

Technical Report

THE EFFECTS OF HURRICANE KATRINA AND THE NEW ORLEANS CHARTER SCHOOL REFORMS ON YOUTH CRIME

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FOR NEW ORLEANS

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The Effects of Hurricane Katrina and the New Orleans Charter School Reforms on Youth Crime

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Abstract: *Hurricane Katrina, in addition to being one of the nation's worst natural disasters, triggered various policy changes in New Orleans, including extensive charter-based school reforms. We provide some of the first evidence on the effects of these combined events on the rate of juvenile adjudications and criminal convictions of students who attended New Orleans publicly funded schools. First, we find that Katrina as a whole did not increase youth crime and probably reduced youth crime. We then use two novel variations of Synthetic Control Group (SCG) analysis to isolate the effects of the school reforms: (a) adapting SCG in the spirit of a triple-difference analysis; and (b) examining the identifying assumptions of SCG using extensive qualitative data collection. Applying these methods with statewide student-level longitudinal data from Louisiana, we find that the charter school reforms were likely the main driver of the reduction in convictions and this was driven by actual reductions in crime, not changes in policing and prosecution.*

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I. Introduction

Hurricanes Katrina and Rita destroyed much of the city of New Orleans and triggered a wide variety of policy changes when they struck in 2005. This led to a considerable body of research on the hurricane (Elliott and Pais, 2006; Frey et al., 2007; Fussell, 2015; Fussell et al., 2010; Groen and Polivka, 2008, 2010; Kates et al., 2006; Hamel, Firth, and Brodie, 2015; etc.). In this study, we focus on the effects of Katrina on an outcome that has not been previously studied: youth crime.¹

The storms might have influenced youth crime through a number of mechanisms. Here, we are especially interested in the post-Katrina school reforms, arguably the most aggressive market-based school reforms in modern U.S. history (Harris, 2020). The results to date suggest that the net effects of Katrina involved large increases in student test scores, graduation, and college entry and that these effects were mainly driven by the subsequent school reforms (Harris & Larsen, forthcoming). Studies of charter schools nationally (CREDO, 2015; Chen & Harris, 2021) and in other specific locations (e.g., Abdulkadiroğlu et al, 2011; Booker et al., 2011; Dobbie & Fryer, 2015; Angrist et al., 2016) also increasingly show positive academic results. Other research and reviews show indirect benefits of charter schools in terms of competition (Zimmer & Buddin, 2009; Cohodes and Parham, 2021) and induced closure of traditional public schools (Chen & Harris, 2021). However, like much of the broader school reform literature, we know much less about non-academic outcomes, such as crime.

This paper adds to these strands of literature by studying two questions. First, what were the effects of Katrina as a whole on the criminal convictions of New Orleans youth after the storm?

¹ Some prior post-Katrina crime literature has been descriptive and mostly concentrated on adult crimes (Brezina and Kaufman, 2008; Curtis et al., 2013; Doucet and Lee, 2015; Frailing and Harper, 2016; Barton et al., 2020; etc.).

Here, we find causal evidence that the hurricane reduced youth crime. Second, to what degree were these effects attributable to various mechanisms, including population change and the school reforms? Our analysis suggests that the overall reductions in crime were mainly due to the school reforms. This is noteworthy given the unprecedented nature of the school reforms.

After Katrina, the state took over almost all publicly funded schools in New Orleans and gave control to its Recovery School District (RSD), which gradually turned management over to non-profit charter organizations. All teachers were fired, and the union contract was allowed to expire, giving schools almost complete autonomy over personnel. The usual attendance zones were essentially eliminated, and families were free to choose almost any public school using a lottery-based centralized enrollment system. School funding levels also increased by 13 percent from pre-Katrina levels (Buerger & Harris, 2021). No school system, before or since, has upended the traditional public school system to the degree New Orleans has.

Our findings build on a number of others that have studied school choice effects on crime. A higher degree of Tiebout school choice, instrumented by the prevalence of rivers and streams in the area, has been shown to reduce teenage arrest rates (Dills and Hernández-Julián, 2011). Cullen, Jacob, and Levitt (2006) and Deming (2011) exploit admission lotteries in district-wide open enrollment systems to study the effects of attending a preferred school on arrests and/or incarcerations.² Other studies of choice among traditional public school systems (MacDonald and Nicosia, 2015)³ and between traditional public schools and private school vouchers (DeAngelis & Wolf, 2016, 2019) rely on matching methods and/or focus on self-reported crime. All of the above

² Of the two, Cullen, Jacob, and Levitt (2006) only looked at self-reported arrests while Deming (2011) considered both arrests and incarcerations from administrative records.

³ MacDonald and Nicosia (2015) find that charter schools may have reduced crime in the Census blocks near where they open. However, this analysis does not address whether *attending* a charter school has an effect.

studies have found that school choice reduced crime, though the methodological issues may mean that their estimates are biased toward finding crime reductions. With matching methods, we might expect that choosers of alternative options have unobserved characteristics that make them less likely to commit crimes. Also, school choice may affect the likelihood of responding to surveys and/or accurately reporting criminal behavior. For example, students using vouchers to move to religious private schools might be less likely to report misbehavior. Under-reporting of one's own delinquent activity is a well known problem (Thornberg and Krohn, 2003).⁴

We are aware of two studies about the crime effects of charter schools specifically (Dobbie and Fryer, 2015; McEachin et al., 2020). Both find crime-reduction effects but also suffer from similar methodological issues as other school choice papers above. Although Dobbie and Fryer (2015) use a quasi-experimental design, the authors only focus on self-reported incarceration for students attending a single high-performing charter middle school in New York City. McEachin et al. (2020) use administrative data but rely on matching on observable characteristics to estimate charter school effects. In this study, we instead examine the effects of charter schools on youth crime using a quasi-experimental analysis of administrative data that also accounts for unobserved factors.

Another contribution of the present study is advancing quasi-experimental methods for causal analysis. We start with the Synthetic Control Group (SCG) method, which accounts for time-constant factors to identify the effects of Katrina as a whole. This and all the analyses rely on statewide student-level data from Louisiana that links individual students to juvenile adjudications and criminal convictions in the juvenile and adult courts, respectively, over the years 2001-2018. We carry out two versions of each analysis: (a) panel analysis that tracks individual students in

⁴ Thornberg and Krohn (2003) also report “looking at the issue of differential validity, there are some disturbing differences by race” (p.61), which correlates with participation in choice programs.

grades 4-6 before Katrina through the post-Katrina period; and (b) pooled analysis that compares successive cohorts of students in grades 7-9. The panel and pooled methods require somewhat different assumptions, which provides an additional check on identification. We are also able to separate criminal convictions by crime type (violent, drug, property, and other crimes), some of which are more likely to satisfy the identifying assumptions than others.

The above SCG is now fairly standard, but to isolate the role of the school reforms from the overall Katrina effect, we adapt the SCG method in the spirit of a triple-difference. In doing this, we switch the dependent variable from the *conviction rate* to what we call the *conviction ratio*: that is, the youth crime rate divided by the crime rate for young adults (ages 25-34). This is a useful comparison group, as both youths and young adults were affected by Katrina and generally faced the same community conditions affecting crime. The main difference between the two groups is that our population of interest (youths) experienced the school reforms while young adults did not.

Still, it is possible that changes in the social, economic, and political context affected youth crime differently than adult crime in New Orleans, making it difficult to separate the overall Katrina effect from the school reform effect. For this reason, we develop an additional method for examining the identifying assumptions. This involves extensive qualitative data collection, including public document searches, a focus group, and interviews with practitioners directly involved in the criminal justice system in New Orleans. While qualitative methods are increasingly coupled with quantitative analysis in “mixed methods” analysis to understand the mediators of causal effects, we are aware of few examples where such methods are used to examine the identifying assumptions of quasi-experimental methods, and those few cases we could find say

relatively little about the method.⁵ We describe how this method can be used in a variety of natural experiments with public policy.

Our results suggest that Katrina reduced the youth conviction rate but reduced the conviction ratio to young adults even more. This conclusion holds in both the panel and pooled analyses and in a variety of other robustness checks. In the triple-difference analysis, the identifying assumption is that there was no change in non-school factors that affected the ratio of youth-to-young-adult crime differently in the treatment versus comparison groups. For the pooled analysis, we perform a quantitative test of the assumption for one specific non-school mechanism that could have affected the youth conviction rate differentially: changes in the population of students attending publicly funded schools in New Orleans compared to those of the young adults sample. We find no evidence that undermines the crime reduction in our estimates.⁶

The conclusion that the school reforms drove the crime reductions is also reinforced by the qualitative results. Though we discuss a large number of possible violations (e.g., changes in policing, prosecution, or detention), the document analyses, focus group, and interviews suggest that only two possible threats to identification. The first threat, trying youths as adults, would bias the estimates toward finding increases in crime. The second threat, a pretrial risk assessment program for juveniles, might contribute to a decrease in New Orleans youth conviction rate but cannot explain the significant crime reduction effects on the conviction ratio for violent crime in the later part of our panel. All of our analyses therefore suggest that the combination of Katrina

⁵ In addition to standard searches of research databases, we solicited ideas on this through economics and policy research groups on social media. The examples that others volunteered were examples of general mixed methods analyses, where the qualitative analysis was aimed at understanding program implementation and mechanisms, not identifying assumptions. In general, it seems researchers sometimes discuss the policies they are studying with policymakers, program managers, and/or participants, but in informal ways and usually without focusing on the identifying assumptions.

⁶ We use Census data to calculate a triple-difference on an extensive array of demographic factors. The only significant third difference is in household income, which would have caused an increase in the conviction ratio of New Orleans compared to the comparison group, the opposite of our findings.

and the changes it triggered probably reduced crime and that the school reforms were the main mechanism.

This finding is plausible given prior evidence of the effects of school choice on crime (Dobbie and Fryer, 2015; McEachin et al., 2020) and what we know about how the student experience changed in New Orleans as a result of the reforms. In addition to the city's increase in academic outcomes (Harris & Larsen, forthcoming), the reforms increased the amount of time students spent in school (Harris & Larsen, 2019) and therefore have reduced the amount of unsupervised time students had to engage in criminal activity. While we find evidence that students in individual schools that relied on exclusionary discipline were more likely to commit crime, the overall use of exclusionary discipline in the city as a whole was about the same before and after Katrina (Hernández, 2019). Finally, school choice might improve the match between student needs and educational options in ways that affect the propensity to commit crime (e.g., some students might benefit from strict, academically focused schools while others benefit from more relaxed environments that balance academics with extracurricular activities and/or experiential learning). While there are also theories about how this type of reform could increase crime (see above regarding exclusionary discipline), the empirical evidence suggests that these have been more than offset by crime-reducing factors. Whatever the reasons, school choice appears to reduce crime.

Section II describes our data and quantitative methods. In section III, we describe our qualitative approach to testing the quantitative identifying assumptions. That discussion also provides relevant context about New Orleans. Sections IV and V show our results on the overall effects of Katrina and the different mechanisms, respectively. Section VI concludes.

II. Data and Empirical Framework

II. A. Data

Our dependent variable is combined juvenile adjudications⁷ and criminal convictions (henceforth referred to as convictions) from 2001 to 2018. These come from administrative data from the State of Louisiana's Department of Corrections (DOC) and Office of Juvenile Justice (OJJ). The DOC data have the date the crime was reported. In the OJJ data, the crime date is not available, and we are forced to use the petition date when charges were filed. The crime data include a unique individual identifier that allows us to match individuals to records from the Louisiana Department of Education (LDOE), which include information on school enrollment, demographics, achievement, and school discipline incidents (suspensions and expulsions). Student data from LDOE are only available through 2012 due to state data-sharing restrictions that prevent student data from being linked with other data in more recent years. Throughout the analysis, we assign convictions to academic, rather than calendar, years using the dates August 1 - July 31.⁸ Henceforth, we use spring year to denote the school year, e.g., 2008 means the 2007-08 school year.

Our next step is to use the above data to create the conviction rate for students, i.e., the

⁷ The juvenile justice system handles two types of cases for youths: (1) families in need of services (FINS) petitions and (2) delinquent acts. FINS petitions involve conducts that would not be a crime if committed by an adult but involve behavior by a youth that consists of some other type of violation or infraction or a violation of law by a child under age 10. Delinquent acts include those that would be considered a crime if committed by an adult, but are classified as delinquent acts if committed by a child 10 years of age or older. During the study period, youth are defined as those under the age of 17, however for some serious offenses prosecutors have discretion to prosecute 15- and 16-year-olds as adults. For simplicity throughout the paper, all acts related to the juvenile or adult systems are referred to collectively as crimes, and adjudications of those crimes are referred to as convictions. We do not observe any specific markers to distinguish FINS petitions and delinquent acts in our data. However, we categorized juvenile adjudications to align with the categories of adult crime, so delinquent acts must have been added to their appropriate categories and FINS petitions must have been considered "other crimes."

⁸ Because some students will change schools, or move into an area well into the school year, we only consider crimes after the entry date for their main school of the year (up through July 31).

number of convictions divided by the number of students in the relevant sample. We then carry out the analogous calculation for young adults, aged 25-34, to construct the student-to-young-adult conviction ratio. As explained further in subsection II.C, the major post-Katrina difference between this second group and our subjects of interest is that they did not experience the school reforms. Hence, the analysis of this variable allows us to partially decompose the different mechanisms of the combined Katrina effects, in particular, school and non-school factors.

In our main analyses, we include all types of crime, but we also rely on convictions by type of crime as part of our identification strategy. The state data include 990 unique crime categories, which we divide into violent, property, drug, and other crimes according to the FBI's categorization. 6.2% of the conviction observations in our analysis sample omit the crime type; these cases with missing crime type information are included in the all-crime analyses, but excluded from the analyses of crime categories.

We carry out two types of analyses using (a) a panel of students who returned to (or stayed in) their pre-hurricane districts (i.e., returnees); and (b) pooled cross-sections of student cohorts who were in specific grades pre- and post-Katrina. The panel analysis follows the same individuals into early adulthood through the end of the crime data in 2018, allowing us to account for unobservable differences across individuals. Furthermore, the long horizon of this analysis captures both the short- and long-term effects of Hurricane Katrina on returnees until they were 23-25 years old. The weakness of the panel analysis lies in its limited statistical power and generalizability, as returnees make up only a limited, non-random subsample of the original population.

In contrast, the pooled analysis includes almost all students who were in New Orleans public schools pre- and post-Katrina in grades 7-9, spanning the 2001-2012 school years. This

pooled analysis also complements the panel analysis in two ways. First, the net effects of Katrina on the pooled cross-sections include the composition effects of population changes (if any) that are absent from the panel analysis.⁹ Thus, the differences between the two results are informative about the fraction (if any) of the Katrina effects that can be attributed to population change alone. Second, the pooled analysis focuses on criminal behavior while of school age, which we can only observe in the earlier years for the panel.

For the panel analysis, we construct a sample of students who were in grades 4-6 for the first time during the 2005 school year who returned to their pre-Katrina school districts for at least three years post-Katrina. We observe these returnees for several years in the education data pre-Katrina and match them to the conviction records in all years up to 2018 to track criminal activities, even if they dropped out and/or eventually transferred out of their home districts.

Table 1 compares the characteristics of the panel of returnees to all first-time 4th-to-6th graders in the school year 2005 for both New Orleans and other Louisiana school districts. Compared to the overall New Orleans student population, the returnee panel consists of slightly lower proportions of Black, lower-income, and special-education students and higher proportions of White students, students of other races, and English Language Learners. Returnees also experienced fewer disciplinary incidents on average than non-returning students. None of these differences are statistically significant. Nevertheless, we are cautious in extrapolating our panel findings to the broader pre-Katrina population.

Figure 1A compares the trend of total conviction rates for returnees in New Orleans and in the rest of the state (see Appendix A for all results separated by crime type). The trend over time reflects not only the year-to-year changes in the environment but also the effect of the same

⁹ Changes in the student population (if any) might still affect the panel analysis through peer effects, as we elaborate later in Section II.C.

group of returnees growing older. The conviction rates peaked in 2010 for the rest of the state when the returnees would have been in grades 9-11. In comparison, the patterns for New Orleans are more erratic, due in part to the smaller sample involved with just one city, but total conviction rates were lower in New Orleans for all years. Our second dependent variable, the student-to-young-adult conviction ratio (Figure 1B), follows a similar pattern. The spike in New Orleans's ratio for total crime and drug crime in 2015 coincided with the DA's reelection campaign, which focused on juvenile, instead of adult, crime. Except for this one instance, total crime, as measured by the ratio, was lower in New Orleans than in the rest of the state for all years. The differences were large in the years immediately after Katrina, but the outcomes converged over time. By last year, 2018, when the returnees were 23-25 years old, the conviction ratio in New Orleans was close to (although still lower than) that in the rest of Louisiana.

For the pooled analysis, we focus on first-time 7th-to-9th graders and the convictions for crimes they committed during the academic years they were in those grades, assigning students to the districts in which they spent the majority of their time that year.¹⁰ The focus on these specific grades is helpful for understanding the mechanisms within schools. Prior research suggests that dropouts are disproportionately likely to commit and be convicted of crime (Harlow, 2003; Lochner, 2011; Raphael and Sills, 2008)), and the school reforms reduced the probability of dropout by 3-9 percentage points (Harris & Larsen, forthcoming). This means that, if we were to focus on students in later grades, the estimates would be biased towards finding an increase in crime as marginal students who would have dropped out without the reforms (and likely committed more crimes) stay in the sample. We use three grades to reduce statistical noise but avoid using earlier grades because convictions are so infrequent among young children.

¹⁰ While switching schools is common, switching districts is somewhat rare, especially in Louisiana where districts are small in number and geographically large.

Table 2 summarizes student characteristics in New Orleans and other Louisiana school districts before and after Katrina. During the 2004-2005 school year, the final year before Katrina and the subsequent reforms, New Orleans was disproportionately African-American and low-income (and, to a much lesser extent, English Language Learners) compared to the rest of Louisiana. By the 2011-2012 school year, the end of our sample period, the student demographics in the New Orleans school district were statistically similar to those before Katrina. The same is true for the rest of Louisiana. Furthermore, the last column shows that most of the differences-in-differences in demographics between New Orleans and other school districts pre- and post-Katrina are small in magnitude, and none are statistically significant.¹¹

Figure 2A compares the trend of total student conviction rates (grades 7-9 only) in New Orleans and the rest of the state. (The trend over time is markedly different from that of the panel analysis because the pooled analysis keeps the age range of the sample constant whereas the panel sample grew older over time.) Given the small sample and influence of Hurricane Katrina, which made landfall in late August of 2005, the patterns are predictably erratic for New Orleans. The student-age conviction rate plummeted in the 2005-06 school year when many students were evacuated. However, it returned to roughly the previous level by 2008. In the rest of the state, the conviction rate was more stable compared to New Orleans, gradually increasing until 2009 then declining. Apart from violent crime, for which New Orleans's rate started to exceed other

¹¹ There might be concerns that due to population change, the post-Katrina cohorts in New Orleans were systematically different from their pre-Katrina counterparts in other aspects that were not captured in observed demographic variables but nevertheless affected their tendency to commit crime. To address this, we also consider their baseline discipline records in 2005 when the 2005 cohorts were in grades 7-9 and the 2012 cohorts were in grades K-2 (see the end of Table 2). The difference pre- and post-Katrina can be attributed mainly to their age difference. This is true for both New Orleans and the comparison group and, therefore, accounted for in the difference-in-differences results. Although they are not significant, the positive differences-in-differences in baseline discipline rates (for all incidents and suspensions) suggest that, if anything, the post-Katrina cohort in New Orleans is more prone to crime than the same cohort in other Louisiana districts (comparative to their respective pre-Katrina cohorts).

Louisiana school districts' as early as 2007, the conviction rates by crime type were lower in New Orleans for most years post-Katrina (Figure A3).

Our second dependent variable, the student-to-adult conviction ratio shown in Figure 2B, provides stronger evidence of a potential reduction in youth convictions in New Orleans post-Katrina compared to young adults. Except for the spike in property crime in 2011, the ratios in New Orleans were lower or close to those in other districts for most years post-Katrina (Figure A4).

At first glance, these figures suggest an initial decline in youth convictions in New Orleans followed by a return to the levels of the rest of Louisiana. However, even before Katrina, there were already differences between New Orleans and the unweighted comparison groups. These trends cannot be considered causal effects of Hurricane Katrina. In what follows, we describe the Synthetic Control Group method as a way of providing a valid counterfactual and discuss the identifying assumptions for this approach.

II.B. Synthetic Control Group Method

The Synthetic Control Group method (SCG) is designed to estimate the effects of an intervention that has been given to only one treatment group (Abadie et al., 2010). Although New Orleans was not the only Louisiana school district experiencing Hurricane Katrina, both the extent of damage and the subsequent policy changes, such as the school reforms, were unique to the city. To recover the net effect of Katrina on New Orleans, we need to create a Synthetic Control Group that best approximates: i) the counterfactual conviction rates in New Orleans in the absence of Hurricane Katrina and ii) the pre-treatment evolution of other characteristics that may be related to conviction rates. When we apply the SCG method to our second dependent variable, the youths-to-young-adults conviction ratio, the analysis becomes a kind of triple difference: the third

“difference” being between youths and young adults in addition to the two differences inherent in the SCG methods (between the treatment group and the synthetic control, pre- and post-treatment). For convenience, for the rest of this subsection, we describe SCG as it applies to the conviction rates.

Following Abadie et al. (2010), we create a Synthetic Control Group for New Orleans students, which is the weighted combination of a donor pool consisting of other Louisiana school districts that minimizes the mean squared prediction errors of the outcome variables during 2001-2005. We define X_1 as a vector of observable characteristics in New Orleans before Hurricane Katrina and X_0 is the matrix of these characteristics in other school districts. The SCG method involves, first, choosing a vector V that weights characteristics according to their predictive power on the outcome. Then, SCG chooses a vector of weights, W^* , that minimizes the expression outlined in equation (1), which represents the distance between the pretreatment characteristics of New Orleans and that of other school districts.

$$\|X_1 - X_0W\| = \sqrt{(X_1 - X_0W)'V(X_1 - X_0W)} \quad (1)$$

The synthetic control estimator of the effect of the school reforms is given by $Y_1 - Y_0W^*$, or the weighted difference in post-reform outcomes between New Orleans and its synthetic control.

For the pooled analysis, we use the conviction rates (or student-to-young adult conviction ratios) for students in grades 7-9 of each school year to construct a SCG for New Orleans. For the panel analysis, it is not possible to match because these students were very young pre-Katrina, so their pre-Katrina conviction rates were essentially zero in all districts (see Figure 1). Hence, we construct the synthetic control weights based on pre-treatment *district-level discipline* (e.g., expulsion) rates (see Figure A13), under the assumption that they contain indications of the students’ behaviors that would be informative of their counterfactual conviction rates in the post-

treatment period in the absence of Katrina. The same set of weights obtained through this procedure is then used to calculate the post-Katrina conviction rates of the synthetic control.¹²

With the SCG framework, it is not possible to use traditional statistical inference approaches due to small samples as well as the lack of randomization and probabilistic sampling (Abadie, Diamond and Hainmueller, 2015). Instead, we use placebo tests. Following Abadie, Diamond and Hainmueller (2015) and McClelland and Gault (2017), we assign treatment status to each school district other than New Orleans, conduct SCG analyses using each placebo treatment, and obtain “effect” estimates. We then calculate the Root Mean Squared Prediction Error (RMSPE) for each district. As shown in equation (2), Y_{1t} measures the crime outcome of the district, and $\sum w_j^* Y_{jt}$ measures that of its corresponding synthetic control. The RMSPE for a time period measures the magnitude of the gap in conviction (or discipline) outcomes between each school district and its synthetic counterpart over that period.

$$RMSPE = \sqrt{\frac{1}{T} \sum_{t=1}^T \left(Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt} \right)^2} \quad (2)$$

Then, we calculate the ratio of post-reform $RMSPE_1$ to pre-reform $RMSPE_0$ for each district $R_d = \frac{RMSPE_{1d}}{RMSPE_{0d}}$. We use this ratio to compare the size of the total hurricane effect to that of other placebo districts and calculate a p -value for a two-sided test for whether the effect in New Orleans is significantly different from zero. The p -value is:

$$p = \frac{\sum I[R_d \geq R_{NOLA}]}{N} \quad (3)$$

where $I[R_d \geq R_{NOLA}]$ is an indicator for when the ratio of a placebo district R_d is at least as large as the New Orleans ratio R_{NOLA} and N is the total number of districts in the donor pool. If there is

¹² For a list of school districts that contribute to the synthetic control and corresponding weights, see Appendix Table A4.

an actual effect, this should be infrequent and the p -value should be small. As in traditional inference, a small p -value provides more confidence that the result is not by chance. For a negative (positive) one-sided test, which may possess considerably more power than two-sided inference, $RMSPE_{1d}$ only counts the negative (positive) part of the errors (Abadie, 2021).¹³

II.C. Identifying Assumptions

To answer our two research questions, we require different sets of identifying assumptions. As explained in this subsection, we are confident that we are causally identifying the overall effect of Hurricane Katrina on New Orleans youth convictions using the SCG method. Although we cannot cleanly identify the effect of each individual mechanism through which Katrina operated, we can attempt to decompose the net effect under reasonable assumptions.

For the overall Katrina effect, the counterfactual is the trajectory of New Orleans youth convictions that would have manifested if the city had not experienced Katrina. Ideally, the treatment would be conditionally exogenous; that is, its assignment was not based on unobserved factors that are correlated with youth convictions. The location and timing of the storm itself obviously satisfy this requirement. For other changes triggered by Katrina that were unique to New Orleans, such as the school reforms, the SCG method can account for unobserved differences between control and treatment districts that are time constant. The only remaining concern is possible time-varying unobserved factors affecting youth convictions that were unrelated to Katrina, of which there is little evidence.

We therefore turn to the more challenging empirical analysis of the four main mechanisms through which Katrina might have affected youth convictions: changes in the population, the crime

¹³ For each type of analysis, we impose the minimal restriction that allows us to run the placebo analyses discussed for all districts included in the donor pool. Specifically, for the panel sample, we exclude districts with more than three zero expulsion years pre-reform. For the pooled sample, we exclude districts with zero adult crime in any year or with more than one zero youth “crime” rate in pre-treatment period.

environment, the criminal justice system, and the New Orleans school reforms. Since we are primarily interested in the school reform effect, the main threats to identification are the first three mechanisms (population change, etc.). Below, we will elaborate on each of the non-school mechanisms and argue that our second dependent variable (the conviction ratio) can substantially account for them to isolate the effect of the New Orleans school reforms.

First, the population of the city changed after Hurricane Katrina (The Data Center, 2014; Vigdor, 2008). Eighty percent of the city was flooded, with effects being somewhat worse in low-income areas; also, public housing projects were closed down, bulldozed, and eventually rebuilt. For this and other reasons, low-income residents may not have returned. If the population did change, especially in unobservable ways, the concern would be that different populations have different underlying probabilities of criminal behavior.

The change in population has two potential effects on our analyses: composition effect and peer effect. First, in the pooled analysis, which compares different student cohorts, the composition effect means that there might be fewer low-income students in our analysis sample post-Katrina (compared to pre-Katrina). Since household income is a strong predictor for youth crime (Bjerk, 2007), this by itself could have improved crime rates (and subsequently conviction rates) in New Orleans. Second, even when the sample is restricted to the same set of individuals over time as in the panel analysis, the change in the broader population means that these individuals might have been exposed to and influenced by different peers.¹⁴ They may thus exhibit different behaviors due to peer effects. Note that the composition effect is more direct and only affects the pooled analysis, so any differences between the panel and pooled results provide a partial test of the population

¹⁴ Imberman, Kugler, and Sacerdote (2012) looked at the peer effects that Katrina evacuees exerted on incumbent students of the schools which received them. However, we are not aware of any research directly addressing the peer effects that evacuees faced upon returning post-Katrina to a different student composition in their home districts.

change effect.

The remaining two threats to identification—the crime environment and the criminal justice system—affect both the pooled and panel analyses in similar ways. Even for the same students pre- and post-reform, as in the panel of returnees, changes in the environments outside of schools following Katrina might have affected the underlying probability of committing crimes. For example, Katrina might have disrupted gang affiliations or altered the demand for illicit drugs.

Finally, conditional on committing crime, the probability of being caught and convicted could also have changed due to changes in the criminal justice system. In the qualitative analysis in the next section, we therefore consider changes in policing and prosecution. As we only observe convictions instead of actual crimes, these changes may also prevent us from drawing implications about the underlying criminal behavior.

As noted earlier, using the ratio of student to young adult crime, i.e., the triple-difference SCG design, partially accounts for all three non-school mechanisms above. The identifying assumption is that any changes in the population, crime environment, and criminal justice system affected each age group proportionally (within their respective locations). Below, we test this assumption quantitatively with respect to population changes using Census data and find any violation likely would have attenuated the reform effects on crime.

As previously described, Table 2 shows that, even though the size of the New Orleans student population shrank, the 2012 cohort in the pooled sample was statistically similar to the pre-storm (2005) cohort on a variety of demographic dimensions. The difference-in-differences between New Orleans and the rest of the state also shows statistically insignificant effects of Katrina on population demographics (Table 2, last column).¹⁵ We obtain similar results when

¹⁵ Harris and Larsen (forthcoming) also performed a similar analysis comparing the 2005 and 2014 cohorts and found no significant differences.

using the weights obtained from the pooled synthetic control analyses for crime rates and crime ratios (see Appendix Tables A2 and A3, respectively). This gives us some confidence that the significant crime reduction in 2012 is unlikely to be due to demographic shifts. If anything, the direction of the results on baseline disciplines suggests that if population change had played a major role, we should have seen an increase in New Orleans conviction rates in 2012.

As further evidence of the identification assumptions for the triple-difference SCG, we use data commissioned from the US Census Bureau for the 2000 Census and 2010-2012 American Community Survey to conduct a triple difference analysis of population change. We compare household incomes and other characteristics of the head of household between (1) relevant-aged adult-headed households (i.e., adult sample) v. households with students in public schools (i.e., student sample);¹⁶ (2) before v. after Katrina; and (3) New Orleans v. all other Louisiana school districts. Table 8 shows the results when we consider students in all K-12 grades, but similar patterns can be found when we restrict to grades 5-8 and 9-12, respectively.¹⁷ In terms of household income, both New Orleans and all other districts started out with the adult sample earning more on average pre-Katrina. These differences decreased (or even became negative) post-Katrina, but the decrease was larger in magnitude for other districts, resulting in a positive third difference. In other words, households in the student sample became wealthier relative to those in the adult sample for both New Orleans and the control districts, but the extent of this was larger for the latter. Conventional wisdom suggests this change would have caused the comparison group to experience a greater reduction in student-to-adult conviction ratio than New Orleans, the opposite of our results. In terms of race, the third differences are not significant.

¹⁶ Due to data restriction, household incomes are calculated for household heads between 25 and 44 years old. For all other characteristics, we consider 25-34 year-olds as in the conviction ratio.

¹⁷ The breakdown of commissioned Census data does not allow us to exactly match the grades 7-9 in our pooled sample.

In the following section, we discuss how qualitative methods can be used to examine identifying assumptions in general and then apply this method and test for threats to identification due to changes in the crime environment and criminal justice system.¹⁸

III. Qualitative Analysis of the Identifying Assumptions

III.A. General Method

Quantitative research naturally relies on quantitative tests of identifying assumptions, but we argue that this is unnecessarily limiting. In some cases, identifying assumptions cannot be tested in a convincing way using quantitative methods. In these cases, qualitative evidence can be a useful supplement. Our approach is perhaps closest to the "convergent design" according to Creswell and Plano Clark (2017)'s typology but still different from what they intend and from typical uses of the convergent design in the literature.¹⁹ In our case, we use qualitative evidence to determine the validity of identification assumptions as qualitative tests. Below, we describe the steps involved in doing so, discuss cases where qualitative methods may be most useful, and then apply these methods in the present SCG analysis in New Orleans.

¹⁸ An additional potential threat is the disruption and trauma of the hurricane itself (Harris & Larsen, forthcoming). To address this, we carry out robustness checks that restrict the donor pool to districts that were also affected by Hurricane Katrina. More generally, if Hurricane Katrina had a common effect across other hurricane districts, other than the school reforms, then restricting to this control group should attenuate the estimated effects. However, due to the donor pool being too small (consisting of 3 or 7 districts, depending on whether we require them to be comparable in size to New Orleans), we could not obtain a synthetic control that resembled pre-Katrina New Orleans closely enough for any meaningful estimates.

¹⁹ Creswell & Plano Clark (2017) define the convergent design as "a mixed methods design in which the researcher collects and analyses two separate databases--quantitative and qualitative--and then merges the two databases for the purpose of comparing or combining the results." As such, their definition is broader than the literature's typical use of the term (triangulation using both types of data) but still does not quite encompass the approach we lay out in this section.

Step 1: Determine the quantitative causal identification strategy and associated assumptions. Identification strategies often rely on discontinuities, and this is where we focus our discussion. With regression discontinuity (RD) designs, the main identifying assumption is that there are no unobserved differences in participants on either side of the threshold, e.g., because of manipulation of the forcing variable (Imbens & Lemieux, 2008). In difference-in-differences (DD) designs, and the close cousin of SCG, the main identifying assumption is that the treatment group would have followed the same trend as the comparison group in the absence of treatment. That analysis naturally starts with a formal pre-trends test, but even passing this test does not guarantee the counterfactual parallel trends. The fact that treatment occurred in one place and time may not be random. A related threat to identification in the DD case is the anticipatory effect. Even before policies are adopted, affected groups sometimes respond, either because of a misunderstanding about when a policy takes effect or because of a desire to move ahead with changes that they know or expect they will have to make in the future. These assumptions can be investigated with qualitative data.

Step 2: Review of public records. Given the assumptions from Step 1, the researchers can determine the potential threats to identification and search for them in the media, published reports from various organizations, and the related academic literature. For certain applications, the threat(s) to search for may be specific. However, to avoid missing factors that may not occur to the researchers *a priori*, we recommend being as general as possible. For instance, instead of only searching for specific policy changes, one should search for *any* changes in the relevant policy sphere then determine whether each of those changes affect identification.

Step 3: Identify and recruit key actors. At least four broad groups seem relevant to examining the identifying assumptions in evaluation research: policymakers, program

administrators, advocacy groups/think tanks, and program participants. It can be useful to talk with the policymakers who put the program/policy in place to better understand factors that led to its adoption and timing. Interviewing program administrators can be useful as they are perhaps best positioned to provide information about when policies were first implemented and whether implementation was relatively steady over time, which is often an implicit assumption in quasi-experimental studies. Leaders of advocacy groups and think tanks can also be useful as they are often well informed about both the policy/program and its adoption.

With both policymakers and administrators, as well as advocacy and think tank experts, it seems advisable to interview people who see and experience the program from different vantage points (e.g., someone involved in day-to-day operations versus someone involved in more distant management and oversight) and have different views on program value and efficacy. The elected official who sponsored the bill, for example, might have a different stated interpretation of why it was adopted than others, in ways that could affect the researcher's understanding of the identifying assumptions. To this end, interviewing officials with varying perspectives and interests in the policy seems advisable.

We have emphasized policymakers and program administrators to this point, for three reasons. First, the names and contact information for the former groups are generally publicly available. In situations where the policies were adopted in the distant past, current officials can identify past ones. In contrast, it is often more difficult to identify participants or their contact information. Second, many public policies are aimed at vulnerable populations (e.g., children, people with disabilities, and prisoners) who are protected by human subjects rules. For this reason, contacting officials will often be the only practical option. Finally, any individual participant is not likely to have much knowledge of the key threats to identification (e.g., the

reasons why a law was passed, the timing of policy adoption and implementation, and issues of data reporting).

This last point highlights why the type of qualitative analysis commonly conducted in program evaluations is not what is necessary to test identifying assumptions. We are often interested in qualitative analysis for understanding implementation and, in that case, interviewing program participants is generally much more important than interviewing policymakers. The reverse appears to be true for using qualitative analysis to examine identifying assumptions.

There is a natural tendency to focus on the treatment group, but understanding the counterfactual, or business-as-usual, is also important. With the comparison group, for example, other interventions might have been instituted to address the same problem or goal as the treatment. One way to capture both control and treatment perspectives is to interview people who have knowledge of both (e.g., for a policy implemented in a particular school district that is being compared with other districts within the state, interviews could be conducted with state officials).

Step 4: Develop interview protocols. The above steps establish who is being interviewed and what the general topics need to be, i.e., the identifying assumptions. For instance, in the RD case, we might ask program administrators about the forcing variable. In the DD case, we might ask the sponsor of a bill why they were pushing for this policy change and how long the political process took (which affects the timing of adoption).

It is often useful in interviews to start with general questions to establish the interviewee's specific areas of relevant knowledge and roles pertaining to the policy/program of interest. This is necessary to be clear about the types of knowledge they are most likely to bring

to bear. Subsequent questions should address *specific* potential threats to identification and be semi-structured to leave room to probe about potential threats to identification that the researcher might not have thought of beforehand. In the RD case where a test score is a forcing variable, for example, we might ask about test retakes, whether any students might have received extra assistance, and how aware key actors were of the threshold. In DD, we can ask elected officials how long the policy was debated before it was adopted, how the idea evolved, and why other places did not adopt the policy.

Step 5: Document the analysis method and results. As in quantitative methods, it is important not only to carry out the above steps, but carefully document them. As these studies are primarily quantitative in nature, the qualitative analysis might be briefly summarized in the main text with details left to the appendix.

These guidelines are designed to provide particular guidance on this somewhat new use of quasi-experimental methods. As in quantitative methods, there are other considerations involved in carrying out valid and reliable qualitative analyses, and so quantitative researchers may wish to consult standard methodological texts. With that in mind, we document below the process we used in our analysis of New Orleans.

III.B. Qualitative Analysis of Identification in New Orleans

We are studying the effects of Hurricane Katrina on the crime rate of school-age children within a DD-SCG framework. We are also especially interested in isolating the role played by the school reforms, as distinct from other mechanisms (i.e., changes in population, the crime environment, and the criminal justice system). Population change is best addressed through quantitative tests that we provide in Section II.C.. The latter two are partially addressed by using

the youth-to-young-adults ratio in the SCG analysis (see later), but the SCG also requires assumptions that we can examine qualitatively.

Step 1: Identification strategy and assumptions in Katrina analysis. We apply a triple-difference SCG analysis to isolate the effects of the school reforms. Given this design and the potential for changes in the crime environment and criminal justice system, we asked respondents to talk about policies and other factors that: (a) changed over time; (b) could have influenced juveniles and adults differently; and (c) could have affected New Orleans differently from other parishes/school districts. All three of these conditions would have to hold in order for a factor to pose a threat to identification with respect to the school reforms.²⁰ We focus particularly on changes and discrepancies between the outcome of primary interest, actual criminal *incidents*, and our data on criminal *convictions*. Much of our qualitative analysis therefore involves understanding changes in policing and prosecution—two areas of policy that link incidents to convictions.

Step 2: Review of public records. We searched the two main local newspaper/media sites (nola.com and thelensnola.org) for the news published between 9/1/2005 and 7/31/2018. Our search included both general and specific terms related to crime and the justice systems (for both youths and adults).²¹

Step 3: Key actors in New Orleans. After the public document search, we held a focus group that included 13 criminal justice and education experts. In the focus group, we described

²⁰ We also provide information on whether a factor affected New Orleans youth conviction rate (our main interest) in the last column of Table 3. However, whether this affects our identification assumptions is already summarized in the three aforementioned conditions. For instance, if a factor affects *only* New Orleans youths, then condition (b) is met, that is, this factor influenced juveniles and adults differently.

²¹ Refer to Appendix B for more details.

this project and asked general questions about policy changes and changes in the social, economic, and political environment that could have affected crime, particularly youth crime, during the relevant time period. We also asked focus group participants to recommend other officials and community leaders for follow-up interviews. From this information, we determined that we needed interviews with officials with expertise in both policing and prosecution. Specifically, we interviewed one high-level leader in the New Orleans District Attorney's (DA) office, another high-level leader from the police department, and a third interviewee from a non-profit organization focused on juvenile justice.²²

Step 4: Interview protocols for key actors. We created semi-structured interview protocols (available upon request) for the three key interviews. They varied due to the interviewees' different areas of expertise. The protocols were based on what we knew from the initial focus group and analysis of public documents. After each interview, we adapted the protocols for the subsequent interview(s) based on what we learned.

Step 5: Results. Table 3 summarizes the qualitative results. The table lists each of the policies that were raised in our literature review, document analysis, focus group, and interviews, along with our assessment of whether that policy poses a threat to identification. We organize these into three broad categories. The policing-related topics included: reduction in the number of police officers, a federal consent decree imposed on the New Orleans Police Department (NOPD), changes in truancy and curfew enforcement, and the use of school resource officers (SROs). The prosecution-related topics included: diversion programs, trying youths as adults,

²² We did not seek out interviews with elected officials regarding why the New Orleans school reforms were put in place, which was largely irrelevant. The school reforms were largely driven by the location and timing of Katrina, which was exogenous.

and shifting lesser crimes to municipal courts. Finally, the only detention-related topic of note is the implementation of pretrial risk assessments.

The right-hand columns summarize what we learned and how each policy pertains to the identifying assumptions. Recall that all three conditions have to hold for there to be a threat to the identifying assumptions in a triple-difference SCG design. There are only two policy changes we could find that might meet these conditions, an increased tendency to try youths as adults after District Attorney Leon Cannizzaro took office in 2008 and the implementation of a pretrial risk assessment program in New Orleans and four other Louisiana parishes starting in 2006. For the former, according to our interviewee with expertise in prosecution, juvenile courts in New Orleans tend to be more lenient than adult courts, so we would expect this practice to increase the number of youth convictions. This would bias our effect estimates toward finding that the school reforms increased crime. For the latter, pretrial risk assessments allowed some arrested youths to remain in the community instead of being detained while awaiting trial, which likely contributed to reducing youth conviction rates. We return to these possible violations later in the results. See Appendix B for additional details.

IV. Results: Overall Effects of Hurricane Katrina

IV.A. Panel of Returnees

As previously discussed, we construct the synthetic control for our panel analysis by matching the pre-Katrina discipline rates of New Orleans returnees (see Figure A13). We use expulsion rate because it is less dependent on variations in local discipline policies and thus more likely to be indicative of behaviors that predict the future propensity to commit crimes. We find a set of weights to construct a synthetic control whose expulsion rate closely matched that of New

Orleans from 2001 to 2005 (pre-reform period). These weights will be used throughout the analyses we show in this subsection.

Figure 3A and Table 4 (column 1) present SCG results for total conviction rates for returning students only (ignoring young adults). Conviction rates for returnees in the synthetic control rose steadily between 2005 and 2011 as they aged, whereas there was a much more gradual increase for New Orleans. After reaching its peak in 2011, the conviction rate began to decline for the synthetic control but continued to rise for New Orleans. Hence, by the final year, 2018, when the returnees were 23-25 years old, although New Orleans still had a lower conviction rate, the difference was smaller. The only year in which New Orleans saw a similar conviction rate to the SCG was 2015, which might be a result of the DA's reelection campaign. A similar pattern emerges in each of the categories except violent crime, for which New Orleans's conviction rate was similar and at times higher than the SCG's between 2008-2012 before becoming lower in the last few years of the panel (Figure A5 and Table 4 (columns 2-5)).

To assess the level of confidence in the net Katrina effects on youth conviction rates, we also carry out a series of placebo studies where each district in the donor pool (other Louisiana school districts) takes turns to become a "placebo" treatment group. Figure 3B shows that the difference between the total conviction rate of New Orleans (dark line) and its synthetic control is more negative than the majority of the corresponding values for other school districts in Louisiana (gray lines) for most years post-Katrina. Tables 5A and 5B report the empirical p-values associated with these placebo studies for a two-sided test and a negative one-sided test, respectively. For the total conviction rate, the effects of Katrina were significant for the first year post-Katrina (2006), when major disruptions still persisted, as well as a later period (2011-2013), when the city had more fully recovered. With the gain in power of the one-sided test, we additionally find negative

significant effects for 2010, 2014, 2016, and 2017. Given the test of significance by crime type, the short- and long-run overall effects seemed to be driven by the “other” crime category,²³ while the medium-run effects were driven at least partly by property crime.

Here, and throughout the remaining analysis, we focus our attention on the medium- and long-term effects (after 2010 and up to 2014-2018) for several reasons. First, there were certainly temporary changes in the crime environment and criminal justice system for several years after Katrina, most of which gradually faded away and are not of general interest. Second, it took some time for the school reforms to take hold. Third, criminal convictions are more common when students are older, so these estimates are likely to have greater reliability. With the panel analysis, we see effects many years after Katrina, which is also true in most of the pooled analyses that follow.

IV.B. Pooled Cross-Sections

For the pooled cross-sections, we estimate the SCG for all crimes and for each of the four crime types. The model is run separately for each crime type, implying that the all-crime result is not necessarily the weighted sum of the sub-category results.

Figure 4A and Table 6 (column 1) present SCG results for total convictions only for students showing up in the data while they were in grades 7-9 (ignoring young adults). After an initial drop in New Orleans, the overall trend shows a convergence between the outcomes of New Orleans and the synthetic control: there was a temporary reversal in 2011, but in the last year, 2012, crime in New Orleans was once again lower than the synthetic control. For violent crimes,

²³ As noted in the data section, all FINS petitions are under this category. Some additional examples of “other” crimes are operating a vehicle while intoxicated, possession of firearm or carrying concealed weapon by a person convicted of certain felonies, and introducing/possessing/sending contraband into or upon the grounds of any state correctional institution.

the reversal in 2011 persisted into the last year so that Katrina seems to have increased crime in the last two years of the data (Figure A7 and Table 6 (columns 2-5)).

To gauge confidence in these patterns, we carry out placebo studies in a similar fashion to the panel analysis. Figure 4B shows the results for all convictions, which suggest that the break from the pre-reform trajectory in New Orleans was larger than the vast majority of districts in the initial years, but we have less confidence of its persistence into the later years. The empirical p-values for the two-sided tests reported in Table 7A imply statistical significance (at a 10% level) for only the first two years post-Katrina (2006 and 2007) for total convictions, likely driven by property and other crimes. The effects on property crimes were also significant for 2008-2009 as well as overall for the analysis period. With the gain in power of one-sided inference (Table 7B), we additionally find significant negative effects in 2012 on overall crime, also concentrated in property and other crimes.

Note that we find significant effects immediately after Katrina both for the panel analysis (fixed population) and pooled analysis (allowing for population change), and it is reasonable to argue that at least part of the overall effect in the very short run was driven by the major disruption affecting both crime environment and the criminal justice system.

V. School Reform as a Potential Mechanism

We showed in the previous section that Hurricane Katrina reduced crime in New Orleans immediately and many years after the storm and that this is unlikely due to changes in the student population. In this section, we further address non-school mechanisms, including changes in the crime environment and criminal justice system, by conducting SCG analyses using the conviction ratio (students-to-young-adults rates) instead of the raw conviction rate as the outcome variable. Below, we discuss the results of the SCG method using the ratio of conviction rates for the panel

and pooled analysis, respectively. The pattern of findings gives us some confidence that the school reforms were a key driver of the overall Katrina crime reductions. Then, we briefly discuss the mechanisms within schools.

V.A. Panel of Returnees

For the panel analysis, we observe similar patterns of results for the ratio, compared to the raw conviction rates, as shown in Figure 5A and Table 8 (column 1), confirming that the Katrina effects we found previously do not go away when non-school factors are controlled for. The placebo studies (Figure 5B) show the same result of significant reductions in the very short run as well as the medium and long runs for total convictions. We also observe significant negative effects for more years for both property crimes and other crimes (Tables 9A and 9B). Furthermore, both total crime and the other-crime category show significant reductions in the overall test for the entire post-Katrina period here, whereas they do not for the conviction rate. We do not have as much power for violent crimes and, to a lesser extent, drug crimes as we do for other categories.²⁴ However, we still find significant reductions in violent crimes for three out of the last four years of the sample period.

To sum up, we find stronger evidence of negative effects when non-school factors have been partially accounted for by using the youth-to-young adult ratio of convictions, which strengthens our confidence that the New Orleans school reforms did indeed contribute to the reductions in youth conviction post-Katrina. The significant reduction of violent crimes in 2015, 2016, and 2018 also alleviates some of our remaining concerns regarding the differential change in the criminal justice system for juveniles and adults for two reasons. First, by 2015, the youngest

²⁴ As these crimes happen less frequently, fewer districts have enough non-zero values for us to use as placebos.

individuals in our returnee sample were 20 years old, meaning that all returnees were fully in the adult system, precluding the change we are most concerned about from our qualitative test: the pretrial risk assessment program. Second, for this more serious crime category, reporting and convictions tend to be handled more consistently than non-violent crimes,²⁵ so our findings are likely to be more reflective of underlying criminal behaviors.

V.B. Pooled Cross-Sections

For the pooled cross-sections, we similarly conduct SCG analysis for the ratio of student-to-young adult conviction rates (Figure 6A and Table 10). The placebo studies and corresponding p-values are shown in Figure 6B and Tables 11A and 11B. As with the panel analysis, where non-school factors are partly controlled for, there are more years with significant (negative) effects compared to the raw conviction rates, which further strengthens the argument that the New Orleans school reforms did contribute to the reduction in student conviction rates.

V.C. The “School-to-Prison” Pipeline and Other School Factors

In the previous parts of this section, we show that the school reforms were a key driver of New Orleans’ post-Katrina reduction in youth crime. To better understand how school reform affects crime, we take the analysis a step further and try to understand how one element of school reform—school discipline—might contribute to what some have called the school-to-prison pipeline. Then, we discuss other school-related factors that might have reduced crime in light of our findings and the previous literature.

²⁵ Victims tend to report severe offenses at higher rates than non-severe offenses (Bachman, 1998; Birbeck et al., 1993; Goudriaan et al., 2004; Hart and Rennison, 2003; Kilpatrick, et al., 1987; Lizotte 1985; Skogan 1976, 1984). In terms of prosecutions, non-violent cases may be eligible for the diversion program while violent cases are not.

In theory, the school reforms may increase crime by expanding the “no excuses” approach to schooling that favors high expectations and strict behavioral standards (Fryer, 2014). While this seems to generate higher achievement (Angrist, Pathak, and Walters, 2013), it may also lead to more suspensions and expulsions, which increase unsupervised time and may reduce educational prospects. Strict discipline might also increase other negative behaviors, including smoking, drinking, and fighting (Zimmerman & Rees, 2014), which are associated with future criminal incidents. Charter schools, including those in New Orleans, also typically hire young teachers who have less experience and training and are of different racial groups from their students, which may affect their discipline practices (Lindsay & Hart, 2017) and make it difficult for teachers to connect with students and provide culturally relevant instruction (Ladson-Billings, 1995; Redding, 2019). The most direct evidence of this school-to-prison pipeline in the previous literature comes from Bacher-Hicks et al. (2020) who find substantial increases in adult arrests and incarcerations for students who went to schools with higher suspension rates. In what follows, we explore the effects of discipline on youth crime within the context of New Orleans and find a similar relationship.

We measure the propensity of each school to take disciplinary actions and leverage school-level (within-city) variance in discipline, assuming household school choices are conditionally independent of the probability of incarceration. Conceptually, the treatment group becomes New Orleans students in schools with high usage of discipline and the comparison group becomes New Orleans students with low usage of these policies, although we include the discipline propensity as a continuous measure.

We use student-level data from our pooled analysis sample (grades 7-9) for the years 2006-2012 to construct school-level discipline propensity. The discipline propensity of school s over this period is the school fixed effect θ_s obtained from the following regression:

$$Y_{ist} = \alpha + \beta X_{ist} + \gamma_{ist} + \theta_s + \epsilon_{ist} \quad (2)$$

where Y_{ist} is the discipline outcome for student i in school s in year t ; X_{ist} is a vector of students characteristics, including gender, race, eligibility for free or reduced-priced lunch, English-language-learner and special-education statuses (and test scores in grade 6 mathematics and English Language Arts); and γ_{ist} includes other controls (year and grade fixed effects).

We create six versions of discipline propensity, using three different discipline outcomes (numbers of all discipline incidents, suspensions, and expulsions) and two different specifications (with and without controlling for grade-6 test scores). We then regress the number of contemporaneous convictions of 7th-to-9th graders in our pooled sample on the (standardized) discipline propensity of the schools they attended, controlling for student demographics, prior achievements, and year and grade fixed effects. Table 9 shows the results of this regression. Across all six measures, we see that schools with higher discipline propensities significantly increased the number of contemporaneous convictions for their students. The magnitude of the effect is much larger for expulsion propensity than for suspension. Within each type of disciplinary incident, we also observe slightly stronger effects when the propensity measures have taken the students' prior achievement into account (columns 2, 4, and 6).

Although there was a positive relationship between schools' discipline rates and their students' contemporaneous convictions, it was unlikely that charter schools' discipline practices increased youth crime. Hernández (2019) finds only a temporary increase in expulsion rates in New Orleans public schools immediately after Katrina until 2009, but it was driven by schools directly run by the Recovery School District (RSD) during the transition instead of charter schools. Expulsion rates decreased sharply in 2010 under public and legal pressures and returned to pre-

Katrina levels by 2012 before a new centralized expulsion system was implemented (Hernández, 2019).

Yet, we still find significant crime reductions for our pooled sample over the period of increased disciplines.²⁶ This suggests that non-disciplinary aspects of the reforms drove the crime reduction. The most salient effect of the school reforms was the substantial improvement in academic outcomes (Barnes and Harris, forthcoming). Given both the theoretical support (Becker, 1968; Lochner, 2004) and empirical evidence of the crime-reduction effect of educational attainment (Lochner and Moretti, 2004), this is likely one of the channels through which the reforms reduced crime. Charter schools also allow students less unsupervised time (e.g., because of longer school days and school years, longer bus rides to school, and reduced dropout rates). On the intensive margin, Jacob and Lefgren (2003) and Luallen (2006) found that spending an extra day in schools reduced juvenile property crime (incapacitation effect) and increased juvenile violent crime (social interaction effect). On the extensive margin, an increase in compulsory schooling ages led to a reduction in the number of arrests both for property and violent crimes in the US context (Anderson, 2004) and in property crime convictions in England and Wales (Machin, Marie, and Vujić, 2011).

VI. Conclusions

We provide some of the first evidence on the effects of Hurricane Katrina and the subsequent changes it triggered in New Orleans on the rate of criminal convictions of students who attended the city's public schools. From the simple SCG analysis, we conclude that Katrina as a whole significantly reduced youth conviction rates in the medium- to long-term. From the triple-

²⁶ As argued previously, within this period, we do not put much weight on the results for 2006 and 2007 because it was early in both the post-Katrina recovery and reform implementation.

difference SCG, we conclude that this is probably attributable to the school reforms. The qualitative analysis reinforces the role of the school reforms and provides more confidence that the effects on convictions likely reflect actual reductions in crime, not just convictions.

Research generally suggests that education reduces crime and a more limited literature suggests that market-based school reform reduces crime as well. However, these studies have been hampered by reliance on self-reported crime and/or matching methods. We avoid both problems using conviction data and developing novel methods to improve causal identification, i.e., the triple-difference SCG and qualitative examination of identifying assumptions. Our results reinforce the consistent finding that school choice policies reduce crime.

We also shed light on the mechanisms within schools that might drive these crime reductions. In particular, we address concerns about the “school-to-prison” pipeline. Although we find a positive relationship between stricter discipline and students’ convictions in our setting, consistent with the previous literature, the temporary increase in discipline post-Katrina was offset by positive factors of the reforms. Furthermore, stricter discipline was driven by RSD’s direct-run school, not charter schools (Hernández, 2019). This means that discipline was not a positive, direct contributor to the crime reduction. Improved academic outcomes, increased college-going chances, and reduced unsupervised time are therefore the likely mechanisms.

Given the broad ways in which education improves private and social welfare, further examination of student outcomes, beyond student academic outcomes, is important. Crime and discipline are good examples. They are difficult to study, but we believe our analysis has provided new paths forward to do so.

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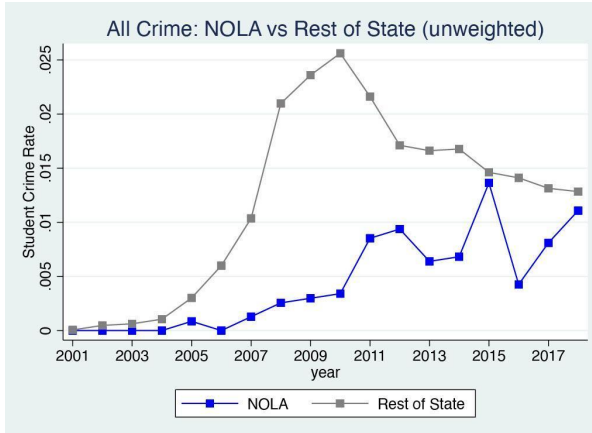
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Figure 1 – Panel Analysis: Descriptive Trends in All Crime

1A. Returnee Only Conviction Rates



1B. *Ratio* of Conviction Rates

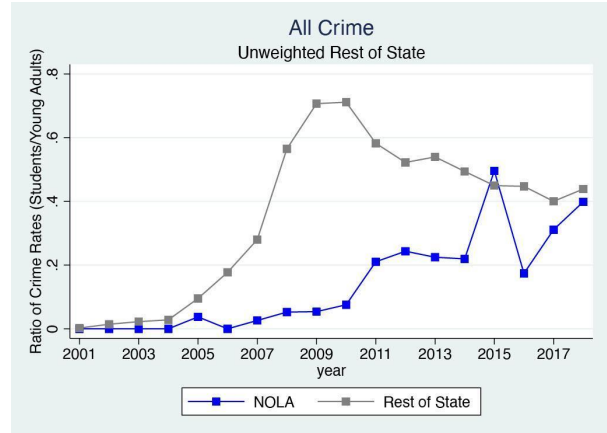
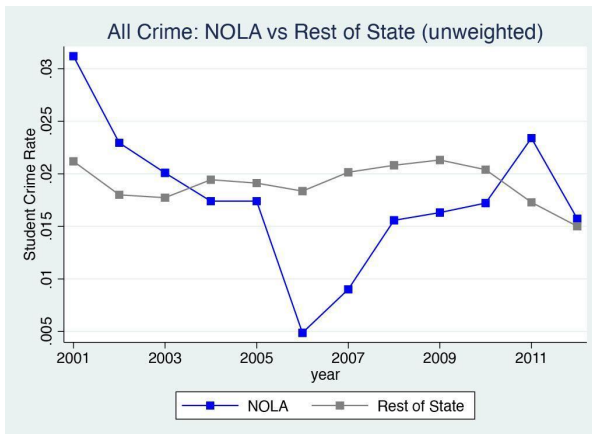
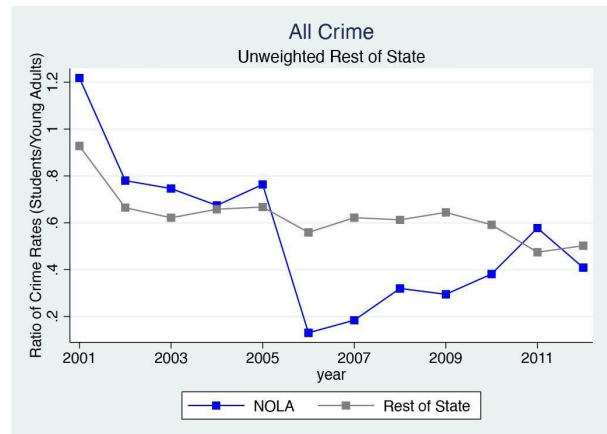


Figure 2 – Pooled Descriptive Trends in All Crime

2A. Students Only Conviction Rates

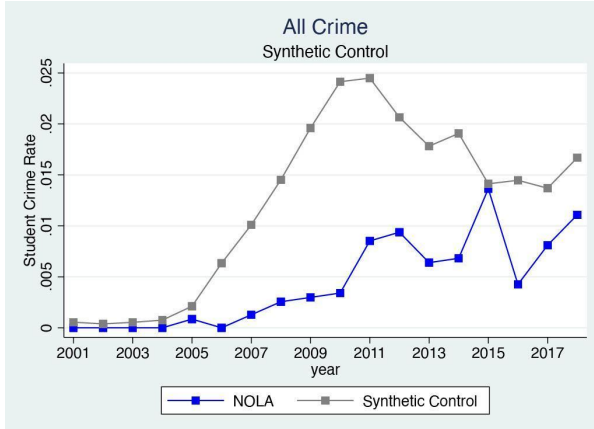


2B. *Ratio* of Conviction Rates

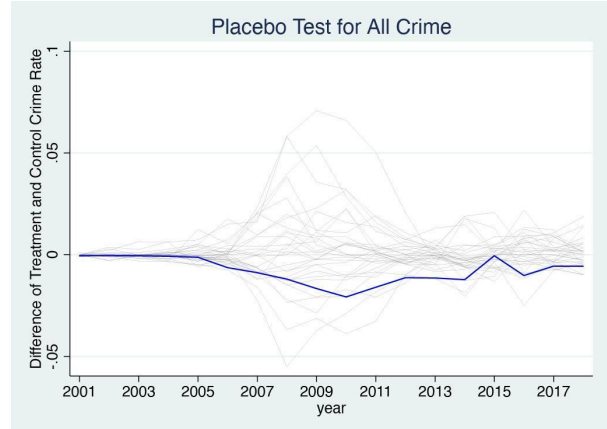


**Figure 3 - Panel Analysis: Synthetic Control for Returnee Only Conviction Rates
(All Crime)**

3A. Effects

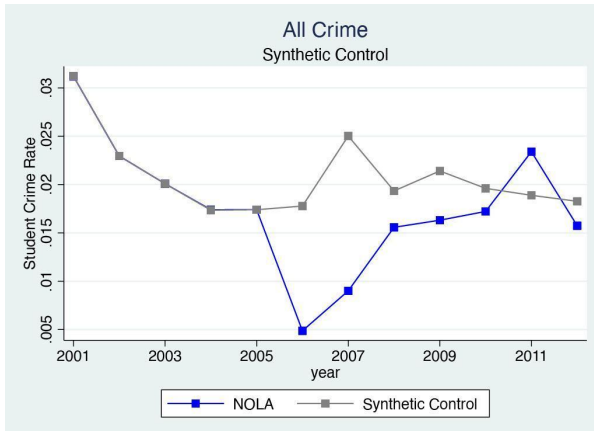


3B. Placebo Studies



**Figure 4 - Pooled Analysis: Synthetic Control for Student Only Conviction Rates
(All Crime)**

4A. Effects



4B. Placebo Studies

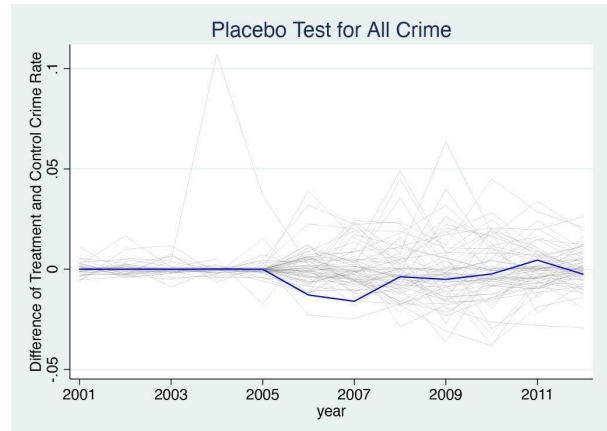
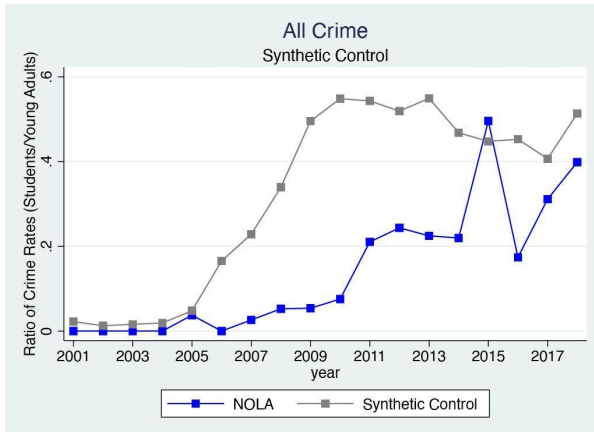


Figure 5 – Panel Analysis: Synthetic Control for *Ratio* of Conviction Rates (All Crime)

5A. Effects



5B. Placebo Studies

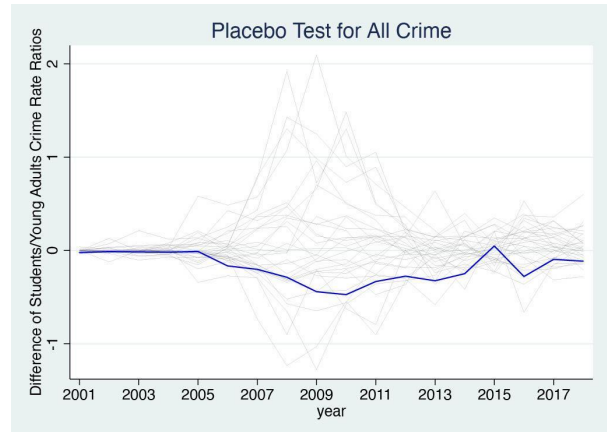
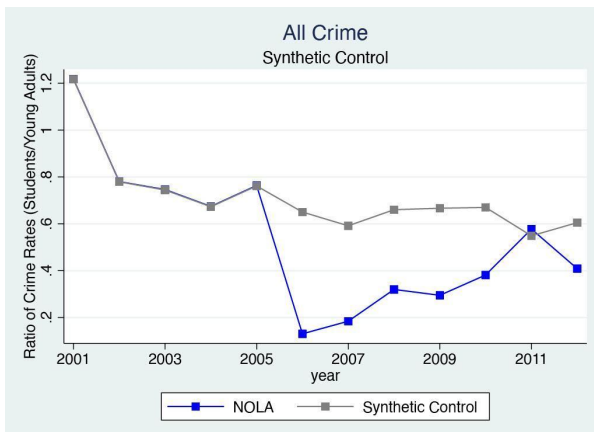


Figure 6 – Pooled Analysis: Synthetic Control for *Ratio* of Conviction Rates (All Crime)

6A. Effects



6B. Placebo Studies

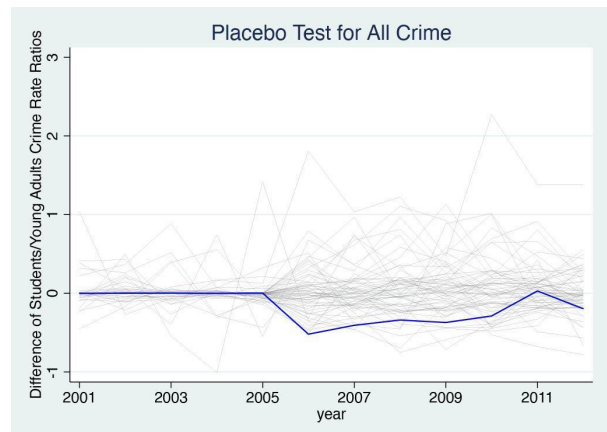


Table 1
Descriptive Statistics for Panel of Returnees Relative to All Students at Baseline (2005),
New Orleans and Comparison Groups

	New Orleans			All Other Louisiana Districts			DID: New Orleans Minus Comparison
	Returnees	All	Diff.	Returnees	All	Diff.	
<i>Demographics</i>							
Male	0.416 (0.493)	0.500 (0.500)	0.084 (0.702)	0.504 (0.500)	0.511 (0.500)	0.007 (0.707)	0.076 (0.996)
Black	0.873 (0.334)	0.934 (0.248)	0.061 (0.416)	0.435 (0.496)	0.429 (0.495)	-0.006 (0.701)	0.067 (0.815)
White	0.069 (0.253)	0.036 (0.187)	-0.032 (0.315)	0.530 (0.499)	0.533 (0.499)	0.003 (0.706)	-0.036 (0.773)
Hispanic	0.012 (0.107)	0.012 (0.111)	0.001 (0.154)	0.016 (0.125)	0.018 (0.133)	0.002 (0.183)	-0.001 (0.239)
Other race	0.047 (0.212)	0.017 (0.130)	-0.030 (0.249)	0.019 (0.138)	0.020 (0.140)	0.001 (0.196)	-0.031 (0.317)
FRPL	0.728 (0.445)	0.805 (0.396)	0.077 (0.596)	0.633 (0.482)	0.631 (0.483)	-0.002 (0.682)	0.079 (0.905)
Special ed	0.047 (0.212)	0.102 (0.303)	0.055 (0.370)	0.130 (0.336)	0.133 (0.340)	0.003 (0.478)	0.052 (0.605)
ELL	0.047 (0.211)	0.020 (0.141)	-0.027 (0.254)	0.008 (0.087)	0.008 (0.088)	0.000 (0.124)	-0.027 (0.283)
<i>Discipline</i>							
All	0.157 (0.532)	0.387 (0.957)	0.230 (1.095)	0.385 (1.207)	0.445 (1.326)	0.060 (1.793)	0.170 (2.101)
Suspension	0.157 (0.532)	0.385 (0.953)	0.228 (1.092)	0.380 (1.186)	0.437 (1.297)	0.057 (1.757)	0.171 (2.069)
Expulsion	0.000 (0.000)	0.002 (0.047)	0.002 (0.047)	0.005 (0.079)	0.008 (0.100)	0.003 (0.127)	-0.001 (0.135)
N (Students)	2346	13892		106119	142157		

Notes: This table reports the demographics and discipline records for students in grades 4-6 in the school year 2005 for New Orleans and all other Louisiana school districts, excluding special school districts outside of Orleans. The first difference shows the means for all students minus those of the returnees in the panel sample. The last column shows the differences between the change in New Orleans compared to that in other school districts.

Table 2
Descriptive Statistics for New Orleans Relative to Comparison Groups,
Pooled Sample - Before and After Katrina

	New Orleans			All Other Louisiana Districts			DID: New Orleans Minus Comparison
	Mean 2005	Mean 2012	Change	Mean 2005	Mean 2012	Change	
<i>Demographics</i>							
Male	0.491 (0.500)	0.503 (0.500)	0.011 (0.707)	0.509 (0.500)	0.509 (0.500)	0.001 (0.707)	0.011 (1.000)
Black	0.940 (0.238)	0.890 (0.313)	-0.050 (0.393)	0.413 (0.492)	0.412 (0.492)	-0.002 (0.696)	-0.048 (0.800)
White	0.031 (0.173)	0.061 (0.240)	0.030 (0.296)	0.549 (0.498)	0.532 (0.499)	-0.017 (0.705)	0.048 (0.764)
Hispanic	0.009 (0.094)	0.012 (0.110)	0.003 (0.145)	0.017 (0.131)	0.023 (0.151)	0.006 (0.200)	-0.003 (0.246)
Other race	0.020 (0.141)	0.037 (0.188)	0.016 (0.235)	0.020 (0.141)	0.034 (0.180)	0.013 (0.228)	0.003 (0.328)
FRPL	0.674 (0.469)	0.842 (0.365)	0.168 (0.594)	0.554 (0.497)	0.632 (0.482)	0.078 (0.693)	0.090 (0.913)
Special ed	0.112 (0.316)	0.108 (0.310)	-0.004 (0.443)	0.122 (0.327)	0.109 (0.311)	-0.013 (0.452)	0.009 (0.633)
ELL	0.014 (0.116)	0.010 (0.101)	-0.003 (0.154)	0.005 (0.071)	0.008 (0.091)	0.003 (0.115)	-0.007 (0.192)
N (Students)	14248	8642		147487	139146		
<i>Baseline Discipline (in 2005)</i>							
All	0.481 (1.019)	0.182 (0.715)	-0.299 (1.245)	0.737 (1.640)	0.091 (0.568)	-0.646 (1.736)	0.347 (2.136)
Suspension	0.470 (1.000)	0.182 (0.713)	-0.289 (1.228)	0.721 (1.602)	0.091 (0.564)	-0.630 (1.698)	0.342 (2.096)
Expulsion	0.011 (0.107)	0.001 (0.024)	-0.010 (0.110)	0.017 (0.137)	0.001 (0.026)	-0.016 (0.139)	0.006 (0.177)
N (Students)	14248	8602		147487	138580		

Notes: This table reports the demographics and baseline discipline records for grades 7-9 cohorts before and after Hurricane Katrina (i.e., in 2005 and 2012, respectively) in New Orleans and all other Louisiana school districts, excluding special school districts outside of Orleans. The last column shows the differences between the change in New Orleans compared to that in other school districts.

Table 3
Qualitative Tests of Identifying Assumption Regarding the Criminal Justice System

<i>Broad Category of Policy</i>	<i>Policies/Factors</i>	<i>Change in Policy from pre- to post-Katrina</i>	<i>If yes, In New Orleans only?</i>	<i>If yes, different effects on youth vs young adults?</i>	<i>Change on all three?</i>	<i>Likely affected New Orleans youth conviction rate?</i>
Policing	Reduction in police officers	Yes	Yes	No	No	Yes
	2012 Consent decree	Yes	Yes	No	No	No
	Temporary increase in truancy/ curfew enforcement	Yes	Yes	No	No	No
	School Resource Officers (SROs)	No	NA	NA	No	NA
Prosecution	Shifting lesser crimes to municipal courts	Yes	Yes	No	No	No
	Diversion programs	Yes	Yes	No	No	Maybe (non-violent only)
	Trying youths as adults	Yes	Yes	Yes	Yes	Yes
Detention	Risk assessment for pre-trial detention	Yes	New Orleans and four other parishes	Yes	Yes	Yes

Table 4
Panel Analysis: Returnees Only Conviction Rate
SCG Effect Point Estimates

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	-0.0063	-0.0005	-0.0003	-0.0012	-0.0033
2007	-0.0088	-0.0007	-0.0011	-0.0011	-0.0038
2008	-0.0120	0.0000	-0.0012	-0.0037	-0.0050
2009	-0.0166	-0.0004	-0.0030	-0.0050	-0.0067
2010	-0.0207	0.0000	-0.0045	-0.0081	-0.0078
2011	-0.0160	0.0016	-0.0047	-0.0089	-0.0036
2012	-0.0113	0.0008	-0.0054	-0.0050	-0.0017
2013	-0.0114	0.0004	-0.0043	-0.0070	-0.0005
2014	-0.0123	0.0004	-0.0078	-0.0041	-0.0008
2015	-0.0005	-0.0015	0.0013	-0.0015	0.0012
2016	-0.0102	-0.0008	-0.0050	-0.0016	-0.0028
2017	-0.0056	0.0001	-0.0040	-0.0009	-0.0007
2018	-0.0056	-0.0006	-0.0056	0.0008	-0.0001

Table 5A
Panel Analysis: Returnees Only Conviction Rate
SCG Placebo Studies: p-values for two-sided test

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.033**	0.136	0.560	0.207	0.067*
2007	0.267	0.318	0.240	0.483	0.233
2008	0.333	1.000	0.440	0.241	0.367
2009	0.167	0.636	0.240	0.138	0.233
2010	0.100	1.000	0.240	0.172	0.200
2011	0.033**	0.364	0.200	0.069*	0.400
2012	0.033**	0.364	0.240	0.241	0.233
2013	0.033**	0.591	0.240	0.103	0.633
2014	0.100	0.682	0.240	0.034**	0.500
2015	0.867	0.273	0.720	0.310	0.233
2016	0.133	0.273	0.360	0.345	0.100
2017	0.133	0.909	0.400	0.414	0.400
2018	0.267	0.455	0.240	0.345	0.933
Overall (2006-2018)	0.133	0.545	0.320	0.172	0.267
Overall (2010-2018)	0.033**	0.636	0.320	0.172	0.233
N (districts)	30	22	25	29	30

Table 5B
Panel Analysis: Returnees Only Conviction Rate
SCG Placebo Studies: p-values for one-sided test for significant negative effects

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.033**	0.136	0.320	0.103	0.033**
2007	0.167	0.227	0.200	0.241	0.100
2008	0.167	0.591	0.200	0.103	0.167
2009	0.133	0.455	0.160	0.103	0.100
2010	0.067*	1.000	0.120	0.103	0.100
2011	0.033**	1.000	0.120	0.069*	0.233
2012	0.033**	1.000	0.120	0.207	0.133
2013	0.033**	1.000	0.160	0.069*	0.333
2014	0.067*	1.000	0.160	0.034**	0.333
2015	0.533	0.182	1.000	0.138	1.000
2016	0.067*	0.045	0.200	0.103	0.067*
2017	0.067*	1.000	0.200	0.207	0.167
2018	0.133	0.273	0.120	1.000	0.467
Overall (2006-2018)	0.100	0.500	0.160	0.138	0.133
Overall (2010-2018)	0.033**	0.500	0.160	0.138	0.133
N (districts)	30	22	25	29	30

Table 6
Pooled Analysis: Students Only Crime Rate
SCG Effect Point Estimates

	By Crime Type				
	All	Violent	Drug	Property	Other
2006	-0.0129	-0.0006	0.0001	-0.0055	-0.0056
2007	-0.0160	-0.0003	-0.0017	-0.0024	-0.0060
2008	-0.0038	-0.0021	-0.0013	-0.0042	-0.0017
2009	-0.0051	0.0012	-0.0029	-0.0053	-0.0017
2010	-0.0024	-0.0005	-0.0020	-0.0033	-0.0064
2011	0.0045	0.0018	0.0000	0.0016	-0.0012
2012	-0.0025	0.0012	-0.0001	-0.0016	-0.0035

Table 7A
Pooled Analysis: Students Only Crime Rate
SCG Placebo Studies: p-values for two-sided test

	By Crime Type				
	All	Violent	Drug	Property	Other
2006	0.078*	0.377	1.000	0.047**	0.047**
2007	0.078*	0.475	0.724	0.109	0.125
2008	0.281	0.279	0.759	0.094*	0.313
2009	0.203	0.426	0.690	0.047**	0.344
2010	0.250	0.459	0.724	0.141	0.141
2011	0.172	0.180	0.983	0.266	0.328
2012	0.234	0.230	0.862	0.172	0.141
Overall (2006-2012)	0.141	0.328	0.828	0.094*	0.188
Overall (2010-2012)	0.281	0.295	0.845	0.203	0.172
N (districts)	64	61	58	64	64

Table 7B
Pooled Analysis: Students Only Crime Rate
SCG Placebo Studies: p-values for one-sided test for significant negative effects

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.047**	0.213	1.000	0.031**	0.047**
2007	0.031**	0.328	0.552	0.047**	0.109
2008	0.172	0.262	0.500	0.078*	0.188
2009	0.125	1.000	0.431	0.047**	0.234
2010	0.156	0.295	0.466	0.109	0.141
2011	1.000	1.000	0.552	1.000	0.188
2012	0.078*	1.000	0.448	0.078*	0.078*
Overall (2006-2012)	0.063*	0.393	0.690	0.063*	0.141
Overall (2010-2012)	0.141	0.443	0.638	0.141	0.156
N (districts)	64	61	58	64	64

Table 8
Panel Analysis: Returnees-to-Young-Adults Conviction Ratio
SCG Effect Point Estimates

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	-0.1656	-0.2373	-0.0164	-0.1091	-0.7057
2007	-0.2023	-0.2221	-0.0521	-0.0924	-0.5370
2008	-0.2870	-0.0330	-0.0431	-0.3682	-0.8042
2009	-0.4417	-0.3418	-0.1642	-0.4566	-1.0029
2010	-0.4730	-0.3041	-0.1720	-1.1100	-1.0070
2011	-0.3327	0.0870	-0.2298	-0.7087	-0.4142
2012	-0.2757	-0.4088	-0.2664	-0.2975	-0.2805
2013	-0.3245	-0.5755	-0.2036	-1.2289	-0.1014
2014	-0.2486	-0.7211	-0.3857	-0.1968	-0.1132
2015	0.0484	-1.0030	0.2722	-0.4294	0.2341
2016	-0.2787	-0.5748	-0.2286	-0.1907	-0.3162
2017	-0.0954	-0.2555	-0.0791	-0.0481	-0.0792
2018	-0.1148	-0.7478	-0.2945	0.2318	-0.1063

Table 9A
Panel Analysis: Returnees-to-Young-Adults Conviction Ratio
SCG Placebo Studies: p-values for two-sided test

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.033**	0.227	0.600	0.276	0.034**
2007	0.133	0.273	0.200	0.345	0.033**
2008	0.300	0.909	0.800	0.138	0.133
2009	0.067*	0.364	0.320	0.207	0.167
2010	0.100	0.500	0.400	0.138	0.100
2011	0.067*	0.773	0.240	0.103	0.067*
2012	0.100	0.409	0.320	0.207	0.067*
2013	0.033**	0.409	0.640	0.069*	0.167
2014	0.133	0.409	0.440	0.345	0.100
2015	0.433	0.136	0.400	0.103	0.033**
2016	0.067*	0.227	0.440	0.345	0.067*
2017	0.300	0.455	0.800	0.759	0.267
2018	0.200	0.273	0.400	0.138	0.200
Overall (2006-2018)	0.100	0.545	0.480	0.172	0.133
Overall (2010-2018)	0.067*	0.545	0.480	0.138	0.067*
N (districts)	30	22	25	29	30

Table 9B
Panel Analysis: Returnees-to-Young-Adults Conviction Ratio
SCG Placebo Studies: p-values for one-sided test for significant negative effects

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.033**	0.182	0.320	0.172	0.033**
2007	0.100	0.227	0.200	0.172	0.033**
2008	0.167	0.545	0.440	0.103	0.067*
2009	0.067*	0.273	0.160	0.103	0.100
2010	0.067*	0.318	0.080*	0.069*	0.100
2011	0.067*	1.000	0.200	0.069*	0.033**
2012	0.067*	0.318	0.120	0.138	0.033**
2013	0.033**	0.318	0.480	0.069*	0.133
2014	0.100	0.227	0.200	0.103	0.067*
2015	1.000	0.091*	1.000	0.069*	1.000
2016	0.033**	0.045**	0.200	0.103	0.033**
2017	0.100	0.227	0.360	0.517	0.067*
2018	0.067*	0.091*	0.160	1.000	0.100
Overall (2006-2018)	0.067*	0.318	0.320	0.103	0.067*
Overall (2010-2018)	0.033**	0.318	0.280	0.103	0.067*
N (districts)	30	22	25	29	30

Table 10
Pooled Analysis on Students-to-Young-Adults Conviction Ratio
SCG Effect Point Estimates

	By Crime Type				
	All	Violent	Drug	Property	Other
2006	-0.5197	-0.6617	-0.0373	-0.4526	-1.8473
2007	-0.4072	-0.0740	-0.0728	-0.2946	-1.1294
2008	-0.3403	0.0247	-0.1621	-0.3198	-0.5794
2009	-0.3716	-0.5307	-0.1692	-0.5164	-0.5894
2010	-0.2884	-0.2388	-0.1120	0.0176	-0.3903
2011	0.0290	-0.1439	-0.0456	0.2980	-0.9072
2012	-0.1963	-0.0561	-0.0698	0.0777	-1.3531

Table 11A
Pooled Analysis on Students-to-Young-Adults Conviction Ratio
SCG Placebo Studies: p-values for two-sided test

	By Crime Type				
	All	Violent	Drug	Property	Other
2006	0.049	0.500	0.421	0.043**	0.064*
2007	0.098	0.900	0.316	0.087*	0.043**
2008	0.148	0.850	0.211	0.087*	0.149
2009	0.098	0.550	0.211	0.109	0.170
2010	0.213	0.750	0.211	0.413	0.298
2011	0.377	0.900	0.737	0.130	0.106
2012	0.197	0.950	0.579	0.326	0.043*
Overall (2006-2012)	0.197	0.700	0.368	0.152	0.149
Overall (2010-2012)	0.295	0.900	0.526	0.217	0.128
N (districts)	61	20	19	46	47

Table 11B
Pooled Analysis on Students-to-Young-Adults Conviction Ratio
SCG Placebo Studies: p-values for one-sided test for significant negative effects

	All	By Crime Type			
		Violent	Drug	Property	Other
2006	0.016**	0.300	0.158	0.022**	0.043**
2007	0.082*	0.450	0.105	0.065*	0.021**
2008	0.066*	1.000	0.158	0.087*	0.128
2009	0.049**	0.350	0.105	0.087*	0.149
2010	0.115	0.500	0.158	1.000	0.255
2011	1.000	0.650	0.368	1.000	0.085*
2012	0.115	0.600	0.368	1.000	0.021**
Overall (2006-2012)	0.082*	0.600	0.211	0.087*	0.085*
Overall (2010-2012)	0.148	0.700	0.368	1.000	0.085*
N (districts)	61	20	19	46	47

Table 8
Quantitative Test on Population Change: Triple Differences

	New Orleans			All Other Louisiana Districts			
	1st Diff. 2000	1st Diff. 2012	Change (2nd Diff.)	1st Diff. 2000	1st Diff. 2012	Change (2nd Diff.)	3rd Diff.
Income	10394	1802	-8593	2210	-16025	-18236	9643
(2012 \$)	(15)	(1)	(4)	(6)	(16)	(17)	(4)
Prop. White	25.30	34.87	9.57	14.60	12.79	-1.81	11.38
	(26.28)	(51.88)	(8.15)	(24.30)	(24.04)	(2.26)	(8.00)
Prop. Black	-26.45	-36.95	-10.50	-14.95	-12.70	2.25	-12.74
	(25.18)	(38.64)	(7.39)	(25.49)	(22.47)	(2.76)	(7.78)
Prop. < HS	-16.00	-8.37	7.63	-9.00	-3.09	5.91	1.72
	(27.48)	(5.89)	(4.97)	(33.77)	(4.82)	(8.53)	(1.02)
Prop. HS	-8.18	-8.42	-0.24	-6.64	-5.23	1.40	-1.65
	(13.70)	(5.15)	(0.14)	(21.54)	(7.03)	(1.74)	(0.86)
Prop. < BA	3.92	-6.37	-10.30	4.75	-0.29	-5.04	-5.26
	(6.28)	(3.96)	(5.96)	(15.63)	(0.37)	(5.98)	(2.74)
Prop. BA	12.90	10.27	-2.63	9.23	-0.65	-9.88	7.25
	(28.07)	(9.56)	(2.25)	(38.81)	(1.22)	(16.90)	(5.55)
Prop. > BA	6.39	-0.84	-7.23	1.04	-2.52	-3.55	-3.68
	(18.34)	(1.20)	(9.24)	(6.76)	(6.26)	(8.25)	(4.12)

Notes: For each location (New Orleans or all other districts), the first two columns show the first difference between households with heads in the relevant age range (25-44 year-old for household income and 25-34 year-old for all other characteristics) and households with children in public schools (K-12). The second difference is the change between pre- and post-reform: the pre-reform Census year is 2000 and the post-reform period averages data from the American Community Survey from 2010-2012. The third difference is between New Orleans and all other districts.

Table 9
Impacts of school-level discipline propensity on contemporaneous conviction
Pooled sample: 2006-2012

	Numbers of contemporaneous convictions					
	(1)	(2)	(3)	(4)	(5)	(6)
Discipline propensity (overall)	0.010*	0.011*				
	(0.006)	(0.006)				
Suspension propensity			0.009*	0.010*		
			(0.005)	(0.005)		
Expulsion propensity					0.032***	0.037***
					(0.005)	(0.006)
Number of observations	31811	31798	31811	31798	31811	31798
Demographic controls	Y	Y	Y	Y	Y	Y
Prior achievement controls	Y	Y	Y	Y	Y	Y
Grade FEs	Y	Y	Y	Y	Y	Y
Year FEs	Y	Y	Y	Y	Y	Y

Notes: The propensity measures used in columns (1), (3), and (5) are calculated without controlling for prior achievements whereas the measures in columns (2), (4), and (6) have taken prior achievements into account.

Appendix A: Additional Tables and Figures

Table A1
Descriptive Statistics for Panel of Returnees Relative to All Students at Baseline (2005),
New Orleans and Comparison Groups (Weighted by Panel SCG Weights)

	New Orleans			All Other Louisiana Districts			DID: New Orleans Minus Comparison
	Returnees	All	Diff.	Returnees	All	Diff.	
<i>Demographics</i>							
Male	0.416 (0.493)	0.500 (0.500)	0.084 (0.702)	0.502 (0.500)	0.509 (0.500)	0.007 (0.707)	0.077 (0.997)
Black	0.873 (0.334)	0.934 (0.248)	0.061 (0.416)	0.230 (0.421)	0.232 (0.422)	0.001 (0.596)	0.060 (0.727)
White	0.069 (0.253)	0.036 (0.187)	-0.032 (0.315)	0.737 (0.440)	0.732 (0.443)	-0.006 (0.624)	-0.027 (0.699)
Hispanic	0.012 (0.107)	0.012 (0.111)	0.001 (0.154)	0.016 (0.127)	0.019 (0.136)	0.003 (0.186)	-0.002 (0.242)
Other race	0.047 (0.212)	0.017 (0.130)	-0.030 (0.249)	0.016 (0.125)	0.017 (0.131)	0.002 (0.180)	-0.032 (0.307)
FRPL	0.728 (0.445)	0.805 (0.396)	0.077 (0.596)	0.441 (0.496)	0.444 (0.497)	0.004 (0.702)	0.073 (0.921)
Special ed	0.047 (0.212)	0.102 (0.303)	0.055 (0.370)	0.148 (0.355)	0.150 (0.357)	0.002 (0.503)	0.054 (0.625)
ELL	0.047 (0.211)	0.020 (0.141)	-0.027 (0.254)	0.007 (0.082)	0.007 (0.084)	0.000 (0.117)	-0.027 (0.280)
<i>Discipline</i>							
All	0.157 (0.532)	0.387 (0.957)	0.230 (1.095)	0.274 (0.929)	0.320 (1.039)	0.046 (1.394)	0.184 (1.773)
Suspension	0.157 (0.532)	0.385 (0.953)	0.228 (1.092)	0.273 (0.926)	0.318 (1.035)	0.046 (1.389)	0.183 (1.767)
Expulsion	0.000 (0.000)	0.002 (0.047)	0.002 (0.047)	0.001 (0.029)	0.001 (0.039)	0.001 (0.048)	0.002 (0.067)
N (Students)	2346	13892		9945	13174		

Notes: This table reports the demographics and discipline records for students in grades 4-6 in the school year 2005 for New Orleans and all other Louisiana school districts, excluding special school districts outside of Orleans. The comparison groups have been weighted using the same weights as the panel SCG analysis. The first difference shows the means for all students minus those of the returnees in the panel sample. The last column shows the differences between the change in New Orleans compared to that in other school districts.

Table A2
Descriptive Statistics for New Orleans Relative to Comparison Groups,
Pooled Sample (Weighted by Pooled Crime Rate SCG Weights) - Before and After Katrina

	New Orleans			All Other Louisiana Districts			DID: New Orleans Minus Comparison
	Mean 2005	Mean 2012	Change	Mean 2005	Mean 2012	Change	
<i>Demographics</i>							
Male	0.491 (0.500)	0.503 (0.500)	0.011 (0.707)	0.507 (0.500)	0.514 (0.500)	0.007 (0.707)	0.004 (1.000)
Black	0.940 (0.238)	0.890 (0.313)	-0.050 (0.393)	0.392 (0.488)	0.410 (0.492)	0.018 (0.693)	-0.069 (0.797)
White	0.031 (0.173)	0.061 (0.240)	0.030 (0.296)	0.568 (0.495)	0.533 (0.499)	-0.035 (0.703)	0.066 (0.763)
Hispanic	0.009 (0.094)	0.012 (0.110)	0.003 (0.145)	0.015 (0.121)	0.018 (0.134)	0.003 (0.180)	0.000 (0.231)
Other race	0.020 (0.141)	0.037 (0.188)	0.016 (0.235)	0.025 (0.157)	0.039 (0.194)	0.014 (0.249)	0.003 (0.343)
FRPL	0.674 (0.469)	0.842 (0.365)	0.168 (0.594)	0.553 (0.497)	0.646 (0.478)	0.093 (0.690)	0.075 (0.911)
Special ed	0.112 (0.316)	0.108 (0.310)	-0.004 (0.443)	0.125 (0.331)	0.106 (0.308)	-0.019 (0.452)	0.014 (0.632)
ELL	0.014 (0.116)	0.010 (0.101)	-0.003 (0.154)	0.003 (0.056)	0.007 (0.085)	0.004 (0.102)	-0.007 (0.184)
N (Students)	14248	8642		144479	136114		
<i>Baseline Discipline (in 2005)</i>							
All	0.481 (1.019)	0.182 (0.715)	-0.299 (1.245)	0.733 (1.607)	0.081 (0.513)	-0.652 (1.687)	0.353 (2.096)
Suspension	0.470 (1.000)	0.182 (0.713)	-0.289 (1.228)	0.722 (1.582)	0.080 (0.510)	-0.642 (1.662)	0.353 (2.066)
Expulsion	0.011 (0.107)	0.001 (0.024)	-0.010 (0.110)	0.011 (0.109)	0.000 (0.024)	-0.010 (0.112)	0.000 (0.157)
N (Students)	14248	8602		144479	136114		

Notes: This table reports the demographics and baseline discipline records for grades 7-9 cohorts before and after Hurricane Katrina (i.e., in 2005 and 2012, respectively) in New Orleans and all other Louisiana school districts, excluding special school districts outside of Orleans. The comparison groups have been weighted using the same weights as the pooled SCG analysis on crime rates for all crime. The last column shows the differences between the change in New Orleans compared to that in other school districts.

Table A3
Descriptive Statistics for New Orleans Relative to Comparison Groups,
Pooled Sample (Weighted by Pooled Crime Ratio SCG Weights)- Before and After Katrina

	New Orleans			All Other Louisiana Districts			DID: New Orleans Minus Comparison
	Mean 2005	Mean 2012	Change	Mean 2005	Mean 2012	Change	
<i>Demographics</i>							
Male	0.491 (0.500)	0.503 (0.500)	0.011 (0.707)	0.509 (0.500)	0.510 (0.500)	0.001 (0.707)	0.010 (1.000)
Black	0.940 (0.238)	0.890 (0.313)	-0.050 (0.393)	0.395 (0.489)	0.388 (0.487)	-0.006 (0.690)	-0.044 (0.795)
White	0.031 (0.173)	0.061 (0.240)	0.030 (0.296)	0.565 (0.496)	0.552 (0.497)	-0.013 (0.702)	0.044 (0.762)
Hispanic	0.009 (0.094)	0.012 (0.110)	0.003 (0.145)	0.019 (0.137)	0.026 (0.158)	0.007 (0.209)	-0.003 (0.255)
Other race	0.020 (0.141)	0.037 (0.188)	0.016 (0.235)	0.021 (0.143)	0.034 (0.182)	0.013 (0.232)	0.003 (0.330)
FRPL	0.674 (0.469)	0.842 (0.365)	0.168 (0.594)	0.537 (0.499)	0.618 (0.486)	0.081 (0.696)	0.087 (0.915)
Special ed	0.112 (0.316)	0.108 (0.310)	-0.004 (0.443)	0.120 (0.325)	0.106 (0.308)	-0.014 (0.448)	0.009 (0.630)
ELL	0.014 (0.116)	0.010 (0.101)	-0.003 (0.154)	0.004 (0.067)	0.008 (0.089)	0.003 (0.111)	-0.007 (0.190)
N (Students)	14248	8642		143651	135645		
<i>Baseline Discipline (in 2005)</i>							
All	0.481 (1.019)	0.182 (0.715)	-0.299 (1.245)	0.757 (1.689)	0.086 (0.559)	-0.671 (1.779)	0.372 (2.171)
Suspension	0.470 (1.000)	0.182 (0.713)	-0.289 (1.228)	0.741 (1.648)	0.086 (0.554)	-0.655 (1.739)	0.367 (2.129)
Expulsion	0.011 (0.107)	0.001 (0.024)	-0.010 (0.110)	0.016 (0.137)	0.001 (0.025)	-0.016 (0.140)	0.005 (0.178)
N (Students)	14248	8602		143651	135645		

Notes: This table reports the demographics and baseline discipline records for grades 7-9 cohorts before and after Hurricane Katrina (i.e., in 2005 and 2012, respectively) in New Orleans and all other Louisiana school districts, excluding special school districts outside of Orleans. The comparison groups have been weighted using the same weights as the pooled SCG analysis on crime ratios for all crime. The last column shows the differences between the change in New Orleans compared to that in other school districts.

Table A4
Synthetic Control Weights

Panel		Pooled Conviction Rate (All Crimes)		Pooled Conviction Ratio (All Crimes)	
School District	SCG Weight	School District	SCG Weight	School District	SCG Weight
St. Tammany	0.5670	Iberia	0.2960	Vernon	0.0910
St. James	0.1630	Jefferson Davis	0.1250	Lincoln	0.0720
Iberia	0.1360	St. Charles	0.0810	Madison	0.0290
Lincoln	0.1330	Tensas	0.0720	St. Martin	0.0290
				West Feliciana	0.0260
				Iberia	0.0220
				Lafourche	0.0220

Notes: This table lists the school districts from the donor pool that contribute at least 2% to the synthetic control in each analysis, along with their corresponding weights.

Figure A1 – Panel Analysis: Trends in Returnee Only Conviction Rates, by Type

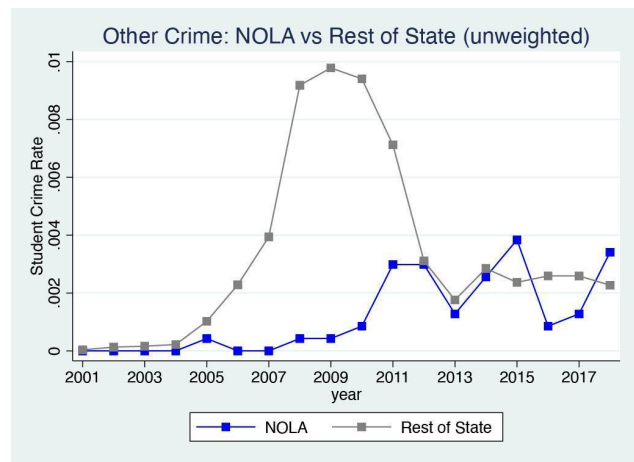
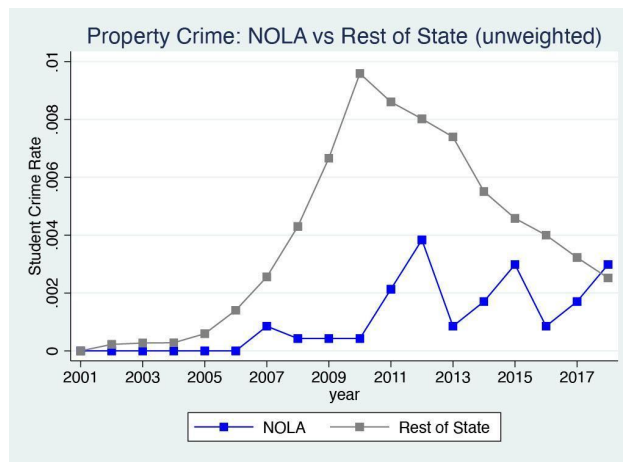
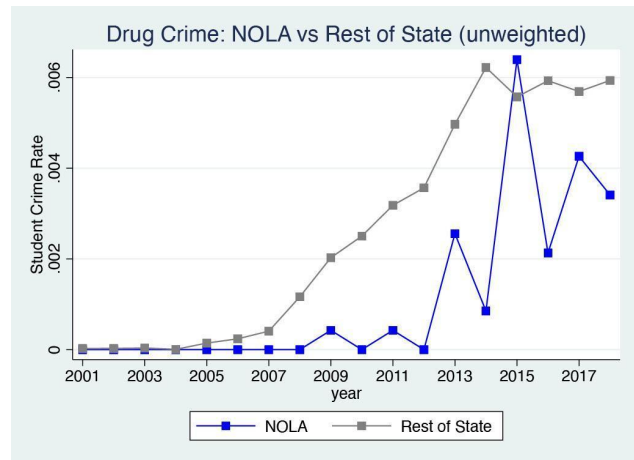
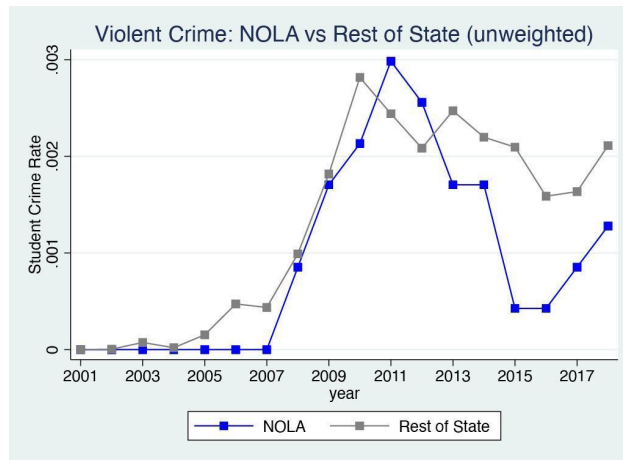


Figure A2 – Panel Analysis: Trends in *Ratio* of Conviction Rates, by Type

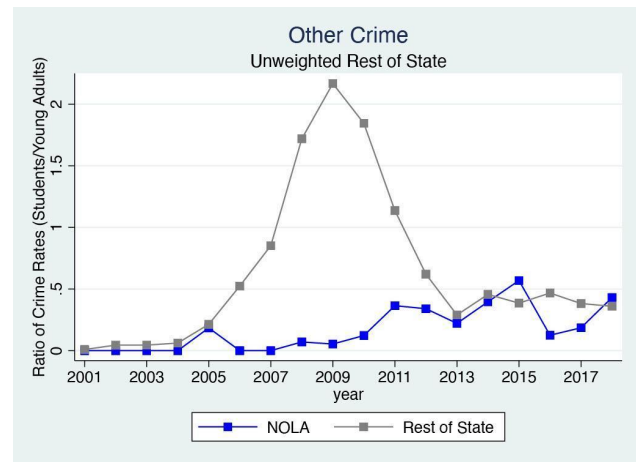
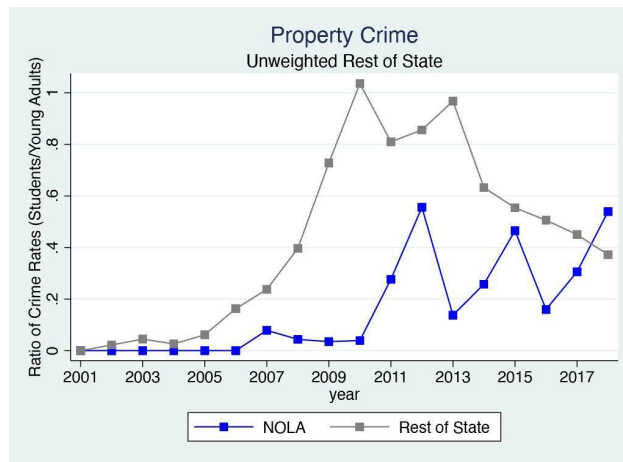
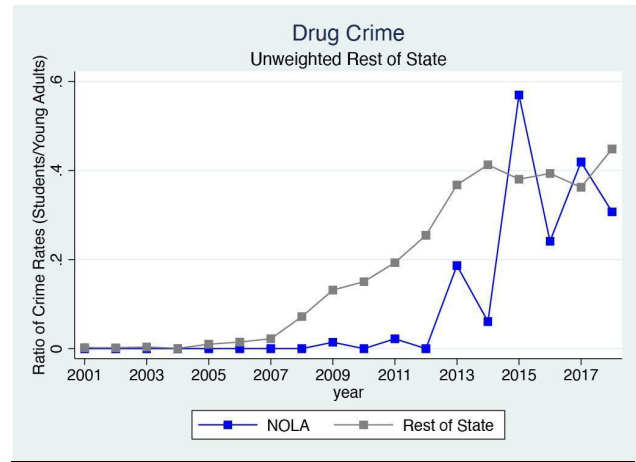
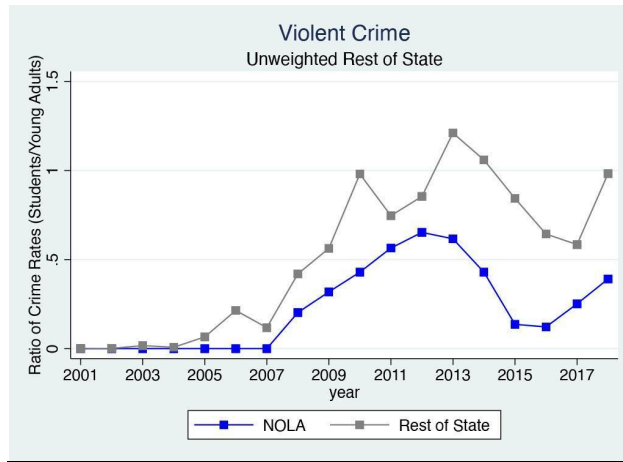


Figure A3 – Pooled Analysis: Trends in Student Only Conviction Rates, by Type

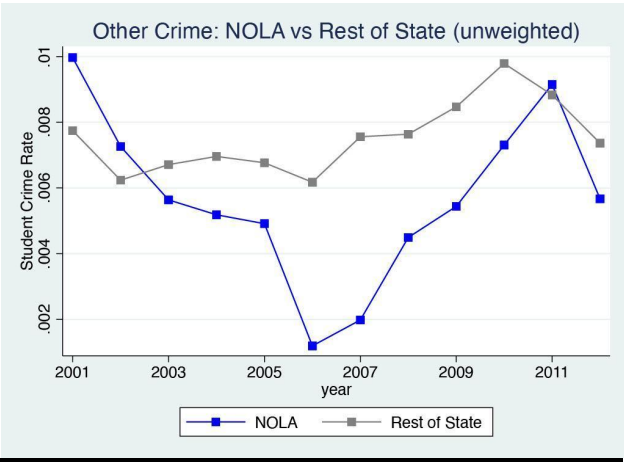
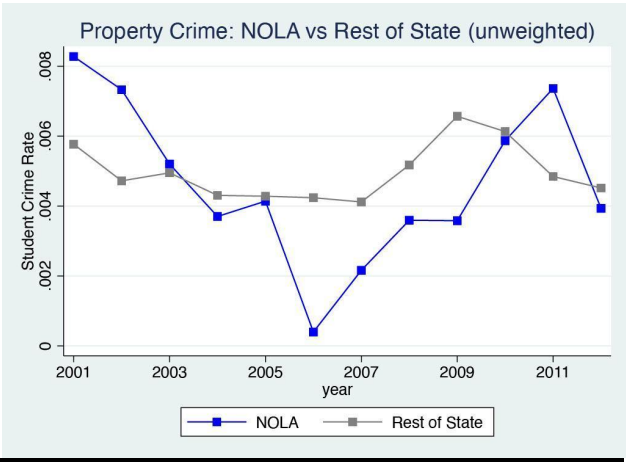
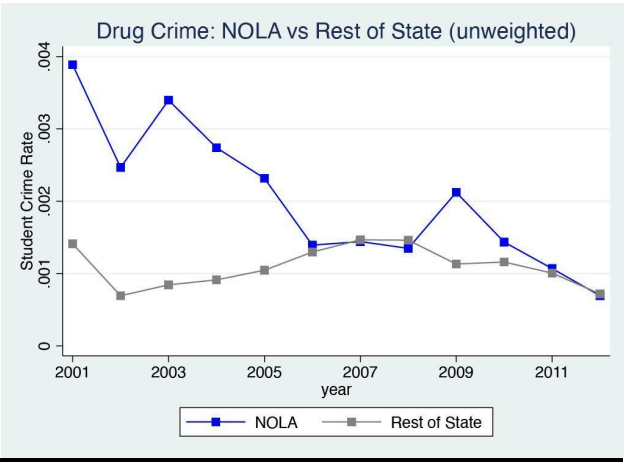
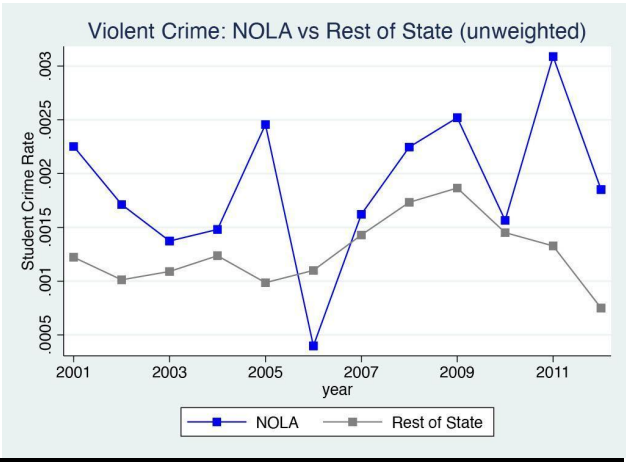


Figure A4 – Pooled Analysis: Trends in *Ratio* of Conviction Rates by Type

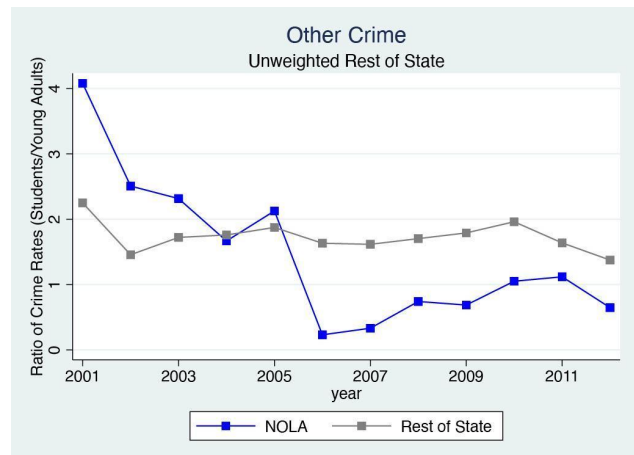
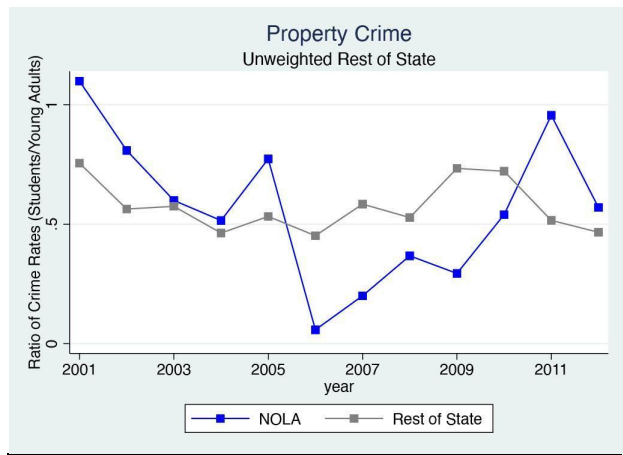
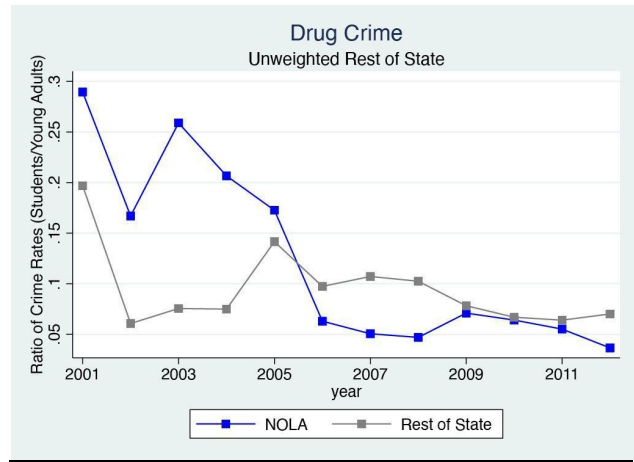
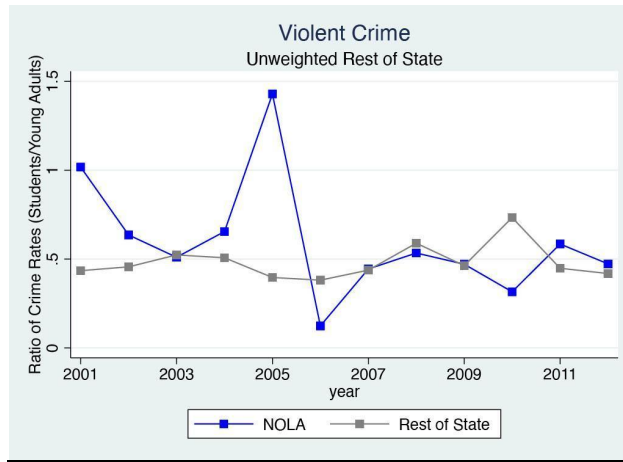


Figure A5 - Panel Analysis: Synthetic Control Effects on Returnee Only Conviction Rates, by Type

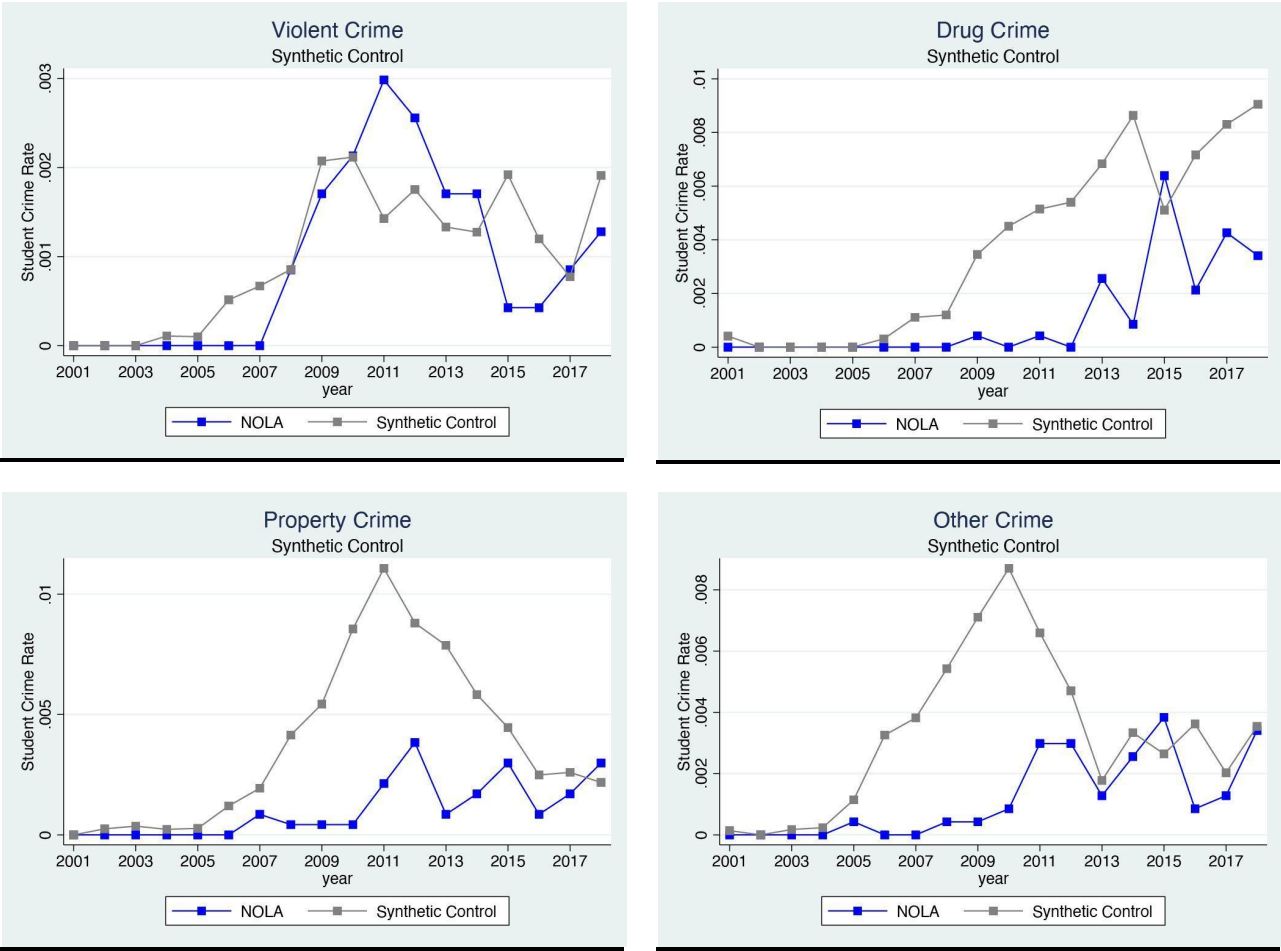


Figure A6 – Panel Analysis: Placebo Studies of Returnee Only Conviction Rate, by Type

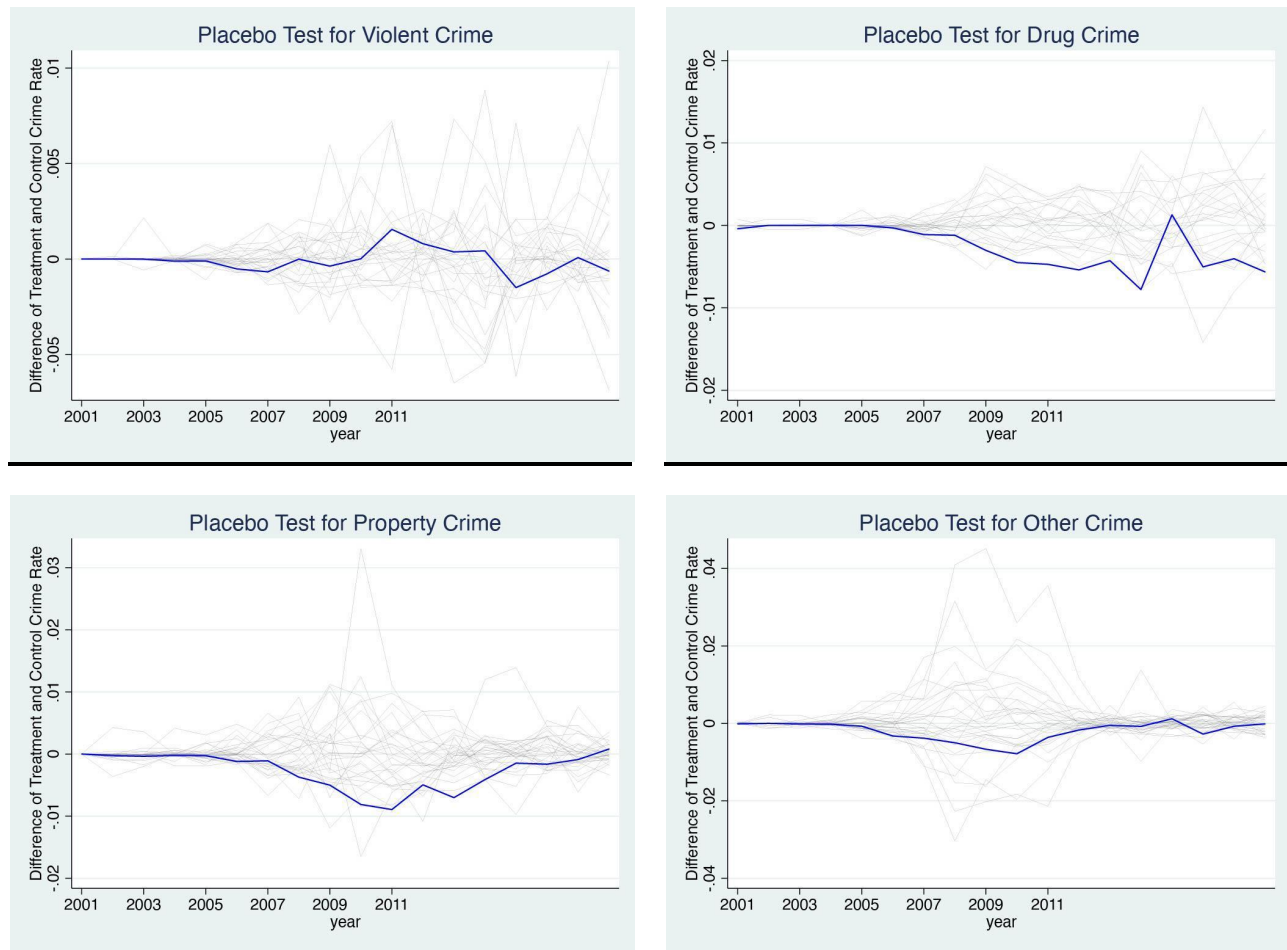
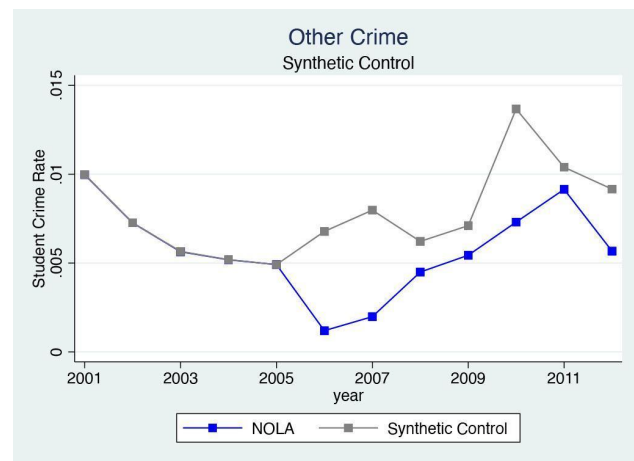
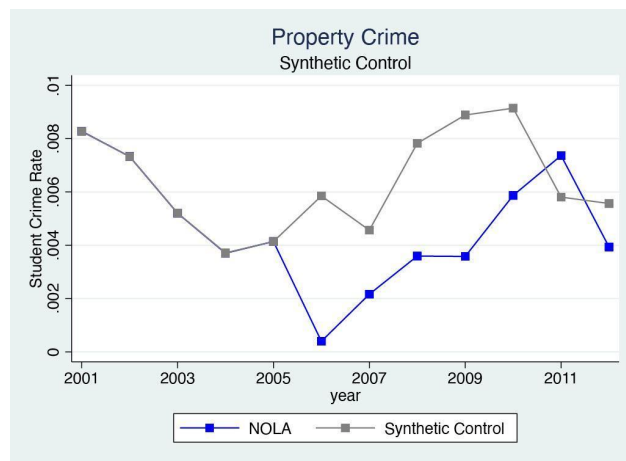
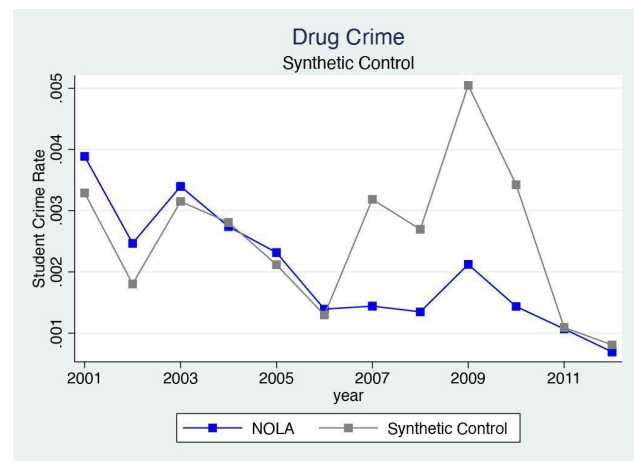
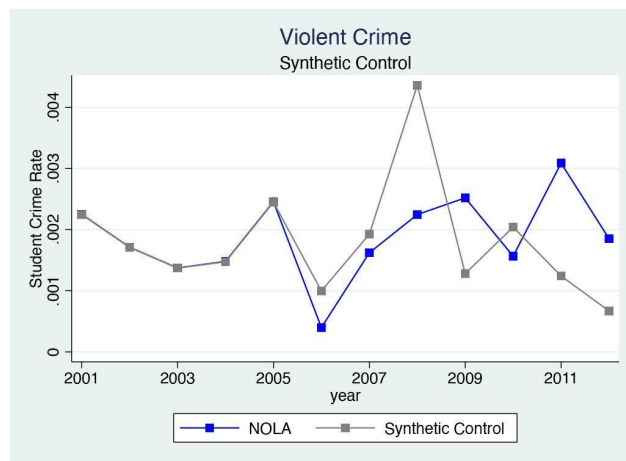


Figure A7 – Pooled Analysis: Synthetic Control Effects on Student Only Conviction Rates, by Type



**Figure A8 – Pooled Analysis: Placebo Studies Student Only Conviction Rates,
by Type**

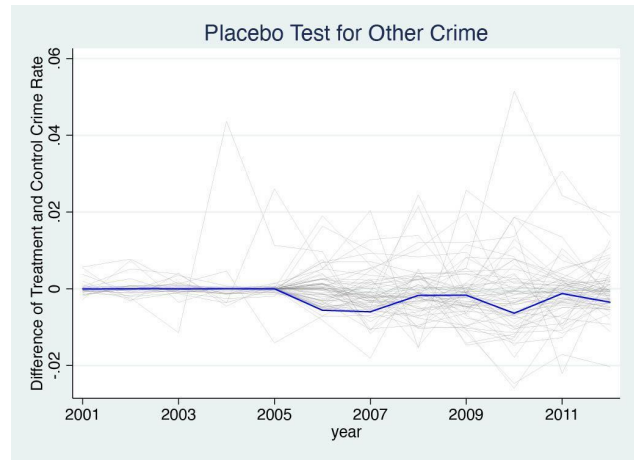
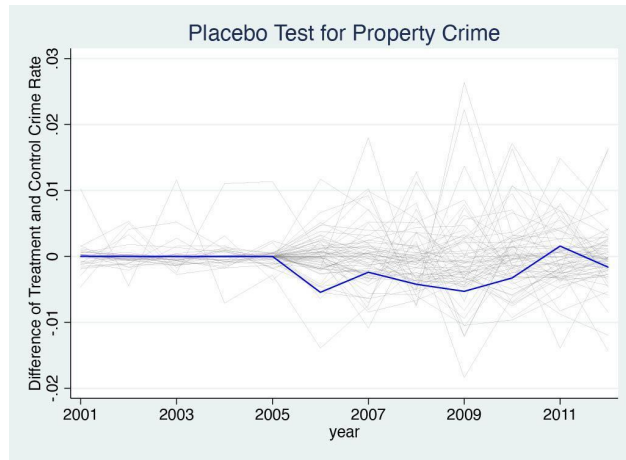
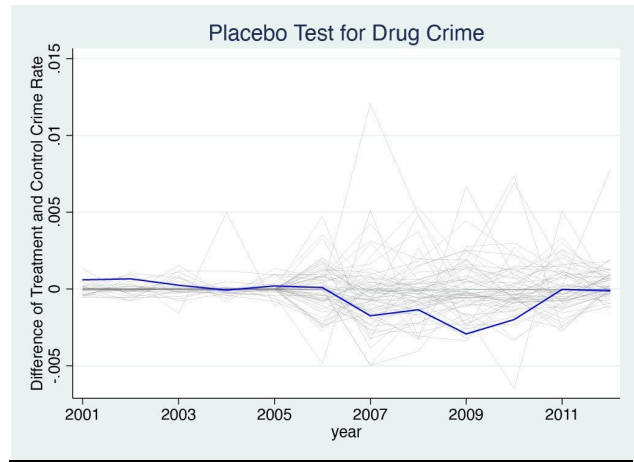
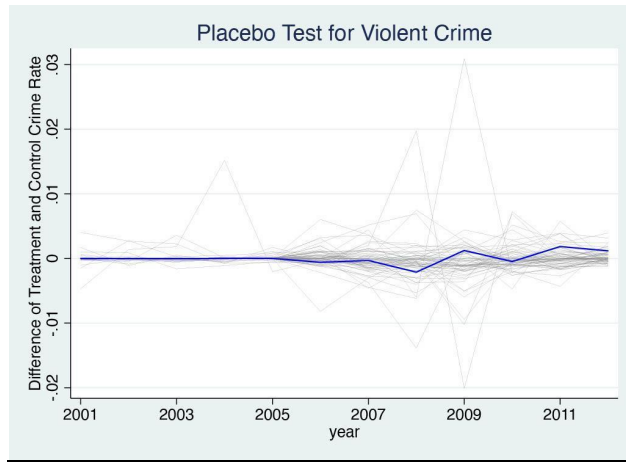
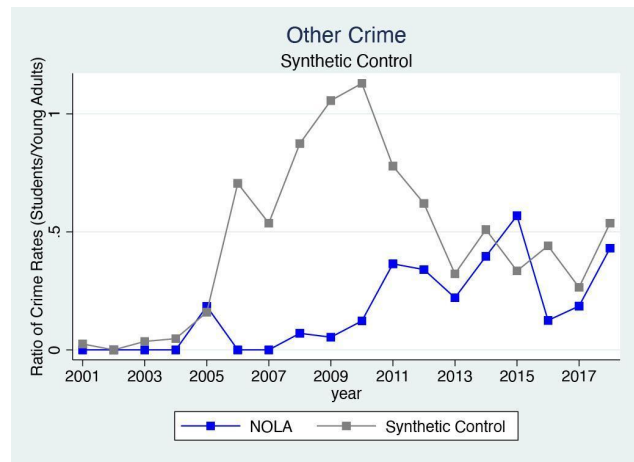
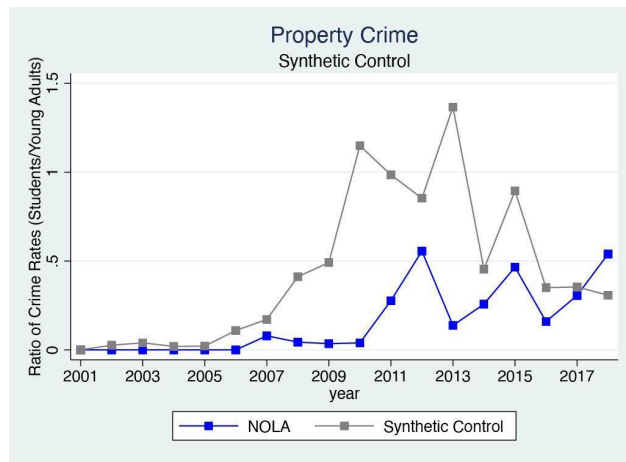
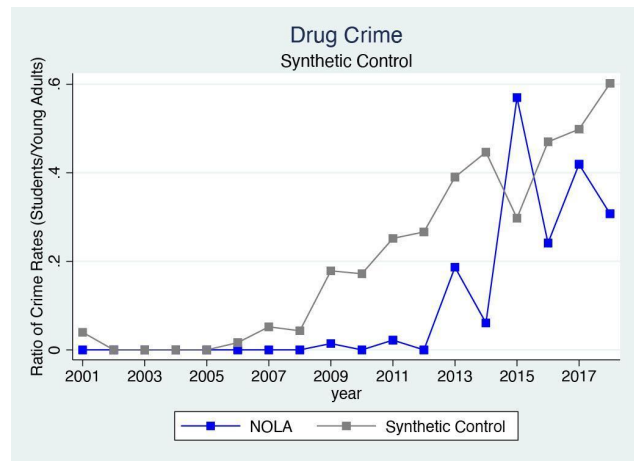
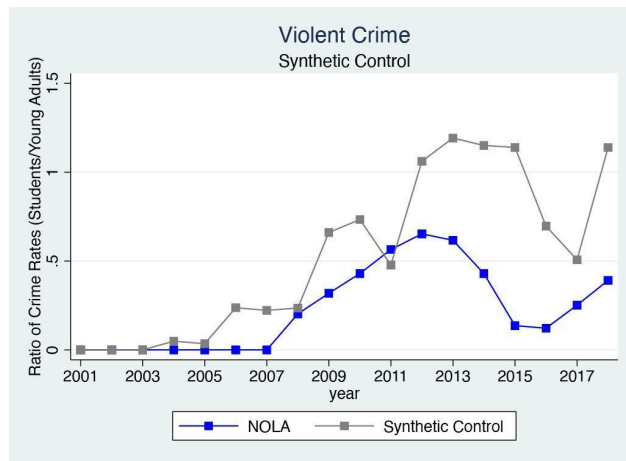
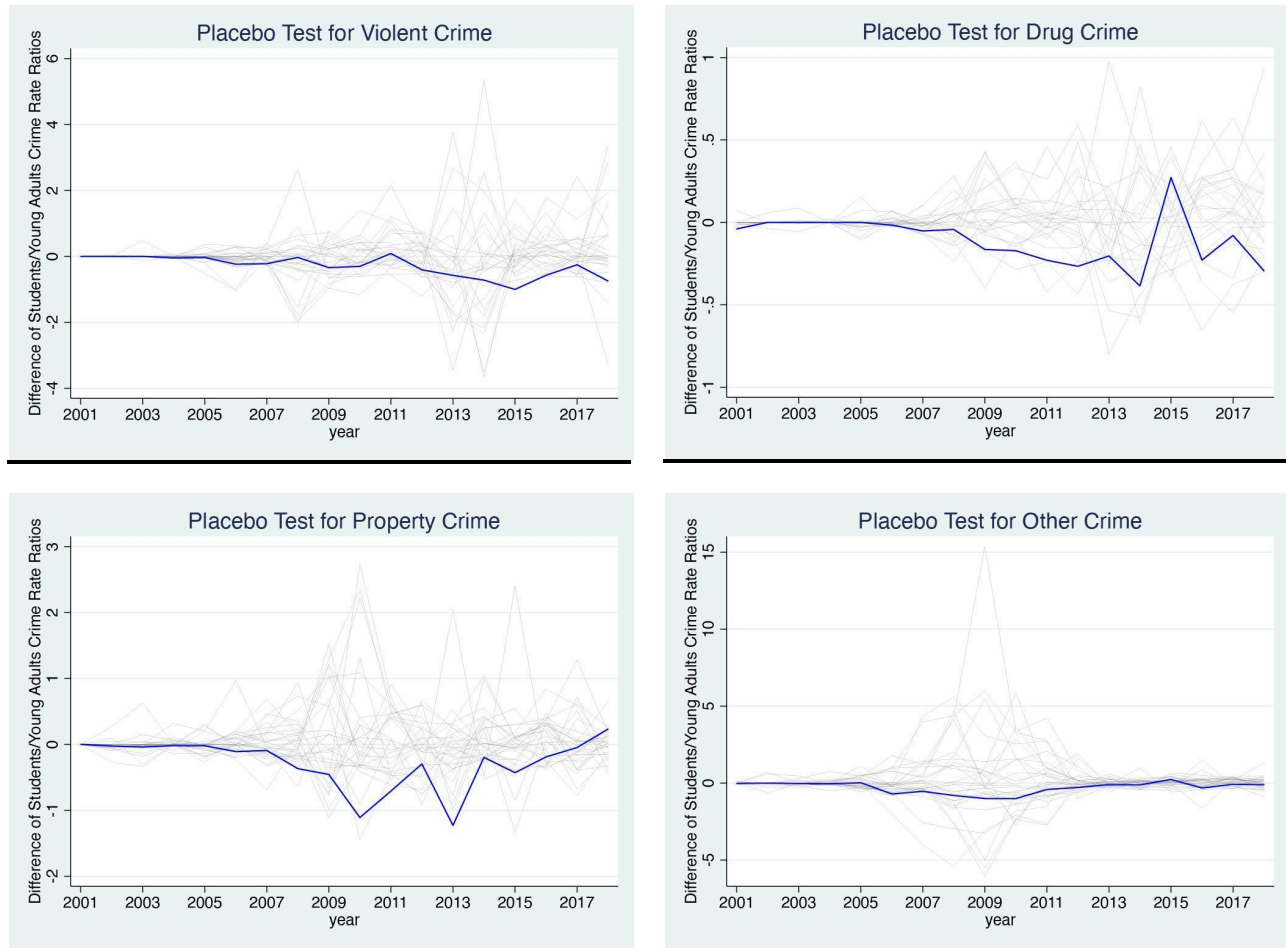


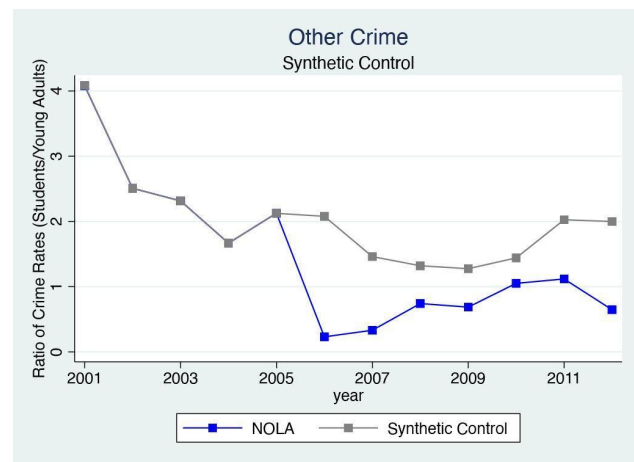
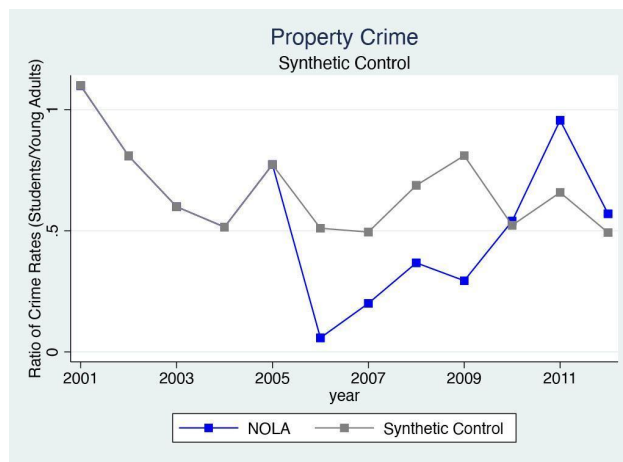
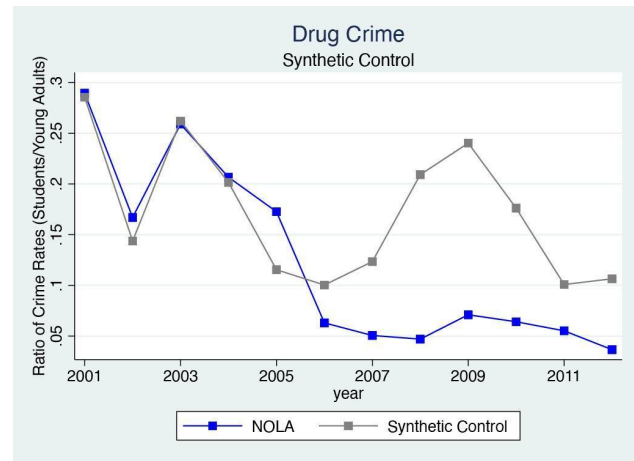
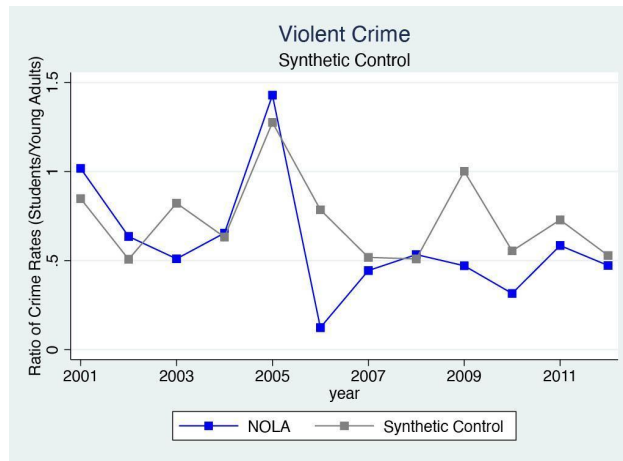
Figure A9 – Panel Analysis: Synthetic Control Effects for *Ratio* of Conviction Rates, by Type



**Figure A10 – Panel Analysis: Placebo Studies of *Ratio* of Conviction Rates,
by Type**



**Figure A11 – Pooled Analysis: Synthetic Control Effects for *Ratio* of Conviction Rates,
by Type**



**Figure A12 – Pooled Analysis: Placebo Studies of *Ratio* of Conviction Rates,
by Type**

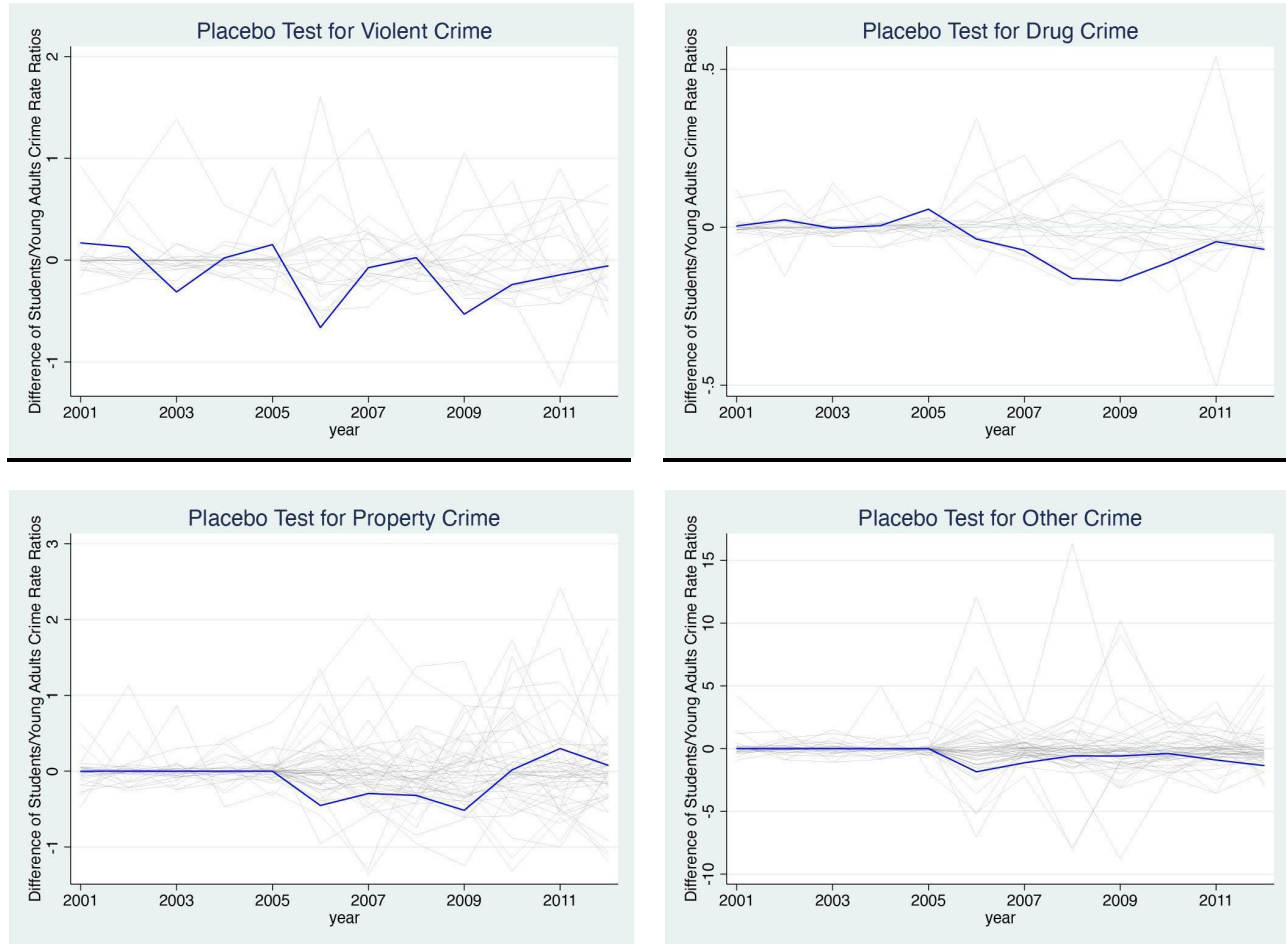
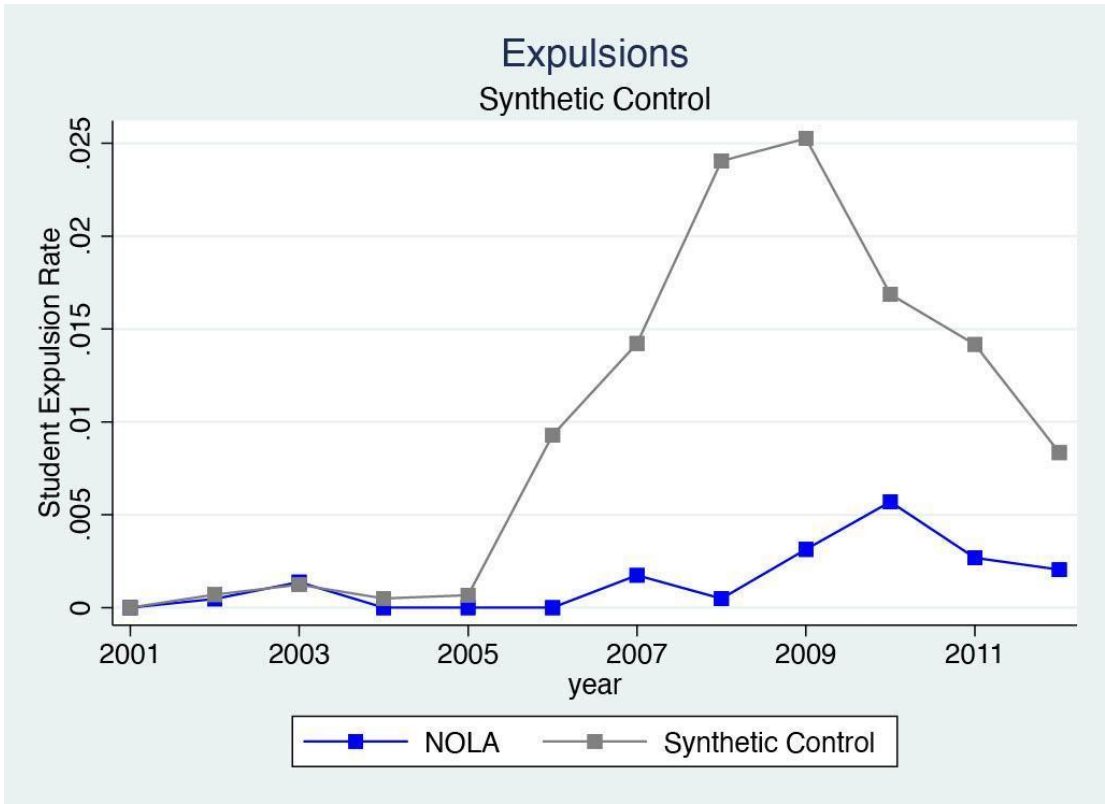


Figure A13 - Panel Analysis: Synthetic Control Group based on Expulsion Rates



Appendix B: Details on Qualitative Identification Tests

We are interested in the effects of Hurricane Katrina on the crime rate of school-age children. It is therefore useful to begin by understanding the wide range of factors potentially affecting crime and convictions in the aftermath of the storm.

Given our particular interest in the New Orleans school reform as one potential mechanism, we also apply a triple-difference design to isolate this factor. Our qualitative analysis therefore was designed to test the identifying assumptions for this design. We asked respondents to talk about factors that met three conditions: (a) changed over time; (b) could have influenced juveniles and adults differently; and (c) could have affected New Orleans differently from other similar districts. All three of these conditions would have to hold in order for a factor to pose a threat to identification with respect to the school reforms.²⁷

We began with a search of media and public documents to identify changes in the crime environment and criminal justice system. We conducted a search on nola.com and thelensnola.org for the news published between 9/1/2005 and 7/31/2018. Our search started with general terms related to juvenile justice (reform), which involved different combinations of “juvenile”/“youth” and criminal-justice-related terms such as “court,” “justice,” and “policing.” We then moved to specific terms that we either considered relevant *a priori* (e.g., “school resource officers”) or found during our general search (e.g., “Raise the Age” and “Juvenile Justice Intervention Center”). We also searched for changes in criminal justice laws and policies with the terms “law change,” “law passed,” and “bill passed.” We focused on policy changes that

²⁷We also provide information on whether a factor affected New Orleans youth conviction rate (our main interest) in the last column of Table 3. However, whether this affects our identification assumptions is already summarized in the three aforementioned conditions. For instance, if a factor affects *only* New Orleans youths, then condition (b) is met, that is, this factor influenced juveniles and adults differently.

directly affected the probability of arrest and conviction, but also considered changes in sentencing, detention, and incarcerations that might have indirect effects on the later part of our panel, e.g., through recidivism.

We also held a focus group with community leaders involved in the criminal justice system, in which we introduced our project along with an overview of the methodology and preliminary results. We obtained information from the participants through the questions and comments during our presentation of the project as well as through explicitly asking about post-Katrina changes in context and the justice system. We followed up with interviews of three individual leaders who, during the post-Katrina period, held high-level positions in the District Attorney's office, police department, and a non-profit focused on juvenile justice in New Orleans, respectively. Interview questions pertained to the main changes in context, policing, and prosecution.²⁸ In some cases, these interviews allowed us to identify additional public documents, which we also reviewed.

We are concerned with changes during the years 2005-2012 for juvenile justice and 2005-2018 for adult criminal justice, aligning with the time periods of our quantitative analysis. Changes in the juvenile justice system after 2012 are not relevant because the youngest cohort in our returnee panel turned 17 in 2012, when they would belong to the adult system. Our pooled analysis sample also ended in 2012. Our interest in the adult criminal justice system extended to 2018 because changes in this system would affect both the conviction rate and conviction ratio in the panel analysis. Similarly, we asked interviewees to limit their comments

²⁸Protocols are available upon request.

to the pre-2018 period. When interviewees discussed changes, we asked for the specific years of these changes to ensure that they were relevant to the timeline and chain of events.

We summarize the policies and factors that showed up in our qualitative analysis in Table 3 and provide more details on each factor below, in the order of their appearance in the table.

B.I. Changes in Policing

The FBI reports that, from 2004 (just before Katrina) to 2018, the number of police officers in New Orleans declined from 3.5 to 2.9 per thousand residents (FBI:UCR, 2022). The trajectory involves a large increase to 6.4 officers per thousand residents in 2007 (due mostly to a sharp, temporary drop in the post-Katrina population), followed by a steady decline to 2.9. In absolute numbers, the media reported a 22 percent drop in the number of officers between 2010 and 2013. An expert on policing we spoke with also discussed this police force decline without our prompting and lamented the difficulties it created. While this decline was larger in New Orleans than in most nearby parishes, there was no indication that this affected adults and juveniles differently.²⁹

On July 24, 2012, New Orleans Police Department (NOPD) agreed to a consent decree, following a two-year federal investigation into the department, in which the U.S. Department of Justice found a pattern of excessive force and discriminatory policing. The decree imposed unprecedented changes in all areas of policing in New Orleans, coupled with federal oversight. An expert from the DA's office told us that the decree had constrained the NOPD and speculated that this might have contributed to reductions in drug and property crimes convictions in the

²⁹ We considered Jefferson, Calcasieu, St. Tammany, St. Bernard, Plaquemines, Vermilion, and Cameron (FBI:UCR, 2022). Only St. Bernard showed a slightly similar trend to New Orleans, but the increase in 2007 and drop afterward were much sharper than in New Orleans, resulting in an overall increase in number of officers per resident compared to pre-Katrina levels.

preliminary quantitative results that we showed. The interviewee gave examples, including police neither being allowed to stop and frisk nor give chase once suspects got in a car and exceeded a certain speed. Anecdotally, this created frustration within the NOPD, demotivated the officers, and further reduced the number of arrests. Again, however, there is no evidence of differential effects on adults and juveniles.

When District Attorney (DA) Leon Cannizzaro took office in 2008, the DA's office worked with the police to begin enforcing juvenile curfew and truancy laws more strictly. However, under Mayor Mitch Landrieu, whose term started in 2010, police enforcement of curfew and truancy laws was stopped (note that the DA can only get involved in cases brought by police, which are under mayoral control). Experts from both the DA's office and police department agreed that these curfew and truancy laws were not meant to punish students, but to keep youth in a positive environment (e.g., school), ensure their safety, and prevent the possibility of serious crime. A policing expert also pointed out that, for these laws to work as intended, there needed to be adequate facilities and social workers, and the system fell short in this respect. Given that this increase in curfew and truancy enforcement was brief and occurred in the middle of the panel, which is of less interest in our analysis, this, too, should not introduce bias in our estimates.

We also investigated policing within schools through the presence of school resource officers (SROs), officers of the sheriff's office or police department who are specifically trained in school-based law enforcement and crisis response, and stationed within schools. An increased presence of SROs could increase the chances that incidents within schools are handled by the criminal justice system. A policing expert confirmed that, given the multiple issues the NOPD had to deal with after Katrina, including the reduction in the size of the police force, providing SROs

was very low on the department’s priority list. None of the experts saw changes in SRO use, and we did not find direct evidence of their use either before or after Katrina.³⁰

We therefore see little evidence of changes in policing that would affect the conviction ratio and introduce bias into our estimates of school reform effects.

B.II. Changes in Prosecution

Whether crimes show up in our data as convictions³¹ also depends on how the justice system prosecutes crimes. In many cities, such as New Orleans, criminal cases are prosecuted by the DA and more minor offenses are handled by the City Attorney in municipal court, though the specific types of crimes handled by each agency changed somewhat over time.

Prompted by problems in the criminal justice system and essentially all public agencies in the wake of Katrina, most low-level offenses in New Orleans eventually fell under the jurisdiction of the municipal instead of the criminal court. Roman, Irazola, & Osborne (2007) and Martin (2020) reported the overwhelmed criminal justice system’s inability to handle new cases in a timely fashion in New Orleans immediately after Katrina. In 2006, more than three thousand suspects,³² including those accused of violent crimes, were released according to state Article 701 on the rights to speedy trials (Roman, Irazola, & Osborne, 2007) , effectively making their prosecution impossible. Cannizzaro, who was elected DA in 2008, chose to focus DA resources on the most serious and violent crimes. He transferred marijuana possession cases to municipal

³⁰ We extended our media search specifically for the term “school resource officer(s)” and its acronym further back to ascertain this. The search in nola.com and thelensnola.org between 9/1/2000 and 7/31/2018 yielded one passing mention in a 2011 article that summarized a school board meeting.

³¹ Note that we combine juvenile adjudications and criminal convictions in our analysis and refer to them as “convictions” for convenience.

³² Compared to 180 in 2003 and 2004.

court in 2009 and, eventually, most misdemeanor cases were prosecuted by the City Attorney in the municipal court. This transfer to municipal court was apparently unique to New Orleans (i.e., not a state policy), and it affected adults and juveniles who were transferred into the adult system. An expert we interviewed confirmed that the policy put a quick stop to the “[Article] 701 [speedy trial] problem,” but our interviewee did not mention any long-run effects of the transfer of minor offenses to municipal court.

A related change is the 2010 ordinance stipulating that individuals caught for lesser offenses will be issued summons to appear in court rather than arrested.³³ This applied only to the adult system until August of 2017 when a similar change was made for juveniles. Neither should have any effects on the probabilities of conviction.

The DA office leader we interviewed indicated that Cannizzaro’s term also marked the expansion of diversion programs designed to keep low-level offenders out of prison and to provide support and incentives to avoid further criminal incidents. Specifically, to avoid charges, defendants were required to: obtain a GED or job skills, maintain contact with the program counselor, and avoid further trouble. This interviewee argued that successful completion led to a decline in recidivism, but that this did not affect conviction rates differentially between adults and juveniles who were both eligible for diversion.

From the perspective of the non-profit expert we interviewed, however, some accused juveniles apparently rejected the diversion programs, opting for prosecution in juvenile court, because unlike an adult criminal conviction, a juvenile adjudication is less problematic as juvenile records are sealed. So, it is possible that the expansion of the diversion program, which lasted until

³³ The ordinance was referenced in several media articles. However, we could not find any direct reporting at the time the ordinance was issued. The one link provided in an article on a related issue was broken.

the start of the COVID-19 pandemic, had a larger effect on adults. The diversion program might have reduced the share of defendants prosecuted in court more for adults than for juveniles, which could influence the total number of adjudications and convictions observed. However, we would only expect this to affect non-violent crimes because all the expert interviewees confirmed that the diversion program was never used with violent crimes.³⁴

An additional policy change, according to a Southern Poverty Law Center (SPLC) report in 2016, involves trying juveniles as adults. This was relatively rare before 2009, but the numbers increased under DA Cannizzaro, making New Orleans an outlier compared to the rest of Louisiana. SPLC (2016) quoted an unpublished report by the Louisiana Center for Children’s Rights, the juvenile public defender in New Orleans, that from 2011 to 2014, 83 percent of eligible juveniles (aged 15-16) were tried in adult court. There are two ways this might affect our results. First, this practice may have increased the youth conviction rate. To quote the report:

“Although 87 percent of New Orleans’ transferred children [juveniles who are transferred to the adult system] are convicted of some offense in criminal court, this statistic is misleading without context. Ninety-six percent of these convictions come from guilty pleas. Young people have been shown overwhelmingly to accept plea deals, even when they are innocent, to avoid the risk of trial and longer adult sentences. Seventy-five percent of transferred youths who plead guilty plead to lesser offenses – most of which would not have been transferrable – including nonviolent offenses and even misdemeanors.”

³⁴ Such programs also existed in other Louisiana districts, being entirely up to the discretion of the corresponding DAs, but we did not find any sign of significant changes for these districts in the public records during our sample period.

In other words, the transfer to adult criminal court led to longer potential sentences, which increased the odds that youth offenders would plead down (often to offenses that would not have been eligible for transfer).³⁵ This could increase the youth conviction rate relative to the adult rate. Other changes in the approach to plea deals could also affect the conviction rate and crime categories, but we saw no other evidence of this or other ways in which this would disproportionately affect juveniles.

Another way this practice might have affected youth conviction rate is through changing their inclination to commit crimes in the future. SPLC (2016) noted that juvenile courts also provide rehabilitative services, education, and psychological treatment that adult criminal courts in New Orleans do not. To the extent these are effective, the transfer of cases to criminal courts could reduce access to these services and increase the odds of future crime.

For prosecutions, therefore, the factor that most likely affects our triple-difference design is the practice of trying juveniles as adults that had become prevalent under the tenure of DA Cannizzaro (2008-2020). As this practice may have increased the conviction rate for juveniles, but not adults, and the change only occurred in New Orleans, our estimates for the conviction ratio are likely biased towards finding an increase in crime instead of a reduction, especially for violent crimes.

B.III. Changes in Detention

Detention may affect convictions in two ways. First, pretrial detention has been shown to increase the likelihood of contemporaneous convictions through increasing guilty pleas (Dobbie,

³⁵ It might also affect the categories in which crimes are placed since the entire process of pleas affects the crime that shows up in the conviction data. The data show the crime that the accused are convicted of, which might differ from or omit aspects of the crime committed.

Goldin and Yang, 2018). Second, changes in the detention and treatment of system-involved youths in our panel between 2005-2012 could have changed their recidivism rates between 2012-2018.

In terms of pretrial detention, the Juvenile Detention Alternatives Initiative (JDAI) was implemented in New Orleans and four other Louisiana parishes (Caddo, Calcasieu, East Baton Rouge, and Jefferson). Under JDAI, pretrial risk assessments are conducted for arrested youths, which allow those determined to be low-risk to remain in the community instead of being detained while awaiting trial. This might have reduced youth conviction rates in the aforementioned parishes, including New Orleans.

The Louisiana Legislature passed Act 1225 in 2003, a comprehensive state-wide juvenile reform package. Although many components of the law were never realized, one interviewee believed it caused a shift in philosophy for those involved in juvenile justice as there was more awareness of the poor conditions at the overcrowded state secure facilities. Reduced reliance on these facilities, which hold convicted youths for the long term, was the first change this expert discussed. She further stated that there were no marked differences in how the policy rolled out before and after Katrina. Neither were the changes specific to New Orleans. We are aware of other improvements in juvenile detention post-Katrina, such as the establishment of the new Juvenile Justice Intervention Center and Travis Hill School within it (as evidenced in our search terms). However, our research uncovered that these changes came after 2012 and thus did not benefit the individuals in our analyses.

Thus, combined with the discussion of policing and prosecution, this suggests that the two main threats to identification are trying youth as adults and the pretrial risk assessment program. While the former works in the opposite direction of the effects we found and should not lessen the

confidence in our results, the latter leads us to be more cautious in interpreting that all of the crime-reduction effect was due to the school reforms.