Examining the Relationship between School Closures and Crime Over the Pandemic

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Abstract

Over the course of the COVID-19 pandemic, numerous anecdotal reports of a youth-led crime wave entered the national discourse. Among other things, media outlets pointed to school closures and virtual education as potential explanations for the purported rise in juvenile crime. This paper seeks to investigate the statistical validity of these claims by examining the association between school closures and crime across the United States. To do this, I utilize incident-level crime data from the FBI's National Incident-Based Reporting System as well as smartphone data from Safegraph to act as a proxy for school attendance. I construct a balanced panel dataset at the county-month-year level over the course of 2020-2021 that tracks a wide array of crime metrics differentiating by type of crime, location, and offender characteristics. By leveraging the variation in school closing and reopening timing across counties nationwide, I am able to conduct a generalized differences-in-differences regression with a border-county pair design. My results suggest that there is no statistically significant association between the extent of school closures and crime over the pandemic, except for crimes committed by youth and motor vehicle thefts. This paper is significant in setting the record straight against the narrative of a pandemic-induced youth crime wave and seeks to quantitatively inform policymakers about the cost-benefit utility of school closure policies as it relates to crime going forward.

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1 Introduction

Beginning in March 2020, many state and local governments across the U.S. implemented a variety of lockdown policies designed to curb the spread of the coronavirus. Among other things, these policies initiated temporary school closures and ushered in a nation-wide transition towards distanced learning. Due to the decentralized nature of the U.S. government's response to the pandemic, the decision of when to reopen schools was largely left in the hands of individual counties (Marshall & Dorsey, 2020). Consequently, as the fall 2020 semester began, the method by which different localities conducted education (i.e. virtual, hybrid, or in-person) began to vary extensively across the nation (Parolin & Lee, 2021).

Pandemic-induced school closures were controversial for a wide array of reasons. First and foremost, cultural beliefs and attitudinal disagreements about the risks of the virus contributed to differences in policy (Lehrer-Small, 2021). Moreover, concerns about potential learning losses, the mental health toll on students, and the overall economic consequences of school closures also sparked controversy (Engzell et al., 2021; Lancker & Parolin, 2020; Hanushek & Woessmann, 2020). In the media sphere, one particularly powerful criticism was that the lack of in-person schooling contributed to an increase in juvenile crime. These criticisms typically manifested via news articles and reports documenting a rise in carjackings and violent crime by school-aged children in certain cities nationwide (Westwood, 2022; Nickeas Krishnakumar, 2022; Corley, 2021; Pagones, 2021). The reasoning behind such claims generally followed the sentiment behind the expression: "Idle minds are the devil's playground." In other words, without in-person schooling to serve the most basic function of giving teenagers something to do during the day, juveniles—especially those from low-income families whose parents continued to work over pandemic and therefore had less capacity for supervision—resorted to illegal activity.

Indeed, many city officials speculated that the primary motivation behind the rise in carjackings boiled down to simple thrill or the act of committing a crime being "a game" itself (Robertson, 2022). This notion supports the idea that boredom arising from the lack of in-person classes caused an increase in delinquent behavior rather than any type of economic strain imposed by the pandemic. Putting aside motivation, it is also plausible that the practice of virtual schooling itself reduced the capacity of school officials (i.e. teachers, administrators, and school resource officers) to serve as capable guardians and intervene against illicit decision-making.

With this in mind, the purpose of this paper is to investigate whether an association between pandemic-induced school closures and crime exists. To do this, I construct a balanced panel dataset matching the extent of school closures with a variety of crime metrics at the county-level for any given month throughout 2020 and 2021. The two main control variables I include are the COVID-19 case rate and a measurement of overall foot traffic within each county. My initial mode of estimation exploits the variation in school closing and reopening timing between different regions to obtain results through a generalized differences-in-differences regression. To better account for spatial heterogeneity in crime trends, however, I also estimate differential treatment effects for all applicable contiguous border county pairs present in my dataset.

My results from both methods suggest that the extent of school closures, as tracked by the percentage of schools closed within a county, exhibits no statistically significant correlation with aggregate crime levels. When breaking down this analysis to specific categories of crime (i.e. assaults, vandalism, theft, etc), the extent of school closures remained ineffectual except for a strong association with an increase in motor vehicle thefts. However, in an attempt to better isolate the effect on youth crimes, I also estimated a regression tracking offenses only committed by individuals 18 and under. In this regression, the extent of school closures were unanimously associated with a reduction in crime in all categories except motor vehicle theft. Rather than remaining positive and significant, though, the coefficient for motor vehicle thefts by youths instead became indistinguishable from zero. It is important to note though that under-reporting of offender age characteristics in my data is cause for legitimate concern regarding the credibility of this final result.

These findings are important because they reject the narrative of a youth-led crime wave during the pandemic on a nationwide scale. Following the slew of media reports detailing the rise in carjackings across cities early on during the pandemic, numerous state governments and even Congress entertained calls to scale back youth-justice reforms and institute punitive counter-measures on a broader scale (Altimari, 2022; "Senate committee passes", 2022; "Federal Support", 2022). Although the merit of such proposals should rely on the specific circumstances of the region at hand, the lack of conclusive evidence that school closures contributed to a significant rise in any type of crime, including motor vehicle thefts, ought to be considered in future policy making. Outside the realm of legislation, this paper is also paramount in combating any generalized perceptions propagated by the media amongst the public over the pandemic that unjustly criminalize young juvenile minorities.

2 Literature Review

2.1 Crime Over the Pandemic

Since the onset of COVID-19, researchers and media outlets alike have expressed a keen interest in the pandemic's effect on crime patterns. Indeed, due to the extensive variation in the timing, scale, and nature of different regional responses to the virus, the pandemic presents an unprecedented quasi-random natural experiment for researchers to test various criminological theories (Stickle & Felson, 2020). These hypotheses can often conflict with each other, which is why theoretically ambiguous situations like the COVID-19 lockdowns are particularly enlightening to study. As an example, whereas opportunity theory predicts that lower foot-traffic and social interaction resulting from shutdowns would reduce the context (and thereby the propensity) for crime, strain theory posits that the financial stress imposed by pandemic-induced economic hardships would contribute towards an entirely opposite effect (UNODC, 2020).

Therefore, it is unsurprising that the results stemming from the academic literature on crime during the pandemic vary widely based on the offense type, location, and time period under investigation. Initial evidence from one of the first large-scale studies on this topic reported no consistent patterns as to the direction of crime during the pandemic's earliest stages (Ashby, 2020). Using data tracking six major crime categories from over a dozen U.S. cities, Ashby calculated the difference between each metric's actual vs. expected frequency based on previous years' data and found no consistent trends across each location. After the onset of shelter-in-place (SIP) orders, however, crime numbers generally decreased (Boman & Gallupe, 2020). In an analysis of San Francisco and Oakland during the immediate aftermath of SIP issuance in California, Malpede and Shayegh (2022) estimated a 43% and 50% drop in the average daily crime rate, respectively. They add that this reduction in criminal activity was only temporarily significant, as crime gradually returned to just below historic levels within a month of SIP orders. When looking at the effects of COVID-19 response measures on specific categories of crime, a separate study using data from Los Angeles identified significant decreases in offenses such robbery (-24%), shoplifting (-14%), theft (-21%), and battery (-11%). By contrast, estimates for burglary, domestic violence, stolen vehicles, and homicide remained statistically unchanged (Campedelli et al, 2020).

In certain categories, lockdowns may have actually increased crime. Due to the lack of national crime data during the pandemic, most criminological studies to date have relied on publicly available police databases—the majority of which stem from agencies based in large cities (Boman Gallupe, 2020). Unfortunately, this source of data is particularly vulnerable to under/non-reporting of crime, especially for incidents related to domestic violence (MacDonald, 2022). With this in mind, in an event-study analysis of 14 large US cities during March through May of 2020, Leslie and Wilson (2020) estimated a statistically significant 7.5% increase in domestic violence service calls. This increase strongly coincided with the timing of social distancing and stay-at-home orders, as the simultaneous rise in employment uncertainty and parental time spent at home likely caused a surge in domestic abuse.

As for the purported rise in juvenile crime during the pandemic, I could not find any studies that isolated a trend for youth in particular. This most likely stems from the lack of comprehensive crime data that includes offender age characteristics. The most quantitative piece of evidence I could find on this topic comes from the Chicago Tribune's Data Points column, in which the author utilizes data from victim statements (of which 70% identify an offender age range) in Chicago to identify an estimated 104% increase in the share of carjacking arrests by individuals younger than 18 from 2019 to 2020 (Ludwig, 2021). She elaborates that this surge in carjacking was concentrated in communities where internet access is most limited and school attendance is lowest.

2.2 School Closures and Crime

I was also unable to find any existing literature examining a relationship between pandemic-induced school closures and crime. This is in part due to the aforementioned lack of reliable data, but it also likely stems from the difficulties associated with isolating the impact of school closures from the effects of stay-in-place policies or lockdowns in general. That being said, I was able to find several school closures studies that were tangentially related to crime. For example, Cabrera-Hernandez and Padilla-Romo (2020) examines how COVID-19 school closures reduced the reporting of child maltreatment in Mexico City. Leveraging a difference-in-differences research design that uses non-school aged children as a synthetic control group, they find that child maltreatment reports decreased by between 21 and 30 percent after school shutdowns. Importantly, they assert that this is likely not due to an actual reduction in child maltreatment behavior; rather, it highlights the importance of schools (and more specifically, counselors and educators) as a crucial medium for recognition and intervention against child abuse. A different study conducted by Bacher-Hicks et al. (2021) assesses the impact of the COVID-19 pandemic and school closures on school bullying and cyberbullying. Using Google search data, they find that searches for both school bullying and cyberbullying (which they argue is a proxy for the occurrence of virtual bullying) decreased by 42 percent in areas where schooling remained fully remote. By contrast, search intensity for bullying only dropped by 19 percent in areas where they were given an in-person option.

The juxtaposition of these two results shows the potentially ambiguous effect that school closures might have on criminal behavior. On one hand, as in the Hernandez and Padilla-Romo study, in-person instruction may act as a positive force against "crime" if school officials (i.e. teachers, administrators, and school police officers) serve as de facto guardians capable of preventing abuse and the long-term deleterious effects stemming from poor circumstances at home. On the other hand, as in the Bacher-Hicks et al. paper, school closure may provide a more immediate disruption to established social patterns and could therefore act as a driving force in reducing crime in the short-run.

Ultimately, prevailing criminological theory seems to suggest that school closures would reduce crime overall. For example, according to routine activities theory, "offenders make choices about whether or not to commit a crime based on their access to a suitable target and the presence—or lack thereof—of capable guardianship that could potentially bring repercussions to the offender" (Cohen Felson, 1979). Consequently, closing schools and reducing the presence of students in schools may generate fewer potential victims for motivated offenders to target. This is the same rationale as to why reduced social interactions stemming from lockdowns decreases crime. Furthermore, closing schools could also remove access to peer groups which may subsequently eliminate much of the impetus behind deviant behavior from youth in the first place (Osgood et al., 1996). This is substantiated by the findings of Gottfredson et al. (2001), which identified a pattern in which crime would frequently spike after school let out as youthful offenders and gangs could easily congregate.

With all this in mind, I hope to contribute to the existing literature on this topic in two main ways. First, by using school closures as a relatively novel determinant for crime during the pandemic, I hope to provide greater insight as to the existence of any juvenile-specific trends in crime. Of course, this notion presupposes that school closures disproportionately impacted youth crime compared to other sects of the population. Second, since my paper utilizes data from locations other than cities, I seek to obtain a broader idea as to how crime during the pandemic changed in a more diverse group of settings. Specifically, I am especially interested in showing whether the reported rise in carjackings in certain cities like Chicago and Washington D.C. also extended to rural areas or represented any type of nationwide trend.

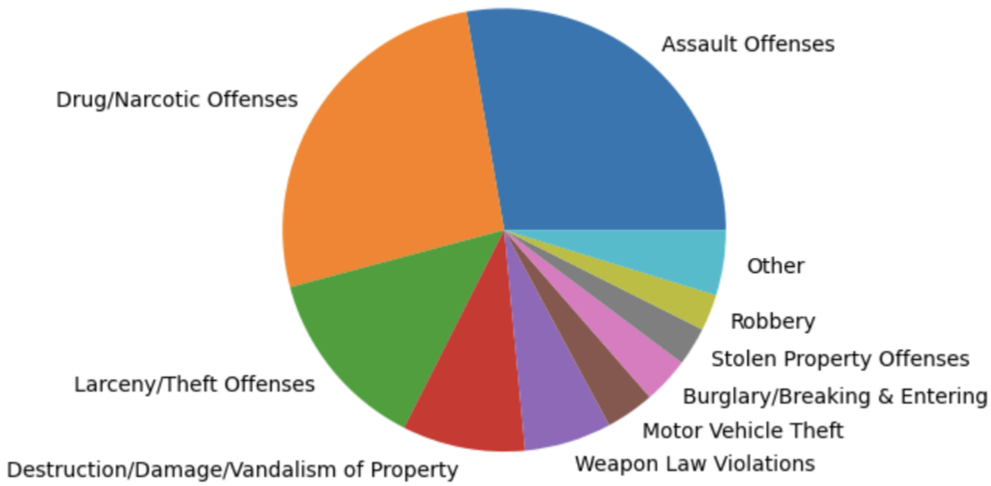
3 Data

3.1 Crime Data

To track crime over the course of the pandemic, I use data from the FBI's National Incident-Based Reporting System (NIBRS). The NIBRS is a comprehensive database that captures incident-level data on each crime reported by police agencies across the United States. In contrast to the FBI's previous data collection protocol, the Summary Reporting System (SRS)—which merely collects monthly aggregated tallies of certain crimes—the NIBRS follows a strict set of standards designed to improve the overall quality of crime data by including numerous details (i.e. separate offenses, location, date, and offender characterstics) associated with each crime incident.

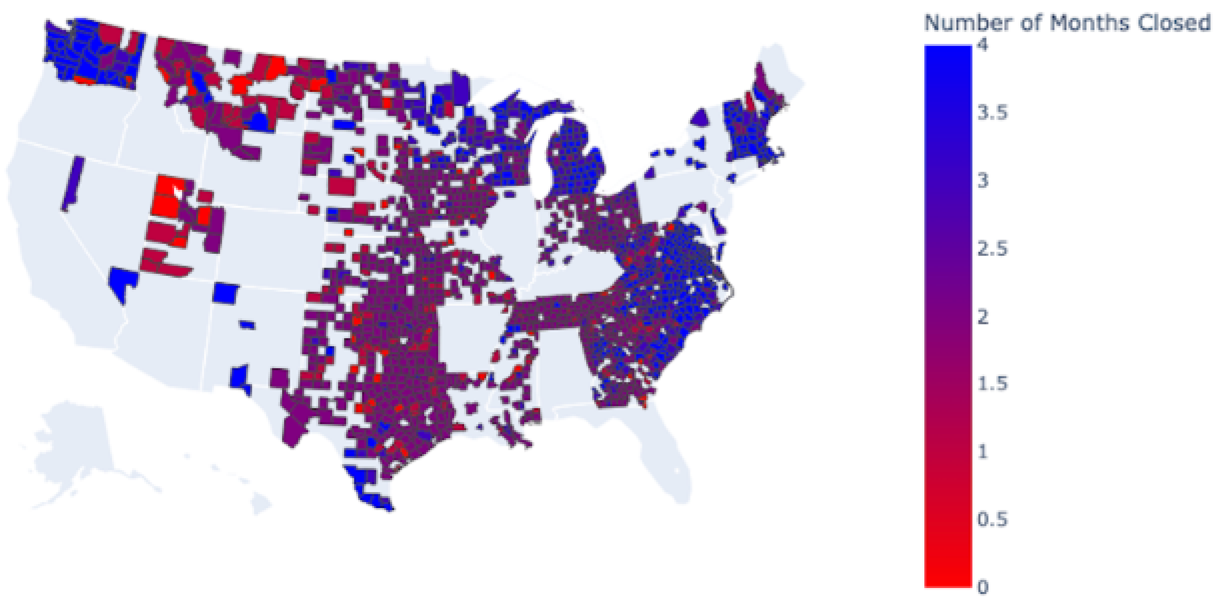
Although the FBI officially initiated the transition from the SRS towards the NIBRS in 2015 and imposed a strict deadline to switch by January 1st, 2021, agency adherence to the updated protocol was limited due its relatively higher administrative burden (Li, 2020). As a result, according to the FBI's NIBRS participation data, only about 54% of agencies nationwide reported data using NIBRS in 2020 and 2021 ("NIBRS Participation Rates", 2022). This roughly equates to a 66% coverage rate of the U.S. population by NIBRS-certified agencies over the course of the pandemic (see Figure 2 for a geographic representation of what parts of the U.S. the data covers). To make matters worse, only a small subset (∼10%) of the data actually includes offender characteristics, and only 3.5% of the data pertains to offenses committed by individuals 18 and under (this corresponds to roughly 500,000 observations). The reason why this data is uniquely omitted is unclear to me, although there does not appear to be any systematic bias towards reporting of any one type of youth crime (see Figure 1). Still, this relatively low sample sample size suggests that my results in Tables 3 and 6 may lack statistical power, but given that the distribution of reported youth crimes still varies heavily in both the types and locations of crimes tracked, meaningful results can still be extracted from them.

Figure 1: Breakdown of Crimes Committed By Youth in NIBRS Data



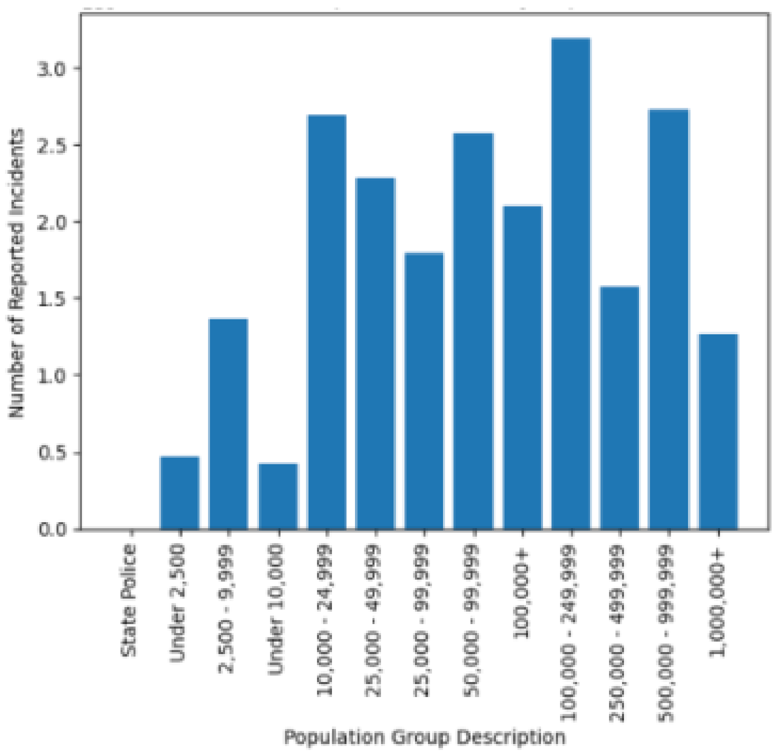
Regarding the gap in geographical coverage, this also poses a potential challenge to the credibility of my results. For example, if adherence to the NIBRS is correlated with administrative capacity, my data may be systematically biased to disproportionately reflect crime patterns reported by smaller agencies that track relatively less crime. I address this concern in two ways. First, by constructing a balanced panel dataset, I increase the likelihood that my results are at least internally valid by comparing outcomes between the same cross-sectional units before and after treatment over a consistent period of time. Specifically, my dataset includes a full panel of observations from 1,704 out of 3,143 total counties in 2020 and 2021 after cleaning. Second, as evidenced in Figure 2, I show that the spread of observations from the NIBRS data is somewhat normally distributed by the population size of the municipality that the reporting agency is located in. Admittedly, an ideal sample would certainly exhibit a higher right-end tail in this figure given that urban areas generally constitute the majority of crime in America. This discrepancy is due to the fact that many agencies in large cities did not submit data to the FBI in 2021 (Li, 2020).[1] That being said, since large cities are usually idiosyncratic in regards to national crime trends, the lack of their data may actually strengthen the external validity of my results. Regardless, the fact that well over half the U.S. population is covered in my study is sufficient for at least a cautionary analysis.

Figure 2: Heatmap of Counties Included in the NIBRS Dataset



[1]i.e. Los Angeles, New York City, and Chicago all did not submit data to the FBI in 2021

Figure 3: Distribution of Reported Crimes by Population Size



3.2 School Closure Data

To track school closure timing for different counties over the pandemic, I use the U.S. School Closure and Distance Learning Database. This dataset, constructed by researchers Zachary Parolin and Emma K. Lee at Columbia University, relies on aggregated, anonymized cell phone data from SafeGraph to act as a proxy for school attendance (Parolin & Lee, 2020). By using high frequency changes in the presence of smartphones on school properties, Parolin and Lee are able to estimate the timing of school closures with a high-degree of accuracy compared to administrative datasets that qualitatively track a school districts' method of instruction (Hansen et al, 2022).[2] To account for seasonality in foot traffic patterns, the School Closure and Distance Learning Database only contains metrics corresponding to a

[2]Hansen et. al (2022) cross-validated the accuracy of this dataset with the COVID-19 School Data Hub—a qualitative dataset tracking mode of instruction as reported by local school district administrations (Halloran et al, 2022).

year-over-year change in in-person visits. Unfortunately, this methodology often resulted in schools during the summer months (i.e. June, July, and August) being labeled as "open." This is because even during the pandemic the change in year-over-year foot traffic did not change too heavily during this time period. This presented a potential challenge to estimation since considering the summer months "open" in my analysis obscured the true effects of pandemic-induced school closures given that youth crime naturally tends to spike during the summer. To address this problem I decided to drop all summer months from my analysis.

All my regressions utilize the same variable as a means of tracking school closures. This measurement is a continuous variable that tracks the percentage of individual schools within a county that are considered closed. A school is considered closed if the mean year-over-year decline in visits to that school in a given month exceeds 50 %. Since there are often multiple school districts in a county, this continuous variable reflects a relatively granular estimate of the extent to which schools are closed in a county. Due to the largely coincident timing of school closures with lockdowns, however, one may argue that this independent variable likely suffers from endogeneity with the unobserved influence of stay-in-place policies on crime. To address this, I include a control variable tracking the overall foot traffic within each county at a given month and year.

3.3 Covariate Data

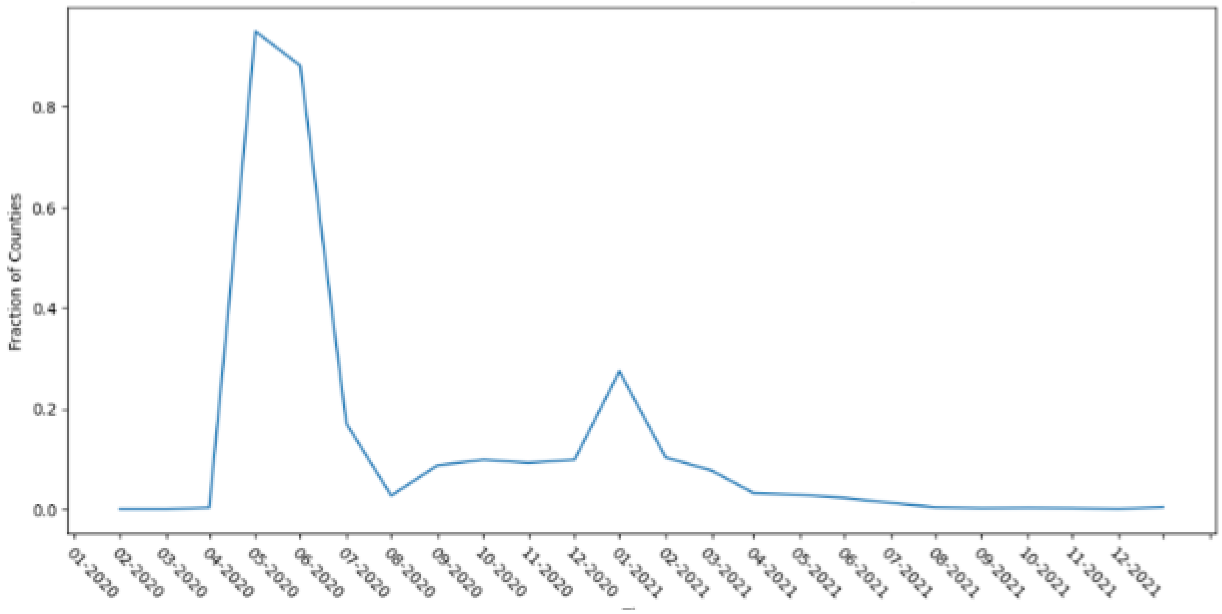
I include two main control variables in my regressions. The first is a time-variant measurement of the number of new COVID-19 cases in a county for each month. I collect this data from the New York Times' COVID-19 Tracker (New York Times, 2022). Under the assumptions that (1) an increase in the number of COVID cases in a month reduces people's willingness to leave their house and socially interact (thereby reducing crime per the aforementioned "opportunity theory") and that (2) school administrations use the local COVID case count as part of their decision of whether or not to open/close schools, then controlling for this variable prevents potential omitted variable bias in my primary estimates.

Regarding assumption (1), a reduction in foot traffic due to high COVID case rates inherently relies on individuals exhibiting a high degree of risk-aversion to contracting the virus. However, varying political beliefs across different geographical regions may preclude this from being the case. As a result, my second control variable attempts to control for different cultural attitudes towards the pandemic by tracking the overall foot traffic in each county over time. Conveniently, this variable also serves as a control for the true effects of shelter-in-place policies, since more people on the street is a heavy indicator of crimes that require social interaction (i.e. assaults). To obtain this data, I utilized the same source of SafeGraph data that was used to construct the measurement of school closures. However, this time I included all visits to any various points-of-interest (POI) within each county. For better interpretability, the actual variable I used was a scaled measurement of the estimated total number of visits to any locations in a county at a given time.

4 Methodology

Due to the largely coincident timing of school closures nationwide in late March to early April of 2020, there is relatively little variation in the school closure data during the early stages of the pandemic. This makes estimation for the spring period difficult due to the low number of observations with a different treatment status in the same month. Beginning in September 2020, however, many state and local governments began adopting vastly different approaches to distanced learning (Parolin and Lee, 2022). As a result, school closures became much more unevenly distributed in the fall (see Figure 3).

Figure 3: Fraction of Counties with a Closed School District by Month



4.1 Generalized Differences-in-Differences

Knowing this, my regressions primarily exploit the difference in reopening times to estimate the effect of school closures on crime. My first method of analysis estimates a treatment effect through a generalized differences-in-differences regression. Unlike a traditional differences-in-differences (DiD) design, where treatment typically occurs all at once and is permanent, a generalized DiD model permits additional covariates that (1) control for effects specific to each cross-sectional unit and (2) allows for the change in outcome to depend on time (Ng, 2020). On their own, these changes effectively describe a two-way county and time fixed effects regression; a generalized DiD distinguishes itself through its inclusion of an interaction term that allows for different treatment timing across units as well.

Figure 4: Generalized Differences-in-Differences Equation

$$Y_{i,t} = \alpha + \delta_i + \pi_t + \rho D_{i,t} + \beta X_{i,t} + \theta Z_{i,t} + \epsilon_{i,t}$$

Figure 4 displays the equation for my generalized DiD. $Y_{i,t}$ is the outcome of interest and represents a crime metric for county i at time t, where t corresponds to a specific month in either 2020 or 2021. The variable δ_i captures individual county-fixed effects, and π_t represents time-fixed effects. $D_{i,t}$ is the interaction term and measures the extent of school closures in the post-treatment period. $X_{i,t}$ and $Z_{i,t}$ are control variables for the number of new COVID cases and the overall foot traffic in a county respectively, and $\epsilon_{i,t}$ is the residual error associated with each observation on average. Interpretation for the primary coefficient of interest, ρ estimates the change in crime associated with a one percentage point increase in the number of schools closed relative to when it's open in comparison to other counties that may close in either the past or future.

4.2 Border County Pair Regression

The downside of the generalized approach is that it necessarily compares counties subject to entirely different crime trends. While controlling for county-fixed effects attempts to account for any unobserved influences on crime that vary across countries (i.e. geographic trends, population size, racial composition, etc), it is an inherently imprecise method of doing so. To address this, I extend my analysis to examine the effect of school closures only in counties that border each other. In theory, this border-county-pair (BCP) matching process effectively eliminates the issue of spatial heterogeneity since contiguous counties are likely to experience similar exogenous shocks as it relates to crime. Importantly, the BCP model is still able to exploit differences in exposure to the treatment variable since school closures are determined by local governments whose jurisdictions extend only to border lines.

I begin this process by restructuring the data in the following way: for each month t, I include two observations corresponding to each border county pair p—one for each county c of the pair. Consequently, the total number of observations in my sample actually increases, since a given county c can appear in the data k times (for each month t) if it is adjacent to k other counties included in the dataset.

Figure 5: Border County Pair Equation

$$Y_{x,y,t} = \alpha + \delta_x + \delta_y + \pi_t + \rho D_{x,y} + \beta X_{x,y,t} + \theta Z_{x,y,t} + e_{x,y,t}$$

Figure 5 displays the equation for the BCP regression. Practically speaking, the model is the same as the generalized DiD regression with one additional variable controlling for pair-period effects (i.e. the effect on crime of being in a specific border-county-pair at a given time). Due to autocorrelation issues, however, I have rearranged the model as a first-differenced equation to more accurately reflect what I input into STATA. With this in mind, $Y_{x,y,t}$ represents the first-difference of a given crime metric between counties x and y at time t. The variables δ_x and δ_y capture any lingering individual county-fixed effects. Similar to Figure 4, π_t represents time-fixed effects. $D_{x,y,t}$ captures the first-difference in exposure to school-closures in the post-treatment period. Lastly, $X_{x,y,t}$ and $Z_{x,y,t}$ represents the first-difference in the number of new COVID cases and the overall foot traffic. Importantly, the equation I have listed above does not need to include a variable capturing pair-period effects since it becomes canceled out due to the first-differenced nature of the regression.

5 Results

The results from the generalized DiD regressions are displayed in Tables 1 through 3 below, whereas the results from the BCP regression are displayed in Tables 4 through 6. I conduct regressions measuring changes in both aggregated (Tables 1 and 4) and individual crime categories (Tables 2, 3, 5, and 6). Tables 3 and 6 specifically correspond to the effects of school closures on crimes only committed by individuals 18 and under. The aggregated categories are all pre-defined by the FBI with one notable exception: "Juvenile Likely" is a custom concatenation of any incidents listed under assault, burglary/breaking & entering, destruction/damage/vandalism of property, drug/narcotic offenses, and larceny/theft. Needless to say, this is an imperfect measurement of crimes committed by juveniles—it is merely included to glean whether these crimes had a more significant change compared to all other crime incidents.

5.1 Generalized DiD Results

In Table 1, there is no significant association between the extent of school closures on any of the aggregated crime categories. The coefficients on the percentage of schools closed in a county for each dependent variable all exhibit a positive sign but are more or less indistinguishable from zero. This would support the notion that pandemic-induced school closures themselves are not a significant predictor of crime. In fact, both control variables were much more strongly correlated with any fluctuations in crime patterns compared to school closures. This explains the relatively high adjusted R-squared value in each of the regressions, as a large share (∼80%) of the variation in crime trends could be explained by the prevalence of COVID or the number of people on the street.

Regarding the COVID variable in particular, the positive association with crime is interesting as one might expect crime to decrease as COVID spread due to fear of in-person interaction. However, the dramatic nature of this result lessens upon realizing that the magnitude of the COVID variable coefficients are simply are not that high when put in perspective. For example, the results indicate that every ten thousand additional COVID cases in a county at a given month would only be associated with only about 2.5 additional crime incidents on average. The magnitude of the foot traffic variable was by far the more significant predictor of crime, which makes sense given the prior literature on the effects of stay-in-place orders on crime over the pandemic.

The results of the generalized DiD for specific categories of crime (Table 2) largely fall in line with one would expect from Table 1 with two notable exceptions: crimes by youth and motor vehicle thefts. Regarding the former, the extent of school closures was associated with a strong reduction in the number of crimes committed by youths. Comparing a county with a 0% closure rate to a county with a 100% closure rate, the county with schools closed would exhibit about six fewer youth crimes on average. Realistically, this number could also be much higher due to the severe under-reporting of youth age characteristics in the NIBRS dataset. Ultimately, this result suggests that school closures did in fact cause a reduction in crime amongst school-aged children in nearly all dimensions.

As for motor vehicle thefts, this was the only category of crime that exhibited any positive association with the extent of school closures. This matches the narrative spread by the media that carjackings increased significantly during periods of school closures during the pandemic. Interestingly, when looking at the equivalent regression in Table 3, which tracks crimes committed by youth exclusively, the association between school closures and carjackings disappears. Of course, this is mostly likely attributable to the large reduction in the dependent variable mean (i.e. there simply were not that many carjackings committed by youths recorded), but it does suggest that the overall increase in motor vehicle thefts may have been falsely attributed to school-aged children. It is plausible that the increase in motor vehicle thefts could have either been committed by youths and were simply not recorded or that they were committed by individuals over 18 (i.e. adolescents between 18 and 24) and children were blamed.

5.2 Border County Pair Regression Results

Estimates from the border county pair regressions were nearly identical to the generalized differences-in-differences model. The extent of school closures still remained statistically unassociated with aggregate crime categories (Table 4), yet there was a strong decrease seen for crimes by youth in particular (Table 5). Moreover, the comparison of school closures amongst contiguous border county pairs also found that motor vehicle thefts increased for counties that exhibited a greater degree of closure. For example, a school with a 0% school closure rate had about 5.3 fewer motor vehicle thefts on average compared to a school with a 100% school closure rate. However, as in the generalized DiD case, this effect disappeared when only considering crimes committed by youth exclusively.

The similarity in results between the generalized and border county pair models provides strong validation regarding the accuracy of my results. The only main notable difference between both regressions was that the significance for both the COVID and foot traffic variables declined for the border county pair results. This suggests that although the difference in these control variables values were not that large between adjacent counties, it ultimately did not matter in terms of how school closures were associated with crime trends. If anything, this bolsters the legitimacy of the generalized differences-in-differences results, as it would appear that the exogenous alternative determinants of crime were largely captured through the use of a county fixed effects term.

6 Conclusion

Upon examination of the relationship between pandemic-induced school closures and crime, I find that the extent of school closures by themselves were not significantly associated with aggregate crime trends. However, counties with a greater degree of school closure did show a significant decline in crimes committed by individuals 18 and under in nearly all crime categories. The only category of crime that increased during periods of school closures was motor vehicle thefts. This result appears to support the media reports during the pandemic that identified a youth crime wave for carjackings. However, my results do not suggest that the uptick in carjackings can be primarily attributable to school-aged children, although further analysis using data that better tracks offender age characteristics is merited. These results are important in combatting the narrative of youth criminalization propagated by the media that suggests that school closures disproportionately increased youth propensity for juvenile delinquency.

Generalized Differences-in-Differences Tables

Table 1: Generalized DiD for Percentage of Schools Closed in County on Aggregated Crimes Without Summer Months

	(1) # of Incidents b/se	(2) Against Persons b/se	(3) Against Property b/se	(4) Against Society b/se	(5) Juvenile Likely b/se	(6) Urban Area b/se
% of Schools Closed in County	0.294	0.063	0.241	0.023	0.179	0.228
	(0.184)	(0.053)	(0.135)	(0.025)	(0.159)	(0.176)
New Cases / 1000	0.256***	0.060***	0.193***	0.035***	0.221***	0.178***
	(0.023)	(0.007)	(0.017)	(0.003)	(0.020)	(0.022)
State-Scaled Visits to POI / 1000	0.076***	0.025***	0.036***	0.025***	0.067***	0.070***
	(0.011)	(0.003)	(0.008)	(0.001)	(0.009)	(0.010)
N	30426	30426	30426	30426	30426	30426
Adj. R-Sq.	0.82	0.80	0.82	0.85	0.82	0.74
DV Mean	351.16	93.98	251.91	66.05	325.04	207.24
County-Fixed Effects	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 2: Generalized DiD for Percentage of Schools Closed in County on Specific Crimes Without Summer Months

	(1) Crimes by Youth b/se	(2) Assault b/se	(3) Burglary b/se	(4) Vandalism b/se	(5) Narcotics b/se	(6) Larceny/Theft b/se	(7) Motor Vehicle Theft b/se
% of Schools Closed in County	-0.063***	0.056	0.019	0.038	-0.010	0.086	0.058***
	(0.005)	(0.050)	(0.019)	(0.023)	(0.019)	(0.064)	(0.016)
New Cases / 1000	-0.004***	0.055***	0.019***	0.033***	0.020***	0.100***	0.026***
	(0.001)	(0.006)	(0.002)	(0.003)	(0.002)	(0.008)	(0.002)
State-Scaled Visits to POI / 1000	0.003***	0.022***	0.002	0.005***	0.019***	0.020***	0.003**
	(0.000)	(0.003)	(0.001)	(0.001)	(0.001)	(0.004)	(0.001)
N	30426	30426	30426	30426	30426	30426	30426
Adj. R-Sq.	0.84	0.80	0.79	0.85	0.85	0.82	0.79
DV Mean	12.81	86.15	28.81	47.00	53.15	118.79	19.93
County-Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3: Generalized DiD for Percentage of Schools Closed in County on Specific Crimes by Individuals 18 and Under

	(1) Assault b/se	(2) Burglary b/se	(3) Vandalism b/se	(4) Narcotics b/se	(5) Larceny/Theft b/se	(6) Motor Vehicle Theft b/se
% of Schools Closed in County	-0.020***	-0.004***	-0.005***	-0.019***	-0.011***	-0.001*
	(0.002)	(0.001)	(0.001)	(0.002)	(0.001)	(0.000)
New Cases / 1000	0.001***	-0.000***	0.000	-0.003***	-0.002***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
State-Scaled Visits to POI / 1000	0.001***	-0.000*	0.000	0.001***	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	30426	30426	30426	30426	30426	30426
Adj. R-Sq.	0.77	0.58	0.58	0.78	0.70	0.60
DV Mean	3.61	0.74	1.11	3.48	2.05	0.43
County-Fixed Effects	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Border County Pair Design Tables

Table 4: Balanced BCP Regression for Percentage of Schools Closed in County on Aggregated Crimes Without Summer Months

	(1) # of Incidents b/se	(2) Against Persons b/se	(3) Against Property b/se	(4) Against Society b/se	(5) Juvenile Likely b/se	(6) Urban Area b/se
% of Schools Closed in County	0.387*	0.074	0.306*	0.026	0.211	0.190
	(0.187)	(0.055)	(0.131)	(0.033)	(0.163)	(0.162)
New Cases / 1000	0.247**	0.055*	0.183**	0.035**	0.216**	0.160*
	(0.090)	(0.023)	(0.065)	(0.013)	(0.078)	(0.075)
State-Scaled Visits to POI / 1000	0.019	0.007	0.002	0.012**	0.015	0.031
	(0.026)	(0.007)	(0.019)	(0.004)	(0.022)	(0.022)
N	68516	68516	68516	68516	68516	68516
Adj. R-Sq.	0.78	0.77	0.79	0.82	0.79	0.72
County-Fixed Effects	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 5: BCP Regression for Percentage of Schools Closed in County on Specific Crimes Without Summer Months

	(1) Crimes by Youth b/se	(2) Assault b/se	(3) Burglary b/se	(4) Vandalism b/se	(5) Narcotics b/se	(6) Larceny/Theft b/se	(7) Vehicle Theft b/se
% of Schools Closed in County	-0.058***	0.066	0.008	0.040	-0.006	0.103	0.053***
	(0.008)	(0.052)	(0.017)	(0.023)	(0.026)	(0.063)	(0.015)
New Cases / 1000	-0.003	0.051*	0.019*	0.030**	0.022*	0.097**	0.022***
	(0.001)	(0.022)	(0.008)	(0.010)	(0.010)	(0.033)	(0.006)
State-Scaled Visits to POI / 1000	0.001*	0.005	-0.001	-0.002	0.009**	0.003	-0.001
	(0.000)	(0.006)	(0.002)	(0.004)	(0.003)	(0.009)	(0.002)
N	68516	68516	68516	68516	68516	68516	68516
Adj. R-Sq.	0.82	0.76	0.76	0.81	0.82	0.78	0.74
County-Fixed Effects	Y	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 6: BCP Regression for Percentage of Schools Closed on Specific Crimes Committed by Individuals 18 and Under

	(1) Assault b/se	(2) Burglary b/se	(3) Vandalism b/se	(4) Narcotics b/se	(5) Larceny/Theft b/se	(6) Motor Vehicle Theft b/se
% of Schools Closed in County	-0.020***	-0.003***	-0.002**	-0.016***	-0.011***	-0.001*
	(0.003)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)
New Cases / 1000	0.001	-0.000	0.000	-0.002***	-0.002***	-0.000*
	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)
State-Scaled Visits to POI / 1000	0.000*	-0.000*	0.000	0.000*	-0.000*	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
N	68516	68516	68516	68516	68516	68516
Adj. R-Sq.	0.74	0.57	0.60	0.75	0.68	0.60
County-Fixed Effects	Y	Y	Y	Y	Y	Y
Time-Fixed Effects	Y	Y	Y	Y	Y	Y

Standard Errors in Parentheses, * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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