left panel). County boundaries are extracted from Manson et al. (2021).

left axis) and arrest rates (shown on the right axis) are reported for exposed and nonexposed cohorts in the same panel. Exposed cohorts experienced a more considerable increase in per-pupil spending. The gap between the two groups is larger for the recent cohorts. Meanwhile, the arrest rates for virtually all categories of crimes reveal a decreasing trend for both cohorts. However, the reduction is slightly larger for cohorts exposed to SFR-induced spending than nonexposed cohorts. Relying on the time series could also be misleading. For instance, the reductions in crime rates of reform states could be attributable to pre-existing secular trends in crime rates. Once we correctly control for these confounding trends, the gap between exposed and nonexposed cohorts' crime rates may close.

4.4. Main Results

To visually show the heterogeneous effects of SFR-induced spending across cohorts and counties within a state, I use an event study model based on the first-stage regression introduced in Equation 3. Specifically, I estimate OLS regressions of the following form:

$$\log(\overline{\text{PPE}}_{5-17})_{bacst} = \sum_{qinc=1}^{4} \sum_{E=-5}^{12} (I_{q_{inc,1990,c}=q_{inc}} \times I_{E_{bs}=E}) \cdot \chi_{E,q_{inc}} + \zeta_c \times t + \gamma_b + \vartheta_{st} + \varepsilon_{bcst}. \tag{7}$$

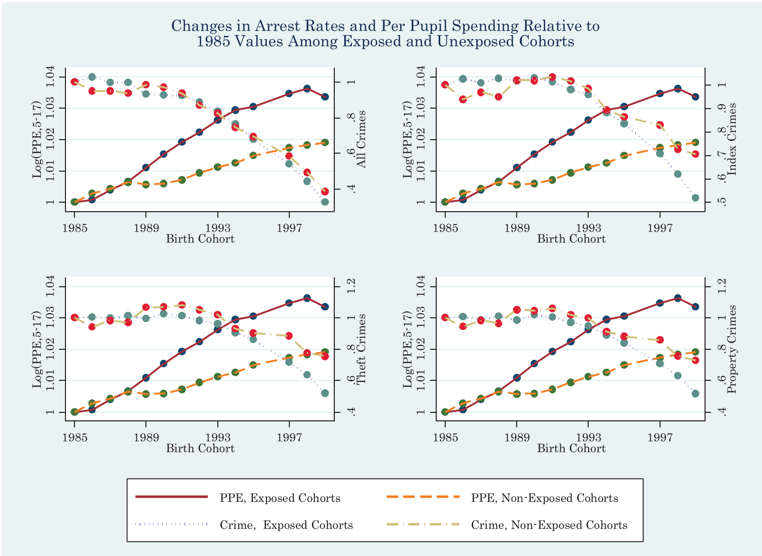

Figure 3. Time Series of Per-Pupil Spending and Crime by Exposure to the Reforms
Notes: The figure compares aggregate measures of crime and school spending for different birth cohorts among reform and nonreform states. Per-pupil expenditure during K-12 education for each birth cohort (in real 2017 dollars) are reported in the left vertical axes while arrest rates for different crime categories are depicted in the right vertical axes. For comparison reasons, all measures are computed as relative to the initial year so that they are equal one at 1985.

All covariates, trends, and fixed effects are as in Equation 3. The variable exposure is allowed to vary between -5 and 12 flexibly.[19] The parameter E is the year an individual turned age 17 minus the year the reform passed in a state. For instance, the California SFR occurred in 2004. Individuals born in 1985 turned age 19 at the time of the reform, 7 years after they turned age 17. Therefore, they are assigned an equal to -2. Cohorts who had been born in 1999, turned age 17 in 2016. The reform occurred 12 years before they turned 17. These cohorts are assigned an E equal to $+12$. In the above formulation, $I_{E_{bs}} = E$ is equal to one if the measure of exposure is E and zero otherwise. These exposure indicators are interacted with county quartile rank within-state distribution of income in 1990

19. I group all cohorts for whom the event-time is less than -5 into one category. This category is represented by one dummy variables in event-study figures. Likewise, I group and build one single dummy for cohorts whose exposure is more than 12.

to capture the *dosage* of SFR effects in different counties. I expect that the reforms do not have a meaningful impact on per-pupil spending of the unexposed cohorts, regardless of how many years before the SFR they turned age 17. Also, I expect that the SFR increases the spending among exposed cohorts and that the rise is more considerable for cohorts with more years of exposure.

The event study results are reported in Figure 4 for counties at the top and bottom quartiles of within-state income rank in 1990. Each point represents the respective coefficient of $\chi_{E,q_{inc}}$ with 95% confidence interval. While there is no consistent and systematic variation in SFR-caused spending among cohorts with negative exposure (who turned 17 at least 1 year before the reform), exposed cohorts experience a significant increase in their spending. The absence of a pre-existing trend in per-pupil spending can be observed by both magnitudes of the point estimates (relative to postreform coefficients) and also the fact that they are statistically insignificant. The postreform increase is slightly larger for cohorts in the bottom quartile compared to the top-three quartiles. Since the coefficient of unexposed cohorts ($E = 0$) are restricted to be zero, all coefficients are relative to this group. Fully exposed cohorts experience roughly 5.9% (top-three quartile) and 6.5% (bottom quartile) increase in their spending compared to nonexposed cohorts (i.e., $E = 0$). These facts confirm the first-stage effects and also the heterogeneous impacts of SFR across counties that are captured by the variable *dosage* in Equation 3.

Before starting with the 2SLS results, I directly document the reduced form effects by investigating the impacts of timing and exposure to reforms on crime rates. In so doing, I use the event study analysis and replace the dependent variable in Equation 8 with arrest rates. The results are reported in Figure 5 for all crimes, Figure 6 for property crimes, Figure 7 for violent crimes, and Figure 8 for simple crimes. Across all crime outcomes, the prereform coefficients for bottom quartile panels (top panels) are close to zero, both economically (relative to postreform effects) and statistically. There is small evidence of a pre-existing trend for the subsample of top-three quartiles (bottom panels). However, the magnitude of the effects is small compared with postreform coefficients. This is more evident for all crimes and simple crimes (Figure 5 and Figure 8, respectively). Overall, while the reforms do not appear to have a discernible effect on cohorts who turned 17

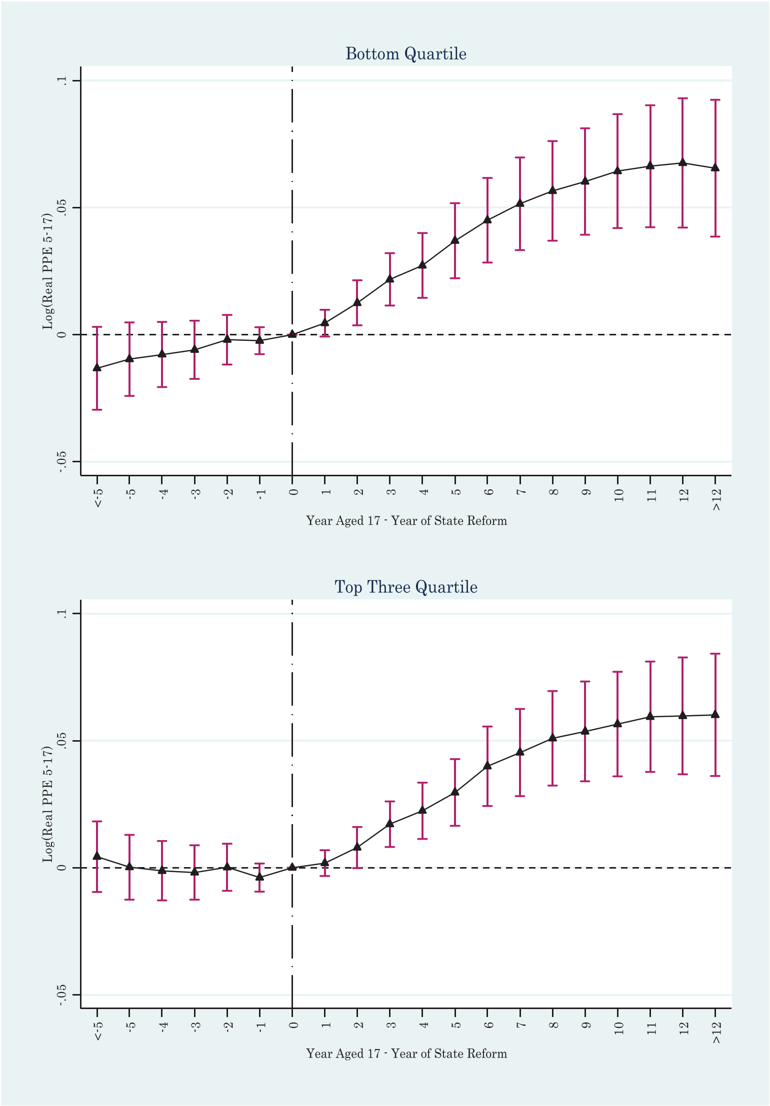

Figure 4. Event Study Analysis of School Finance Reform on Real Per-Pupil Spending by Different Quartiles of 1990 County Income
Notes: Point estimates and 95% confidence intervals are illustrated. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. Quartiles are based on county rank at within-state per-capita income distribution in 1990.

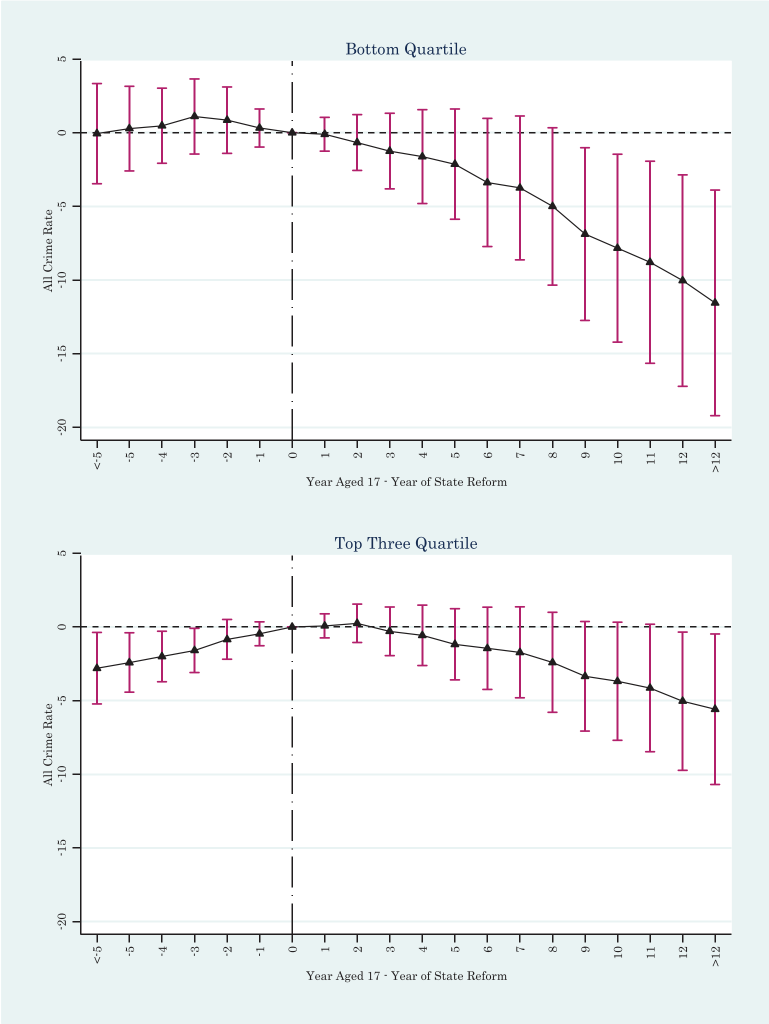

Figure 5. Event Study Analysis of School Finance Reform on Juvenile Arrests of All Crimes by Different Quartiles of 1990 County Income
Notes: Point estimates and 95% confidence intervals are illustrated. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. Quartiles are based on county rank at within-state per-capita income distribution in 1990.

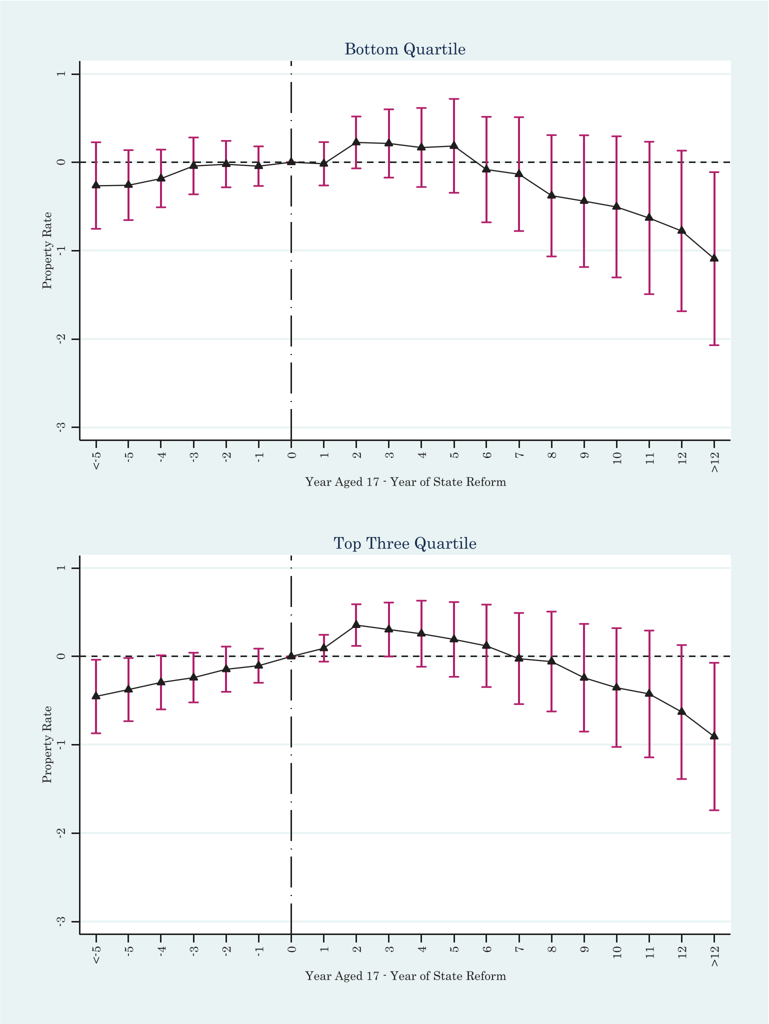

Figure 6. Event Study Analysis of School Finance Reform on Juvenile Arrests of Property Crimes by Different Quartiles of 1990 County Income

Notes: Point estimates and 95% confidence intervals are illustrated. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. Quartiles are based on county rank at within-state per-capita income distribution in 1990.

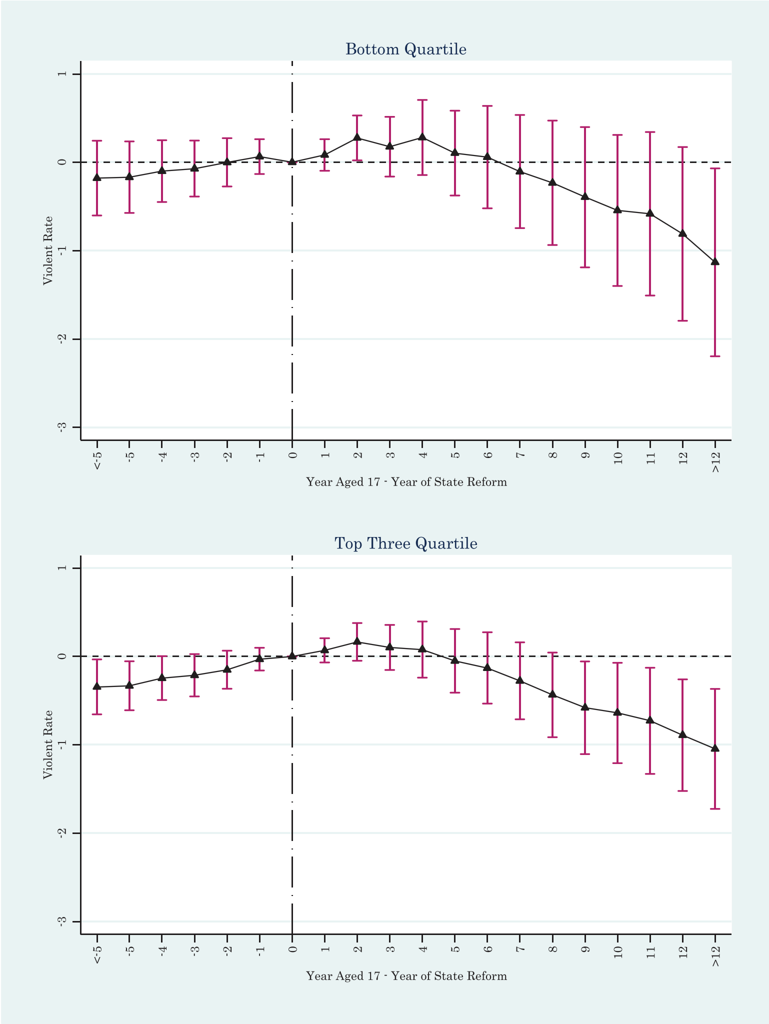

Figure 7. Event Study Analysis of School Finance Reform on Juvenile Arrests of Violent Crimes by Different Quartiles of 1990 County Income

Notes: Point estimates and 95% confidence intervals are illustrated. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. Quartiles are based on county rank at within-state per-capita income distribution in 1990.

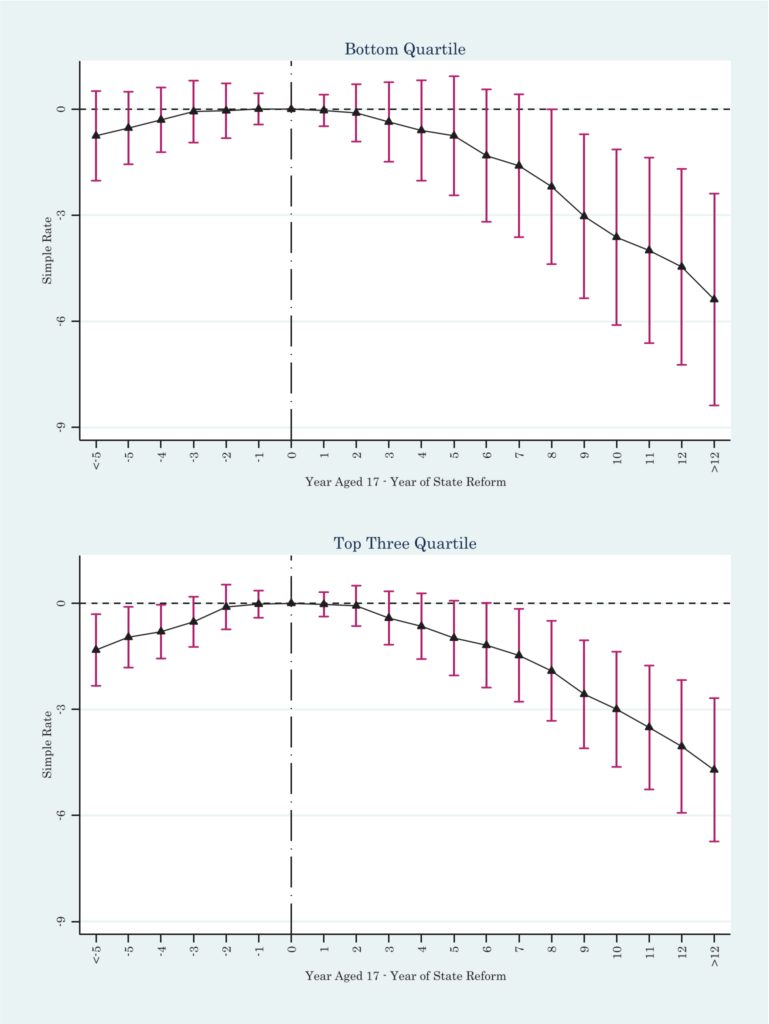

Figure 8. Event Study Analysis of School Finance Reform on Juvenile Arrests of Simple Crimes by Different Quartiles of 1990 County Income

Notes: Point estimates and 95% confidence intervals are illustrated. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. Quartiles are based on county rank at within-state per-capita income distribution in 1990.

before the reforms, they negatively affect arrest rates of exposed cohorts. The effects are economically and statistically significant for fully exposed cohorts at 95%.

On the contrary to the event study analysis, the variable exposure in Equation 3 varies between 0 (all unexposed cohorts) and 12 (all fully exposed cohorts). Any value between 0 and 12 refers to partially exposed cohorts. The primary estimations of the 2SLS model introduced in Equations 3 and 4 are reported in Table 3.[20] In column 1, I show the OLS estimates of school spending on crime rates. The coefficients are mainly insignificant and quite small in magnitude. Suppose nonreform states attempt to substitute SFR with other policies to promote education, such as increasing the minimum dropout age, which leads to a reduction in crime. In that case, we should expect a spurious positive correlation between spending and crime rates. Moreover, black families face higher property taxes. This racial gap is driven by differential behavior of property assessments as well as differential outcomes of court appeals (Avenancio-Leon and Howard, 2019). The higher property tax may lead to higher spending on education. On the other hand, crime rates are higher among blacks for other (observable and unobservable) reasons. Thus, one might observe a spurious negative correlation between spending and crime.

The estimated effects of SFR-based spending on arrest rates are reported in columns 2 and 3. In the full specification (column 3), for all categories of crime combined, a 10% rise in per-pupil school spending decreases total arrest by 7.4 incidences per 1,000 population of individuals aged 15–19. This effect means a 12.7% reduction from the mean of juvenile arrest rates among reform states over the years 2000–18. In 2016, total arrests of individuals ages 15–19 in reform states were 738,633 counts. To put it into perspective, a 10% increase in per-pupil spending is associated with approximately 90,806 fewer arrests annually.[21]

20. First stage effects are reported and discussed in Appendix B.

21. As one can notice, there is a relatively big jump in the marginal effects from column 2 to 3, where we add state-year fixed effects. We observe a similar pattern excluding/including county-year linear trend. To explore the importance of state-year dummies and county trend, I show a balancing test (replicating Table 6, see Section 5.1) without these controls. The fact that the balancing test fails in the absence of these controls highlights the importance of a full specification in interpreting the causal links.

Table 3. Per-Pupil School Spending and Juvenile Arrest Rates

Outcome variable	OLS (1)	2SLS-IV (2)	2SLS-IV (3)
All crime	0.14345	−29.26034**	−74.53569***
	(1.96553)	(14.7033)	(26.31451)
Property crime	0.08424	−2.768	−11.02831**
	(0.32142)	(2.64968)	(4.5418)
Violent crime	−0.21053	−3.07204	−12.69906***
	(0.31644)	(2.23128)	(4.73766)
Index crime	0.10995	−4.02933	−15.48466***
	(0.3785)	(3.19571)	(5.54336)
Simple crime	−0.04333	−11.09024**	−39.94965***
	(0.76699)	(5.99655)	(12.07167)
Drug crime	−0.83903	−6.86794	−10.90466
	(0.51749)	(4.66521)	(7.02481)
Financial crime	0.02299	−0.40074**	−0.295
	(0.02195)	(0.18578)	(0.2269)
Sexual crime	−0.00514	−1.90205**	−1.26702*
	(0.08554)	(0.90879)	(0.72075)
Fixed effects	Yes	Yes	Yes
State-year FE	Yes	No	Yes
Observations	423,351	413,255	413,255

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The arrest data cover the years 2000–18. The arrest rates for all crime categories are per 1,000 persons of county population aged 15–19. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Over the sample period, the increase in school spending is about 33% higher in reform states than nonreform states. Using the 33% relative rise in per-pupil spending as the benchmark shock, arrests for property crimes, violent crimes, index crimes, and simple crimes decrease by 3.6, 4.1, 5.1, and 13.1 fewer incidences per 1,000 age-group population. The coefficients are significant at conventional levels. Simple crimes are a prevalent form of crime among juveniles. As expected, the impact of education spending on this category of crime is higher than in other types. One anomaly is the drug-related crimes. Although the impacts are large and negative, they are insignificant at conventional levels. Drug crimes are among the major school crimes. It is also associated with social interactions. If school spending encourages potential criminals to continue education and stay at school, their social interaction can increase drug use among students. This social interaction effect can partly offset the impact of schooling quality on drug arrests.

These results are in line with the findings of Anderson (2014), who documents that exposure to a minimum dropout age of 18 decreases juvenile arrests by 10.3 cases per 1,000 age-group population. This value is quite similar to a 10% increase in per-pupil spending. Similarly, he does not find evidence that an increase in the minimum dropout age affects drug-related crimes. In another work, Deming (2011) finds that school quality could reduce drug-related felonies only for individuals at top risk quintiles during high school years.

On the contrary, Bennett (2018) uses Danish administrative data and shows that completing upper secondary school reduces narcotics crimes by 2.4 percentage points. In addition, Beatton, Kidd, Machin, and Sarkar (2018) investigate the effect of an educational reform in Queensland, Australia, on youth crime. They also find a negative and statistically significant impact of the reform on drug-related crimes among juveniles.

Table 4 explores the heterogeneity of the effects by gender. The magnitude of the coefficients on male arrest rates (column 1) is more than three times those of female arrest rates (column 2). A 10% rise in per-pupil spending is associated with 11.8 and 3 fewer arrests annually among male and female juveniles, respectively. The effects are more pronounced for simple crimes. A 10% rise in per-pupil spending reduces simple crimes by 6.1 and 1.9 incidences per 1,000 population of 15- to 19-year-old males and females, respectively. There is also evidence that per-pupil spending has a significant effect on financial crimes among females and sexual crimes among males.

As shown in column 2, school spending has negative effects on crimes for females. This fact contradicts the findings of Anderson (2014) that minimum dropout age does not have a crime-reducing effect among females. The minimum dropout age imposes a direct quantity effect, while SFR-induced spending could have both quantity and quality effects. The positive externality of quality impact could be larger than the quantity effect among females, and so one can observe a negative and significant effect on their arrest rates. Moreover, females are responsible for about 20% of total crimes in the sample. The gender-age specific average arrests among females are roughly one-third of male arrest rates. Therefore, it is not surprising that the

Table 4. Per-Pupil School Spending and Juvenile Arrest Rates, By Gender and Crime Subcategory

	Subsamples	
	Males	Females
Outcome variables	(1)	(2)
All crime	−118.83084***	−30.2267**
	(40.72899)	(13.47381)
Property crime	−16.81385**	−5.23684*
	(6.63928)	(3.02157)
Violent crime	−19.75927***	−5.63812*
	(6.73474)	(3.18773)
Index crime	−23.19348***	−7.77035**
	(8.24839)	(3.38897)
Simple crime	−61.30778***	−18.58598***
	(18.21589)	(6.54946)
Drug crime	−18.81215	−2.99552
	(11.6365)	(2.71037)
Financial crime	−0.0493	−0.5413*
	(0.25401)	(0.27729)
Sexual crime	−1.84862*	−0.68454
	(1.0155)	(0.6271)
Fixed effects	Yes	Yes
State-year FE	Yes	Yes
Observations	206,629	206,622

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The arrest data cover the years 2000–18. The arrest rates for all crime categories are per 1,000 persons of county population aged 15–19. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

magnitude of the effects is also lower among females. Should we interpret the coefficients as changes from the mean, we reach quite similar numbers for both genders. For instance, a 10% rise in per-pupil spending reduces simple crimes by 6.1 and 1.9 fewer arrests, equivalent to 7.2% and 6.1% reduction from the mean of arrest rates among males and females, respectively.

Table 5 checks the sensitivity of the estimations under different subsamples and alternative specifications. It disaggregates the sample by gender and reports the outcome for major crime categories. For comparison purposes, the first row replicates the estimated coefficient of columns 2 and 4 from Table 4. The second row reports the effects for a sample that is restricted to counties with above-median black rates. The coefficients are substantially larger, especially for violent and property crimes implying that the SFR had a higher externality among black students compared to all other races. Row 3 excludes the selected states with high crime rates. The effects are slightly larger than the main results.

Individuals may commit a crime or become arrested in counties other than their county of residence. These spillover effects can bias the estimated coefficients in two ways. First, the estimated coefficients are biased downward if the county of residence is located in a nonreform state while the county of arrest is in a reform state. Second, rich counties in reform states experience smaller per-pupil spending changes than poor counties. The rich counties might also observe lower crime rates if individuals in poor nearby counties, who experience higher per-pupil spending, choose to commit a crime in their affluent neighboring areas. The within-state movement can overestimate the estimated coefficients. However, the UCR data report aggregated arrests and so lack the information on the county of residence of arrestees. I try to address the spillover effects in several ways. First, to mitigate the across-state movements of arrestees, I exclude those counties that have any border with a county in another state. In this subsample, an individual may still move from an interior county in nonreform states, passes two counties at the state borders, and commit a crime at the interior county of the reform state. Although possible, this subsample is less likely to suffer from the spillover effects. Row 4 reports the results using this sample. The estimated coefficients are only slightly larger than the baseline results. Second, to mitigate the cross-county within-state spillovers, I drop the *dosage* variable and use only *exposure* as the IV in the first-stage estimation. In this setting, counties in a reform state are no more allowed to absorb the SFR-induced spending differently. The estimated coefficients are reported in row 5. They suggest that neither the cross-state nor the cross-county movements drive the main results, although they may slightly underestimate the true effects. Third, I combine the two previous approaches. In row 6, the results of interior counties without the *dosage* variable are reported (combining the strategy of row 5 with the subsample of row 4). Except for the simple crimes, major categories of crimes become unresponsive to educational spending for females. For males, the effects are quite comparable to the baseline estimations. The overall picture of rows 4–6 stands against the concerns over the spillover effects as the driver of the results.

Table 5. Sensitivity of the Effect of School Spending on Juvenile Crime in Different Specifications and Subsamples

	Males				Females			
	All Crimes (1)	Property Crimes (2)	Violent Crimes (3)	Simple Crimes (4)	All Crimes (5)	Property Crimes (6)	Violent Crimes (7)	Simple Crimes (8)
(1) Baseline results from Table 4	−118.83084***	−16.81385**	−19.75927***	−61.30778***	−30.2267**	−5.23684*	−5.63812*	−18.58598***
	(40.72899)	(6.63928)	(6.73474)	(18.21589)	(13.47381)	(3.02157)	(3.18773)	(6.54946)
(2) Above-median county black rate	−246.00319**	−22.76105*	−37.55084**	−93.71056**	−71.81889***	−10.81846**	−10.33003	−29.19041***
	(104.5267)	(13.48666)	(16.45665)	(36.37818)	(25.97958)	(5.22676)	(8.14985)	(10.53274)
(3) Excluding California, Texas, New York, Florida	−128.67782***	−21.04217***	−22.53801***	−81.14815***	−30.69491**	−6.16387*	−7.42725**	−24.4709***
	(42.94737)	(7.39862)	(7.5422)	(20.52153)	(13.92494)	(3.16581)	(3.54075)	(7.351)
(4) Counties not at state borders (interior counties)	−118.24182***	−17.58706**	−21.79175***	−61.15753***	−31.81942**	−4.65005	−6.96617 **	−20.09641***
	(43.10845)	(6.98097)	(6.4313)	(20.1396)	(15.39478)	(3.07699)	(3.08401)	(7.62843)
(5) Without dosage variable	−108.04817**	−13.5087*	−18.40753**	−64.07874***	−23.36291	−0.66616	−3.52503	−19.73303***
	(45.6811)	(7.26544)	(7.20867)	(21.19688)	(15.22977)	(3.33591)	(3.30953)	(7.63792)
(6) Interior counties + without dosage variable	−110.64229**	−13.97066*	−21.31479***	−69.05814***	−26.07939	0.94726	−4.81672	−23.50225**
	(54.32023)	(8.07509)	(7.65105)	(25.58356)	(19.08825)	(3.77523)	(3.49296)	(9.51402)

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The arrest data cover the years 2000–18. The arrest rates for all crime categories are per 1,000 persons of county population aged 15–19. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Furthermore, I also show the effects in a state-by-year aggregated sample (see Appendix E). These results are not affected by within-state spillovers for two reasons. First, the strategy relies only on differential exposure of cohorts within a reform state and not the county-level dosage of the treatment. Second, any within-state spillovers in occurrences of crime are dissolved through state-level aggregations. The fact that the results of the state-aggregated sample are generally similar to the baseline results reassures us that the spillovers are not inducing bias in the estimations.[22]

5. Concerns over Endogeneity

5.1. Postreform Mobility

To register in a specific public school, students must be residents of those school districts. An increase in state funding for one school district might generate an incentive for parents of other districts to relocate to the higher-spending district if the perceived additional benefits cover the extra costs of their relocation. This type of residential mobility can pose a threat to the identification strategy if the families' decision to migrate is affected by some elements that are also correlated with their juveniles' criminal activity. For instance, wealthy families might put higher weights on educational spending and be more willing and able to relocate to districts that experience increases in spending. Meanwhile, juveniles from more affluent families are less likely to engage in criminal activities (for observable and unobservable reasons). Therefore, one might find a negative and endogenous effect of school spending on crime.

Similarly, black families and low-wage earners might find it easier to migrate. Since black juveniles have higher crime rates, their migration to higher-spending areas bias the spending-crime relationship and make the coefficients understate the true effects. To investigate whether these compositional changes impose endogeneity issues in the main results, I use a series of socioeconomic characteristics of the county as the dependent variable of the first-stage regression (Equation 3). Suppose families relocate in

22. In Appendix H, I complement the sensitivity analysis by exploring additional specifications, alternative functional forms, and other subsamples.

response to the timing and location of the reforms. In that case, I expect to find a consistent effect of *exposure* and even *dosage* of the treatment on the demographic and economic composition of the counties. Table 6 provides little evidence of such endogeneity issues. There is no systematic evidence that the timing of the reform, exposure to the reform, and dosage of absorption of state funding has any effect on percentage whites, blacks, other races, Hispanics, average wages, total quarterly wages, and job destruction rates. To further examine the migration issue, I use the original 2SLS formulation of the main results and replace the outcome of the second stage with the county socioeconomic characteristics. This analysis investigates whether reform-induced spending changes the county's demographic and economic composition. The results, reported in Appendix Table F1, confirm the findings of Table 6.

Next, I examine whether SFR-induced changes in spending can explain the demographic-related changes in crime. The results are reported in Appendix Table F2. The predicted propensity to commit a crime (from a regression of arrest rate on socioeconomic variables and fixed effects) reveals no systematic and significant correlation with predicted spending from the first stage. All in all, these results suggest that cohort-level demographic changes are not correlated with SFR-induced spending.

5.2. School Finance Reform and Policing Expenditure

Court orders may depend on states' budget, revenue, and taxation. An increase in state tax revenue can be translated into an increase in state expenditure over different categories, including education. This change may accelerate or even encourage court-ordered reforms. However, the increase in state revenue can also affect the expenditure on policing enforcement. In this case, students who are exposed to the reforms also observe an increase in police officers and policing efforts. The increased number of officers in the streets could lessen the potential delinquents' tendency to commit a crime (Lin, 2009; Klick and Tabarrok, 2015; Atems, 2020). Hence, fewer arrests are observed among these cohorts. Even if the state budgets are constrained, states can change the composition of their expenditure. State authorities may also assign more weight to annual expenditure on education alongside the policing budget. These possibilities have two consequences.

Table 6. Exposure to School Finance Reforms and County Socioeconomic Characteristics

	Outcome						
	%Whites (1)	%Blacks (2)	%Others (3)	%Hispanics (4)	Job Destruction Rate (5)	Log Total Quarterly Wages (6)	Average Weekly Wages (7)
Exposure	−0.00525	−0.00099	−0.0003	−0.00421	−0.00374	−0.00067	0.09039
	(0.0044)	(0.00404)	(0.00292)	(0.00604)	(0.0094)	(0.00279)	(0.32668)
Exposure × dosage dummies:							
2	0.00684	−0.00564	0.00482	−0.00092	−0.00361	0.00313	0.19683
	(0.00647)	(0.00564)	(0.00443)	(0.00784)	(0.01444)	(0.00372)	(0.50606)
3	0.00837	−0.00571	−0.00454	0.01271	0.01518	0.00089	−0.1888
	(0.00748)	(0.00583)	(0.00521)	(0.00908)	(0.01422)	(0.00349)	(0.49503)
4	−0.00406	0.00556	0.00383	0.01996**	−0.00205	0.00439	−0.36315
	(0.00665)	(0.00581)	(0.00406)	(0.00958)	(0.01458)	(0.00344)	(0.55976)
Fixed effects and trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	406,395	406,395	406,395	406,395	406,395	406,395	406,395

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
*** $P < 0.01$, ** $P < 0.05$, * $P < 0.1$.

First, one might observe a spurious positive link between policing expenditure and changes in spending on education through court-ordered reforms. Second, the negative relationship between per-pupil spending and arrests is a spurious link and can be partly explained by increases in policing enforcement.

To check for this possibility, I use a state-year-level panel data fixed effect model to find the correlation between measures of police enforcement and states' reform status. The dependent variable is police employees per 1,000 state-year level population, police sworn officer per 1,000 state-year level population, and police expenditure per capita.[23] The independent variable is a dummy that equals one if the state has passed an SFR (equals one for all postreform years) and zero otherwise. State-year level covariate includes the percentage of blacks, percentage of males, percentage of individuals 15–19, unemployment rate, and log of population. The results are shown in Table 7. Passing an SFR is not correlated with a significant change in per-capita police employee, per-capita officer, and per-capita expenditure. The sign of coefficients is even negative, implying that reform states substituted expenditure on policing with increased spending on education. However, the coefficients are not statistically significant. Overall, there is no evidence that changes in police enforcement generate a negative link between school spending and arrest rates. To further evaluate this source of contamination, I implement a series of event-study analyses in which the event-time is the distance in years relative to the state-specific year of reform. These results are reported and discussed in Appendix K. Overall, there is no consistent and significant pre/post-trend in policing employment and spending. These results are also in line with other studies that evaluate the compositional changes in state expenditure following school finance reforms. For instance, Liscow (2018) shows that while the rises in educational spending as a result of court-ordered reforms remain in place in the following years, there is no evidence of a significant change in noneducational expenditure.

23. Police employee and officer per capita data is extracted from a series of Uniform Crime Reporting Program Data Series provided by Inter-University Consortium for Political and Social Research (ICPSR) (FBI, 2018). Police expenditure data are extracted from Kaplan (2018a).

Table 7. School Finance Reform and State Policing Enforcement

	(1) Police Employee per 1,000	(2) Police Officer per 1,000	(3) Police Expenditure per Capita
SFR status	−1.534	0.452	0.075
	(1.911)	(1.164)	(0.130)
Fixed effect	Yes	Yes	Yes
Covariates	Yes	Yes	Yes
Observations	1,377	1,377	1,377

Notes: The data on police employees comes from FBI (2018). The data on policing per capita expenditure comes from Kaplan (2018a). They cover the years 1992–2018 and all 51 US states. The unit of observation is state-year. The results are from a simple OLS regression of law enforcement measure on a dummy variable (SFR Stat), which is equal to one if court-ordered school finance reform has passed by the state in the year (remains one for all post-reform years) and zero for all pre-reform years. Covariates include the log of the state population, percentage of males, percentage of blacks, percentage of individuals between 15 and 19, and unemployment rate. State and year fixed effects are also included. Standard errors, reported in parentheses, are clustered at the state level.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

6. Mechanisms of Impact

There are three possible stories beyond the observed results. The first one is the quantity effect. School spending offers an incentive for individuals to earn more education because better schooling can increase the relative wages of educated people. If the quality of skills they earn affects the employment opportunity or the wage premium in the job market, then individuals have the incentive to increase their education. Section 6.1 provides some evidence regarding the quantity effect. The second channel is the quality effect. The observed improvement in schooling quality may encourage students to improve the quality of their skills by spending more time on education. Section 6.1 also documents evidence regarding the subjective improvement in education once individuals face an objective improvement in schooling. Using the American Time Use Survey, I find that exposed cohorts in reform states increase their time on educational activity. Third, it decreases individuals' propensity to do risky behaviors. In Section 6.2, I show that the SFR-induced raise in school spending is associated with lower risky behavior at school, higher perception of school safety, and a lower likelihood of drug-dealing at school.

6.1. School Spending and Educational Outcomes

The primary channel between school spending and juvenile crime is education. Education could potentially reduce crime for three general reasons.

First, it increases the opportunity cost of committing a crime by increasing the return in the legal labor market. Second, the time spent on gaining human capital is a substitute for the time learning street capital. The more time a teenager spends on educational activity, the less time is available to participate in criminal activity. Therefore, education has a 'self-incapacitation' effect. Third, education can affect individuals' patience and risk aversion. People with lower discount rates value future earnings and consumption more, and so they are more likely to invest in human capital and accept the immediate costs of education. Less patient individuals care more about today's costs and benefits. They are more likely to drop out of high school and commit a crime (Oreopoulos, 2007). Education can affect the degree of discount factor and make individuals more patient. The new discount rate changes the weight of future earning and probable gain from criminal activity. Therefore, it diminishes the likelihood of committing a crime (Machin Marie, and Vujiae, 2011).

Education could act as a mediatory channel between school spending and crime if increases in school spending encourage education. I investigate the link between per-pupil school spending and education using the state-aggregated method introduced in Equations 5 and 6 and using the Current Population Survey data (2001–17). The main reason to use the state-aggregated 2SLS-DD approach is the very restricted number of identifiable counties in the CPS.[24] The outcome variable of the first-stage regression is per-pupil school spending. In the second stage, I explore three outcomes. First, a binary variable that takes one if the individual has a high school education but has not graduated from high school and zero otherwise. Second, a binary variable that equals one if the individual has earned a high school diploma and zero if it has an education of some high school or less than high school. The third outcome is a binary that takes one if the individual has some college

24. The main drawback of using CPS, as well as other publicly available data sources such as the American Community Survey, is the limited number of identifiable counties. Neither CPS, nor Census (1950-onward) reports county identifier. Although Flood, King, Rodgers, Ruggles, and Warren (2018) identify counties from other low-level geographical variables in the CPS, the identifiable counties vary between 450 and 500, almost one-sixth of all US counties. For this reason, I use the state-aggregated version method (2SLS-DD).

Table 8. School Finance Reform, Education, and Time Spending on Educational Activities

	(1) High School Dropout	(2) High School Graduation	(3) Some College	(4) Any Educational Activity	(5) Duration of All Educational Activities
$\log(\widehat{\mathrm{PPE}}^{R}_{5-17})$	−0.31011**	0.32032**	0.67505	0.25562***	5.45139**
	(0.14379)	(0.14185)	(0.53669)	(0.08334)	(2.52725)
Fixed effect	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes
Observations	565,998	565,998	565,998	155,038	155,038

Notes: The empirical strategy is the same 2SLS-DD structure where the IV is dummies for the exposure variable (no dosage). In column 1, the outcome is a dummy equal to one has the individual attended high school but does not have a high school diploma. In column 2, the outcome is a dummy equal to one has the individual earned a high school diploma and zero otherwise. In column 3, the outcome is a dummy variable equal to one has the individual had some college education and zero otherwise. In column 4, the outcome is a dummy variable equal to one has the individual spent any time doing educational activities during the past 24 h. In column 5, the outcome is the amount of time spent on educational activity (in hours) during the past 24 hours. In addition to all covariates and fixed effects explained in notes of Table 3, the following control variables are also included: gender, race, and dummies for family income. In columns 1–3, the Current Population Survey (2001–17) data are used. For columns 4-5, the American Time Use Survey is used. Standard errors, reported in parentheses, are clustered at the state level.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

education and zero if less than a college education. The sample is restricted to individuals below age 25 to restrict migration issues.[25] The sample is also limited to individuals above age 18 to assure that individuals did finish K-12 schooling. The results are reported in the first three columns of Table 8. School spending decreases high school dropouts and encourages high school completion. A 10% increase in per-pupil spending increases the likelihood of being a high school graduate by 3.2 percentage points. It also has a positive effect on the likelihood of college attendance, although the marginal effects are not precisely estimated.

Hyman (2017) evaluates Michigan's 1994-SFR on students' educational attainments. He finds that a $1,000 rise in spending during grades four through seven leads to a 3.9 percentage point higher likelihood of enrolling in a postsecondary school and 2.5 percentage points higher probability of receiving a degree. Candelaria and Shores (2019) use all *adequacy* reforms and find that a 10% rise in per-pupil revenue increases the likelihood of graduation rates among high-poverty school districts by roughly 5.1 percentage

25. The results are, however, not sensitive to this specific age range limit.

points. One possible reason that the implied effects on high school graduation are smaller in CPS data is the aggregation effect. While these two papers apply a district-level sample, I aggregate the effects on the state level and ignore the variations across counties.[26]

Next, I examine whether increases in school spending have a *self-incapacitation* effect. It is worth noting that in the United States, moving from one grade to the next highly depends on test-based criteria (Hauser, Frederick, and Andrew, 2007). Therefore, an increase in spending may encourage education by increasing time spent on educational activities. Improving educational outcomes demand allocating more time for education. The *self-incapacitation* resulting from the time reallocation can also limit the time available to acquire street capital or engage in criminal activity. Using the American Time Use Survey 2003–17, columns 4–5 of Table 8 show the effect of per-pupil school spending on time spent on educational activity. In column 4, the outcome is a binary variable that equals one has the individual spent any time on educational activity during the last 24 h and zero otherwise. A 10% rise in per-pupil spending increases the probability of spending any time on educational activity by roughly 2.6 percentage points. In column 5, the outcome of the second-stage regression is replaced by a continuous variable that measures the total amount of time (in hours) spent on educational activity during the past 24 h. A 10% increase in spending is associated with 32.4 min more time spent on educational activities. Both coefficients are statistically significant at conventional levels. Overall, the results of this section imply that SFR-caused increase in school spending did have a positive effect on education and provided incentives for individuals to allocate more time for educational activities.

Overall, these facts are in line with predictions of the theoretical framework, as highlighted in Remark 1 (Section 3). Juveniles observe higher spending by changes in the quality (e.g., smaller class sizes, better teaching equipment, more trained teachers, more updated course materials, and curriculum, etc.) or quantity (e.g., more school-days, longer school-hours per day) of education. These outcomes signal a more effective learning process and a higher wage premium in the job market upon graduation. Moreover, a

26. In row 5 of Table 5, I show that the main results of crime rates are robust when I exclude the *dosage* variable and focus only on exposure in a 2SLS-DD approach.

better learning experience improves their placements in colleges and universities, which means an improved wage premium. The higher wage premium incentivizes high school graduation and college enrollment. It also encourages individuals to spend more time on educational activities that, in turn, increase the chances of completing high school. Therefore, increases in school spending lower the share of uneducated individuals. The empirical results of this section confirm this fact. Through this mixed quality–quantity channel, the higher educated individuals have a higher opportunity cost of time and lower incentive to engage in criminal activities. Therefore, one may observe a negative relationship between spending and juveniles' crime rates (Fact 3, Remark 1).

6.2. School Finance Reform and Risky Behavior at School

The results indicate that the reforms are successful in reducing the arrest rates, providing an incentive for students to allocate more time to educational activities, and encouraging high school completion. However, the reduction in arrests could be due to a decrease in potential delinquents' propensity to commit a crime when they observe higher spending and higher expected wage premium. Reducing their tendency to commit a crime also discourages them from engaging in risky behaviors at school.

On the other hand, higher spending could also encourage individuals to stay more at school without reducing their propensity to commit a felony. In the latter case, the problems are transferred from streets to schools. This case is further compounded if such activities do not lead to an arrest. The presence of potential criminals at school can affect the schooling quality in several channels, such as students' perception of school safety. Therefore, students bear the cost of incentivizing potential delinquents to stay at school.

To investigate these possibilities, I use the restricted-use state-identified version of the 2001–15 Youth Risk Behavior Surveillance System (YRBSS) data files. YRBSS is a nationally representative data set of individuals aged 14–18 during high school years. It asks a comprehensive set of questions regarding juvenile health attitudes, risky behavior, drug and tobacco use, sexual behavior, etc.[27] I convert the responses of a selected set of questions

27. For some studies that use YRBSS data see, for example, Anderson (2010), Anderson, Hansen, and Walker (2013), Anderson and Sabia (2018), and Zheng (2018).

Table 9. School Finance Reform and Youth Risky Behavior at School

	(1) All Sample	(2) Females	(3) Males
Drug deal at school	−0.51217***	−0.47863***	−0.55027***
	(0.12729)	(0.12177)	(0.14546)
Observations	105,119	53,328	51,791
Considering suicide	0.00005	−0.03738	0.04327
	(0.04305)	(0.05777)	(0.05597)
Observations	107,720	54,433	53,287
Fighting at school	−0.06035	−0.10029	−0.01997
	(0.06597)	(0.08703)	(0.1004)
Observations	105,997	53,649	52,348
Absence at school out	−0.18366**	−0.15718*	−0.21029*
of fear of safety	(0.08526)	(0.07817)	(0.10567)
Observations	108,142	54,638	53,504
Carry arm at school	−0.09447	−0.08122	−0.10109
	(0.11221)	(0.10683)	(0.13374)
Observations	105,166	53,561	51,605
The threat of an	−0.06427	−0.05577	−0.08286
injury at school	(0.04709)	(0.04056)	(0.06699)
Observations	107,789	54,471	53,318
Fixed effect	Yes	Yes	Yes
Covariates	Yes	Yes	Yes

Notes: The data are extracted from the Youth Risk Behavior Surveillance System (YRBSS). It covers the years 1999-2015 on a biannual basis. Since the YRBSS does not provide county of residence information, the variable *dosage* is omitted. The empirical strategy is then a2SLS-DD with only the exposure dummies as the IV. Each cell represents a separate regression. Controls include dummies for race and a quadratic function of age. The standard errors, reported in parentheses, are clustered on the state level. Outcome variables are binary variables equal to one has the individual answered a positive number to any of the questions in the left panel.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

into binary responses (yes/no) and use the state-aggregates empirical approach in Equations 5 and 6 to find the possible effects of school spending on individuals' risky behavior at school. The only difference of this approach to the main empirical method is that there is no *dosage* within reform states since YRBSS does not ask for the county of residence. The results are reported in columns 1–3 of Table 9 for all respondents, females, and males, respectively. A 10% rise in school spending decreases the probability of involving in a drug deal (offered, given, or sold illegal drugs) at school during the past 12 months by 5.5 and 4.8 percentage points for males and females, respectively. The coefficients are statistically significant at 1% level.

The UCR arrest data do not provide evidence of a drug-reducing effect of spending among juveniles, while YRBSS data do. Note that although the coefficients of males' and females' in UCR analysis (Table 4) are statistically insignificant, they are negative in sign and economically large in magnitude.

A large fraction of drug deals among juveniles may occur at school. The effect of spending on drug deals is more pronounced for male youths, mainly because the school is the primary location of the transactions. Moreover, recall that the theoretical framework (Section 3) suggests that the mediatory channel between spending and crime is education. Therefore, another possibility is that the main effects are among people who choose to stay at school. The coefficients of the YRBSS results are extracted from a sample of individuals who have already chosen to stay at school, while the UCR results are based on the population of all individuals. So, it should not be surprising that YRBSS data reveal stronger effects of spending on drug-related crimes.

The second most affected outcome is individuals' perception of school safety. A 10% SFR-caused increase in per-pupil school spending is associated with 1.8 percentage points lower likelihood of having been absent at school out of fear of safety during the past 30 days. The coefficients of other outcomes are negative in most cases but not statistically significant. The only anomaly in the results is the positive sign for suicidal thoughts among male respondents. The respective coefficient is insignificant with and without covariates and relatively small in magnitude.

Overall, the results imply that the reductions in juvenile arrests are not associated with transferring problems to schools. On the opposite, the increases in spending reduce their marginal tendency to commit a crime and lessen their risky behavior at school.

7. Concluding Remarks

7.1. Discussion

On average, states that passed SFR during the 1990s experienced a 58% increase in real per-pupil expenditure up to the year 2015, an increase equivalent to $5,377 in 2017 dollars. Over the same period and among nonreform states, per-pupil expenditure increased by $4,033 in 2017 dollars. Relative to nonreform states, SFR-passed states increased the spending by $1,345 or roughly 33%. This relative dollar change implies that the SFR-induced increase in per-pupil spending reduces total arrests by roughly

24.6 incidences per 1,000 population aged 15–19, which is about a 44% reduction from the mean of juvenile arrest rates. Total arrests of juveniles aged 15–19 account for 11% of the nation's annual arrests. Also, reform states are responsible for roughly 62% of total juvenile arrests in the sample period. Therefore, the relative change in spending is associated with an approximately 3% reduction in the nation's arrest rates.

In 2017, the annual per inmate cost was estimated at $36,299.[28] Moreover, the average expected sentence length for inmates aged 15–24 is 3.3 years (NCRP, 2018).[29] If each arrest leads to 1 year of prison, then a 3% reduction in arrest rate means a $1,088 reduction in per-capita prison cost. This number is about 20.2% of the increase in per-pupil spending among reform states between the years 1990 and 2015. Although these figures are only back-of-an-envelope calculations, they show large externalities and considerable cost-saving potentials.

Furthermore, a 33% rise in spending is associated with 3.6 fewer arrests for property crimes among juveniles, equivalent to a 33% reduction from the mean of juvenile property crimes. In 2015, juveniles aged 15–19 were responsible for 18.2% of property crimes in reform states. Reform states accounted for roughly 70% of all the nation's property crimes in 2015. Therefore, the relative rise in spending decreases national property crimes by 4.2%. The FBI provides estimations for lost physical capital due to property crimes annually. Based on these estimates, property crimes in 2015 resulted in approximately $14.3 billion in losses. To put it into perspective, a 33% relative rise in spending in reform states results in a reduction of $601 million in damages to the nation's capital due to deterred property crimes. This cost is about three times larger than those found by Anderson (2014), who estimated that the minimum dropout age of 18 could save the nation about $190 million due to a reduction in juveniles' property crimes.

Overall, the results of the article are also impressive in comparison with studies on more conventional and legal measures of crime deterrence like punishment severity (Drago, Galbiati, and Vertova, 2009; Owens, 2009;

28. Source: Federal Register: Annual Determination of Average Cost of Incarceration (2020).

29. It is worth noting that in the United States in the year 2010, the conviction rate (the share of arrests that lead to a conviction) was 93% (*United States Attorneys Annual Statistical Report*, 2010).

Abrams, 2012; Johnson and Raphael, 2012; Hansen, 2015) and policing expenditure and enforcement (Zhao, Scheider, and Thurman, 2002; Evans and Owens, 2007; DeAngelo and Hansen, 2014; Bove and Gavrilova, 2017). For instance, Bove and Gavrilova (2017) find that a 10% increase in the monetary value of federal equipment provided to aid the militarization of police forces decreases total arrests by 5.9 incidences per 100,000 population, a 0.2% reduction from the mean arrest rate of the total population. In comparison, a 10% increase in per-pupil spending is associated with a 13.3% reduction in the mean of juvenile arrests, or a 1.7% reduction from the mean arrest rate of the total population. Although these numbers suggest a higher return to expenditures on education, one may interpret them with caution as they are calculated at the margin and ignore the level of expenditure on both items or the possible nonlinearities of their effects.

7.2. Conclusion

The United States has high rates of crime compared to other developed countries. Juveniles account for a considerable portion of the crimes that are committed in this country. The high rate of recidivism among criminals[30] makes crime-prevention strategies appealing to policymakers, especially concerning juvenile crime. To date, the literature has offered several determinants of crime and alternative strategies for crime prevention among juveniles. Among these determinants are policies that promote education and encourage high school graduation. It is widely documented that education has a negative association with crime and arrest rates. However, little research has been done to investigate the relationship between school spending and crime. This article aimed to fill this gap by investigating the causal effect of per-pupil school spending on arrest rates.

I construct a panel data set on per-pupil school spending and arrest rates covering virtually all counties in the United States over the years 2000–18. Using a 2SLS-DDD approach, I attempt to solve the endogeneity of school spending by taking advantage of the plausibly exogenous timing and location of court-ordered school finance reforms across states. The results provide strong evidence that school spending can alter the juveniles'

30. Alper, Durose, and Markman (2018) estimate that between 2005 and 2014 about 68% of released prisoners were arrested in the first 3 years following their release, 79% within 6 years, and 82% within 9 years.

tendency to commit a crime. The results suggest that a 10% rise in real per-pupil school spending is associated with 7.4 fewer arrests of total crimes per 1,000 population aged 15–19. This effect is equivalent to a roughly 13% reduction from the mean arrest rate in this age group.

The results are quite robust to different specification checks, subsamples, and also across genders. I explore two potential threats to the identification strategy. First, I show that the observable demographic–economic characteristics of counties do not change as a response to the reforms' timing and location. Moreover, postreform residential mobility does not change cohort-level characteristics following reform-induced fiscal changes. Therefore, the shift in arrest rates is not driven by changes in cohort demographic–economic composition. Second, I show that the reforms did not accompany other state-level policies that also influence crime rates. In so doing, I document that there is no statistical evidence that the reforms are associated with changes in per-capita police officers, police employees, or police expenditures.

A simple economic framework provides an intuition for the potential channels of impact. The main channel that affects the exposed cohorts is education. Higher spending points to a better learning process. It suggests higher future wages for high-educated agents. An expected higher wage premium decreases the share of low-educated and low-skilled agents. It also lowers the measures of criminals in society. The empirical analysis confirms the model's predictions. The results show that the reforms did encourage high school completion. In addition, they created an incentive for individuals to spend more time on educational activities.

Another concern is that school spending causes an increase in the quantity of education by, for example, keeping students at school for longer hours. If the spending does not change potential criminals' propensity to commit a crime, then keeping them at school transfers the street problems to schools. The empirical results fail to support this concern. The results show that the SFR-induced increases in school spending decrease risky behavior at school and improve students' perception of school safety. Although coefficients are not large in magnitude, they reject the *displacement effect*. Anderson (2014) finds that a minimum dropout age of 18 causes juvenile arrests to drop by about 17.2% from the mean. However, he finds that keeping potential criminals at school could displace the problems to schools. The students who would have gone to school even without the minimum dropout age law bear the costs of keeping troublesome students at school.

Overall, the results suggest that school spending could be an effective alternative mechanism for juvenile crime prevention compared to traditional criminal justice policies; it could encourage education and reduce the tendency of juveniles to engage in criminal activity. In addition, since it does not displace the trouble to schools but provides an incentive to reduce risky behavior, it could be a better policy than other educational promotion policies like minimum dropout age laws.

Appendix A

For agents of type θ in each category of education, there is a threshold on criminal activities above which they choose to engage in crime. At the reservation rule, agents are indifferent between allocating all their time to the labor market or criminal activity. From Equations 1 and 2, one can extract the reservation rule for high- and low-educated individuals:

$$\log\left(w^H\theta q\left(s\right)\right) = \left(1-q\right)\left[\varphi_r^H + \log\left(w^H\theta q\left(S\right)\right)\right] + q\log\bar{c}$$
$$\Rightarrow \varphi_r^H = \frac{q}{1-q}\left(\log\left(w^H\theta q\left(S\right)\right) - \log\bar{c}\right). \tag{A1}$$

In the same way for low-educated people:

$$\varphi_r^L\left(\theta\right) = \frac{q}{1-q}\left(\log\left(w^L\theta\right) - \log\bar{c}\right) \tag{A2}$$

where index r refers to the reservation return on criminal activity for individuals of type θ with educational level in the set $\{H, L\}$. Higher prison consumption decreases reservation return. Higher wages raise the required return for agents to commit a crime. Individuals with higher innate ability demand higher reservation returns. More importantly, a better quality of education increases the reservation rule.

By rearranging the terms in (A2), one can obtain:

$$\log\bar{c} = \log(w^L\theta) - \frac{1-q}{q}\varphi_r^L(\theta).$$

In the first period, the expected utility of low-educated agents is composed of their leisure from the first period and their expected discounted utility from the second period:

$$U_1^L = E_1\left(\beta U_2^L(\theta)\right) + l. \tag{A3}$$

The first period expected utility of high-educated individuals is the disutility of studying $(d - \log\theta)$ and the expected discounted utility in the second period:

$$U_1^H = -d + \log\theta + E_1\left(\beta U_2^H(\theta)\right). \tag{A4}$$

The second period expected utility can be rewritten as a function of reservation rule by inserting this term in Equation 1 and simplifying:

$$\begin{aligned} U_2^L(\theta) &= \max\left\{\log\left(w^L\theta\right), (1-q)\,\varphi + (1-q)\log\left(w^L\theta\right) + q\log\left(w^L\theta\right)\right. \\ &\quad \left. - (1-q)\log\left(w^L\theta\right) - (1-q)\,\varphi_r^L(\theta)\right\} \\ \Rightarrow U_2^L(\theta) &= \log\left(w^L\theta\right) + \max\left\{0, (1-q)\left(\varphi - \varphi_r^L(\theta)\right)\right\}. \end{aligned} \tag{A5}$$

And with the same procedure for high-educated agents:

$$U_2^H(\theta) = \log\left(w^H\theta q(S)\right) + \max\left\{0, (1-q)\left(\varphi - \varphi_r^H(\theta)\right)\right\}. \tag{A6}$$

Integrating over φ returns the expected value of the second-period utility:

$$EU_2^L(\theta) = \int_{-\bar{\varphi}}^{\bar{\varphi}} \log\left(w^L\theta\right) dF_\varphi + \int_{-\bar{\varphi}}^{\bar{\varphi}} \max\left\{0, (1-q)\left(\varphi - \varphi_r^L(\theta)\right)\right\} dF_\varphi.$$

Recall that φ follows a uniform distribution over $[-\bar{\varphi}, \bar{\varphi}]$:

$$\begin{aligned} &\Rightarrow EU_2^L(\theta) = \log\left(w^L\theta\right)\int_{-\bar{\varphi}}^{\bar{\varphi}} \frac{1}{2\bar{\varphi}}d\varphi + \int_{\varphi_r^L(\theta)}^{\bar{\varphi}} \frac{(1-q)\left(\varphi - \varphi_r^L(\theta)\right)}{2\bar{\varphi}}d\varphi \\ &\Rightarrow EU_2^L(\theta) = \log\left(w^L\theta\right) + \frac{1-q}{4\bar{\varphi}}\left(\bar{\varphi} - \varphi_r^L(\theta)\right)^2. \end{aligned}$$

Since $q < 1$, one can assume that $q^2 \simeq 0$ and so $\left(\varphi_r^L(\theta)\right)^2 \simeq 0$ and simplify the equation:

$$\Rightarrow EU_2^L(\theta) = \log\left(w^L\theta\right) + \frac{1-q}{4\bar{\varphi}}\left[\bar{\varphi}^2 - 2\bar{\varphi}\varphi_r^L(\theta)\right].$$

Inserting from (A2):

$$EU_2^L(\theta) = \left(1 - \frac{q}{2}\right)\log\left(w^L\theta\right) + \left(\frac{1-q}{2}\right) + \frac{q}{2}\log\bar{c} \tag{A7}$$

And with the same procedure for high-educated individuals:

$$EU_2^H(\theta) = \left(1 - \frac{q}{2}\right)\log\left(w^H\theta q(S)\right) + \left(\frac{1-q}{2}\right) + \frac{q}{2}\log\bar{c} \tag{A8}$$

Agents of ability θ^* are indifferent between studying and not studying, i.e., $U_1^H(\theta^*) = U_1^L(\theta^*)$, then this specific θ^* can represent the share of people who remain uneducated if $\underline{\theta} \to 0$. Inserting Equations (A7) and (A8) into Equations (A3) and (A4), one can replicate the following:

$$\log\theta^* = l + d - \beta\left(1 - \frac{q}{2}\right)\log\frac{w^H}{w^L} - \beta\left(1 - \frac{q}{2}\right)\log\left(q(S)\right). \tag{A9}$$

The share of uneducated people provides a measure of the stock of low-educated people as $H_L = \int_0^{\theta^*}\theta d\theta = \frac{\theta^{*2}}{2}$, and the corresponding measure of high-educated people can be computed as $H_H = \int_{\theta^*}^1 \theta d\theta = \frac{1-\theta^*}{2}$. Since markets are perfectly competitive, workers are paid their marginal revenue product, $\mathrm{MRP}_L^L = w^L$ and $\mathrm{MRP}_L^H = w^H$. Using the production function $Y = AH_L^\alpha H_H^{1-\alpha}$, and inserting for the stock of low- and high-educated individuals, one can extract the following:

$$\log\left(\frac{w^H}{w^L}\right) = \log\left(\frac{1-\alpha}{\alpha}\right) + \log\frac{\theta^{*2}}{1-\theta^{*2}}. \tag{A10}$$

Inserting (A10) into (A6), one can get an implicit equation for θ^*:

$$\log\theta^* - l - d + \beta\left(1 - \frac{q}{2}\right)\left[\log\left(\frac{1-\alpha}{\alpha}\right) + \log\left(\frac{\theta^{*2}}{1-\theta^{*2}}\right) + \log\left(q(S)\right)\right] = 0. \tag{A11}$$

Differentiating (A11) with respect to school spending S:

$$\frac{\partial\theta^*}{\partial S} = \frac{1}{\frac{1}{\theta^*} + \beta\left(1 - \frac{q}{2}\right)\left(\frac{2}{\theta^*\left(1-\theta^{*2}\right)}\right)}\left[-\beta\left(1 - \frac{q}{2}\right)\frac{\partial q(S)}{\partial S}\frac{1}{q(S)}\right]. \tag{A12}$$

Since $\frac{\partial q(s)}{\partial S} > 0$, this derivative is generally negative.

Fact 1 The share of uneducated agents responds negatively to school spending at equilibrium, i.e., $\frac{\partial \theta^*}{\partial S} < 0$.

An individual of type θ engages in crime if the return on criminal activity is above the education-specific threshold. This is given by the following probabilities:

$$\text{Prob}\left(\varphi \geq \varphi_r^L(\theta)\right) = \int_{\varphi_r^L(\theta)}^{\bar{\varphi}} dF_\varphi = \frac{1}{2\bar{\varphi}}\left[\bar{\varphi} - \frac{q}{1-q}((\log w^L\theta) - \log \bar{c})\right].$$

$$\text{Prob}\left(\varphi \geq \varphi_r^H(\theta)\right) = \int_{\varphi_r^H(\theta)}^{\bar{\varphi}} dF_\varphi = \frac{1}{2\bar{\varphi}}\left[\bar{\varphi} - \frac{q}{1-q}(\log(w^H\theta q(S)) - \log \bar{c})\right].$$

Integrating over the ability for each education category yields the stock of criminals in each category:

$$\int_0^{\theta^*} \text{Prob}\left(\varphi \geq \varphi_r^L(\theta)\right) d\theta$$
$$= \frac{1}{2\bar{\varphi}}\left[\left[\bar{\varphi} + \frac{q}{1-q}(\log \bar{c}) - \frac{q}{1-q}(\log w^L)\right](\theta^*) - \frac{q}{1-q}\int_0^{\theta^*} \log\theta d\theta\right]$$

$$\int_{\theta^*}^{1} \text{Prob}\left(\varphi \geq \varphi_r^H(\theta)\right) d\theta$$
$$= \frac{1}{2\bar{\varphi}} \times \left\{\left[\bar{\varphi} + \frac{q}{1-q}(\log \bar{c} - \log w^H - \log(q(S)))\right](1-\theta^*)\right.$$
$$\left. - \frac{q}{1-q}\int_0^{\theta^*} \log\theta d\theta\right\}.$$

The total measure of criminals is calculated by adding the stock of criminals in each category of education:

$$C = \int_0^{\theta^*} \text{Prob}\left(\varphi \geq \varphi_r^L(\theta)\right) + \int_{\theta^*}^{1} \text{Prob}\left(\varphi \geq \varphi_r^H(\theta)\right)$$
$$\Rightarrow C = \frac{1}{2} + \frac{1}{2\bar{\varphi}}\frac{q}{1-q}$$
$$\left[\log \bar{c} - (1-\theta^*)\log(q(S)) - \log w^H + \theta^* \log\frac{w^H}{w^L} - \int_0^1 \log\theta d\theta\right].$$

Inserting from (A10) into the above equation, one can obtain the measure of criminals as a function of θ^*, S, and model parameters:

$$C = \frac{1}{2} + \frac{1}{2\bar{\varphi}}\frac{q}{1-q}\left\{\log \bar{c} - (1-\theta^*)\log(q(S)) - \log w^H \right.$$
$$\left. + \theta^* \log\left(\frac{1-\alpha}{\alpha}\right) + \theta^* \log\left(\frac{\theta^{*2}}{1-\theta^{*2}}\right) - \int_0^1 \log\theta d\theta \right\}. \quad \text{(A13)}$$

Differentiating with respect to θ^* yields:

$$\frac{\partial C}{\partial \theta^*} = \frac{1}{2\bar{\varphi}}\frac{q}{1-q} \times \left[\log(q(S)) + \log\left(\frac{1-\alpha}{\alpha}\right) + \log\left(\frac{\theta^{*2}}{1-\theta^{*2}}\right) + \frac{2}{1-\theta^{*2}} - \frac{\partial \log w^H}{\partial \theta^*}\right] \quad \text{(A14)}$$

Since in empirical studies $\frac{\partial \log w^H}{\partial \theta^*} \cong 0$ and all other right-hand side terms are strictly positive, one can reach Fact 2:

Fact 2 A decrease in the share of uneducated agents lowers the criminal measure, i.e., $\frac{\partial C}{\partial \theta^*} > 0$.

Differentiation (A13) with respect to S yields:

$$\frac{\partial C}{\partial S} = \frac{1}{2\varphi^-}\frac{q}{1-q}\left\{\frac{\partial \theta^*}{\partial S}\left[\log\frac{\theta^{*2}}{1-\theta^{*2}} + \frac{2}{1-\theta^{*2}} - \frac{\partial \log w^H}{\partial \theta^*}\right.\right.$$
$$\left.\left. + \log\left(\frac{1-\alpha}{\alpha}\right) + \log q(s)\right] - (1-\theta^*)\frac{1}{q(s)}\frac{\partial q(s)}{\partial S} - \frac{\partial \theta^*}{\partial S}\frac{\partial \log w^H}{\partial \theta^*}\right\}. \quad \text{(A15)}$$

Again, I assume $\frac{\partial \log w^H}{\partial \theta^*} \cong 0$ and $\frac{\partial q(S)}{\partial S} > 0$. In addition, from Fact 1, we have $\frac{\partial \theta^*}{\partial S} < 0$. Therefore, the right-hand side terms are all strictly negative.

Fact 3 In equilibrium, school spending can reduce the measure of criminals, i.e., $\frac{\partial C}{\partial S} < 0$.

The three facts can be summarized in Remark 1 in the text.

Appendix B

This appendix documents the first-stage results of the 2SLS model used in the article. The variable exposure in Equation 3 varies between 0 (all unexposed cohorts) to 12 (all fully exposed cohorts). Any value between 0 and 12 refers to partially exposed cohorts. I use four versions of Equation 3 to search for first-stage effects.

First, I drop the *dosage* variable and report the results for a continuous measure of *exposure* (column 1, Appendix Table B1). One year of exposure to reform-induced spending increases the per-pupil spending by 0.5%. To put it into perspective, being exposed to the reform during all school-age years (12 years of exposure) is associated with 6.6% higher spending compared to all nonexposed cohorts. Second, I replace the linear variable with a series of dummy variables for *exposure* (column 2, Appendix Table B1).

Next, to account for nonlinearities in the outcome with respect to both *dosage* and *exposure*, I use dummies for exposure interacted with dummies of dosage as the primary set of instruments. (I use this nonlinear version in the primary analysis in the text.) In these specifications, the dosage is the county rank (quartiles of) within-state income distribution in 1990. These results are reported in Appendix Table B2. As is obvious from columns 1 and 2, the largest effects appear for counties at the lower quartiles of the distribution. Moreover, within each set of counties, the larger effects occur in larger values of *exposure* variable.

Table B1. First-Stage Effect of Exposure to School Finance Reforms on Per-Pupil Spending

	Outcome: Log per Pupil Spending 5–17l	
	(1)	(2)
Linear exposure	0.0053***	
	(0.00099)	
Exposure dummies		
1		−0.00431
		(0.00339)
2		−0.00439
		(0.00456)
3		−0.00124
		(0.00587)
4		0.00088
		(0.00683)
5		0.00702
		(0.00752)
6		0.01605**
		(0.00773)
7		0.02127**
		(0.0084)
8		0.02912***
		(0.00905)
9		0.03865***
		(0.00992)
10		0.04401***
		(0.01046)
11		0.04899***
		(0.011)
12		0.05387***
		(0.01151)
Full fixed effects and trends	Yes	Yes
Observations	429,103	429,103

Notes: Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Table B2. First-Stage Effect of Exposure to School Finance Reforms on Per-Pupil Spending: Exposure and Dosage Dummies

	Outcome: Log per Pupil Spending 5–17l		
	Within-State County Income Rank in 1990		
Exposure dummies	×Quartile 1	×Quartile 2	×Quartile 3
1×	0.01982***	0.00592	0.00461
	(0.00393)	(0.00397)	(0.00343)
2×	0.02226***	0.00867*	0.00711
	(0.00487)	(0.00499)	(0.00432)
3×	0.01811***	0.00562	0.00397
	(0.00649)	(0.00666)	(0.00631)
4×	0.02365***	0.01144	0.0092
	(0.00715)	(0.00713)	(0.00695)
5×	0.0275***	0.01847**	0.01645**
	(0.00808)	(0.00789)	(0.00798)
6×	0.03203***	0.02543***	0.02301**
	(0.00874)	(0.00852)	(0.00894)
7×	0.0352***	0.03123***	0.02743***
	(0.00932)	(0.00925)	(0.00968)
8×	0.0393***	0.03265***	0.02878***
	(0.00976)	(0.00975)	(0.0102)
9×	0.03206***	0.02369**	0.01781
	(0.01177)	(0.01158)	(0.012)
10×	0.03279***	0.02277*	0.01825
	(0.01244)	(0.01213)	(0.01255)
11×	0.03186**	0.02065	0.01479
	(0.01316)	(0.01273)	(0.01321)
12×	0.03918***	0.02429*	0.01806
	(0.01365)	(0.01317)	(0.01377)

Notes: Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Appendix C

In this appendix, I investigate the causal relationship between educational spending and juvenile crime rates using the exposure to and timing of school finance reforms during the so-called *Equity Era* as exogenous shocks to per-pupil spending. I estimate the following system of equations:

$$\log\left(\overline{\text{PPE}}_{5-17}\right)_{bcst} = \alpha_1 \text{Exp}_{bs} + \alpha_2 \text{Exp}_{bs} \times \text{Dosage}_c + \varsigma X_{c,1970} \times t + \gamma_b + \theta_c + \vartheta_{st} + \varepsilon_{bcst}, \tag{C1}$$

$$\text{ArrestRate}_{bcst} = \delta \log(\widehat{\overline{\text{PPE}}_{5-17}})_{bcst} + \beta X_{c,1970} \times t + \pi_b$$

Table C1. Per-Pupil Spending and Juvenile Arrests during the Equity Era (1970–90)

Outcome variables	OLS (1)	2SLS-IV (2)	2SLS-IV (3)
All crime	−0.26647	−55.3772**	−56.32863**
	(0.94435)	(25.4391)	(25.40341)
Property crime	−0.54004*	−9.30693*	−11.36186**
	(0.27802)	(5.05125)	(4.96214)
Violent crime	−0.31524**	−7.79851*	−7.99838**
	(0.15739)	(4.07173)	(4.01866)
Index crime	−0.57995**	−10.77575**	−12.87447**
	(0.27813)	(5.4081)	(5.31042)
Simple crime	−0.5655	−39.54058***	−40.21049***
	(0.49287)	(12.86357)	(12.87662)
Drug crime	−0.10408	−8.0709*	−6.87759*
	(0.13772)	(4.23214)	(4.08277)
Financial crime	0.00439	0.19716	0.20842
	(0.00613)	(0.13922)	(0.14109)
Sexual crime	−0.04608	−0.06302	−0.12343
	(0.0355)	(0.57985)	(0.57273)
Fixed effects	Yes	Yes	Yes
1970 covariates trend	Yes	No	Yes
Observations	318,991	420,363	314,367

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. State, year, birth-cohort, and state-year fixed effects are included in the fixed effect matrix. County covariates in the initial year (1970) are interacted with a linear year trend. These covariates include the percentage of people with less than high school, high school, some college, bachelor, and graduate education, poverty rate, the percentage employed in manufacturing, mining, and construction industries, unemployment rate, percentage whites, blacks, males, the portion of the population aged 15–19 and 25–65. The dollar values are in 1990 dollars. ***$p < 0.01$, **$p < 0.05$, *$p < 0.1$

$$+ \rho_c + \sigma_{st} + \xi_{bcst}. \tag{C2}$$

All common variables and fixed effects are the same except the vector of county covariates in the initial year, which is calculated for the year 1970. Since the primary purpose of *Equity Era* reforms was to equalize state funding across school districts within a state, I use county rank in 1970 distribution of spending as the *dosage* of the treatment. As shown in Appendix Table C1, there is a strong and statistically significant effect of spending on major categories of crime during this period. However, the coefficients are slightly smaller than those documented in Table 3. A 10% rise in spending is associated with 5.6 fewer arrests per 1,000 individuals aged 15–19 (compared with 7.4 during the *adequacy era*).

The sample of this appendix includes birth cohorts of 1970–86 who are observed between the years 1985 and 2005 and were exposed to pre-1990 reforms.[31]

Appendix D

Following Brunner, Hyman, and Ju (2019), I cluster the standard errors on two dimensions: county and state-year. Appendix Table D1 reports the clustering of standard errors at other levels: state (columns 3 and 4) and county (columns 5 and 6). The OLS estimates remain insignificant (as the main results of the article replicated in column 1), while the 2SLS-IV estimates are statistically significant at conventional levels for major categories of crime.

31. Note that in the article the reforms start at 1990 and birth cohorts start at 1985.

Table D1. Robustness of the Main Results to Clustering the Standard Errors on Different Levels

	Two-Way Clustering: County and State-Year		Cluster: State		Cluster: County	
	OLS	2SLS-IV	OLS	2SLS-IV	OLS	2SLS-IV
Outcome variables	(1)	(2)	(3)	(4)	(5)	(6)
All crime	0.14345	−74.53569***	0.14345	−74.53569*	0.14345	−74.53569***
	(1.96553)	(26.31451)	(2.30387)	(40.18551)	(1.64241)	(15.92138)
Property crime	0.08424	−11.02831**	0.08424	−11.02831	0.08424	−11.02831***
	(0.32142)	(4.5418)	(0.40144)	(7.14966)	(0.26496)	(2.28412)
Violent crime	−0.21053	−12.69906***	−0.21053	−12.69906*	−0.21053	−12.69906***
	(0.31644)	(4.73766)	(0.3729)	(7.6311)	(0.28008)	(2.34008)
Index crime	0.10995	−15.48466***	0.10995	−15.48466*	0.10995	−15.48466***
	(0.3785)	(5.54336)	(0.45269)	(8.88804)	(0.32418)	(2.6051)
Simple crime	−0.04333	−39.94965***	−0.04333	−39.94965**	−0.04333	−39.94965***
	(0.76699)	(12.07167)	(0.91019)	(19.4138)	(0.65729)	(6.68356)
Drug crime	−0.83903	−10.90466	−0.83903	−10.90466	−0.83903*	−10.90466**
	(0.51749)	(7.02481)	(0.55165)	(11.07583)	(0.46297)	(4.25579)
Financial crime	0.02299	−0.295	0.02299	−0.295	0.02299	−0.295**
	(0.02195)	(0.2269)	(0.02163)	(0.34428)	(0.0205)	(0.1465)
Sexual crime	−0.00514	−1.26702*	−0.00514	−1.26702	−0.00514	−1.26702**
	(0.08554)	(0.72075)	(0.07672)	(0.88475)	(0.08583)	(0.60174)
Fixed effects and trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	318,991	314,367	318,991	314,367	318,991	314,367

Notes: The baseline results from Table 3 are replicated in columns 1 and 2. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Appendix E

This appendix examines the robustness of the main results of the article to state aggregation. I use the system of Equations 5 and 6 to estimate a 2SLS-DD strategy using UCR data aggregated at the state-birth-cohort-gender-year level. This strategy ignores the variation across counties as in the main results, where the variables were at the county- state-birth-cohort-gender-year level. The first-stage regression results (Equation 5) are reported in column 1 of Appendix Table E1. On average, fully exposed cohorts (compared to nonexposed cohorts) experience a 4.5% increase in spending during K-12 education. The coefficients of *exposure dummies* are quite similar to the first-stage effects of the article (columns 1 and 2, Appendix Table B1). The 2SLS estimates for both genders and major crime categories are reported in columns 2 through 9. The coefficients of male arrest rates are larger than the main results of Table 4 but statistically significant at conventional levels. A 10% rise in spending is associated with 19 fewer arrest rates among male individuals aged 15–19 (compared with 11.8 from Table 4). The coefficients are insignificant for female total crimes and property crimes. However, there are statistically significant effects on simple crimes among females.

Table E1. Per-Pupil Spending and Juvenile Arrests, State Aggregation

	Outcomes in Columns								
	First Stage, OLS	2SLS-DD							
	Log(Real PPE, 5–17)	All Crimes		Property Crimes		Violent Crimes		Simple Crimes	
	All (1)	Male (2)	Female (3)	Male (4)	Female (5)	Male (6)	Female (7)	Male (8)	Female (9)
Exposure dummies									
1	0.00117								
	(0.00277)								
2	0.00123								
	(0.00367)								
3	0.00468								
	(0.00459)								
4	0.00841								
	(0.00525)								
5	0.01347**								
	(0.00573)								
6	0.02013***								
	(0.00611)								
7	0.02441***								
	(0.00685)								
8	0.03024***								
	(0.00756)								
9	0.03386***								
	(0.0082)								
10	0.03716***								
	(0.00874)								
11	0.04138***								
	(0.00922)								
12	0.04533***								
	(0.00987)								
Log(real PPE, 5–17)		−190.0586***	−45.80209**	−9.89834	0.82121	−5.19619	0.14406	−120.5622***	−46.09829***
		(60.61921)	(23.01341)	(8.63685)	(6.51822)	(6.42457)	(3.44287)	(31.66312)	(13.6389)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations:	7,344	3,621	3,621	3,621	3,621	3,621	3,621	3,621	3,621

Notes: Each column represents a separate regression. Standard errors, reported in parenthesis, are clustered at the state level. State, year, birth-cohort, and state-year fixed effects are included in the fixed effect matrix. A series of state covariates in the initial year (1990) are interacted with a linear year trend. These covariates include percentage of urban residents, percentage of people with less than high school, high school, some college, bachelor, and graduate education, poverty rate, percentage employed in manufacturing, mining, and construction industries, unemployment rate, percentage whites, blacks, males, percentage of population aged 15–19 and 25–65. The dollar values are in 2017 dollars.
***$p < 0.01$, **$p < 0.05$, *$p < 0.1$.

Appendix F

This appendix complements Section 5.1 to provide further examinations regarding the possible demographic changes in response to SFR-induced fiscal changes. To check whether changes in spending due to SFR cause a compositional shift in demographic characteristics of cohorts, I replace the outcome of the second stage in the primary empirical strategy (Equation 4) with cohorts' socioeconomic characteristics. The results are reported in Appendix Table F1. No systematic evidence supports the postreforms demographic change issue. Although there is a small change in average weekly wages, the coefficient is only significant at 10% level.

Next, I examine whether the predicted per-pupil spending can explain a fraction of crime rates that are predicted by demographic variables. In so doing, I construct an index for each category of crime from a regression of the raw crime rates on demographic variables and fixed effects. The predicted value of this regression is the predicted propensity to commit a crime. This measure contains part of the actual crime rates that can be explained by demographic characteristics and fixed features of place and time. Exploring the effect of per-pupil spending on the predicted propensity to crime could be more informative in examining endogenous migration if certain associations between county demographics and spending are not necessarily

Table F1. 2SLS Results of School Finance Reforms on County Socioeconomic Characteristics

	%Whites (1)	%Blacks (2)	%Others (3)	%Hispanics (4)	Job Destruction Rate (5)	Log Total Quarterly Wages (6)	Average Weekly Wages (7)
Log(real PPE, 5–17)	−0.04919	−0.36147	0.26648	0.41427	0.05257	0.39155	35.15719**
	(0.32914)	(0.31489)	(0.26193)	(0.55599)	(0.47933)	(0.29444)	(15.87976)
Fixed effects and trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	383,031	383,031	383,031	383,031	383,031	383,031	383,031

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The dollar values are in 2017 dollars.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Table F2. Reform-Induced Spending and Demographic Predicted Propensity to Crime

	Outcome: Predicted Propensity to Crime					
	All Crimes (1)	Violent Crimes (2)	Property Crimes (3)	Index Crimes (4)	Simple Crimes (5)	Drug Crimes (6)
Reform-induced per-pupil spending:						
Log(real PPE, 5–17)	0.03837	−0.00398	−0.00518	−0.00239	0.00038	0.0235**
	(0.03577)	(0.00762)	(0.01261)	(0.01279)	(0.01142)	(0.00961)
Fixed effects and trends	Yes	Yes	Yes	Yes	Yes	Yes
Observations	383,031	383,031	383,031	383,031	383,031	383,031

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The dollar values are in 2017 dollars.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

mirrored in crime rates. If we believe that endogenous selection and relocation *AND* changes in spending is driving the main results, then looking at the (2SLS-IV) association between spending and the actual (rather than predicted) crime rates provide us with a mixed bag of both forces, changes in spending *and* changes in demographic features.

On the other hand, looking at the association between spending and demographic-induced crimes offers a raw estimation of the latter effects, the endogenous migration/relocation. Appendix Table F2 reports the estimated effects of SFR-induced spending on the predicted propensity to commit a crime. There is no statistically significant evidence of such correlations for all crimes, violent crime, property crimes, and drug-related crimes. The only anomaly is the coefficient on the predicted propensity to commit drug crimes. Since we are using an identical empirical strategy and the same variables, we can compare the magnitudes with those of the main results in Table 3. For instance, a 10% rise in per-pupil spending is associated with 0.002 fewer predicted drug arrests per 1,000, while the same shock can be translated into roughly one fewer actual drug arrests per 1,000. As we can see, the marginal impact, though statistically significant, is very small in magnitude, 0.2%. This tiny share not only relieves us from migration concerns but also validates the 2SLS-IV approach in support of the exclusion restriction assumption.

In Appendix Table F3, I revise the first-stage effects of SFR on socioeconomics and demographics using the interaction of dummies for *exposure* and a linear *dosage* variable, i.e., the median county income in the year 1990. Considering overall results from Section 5.1 and reported estimates of Appendix Table F1 through Appendix Table F3, one can conclude that there is no systematic evidence of relocation, migration, and cohort changes in response to the reform-induced fiscal changes.

Table F3. Exposure to School Finance Reforms and County Socioeconomic Characteristics

	Outcome:						
	%Whites (1)	%Blacks (2)	%Others (3)	%Hispanics (4)	Job Destruction Rate (5)	Log Total Quarterly Wages (6)	Average Weekly Wages (7)
Exposure dummies:							
1	−0.02465	0.02246	0.03563	−0.07227	0.17782**	0.01443	1.89708
	(0.05081)	(0.0404)	(0.0366)	(0.23118)	(0.07121)	(0.03694)	(3.84795)
2	0.0381	0.02767	−0.02943	−0.23517	0.17909*	−0.00194	2.47507
	(0.0594)	(0.04716)	(0.03952)	(0.20754)	(0.09419)	(0.03063)	(5.17384)
3	0.03639	−0.04716	−0.03111	0.02341	0.18975*	−0.07049**	3.73468
	(0.06885)	(0.05515)	(0.04176)	(0.2311)	(0.11298)	(0.03188)	(6.23115)
4	0.08116	−0.06294	0.00825	0.06877	0.25314**	−0.03873	3.98102
	(0.07969)	(0.05619)	(0.04505)	(0.20476)	(0.12578)	(0.03222)	(7.06101)
5	0.08496	−0.08331	−0.0107	−0.09628	0.19652	−0.05912	4.68874
	(0.08338)	(0.06534)	(0.04881)	(0.2504)	(0.13771)	(0.03753)	(7.2828)
6	0.09062	−0.0939	−0.01759	−0.07258	0.24874	−0.01362	4.86743
	(0.08896)	(0.06782)	(0.04718)	(0.24087)	(0.15225)	(0.03513)	(6.96796)
7	0.08416	−0.13626**	−0.02157	−0.04221	0.18526	−0.03149	2.8926
	(0.09089)	(0.06836)	(0.05433)	(0.25961)	(0.16157)	(0.04024)	(6.45364)
8	0.09233	−0.10458	−0.00996	0.06402	0.17943	−0.06295	2.39714
	(0.09152)	(0.07089)	(0.05969)	(0.2718)	(0.16681)	(0.0472)	(6.17933)
9	0.12022	−0.12489*	−0.00008	0.07719	0.14404	−0.0728	2.58027
	(0.09198)	(0.06979)	(0.05639)	(0.26674)	(0.17545)	(0.04579)	(5.93514)
10	0.0399	−0.11914	0.05597	0.16465	0.1609	−0.10997**	4.47772
	(0.09422)	(0.07641)	(0.0578)	(0.27857)	(0.17462)	(0.05077)	(5.84054)
11	0.10829	−0.09716	−0.02192	−0.11939	0.17696	−0.07812	6.48095
	(0.09277)	(0.0773)	(0.05941)	(0.28672)	(0.17263)	(0.05282)	(6.08297)
12	0.12504	−0.13224	−0.01142	0.0332	0.08872	−0.0556	10.46414
	(0.09617)	(0.08249)	(0.06264)	(0.30351)	(0.17408)	(0.05347)	(6.37075)

Table F3. (Continued)

	Outcome:						
	%Whites (1)	%Blacks (2)	%Others (3)	%Hispanics (4)	Job Destruction Rate (5)	Log Total Quarterly Wages (6)	Average Weekly Wages (7)
Exposure dummies × median income at 1990 ($1,000):							
1	0.00558	−0.0095	−0.01456	0.01629	−0.06466**	−0.00242	−0.70189
	(0.01889)	(0.01488)	(0.01317)	(0.07998)	(0.02736)	(0.01428)	(1.46002)
2	−0.02059	−0.01489	0.01226	0.06685	−0.06177*	−0.00241	−0.93114
	(0.02306)	(0.01862)	(0.01484)	(0.07315)	(0.03608)	(0.01112)	(1.97462)
3	−0.01927	0.01652	0.00785	−0.03304	−0.06699	0.02667**	−1.41369
	(0.02594)	(0.02152)	(0.0155)	(0.08305)	(0.04328)	(0.0126)	(2.38112)
4	−0.04216	0.01914	−0.01566	−0.07876	−0.08804*	0.01171	−1.51205
	(0.02997)	(0.02051)	(0.01608)	(0.07225)	(0.04794)	(0.01235)	(2.70089)
5	−0.04423	0.02223	−0.00321	0.02446	−0.07118	0.02053	−1.78687
	(0.03135)	(0.02454)	(0.01805)	(0.08793)	(0.05287)	(0.014)	(2.78602)
6	−0.04643	0.02995	0.00406	0.05644	−0.09036	0.0073	−1.85083
	(0.03307)	(0.0255)	(0.01728)	(0.08404)	(0.05858)	(0.01249)	(2.66695)
7	−0.04543	0.04065	0.00759	0.02434	−0.06732	0.01496	−1.08467
	(0.03423)	(0.02596)	(0.02022)	(0.09476)	(0.0622)	(0.01395)	(2.47894)
8	−0.05145	0.03235	−0.00052	−0.01701	−0.06427	0.02353	−0.88523
	(0.03442)	(0.0267)	(0.02238)	(0.09793)	(0.06423)	(0.01685)	(2.37394)
9	−0.0666*	0.03941	−0.00242	−0.03492	−0.05347	0.03276**	−0.93993
	(0.03439)	(0.02623)	(0.02121)	(0.09594)	(0.06764)	(0.01609)	(2.28578)
10	−0.02964	0.03792	−0.02163	−0.05822	−0.05449	0.04853***	−1.70607
	(0.03549)	(0.0288)	(0.02172)	(0.10104)	(0.06715)	(0.01813)	(2.2599)
11	−0.05436	0.02592	0.00898	0.04148	−0.06731	0.03566*	−2.5469
	(0.03503)	(0.02939)	(0.02256)	(0.10375)	(0.06635)	(0.0189)	(2.36799)
12	−0.06673*	0.03797	0.00531	−0.0248	−0.03547	0.02702	−4.22284*
	(0.03656)	(0.03145)	(0.02378)	(0.1124)	(0.06759)	(0.01883)	(2.49699)
Fixed effects and trends	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	394,971	394,971	394,971	394,971	394,971	394,971	394,971

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The dollar values are in 2017 dollars.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Appendix G

Since the UCR data report crime rates at the county level, the school spending data is aggregated at the county level accordingly. This appendix examines to what extent the forgone variations in spending due to aggregation may affect the results. In so doing, I look into the first-stage effects that are not aggregated to the county level and compare the estimates with those county aggregates. Next, I compare the first-stage effects across areas that school districts' boundaries match county boundaries closely with those areas where there is a higher number of school districts in county boundaries. The spending varies at birth cohort b and also school district d.[32] Note that in the following 2SLS system of equations, there is no *year* variable. I modify the first-stage regression of Equation 3 to build an isolated first-stage equation as the following:

$$\log(\overline{\mathrm{PPE}}_{5-17})_{bds} = \alpha_1 \mathrm{ExP}_{bs} + \alpha_2 \mathrm{ExP}_{bs} \times \mathrm{Dosage}_d + \zeta_d \times T_b + \gamma_b + \theta_d + \varepsilon_{bds} \quad \text{(G1)}$$

The *dosage* and 1990-covariates (X) vary at the school district level. The matrix of school district fixed effects (ζ) is interacted with a birth cohort linear trend (T_b). The standard errors are clustered at the school district level. The results are reported in Appendix Table G1 through Appendix Table G3. Column 1 of each table reports the results for all districts, column 2 for the subsample of below median of district-per-county distribution, and column 3 for above-median of district-per-county distribution. First, I interact a dummy for exposure with a linear dosage (county income at 1990). These results are reported in Appendix Table G1. There is considerable heterogeneity in the coefficients of the two following subsamples of columns 2 and 3. The below-median sample reveals coefficients that are almost half the coefficients of the above-median subsample.

Next, I exclude the *dosage* variable and focus on the DD approach by regressing the spending on a series of dummies for *exposure.* The results

32. In arrest data, we have arrest reports by age and year (and place). Therefore, in the main analysis, we observe time dimension and include year fixed effect. However, the spending is at the birth-cohort level and lacks the time dimension and hence time fixed effects. One should note that each cohort is exposed to one set of per-pupil spending during ages 5–17, and this set does not vary by time.

are reported in Appendix Table G2. The coefficients are slightly larger than those of county-level first-stage effects (compare with Appendix Table B1). This suggests that aggregating the spending at the county level is likely attenuating our first-stage effects. Finally, I explore the effect of a linear measure of exposure in Appendix Table G3. The effects are larger than those in column 1 of Appendix Table B1 for county aggregates. However, in this setting, the marginal effects of columns 2 and 3 (districts below-median and above-median district-per-county) are quite similar.

The big picture is that aggregating the spending at the county level is not inflating the first-stage effects in ways that artificially changes the 2SLS results.

Table G1. First-Stage Effects at the District Level, Exposure Dummies Interacted with Linear Dosage

	All Districts (1)	Below Median of District Per-County Distribution- (2)	Above Median of District-Per-County Distribution- (3)
Exposure dummies			
1	0.00879***	0.01708***	0.0166***
	(0.00335)	(0.00559)	(0.00524)
2	0.01228***	0.02203***	0.02808***
	(0.00463)	(0.00748)	(0.00746)
3	0.0178***	0.03098***	0.03973***
	(0.00594)	(0.00937)	(0.00966)
4	0.02333***	0.04337***	0.05561***
	(0.00692)	(0.01128)	(0.01126)
5	0.03906***	0.05844***	0.08033***
	(0.00803)	(0.01303)	(0.01306)
6	0.05878***	0.07009***	0.10134***
	(0.0091)	(0.01454)	(0.01494)
7	0.07551***	0.08909***	0.11723***
	(0.01006)	(0.01607)	(0.01646)
8	0.09455***	0.10897***	0.1349***
	(0.01103)	(0.01766)	(0.01817)
9	0.11061***	0.13182***	0.14835***
	(0.01208)	(0.01919)	(0.02)
10	0.12745***	0.14938***	0.15631***
	(0.01272)	(0.02034)	(0.02142)
11	0.14855***	0.16132***	0.18296***
	(0.01318)	(0.0211)	(0.02263)
12	0.16973***	0.17729***	0.2029***
	(0.01387)	(0.02239)	(0.02397)

Table G1. (Continued)

Exposure dummies	All Districts (1)	Below Median of District Per-County Distribution- (2)	Above Median of District- Per-County Distribution- (3)
Exposure dummies × median income 1990 ($1,000)			
1	−0.00284***	−0.00889***	−0.00245*
	(0.00102)	(0.00205)	(0.00139)
2	−0.00319**	−0.01147***	−0.00371*
	(0.00142)	(0.00274)	(0.00196)
3	−0.004**	−0.01469***	−0.00503**
	(0.00183)	(0.00346)	(0.00254)
4	−0.00197	−0.01782***	−0.0042
	(0.00214)	(0.00421)	(0.00294)
5	−0.00375	−0.02138***	−0.00734**
	(0.00248)	(0.00487)	(0.0034)
6	−0.00548*	−0.02007***	−0.0094**
	(0.00283)	(0.00546)	(0.00392)
7	−0.00789**	−0.02318***	−0.01131***
	(0.00315)	(0.00603)	(0.00436)
8	−0.0108***	−0.02628***	−0.01368***
	(0.00348)	(0.00666)	(0.00483)
9	−0.01357***	−0.03185***	−0.01551***
	(0.00384)	(0.00727)	(0.00535)
10	−0.01774***	−0.03549***	−0.01757***
	(0.00405)	(0.00777)	(0.0057)
11	−0.02405***	−0.03825***	−0.02505***
	(0.0042)	(0.00809)	(0.006)
12	−0.02915***	−0.04135***	−0.02953***
	(0.00445)	(0.00863)	(0.00635)
Observations	180,372	98,617	81,755

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Table G2. First-Stage Effects at the District Level, Exposure Dummies

	All Districts (1)	Below Median of District-Per-County Distribution (2)	Above Median of District-Per-County Distribution (3)
Exposure dummies			
1	−0.00006	−0.0076***	0.00803***
	(0.0011)	(0.00149)	(0.00157)
2	0.00209	−0.00999***	0.01488***
	(0.00156)	(0.00199)	(0.00233)
3	0.00514**	−0.00814***	0.02142***
	(0.00205)	(0.00254)	(0.00312)
4	0.0167***	−0.00528*	0.04008***
	(0.00243)	(0.00306)	(0.00373)
5	0.02681***	−0.00172	0.05443***
	(0.00287)	(0.00364)	(0.00438)
6	0.04082***	0.01205***	0.06821***
	(0.0032)	(0.00415)	(0.00488)
7	0.04941***	0.02152***	0.07701***
	(0.00348)	(0.00455)	(0.0053)
8	0.05935***	0.03276***	0.08629***
	(0.00378)	(0.00495)	(0.00576)
9	0.0676***	0.04107***	0.09419***
	(0.00409)	(0.00537)	(0.00624)
10	0.07113***	0.04946***	0.09408***
	(0.00433)	(0.00566)	(0.00663)
11	0.07435***	0.0548***	0.09457***
	(0.00451)	(0.00587)	(0.00698)
12	0.08109***	0.06427***	0.09768***
	(0.00472)	(0.00611)	(0.00739)
Observations	180,372	98,617	81,755

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Table G3. First-Stage Effects at the District Level, Linear Exposure

	All Districts (1)	Below Median of District-Per-County Distribution (2)	Above Median of District-Per-County Distribution (3)
Exposure	0.00791***	0.00756***	0.00796***
	(0.00038)	(0.00049)	(0.00058)
Observations	180,372	98,617	81,755

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Appendix H

This appendix complements the robustness checks of Table 5 by exploring sensitivity to functional forms, subsamples, and alternative specifications. The results are reported in Appendix Table H1 The results are arguably robust across models and outcomes. For instance, a $1,200 increase in spending (refer to row 8), roughly a 10% rise, leads to 7.2 fewer male arrest rates (compare with 11.8 in the baseline estimates).

Table H1. Sensitivity of the Effect of School Spending on Juvenile Crime in Different Specifications and Subsamples

	Males				Females			
	All Crimes (1)	Property Crimes (2)	Violent Crimes (3)	Simple Crimes (4)	All Crimes (5)	Property Crimes (6)	Violent Crimes (7)	Simple Crimes (8)
(1) Baseline results from Table 4	−118.83084***	−16.81385**	−19.75927***	−61.30778***	−30.2267**	−5.23684*	−5.63812*	−18.58598***
	(40.72899)	(6.63928)	(6.73474)	(18.21589)	(13.47381)	(3.02157)	(3.18773)	(6.54946)
(2) Above-median county population	−133.92724**	−16.25331*	−19.28292**	−69.5411**	−28.73809	−5.66597	−2.61318	−25.11674**
	(66.40229)	(8.89055)	(9.41068)	(27.87888)	(20.53474)	(4.47801)	(4.54565)	(10.16953)
(3) Above-median county arrest rate at 1990	−134.70006**	−20.76311**	−22.29482***	−74.96741***	−28.26533	−4.9646	−5.89546	−27.04127***
	(56.85799)	(8.06613)	(8.31554)	(23.6784)	(19.18691)	(3.99863)	(4.31654)	(9.51666)
(4) Adding age trend	−118.79203***	−16.81862**	−19.76253***	−61.29512***	−30.22334**	−5.23958*	−5.63973*	−18.58379***
	(40.61398)	(6.6302)	(6.7416)	(18.17211)	(13.45563)	(3.01132)	(3.19139)	(6.53117)
(5) Adding state-cohort and cohort-year FE	−61.0563	−20.24611**	−24.50953***	−17.21805	−23.49161	−15.76313***	−14.81412***	−2.10067
	(53.61579)	(8.64398)	(7.46244)	(20.06394)	(20.16017)	(4.69234)	(4.4217)	(7.2207)
(6) Adding county-year FE	−117.74779***	−17.28756**	−20.746***	−61.82342***	−30.23719**	−4.54466	−6.35913*	−19.47155***
	(43.28332)	(6.97107)	(7.18994)	(19.18604)	(14.26703)	(3.13556)	(3.4148)	(6.95015)
(7) Second-stage outcomes in log (columns 1–8)	−0.52312	−1.33408	−3.26261**	−4.75277**	−0.95663	−1.21088	−2.80442*	−4.78154**
	(1.36053)	(1.27166)	(1.33158)	(2.13935)	(1.44852)	(1.45509)	(1.61643)	(2.22717)
(8) First-stage outcome: level of PPE 5–17 ($1,000)	−6.0384**	−0.69418*	−0.95274**	−3.06101***	−1.42752*	−0.20324	−0.25483	−0.95395**
	(2.51371)	(0.39641)	(0.39545)	(1.13078)	(0.83088)	(0.19092)	(0.19425)	(0.41382)
(9) Only reform states (2SLS-DD)	−47.97236	−5.48862	−10.27195*	−11.59082	−19.82028	−0.24548	−3.65709	−3.06707
	(36.9424)	(5.85596)	(5.72753)	(16.00443)	(12.2029)	(2.77143)	(3.00003)	(5.45563)

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The arrest data cover the years 2000–18. The arrest rates for all crime categories are per 1,000 persons of county population aged 15–19. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression.
***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Appendix I

In Columns 2 and 3 of Table 3, there is a large jump in the effects as I include state-by-year dummies. I also observe a similar jump when I exclude/include county-trend. To see why the inclusion of county trend and state-year fixed effects is important, I replicate the endogeneity analysis (in Table 6) for a parsimonious specification where state-year dummies and county trends are excluded. The results are reported in Appendix Table I1 The balancing test fails across several outcomes and coefficients.

Table I1. Exposure to School Finance Reforms and County Socioeconomic Characteristics, Excluding State-Year Fixed Effects and County Trend

	Outcome:						
	%Whites (1)	%Blacks (2)	%Others (3)	%Hispanics (4)	Job Destruction Rate (5)	Log Total Quarterly Wages (6)	Average Weekly Wages (7)
Exposure	0.02304*	−0.03238***	0.01143	−0.0364	0.02585	−0.00494***	0.14728
	(0.01353)	(0.00987)	(0.00786)		(0.01619)	(0.00178)	(0.60843)
Exposure × dosage dummies							
2	0.00834	0.00348	−0.00705	0.02491	−0.0181	0.00207	0.03179
	(0.01426)	(0.0102)	(0.00818)	(0.0202)	(0.01373)	(0.00204)	(0.69903)
3	−0.0136	0.01378	−0.01229	0.06768***	−0.00973	0.00407*	−0.16753
	(0.01582)	(0.0107)	(0.00914)	(0.02201)	(0.01536)	(0.0023)	(0.8529)
4	−0.09507***	0.05369***	−0.01418*	0.10192***	−0.02944**	0.00849***	0.00754
	(0.0199)	(0.01465)	(0.0078)	(0.02351)	(0.01457)	(0.0019)	(0.79837)
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	406,395	406,395	406,395	406,395	406,395	406,395	406,395

Notes: Each column represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression. The dollar values are in 2017 dollars.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

Appendix J

Since there are some state reforms during post-2012 (and not included in the main reform database), one may argue that the new reforms could confound the estimates as we observe cohorts up to 2018 and the effects accumulate over the years. Therefore, in Appendix Table J1, I replicate the main results in a subsample excluding post-2012 observations. The point estimates are similar to the main results of Table 3.

Table J1. Per-Pupil Spending and Juvenile Arrests Pre-2012

Outcome variables	OLS (1)	2SLS-IV (2)	2SLS-IV (3)
All crime	0.00635	−24.39833	−66.31925**
	(2.85541)	(20.19422)	(28.82195)
Property crime	−0.26318	−2.41064	−9.39273*
	(0.48255)	(3.60184)	(4.98852)
Violent crime	−0.55574	−4.34685	−12.57555**
	(0.39773)	(3.41306)	(5.26046)
Index crime	−0.37058	−4.90094	−13.48076**
	(0.50947)	(4.316)	(6.02206)
Simple crime	−0.486	−21.48183**	−44.37796***
	(1.10855)	(9.30393)	(13.66929)
Drug crime	−1.75793***	−5.81768	−11.28034
	(0.67216)	(5.29958)	(7.72639)
Financial crime	0.02741	−0.24646	−0.46851**
	(0.02342)	(0.161)	(0.22738)
Sexual crime	−0.06961	−1.44805**	−1.83353**
	(0.10459)	(0.59695)	(0.83302)
Fixed effects	Yes	Yes	Yes
State-year FE	Yes	No	Yes
Observations	318,991	420,363	314,367

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, state-year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression. The dollar values are in 2017 dollars.

***$P < 0.01$, **$P < 0.05$, *$P < 0.1$.

Appendix K

In this appendix, I re-evaluate the results of Table 7 (Section 5.2) by exploring the effects of reforms on changes in policing employment and spending through a series of event-study analyses. In these analyses, the event time is the distance in years relative to the year the state passes school finance reform. Since the police employee and expenditure data is at the state level, the regressions also include state and year fixed effects. To isolate the reform-induced changes in policing employment and spending, I also control for several state-by-year demographic and economic characteristics including the log of population, unemployment rate, share of males, share of blacks, and share of people aged 15–19. The results are reported in Appendix Figure K1, K2, and K3 for police employee per capita, police

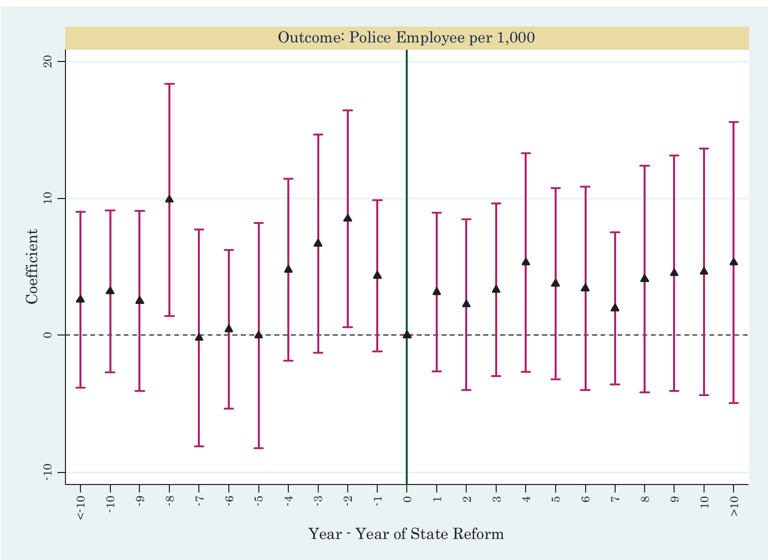

Figure K1. Event-Study Analysis to Explore the Effect of School Finance Reforms on Police Employees per Capita
Notes: The point estimates and 95% confidence intervals are illustrated. All regressions include state fixed effects, year fixed effects, share of males, share of blacks, share of individuals aged 15–19, unemployment rate, and log of population. Standard errors are clustered at the state level.

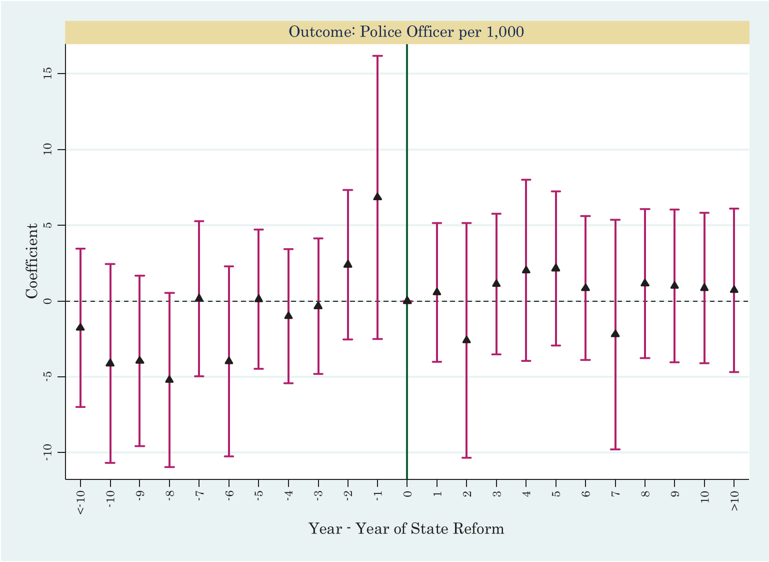

Figure K2. Event-Study Analysis to Explore the Effect of School Finance Reforms on Police Officers per Capita
Notes: The point estimates and 95% confidence intervals are illustrated. All regressions include state fixed effects, year fixed effects, share of males, share of blacks, share of individuals aged 15–19, unemployment rate, and log of population. Standard errors are clustered at the state level.

officer per capita, and police expenditure per capita as the outcome, respectively. The pre/postreform coefficients do not provide consistent, strong, and statistically significant evidence that policing employment and expenditure changed as a prediction of future school reforms or as a response to the passage of reforms in the previous years. These results lend to the exogeneity assumption of the main findings of the article.

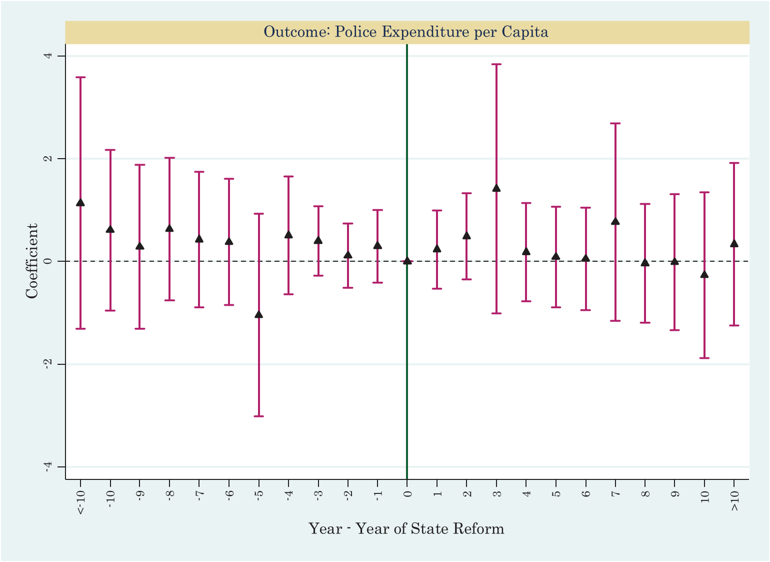

Figure K3. Event-Study Analysis to Explore the Effect of School Finance Reforms on Police Expenditure per Capita

Notes: The point estimates and 95% confidence intervals are illustrated. All regressions include state fixed effects, year fixed effects, share of males, share of blacks, share of individuals aged 15–19, unemployment rate, and log of population. Standard errors are clustered at the state level.

Appendix L

In the main analyses of the article, the district-level spending data are aggregated into the county level as the arrest data are at the county level. I use district-level K-12 enrollment as the weights in aggregating the spending data. In this appendix, I show the results for the case where spending data is aggregated without any weighting scheme. The results, reported in Appendix Table L1, are somewhat larger than the marginal effects reported in Table 3, but economically and statistically significant.

Table L1. Per-Pupil School Spending and Juvenile Arrest for Unweighted Spending Aggregation

Outcome variables	OLS (1)	2SLS-IV (2)	2SLS-IV (3)
All crime	2.36996	−26.04335**	−84.82543***
	(1.5765)	(12.91332)	(28.54493)
Property crime	0.07349	−2.33238	−12.5694**
	(0.24988)	(2.33116)	(4.89555)
Violent crime	−0.36042	−3.14171	−14.51967***
	(0.22218)	(1.93399)	(5.15347)
Index crime	−1.41782***	−3.59465	−17.05309***
	(0.35599)	(2.79821)	(5.9836)
Simple crime	0.33469	−10.69725**	−46.45451***
	(0.58575)	(5.26898)	(13.10195)
Drug crime	2.06827***	−5.01923	−11.49951
	(0.61522)	(4.12583)	(7.86288)
Financial crime	0.09494***	−0.3905**	−0.41906*
	(0.02762)	(0.16912)	(0.24676)
Sexual crime	0.7811***	−1.42545*	−1.33438*
	(0.12856)	(0.81812)	(0.73824)
Fixed effects	Yes	Yes	Yes
State-year FE	Yes	No	Yes
Observations	423,351	413,259	413,259

Notes: Each cell represents a separate regression. Standard errors, reported in parentheses, are two-way clustered at the county and state-year level. County, year, birth-cohort, and gender fixed effects are included in the fixed effect matrix. All regressions include a county-specific linear time trend. The arrest data cover the years 2000–18. The arrest rates for all crime categories are per 1,000 persons of county population aged 15–19. The independent variable is the predicted value of log per-pupil spending in 2017 dollars during K-12 education from the first-stage regression.
$^{***}P < 0.01$, $^{**}P < 0.05$, $^{*}P < 0.1$.

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