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In-Person Schooling and Juvenile Violence

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Abstract[*]

While investments in schooling generate large private and external returns, negative peer interactions in school may generate substantial social costs. Using data from four national sources (Uniform Crime Reports, National Incident-Based Reporting System, National Crime Victimization Survey, National Electronic Injury Surveillance System) and a variety of identification strategies, this study comprehensively explores the effect of in-person schooling on contemporaneous juvenile violence. Using a proxy for in-person schooling generated from anonymized smartphone data and leveraging county-level variation in school calendars — including unique, large, localized changes to in-person instruction during the COVID-19 pandemic — we find that in-person schooling is associated with a 28 percent increase in juvenile violent crime. A null finding for young adults is consistent with a causal interpretation of this result. The effects are largest in larger schools and in jurisdictions with weaker anti-bullying policies, consistent with both concentration effects and a peer quality channel. Back-of-the-envelope calculations suggest that relative to closed K-12 schools, in-person schooling generates $233 million in monthly violent crime costs.

Keywords: schooling; in-person instruction; crime
JEL codes: K42; I21; I28

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"There are too many…schools, too many other everyday places, that have become killing fields, battlefields, here in America."

- President Joe Biden (2022)

1. Introduction

Teenagers spend 810 to 912 hours per academic year, or one-third of their waking hours, attending school (American Time Use Survey 2019). While investments in schooling yield important private and external economic benefits (Angrist and Krueger 1991; Lleras-Muney 2005; Lochner and Moretti 2004; Machin, Marie, Vujić 2011), in-person schooling may generate substantial contemporaneous social costs. Recent studies have documented that negative peer interactions in school may increase the risk of suicidal behaviors (Hansen, Sabia and Schaller 2023; Rees, Sabia, and Kumpas 2022)), substance use (Card and Giuliano 2013; Eisenberg, Golberstein and Whitlock 2014; Lundborg 2006), and school shootings (Sabia and Bass 2017; Klein 2012). Moreover, school policies designed to deter delinquent behaviors among students may themselves have unintended consequences that increase violence and create "pipelines to prison" (Bacher-Hicks, Billings and Deming 2019; Cuellar and Markowitz 2015; Owens 2017).

Violence at school is of grave concern to both parents and policymakers. Approximately one-third (34 percent) of parents express fears about school safety (Brenan 2019) and U.S. President Joe Biden has described school violence as a threat to the nation's fabric.[1] Twenty-two (22) percent of U.S. high school students reported being in a physical fight on school property in the prior month (CDC 2021), 13 percent reported carrying a gun, knife, or club, and 8 percent reported a weapons-related threat on school property.

In 2019, there were 525,300 (nonfatal) violent incidents at public schools (NCVS 2019), with nearly three-quarters (70 percent) of schools recording at least one violent act.[2] Twenty-four (24) percent of these incidents were serious violent offenses (NCVS 2019) — attempts to murder, rob, kill, rape, or assault another with dangerous (or deadly) weapon (Federal Bureau of Investigation 2022) — while the remainder were simple assaults (NCVS 2019).[3] During the 2019-2020 academic year, there were 116 school shootings in the U.S., the highest number recorded in two decades, a

[1] In a speech along these lines, delivered in May 2023, President Biden said, "Our kids should be focused on leading the world in math and science – not learning how to duck and cover."

[2] This translates to about 20.6 violent incidents per 1,000 students ages 12-18. This finding is consistent with the 2019-2020 Survey on School Survey on Crime and Safety, which reported 19 violent incidents recorded by U.S. public schools per 1,000 students during that academic year (NCES 2022).

[3] Approximately 32 percent of schools reported violent incidents to law enforcement (NCVS 2019).

period during which there were 1,188 school shooting-related casualties (NCES 2023). Together, there were 35,472 arrests for violent offenses involving juveniles in the U.S. (Federal Bureau of Investigation 2023), generating social costs in excess of $100 billion per year (CDC 2022).

While the causes of school violence — and the potential unintended consequences of school policies designed to combat such violence (Bacher-Hicks, Billings and Deming 2019; Cuellar and Markowitz 2015; Owens 2017) — encompass individual, family, peer, and neighborhood effects (Billings et al. 2019; Case and Katz 1991; Ludwig et al. 2001), the role of in-person schooling has generated increased attention from scholars (Akee et al. 2014; Jones and Karger 2023; Jacob and Lefgren 2003; Luallen 2006).

The effect of in-person schooling on juvenile violence is, a priori, difficult to sign. On the one hand, time spent in school may reduce violence through increased monitoring by faculty and staff. There may also be "incapacitation effects," whereby time spent in academic (or extracurricular) activities crowds out less purposeful peer interactions that generate violence (Akee et al 2014; Fischer and Argyle 2018; Jacob and Lefgren 2003; Luallen 2006).[4] In addition, there may be positive human capital effects of in-person schooling that reduce incentives for criminal behavior in both the short- and longer-runs (Anderson 2014; Lochner and Moretti 2004; Machin, Marie, Vujić 2011).

On the other hand, in-person schooling could have unintended consequences that increase juvenile violence. This may occur due to a "concentration effect," whereby the high density of peers in schools increases the number of student interactions and the likelihood of physical altercations (Akee, Halliday, and Kwak 2014; Jacob and Legren 2003; Luallen 2006).[5] In addition , the quality of peer interactions in school may increase violence (Billings and Phillips 2017; Billings, Deming, and Ross 2019; Sabia and Bass 2017). Approximately 22 percent of those ages 12-18 report having been bullied in school (National Center for Education Statistics 2021) and such victimization has linked to increased risk of physical altercations, weapons-related threats, and school shootings (Klein 2012; Sabia and Bass 2017).[6]

[4] Another notable "incapacitation effect" of K-12 in-person schooling is limiting interactions with young adult peers ages 18-24, who have the highest arrest rates for violent, property, and drug offenses (Federal Bureau of Investigation 2019).
[5] Relatedly, in-person schooling may lower the opportunity cost of committing violence and increase the potential for criminal socialization (Steinberg, Ukert, MacDonald 2019).
[6] In addition, bullying victimization has been found to be associated with poorer psychological health (Nikolaou 2022; Rees, Sabia, and Kumpas 2022; Liang et al. 2023) and substance use (Muratori and Sabia 2023; Nikolaou 2017), each of which has been linked to increased violence (Anderson, Crost, and Rees 2016; Bondurant, Lindo, and Swensen 2018; Carpenter and Dobkin 2015; Dave, Deza, and Horn 2018; Hansen and Waddell 2018).

Finally, school discipline policies may create pipelines to prison for teenagers and have the unintended effect of increasing violence. For instance, school suspension policies that segregate students with poorer quality peers may generate more anti-social behaviors, including violence (Bacher-Hicks, Billings and Deming 2019; Cuellar and Markowitz 2015; Owens 2017). Moreover, increases in students' in-school interactions with police — resulting from the deployment of law enforcement officers in junior high and high schools to handle routine disciplinary problems — may increase the likelihood of a future arrest.[7] These unintended effects are more pronounced for disadvantaged youth, including Black and Hispanic students (Bacher-Hicks, Billings and Deming 2019; Cuellar and Markowitz 2015).

This study comprehensively examines the effect of in-person schooling on juvenile violence. In doing so, we overcome the lack of consistent, nationwide administrative data on local school calendars by creating a novel, daily measure of likely physical presence on elementary and secondary school campuses.[8] We do this by using point-of-interest anonymized smartphone data from SafeGraph to measure the daily intensity of recorded cellphone "pings." We (1) document that the intensity of smartphone pings is a credible proxy for in-person instruction that captures localized (and temporal) variation in school calendars, (2) use this variation to capture unique, large shocks to in-person schooling that occurred prior to and during the COVID-19 pandemic, and (3) use non-school-related smartphone data on social mobility (as well as COVID-19 health and economic data) to disentangle the effects of pandemic-era school closures from other contemporaneous shocks related to lockdowns, economic downturns, and COVID-19-related health.

Using data from four national sources (Uniform Crime Reports, National Incident-Based Reporting System, National Crime Victimization Survey, National Electronic Injury Surveillance System), we document four key results. First, we find strong evidence of seasonality in juvenile violence over the pre-pandemic period of 2015-2019. Our results show that juvenile violence falls by 12 to 17 percent during the summer months (June, July and August) when elementary and secondary schools are generally not in session. This pattern of violence is not explained by macroeconomic conditions or weather patterns, and is not observed for young adults ages 19-24,

[7] This may be due to (1) police unduly targeting those identified as having prior disciplinary problems (Stolzenberg et al. 2021), (2) a negative behavioral effect of interacting with police (to settle a modest disciplinary matter) at a relatively young age (i.e., a labeling effect) (Lopes et al. 2011; Schur 1973; Wiley and Esbensen 2016).

[8] Recent work by Goldhaber et al. (2024) compares SafeGraph to other centralized databases on school closures and finds SafeGraph outperforms other attempts to scrape or collect school calendars when compared against administrative records from Michigan on actual days schools are open.

consistent with the hypothesis that the academic calendar affects violence committed by juveniles. Auxiliary analysis of crime victimization data (which includes unreported crime) from the National Crime Victimization Survey (NCVS) and of hospitalization data on injuries sustained from assaults from the National Electronic Injury Surveillance System (NEISS) suggest that our UCR- and NIBRS-based results are not driven by increases in the probability of detection or reporting, but rather by increases in violent behavior.

Second, we use anonymized smartphone data to measure cross-county variation in school calendars in the pre-pandemic period. We find that juvenile violence falls by 21 percent more in June (relative to January, when schools are in session) in counties where schools begin summer break in May relative to counties where summer break does not commence until mid-late June. We also find that the decline in juvenile violence is generally smaller in August for counties where school districts start the academic year in August. We find no evidence that kindergarten through 12th grade (K-12) school calendars are related to violent crime arrests involving young adults ages 19-24.

Third, we examine unique, large shocks to K-12 school calendars generated by the COVID-19 pandemic to estimate their effects on juvenile violence. We show that in March 2020, when there was a nationwide shutdown of K-12 schools, juvenile (but not young adult) violence fell by 10 percent in March, 22 percent in April, and 27 percent in May (relative to January 2020) and remained low throughout the summer and fall. This is in sharp contrast to the seasonal pattern of juvenile violence in March, April, and May in the pre-pandemic period and is inconsistent with what is observed in those months for young adult violent crime arrests during the pandemic.

Furthermore, we draw data from 2019-2021 and exploit county and temporal (day-by-month-by-year) variation in K-12 school foot traffic in a generalized difference-in-difference-in-differences framework to estimate the effect of in-person instruction on juvenile vs. young adult violent crime. This approach relies on differences in school closing and reopening policies in the Fall of 2020, Spring of 2021, and Fall of 2021 to identify treatment effects. Controlling for local COVID-19 economic shocks (restaurant and bar foot traffic, COVID-19 deaths, unemployment rate) that are allowed to differentially affect juveniles and young adults and age-specific year-by-month-by-day fixed effects, we find that a shift from fully closed to fully reopened schools is associated with a 28 percent increase in violent criminal incidents involving juveniles offenders. Event-study analyses, including those generated using deChaisemartin and D'Haultfoeuille (2020; 2022) estimates, are consistent with parallel pre-treatment trends and a causal interpretation of findings.

The violent crime effects we find are driven by simple assaults (53 percent) and aggravated assaults (27 percent) and are largest for criminal offenses that occur on school property during school hours, peaking around the time most middle and high school days end, around 3PM-4PM. Moreover, the violent crime effects are driven by jurisdictions with larger pupil-teacher ratios and with weaker anti-bullying laws, the latter of which have been documented to improve the quality of students' peer interactions (Rees et al. 2022). These descriptive findings suggest that both concentration effects and the quality of the peer environment are potentially important channels to explain our findings.

Back-of-the-envelope calculations based on our estimated treatment effects, along with per-crime cost estimates reported by McCollister et al. (2010), suggest that full in-person schooling (relative to fully closed schools) generates contemporaneous juvenile violent crime costs of approximately $233 million per month. We conclude that while investments in schooling generate important economic benefits, the contemporaneous crime costs of in-person schooling are substantial. Our findings suggest that cost-effective policies designed to minimize negative peer interactions are likely to maximize the longer run social benefits of schooling.

2. Background

2.1 Individual and Economic Determinants of Juvenile Violence

There is a wide literature in economics on the individual, family, and economic determinants of juvenile violence. For instance, cognitive ability (Levitt and Lochner 2001), parental income (Heller, Jacob, and Ludwig 2010), and a "traditional" (two parent) family structure (Bezin, Verdier, and Zenou 2022) have been found to be negatively related to youth violence. On the other hand, youths with parents who have criminal records are more likely to commit both violent and property crimes (Case and Katz 1991; Hjalmarsson and Lindquist 2012). Youth psychological health is also a potentially important determinant of violence, with depressive symptomatology and other psychiatric disorders contributing to an increased risk of crime (Cuellar, McReynolds, and Wasserman 2005; Cuellar, Markowitz, and Libby 2004; Anderson et al. 2015).

There is also strong evidence that youth crime responds to economic incentives. For example, randomized control trials suggest that access to summer employment reduces the propensity for youth violence (Gelber, Isen, and Kessler 2015; Heller 2014; Kessler et al. 2022). Quasi-experimental evidence also supports the hypothesis that higher wages (Grogger 1998;

Hashimoto 1987) and more job opportunities (Fone, Sabia, and Cesur 2023) reduce youth criminal behavior.[9]

2.2 Youths' Social Environment, Peer Effects, and Crime

Youths' social environment, including exposure to poor quality peers, has been shown to affect youth crime. A number of quasi-experimental and experimental studies that generate plausibly exogenous improvements in social environments and peers — for example, through the redrawing of school boundaries (Billings, Deming, and Ross 2019), randomized housing lotteries (Kling, Ludwig, and Katz 2005), public housing demolitions (Chyn 2018), and neighborhood assignments for refugees (Damm and Dustmann 2014) — reduce juvenile violent and property crime arrests.

There is also evidence to suggest that peer effects matter. For example, Bayer, Hjalmarsson, and Pozen (2009) use variation within juvenile correctional facilities in the number of peers in the same facility and find that exposure to peers who had committed similar crimes led a to higher rate of recidivism.[10] Finally, there is evidence that exposure to more disruptive peers in school may diminish human capital acquisition (Figlio 2021; Verma and Meiselam 2021), which may increase the risk of criminal behavior.[11]

2.3 Longer-Run Crime Effects of Schooling

Educational investments reduce criminal behavior both contemporaneously (Anderson 2014) and later in life (Lochner and Moretti 2004). Lochner and Moretti (2004) and Machin, Marie, and Vujić (2011) use compulsory schooling laws to estimate the causal effect of educational attainment on criminal behavior. The results of these studies consistently point to education as having a crime-reducing effect for men. The authors attribute their finding to income effects from

[9] We note that property crime seems to be more sensitive to changes in employment opportunities in studies using non-experimental design.
[10] With respect to adults, Billings and Schnepel (2022) use administrative arrest and incarceration records from Charlotte, North Carolina, and find that former inmates were less likely to reoffend when more of their peers were incarcerated while they reintegrated into society.
[11] Moreover, some studies have demonstrated peer effects in risky health behaviors, such as drinking (Eisenberg, Golberstein, and Whitlock 2014; Gaviria and Raphael 2001; Kremer and Levy 2008; Lundborg 2006) and illicit drug use (Gaviria and Raphael 2001; Kawaguchi 2004; Lundborg 2006), each of which have been found to be causal influences ion violence (Anderson, Crost, and Rees 2016; Bondurant, Lindo, and Swensen 2018; Carpenter and Dobkin 2015; Dave, Deza, and Horn 2018; Hansen and Waddell 2018).

increasing returns to legitimate work (raising the opportunity costs of illegal behavior), and education-induced increase in patience and risk aversion.

McNichols, Sabia, and Kumpas (2023) use the 1972 educational amendments to Title IX as an instrument for female high school sports participation rate to examine its impact on the crime rate. They find that a 10 percentage-point increase in female sports participation induced by Title IX resulted in a 1 to 1.5 reduction in property crime arrests and a 2.1 percent reduction in violent crime arrests among affected cohorts of females. The authors attribute this finding to Title IX-induced increases in educational attainment, employment, and earnings.

2.4 In-Person Schooling and Contemporaneous Crime

While the longer-run effects of education on crime may be explained by improvements in labor market opportunities, the mechanisms at work for contemporaneous crime may look quite different. High quality studies exploring the effect of in-person schooling on contemporaneous juvenile crime are notable for their cleverness in disentangling the effect of in-person schooling from other factors associated with both in-person schooling and crime. Such factors include day of the week (i.e., weekend vs weekday), time of day (i.e., morning vs. afternoon vs. night), and time of year (academic year vs summer). For instance, violent crime has been found to be higher on weekends as compared to weekdays (Anderson et al. 2000) as well as on days with higher temperatures (Anderson et al. 2000; Heilmann, Kahh, and Tang 2021) and fewer allergens (Chalfin, Danagoulian, and Deza 2019).

Jacob and Lefgren (2003) overcome omitted variable bias by using plausibly exogenous variation in teacher in-service days as a source of identification. In-service days are days on which students do not attend school due to blocks of time allocated for planning or professional development workshops for teachers. Jacob and Lefgren (2003) pool NIBRS data from 29 treatment cities (school districts), and, controlling for city-by-year-by-month fixed effects, find that in-person schooling is associated with a 28 percent increase in juvenile violent crime and a 14 percent decline in property crime. The authors conclude that the former is driven by a concentration effect while the latter is driven by an incapacitation effect.

Akee et al. (2014) and Luallen (2006) use teacher strikes in Washington and public-school teacher furlough days in Hawaii, respectively, to identify the effect of in-person schooling on contemporaneous crime. Consistent with Jacob and Lefgren (2006), they find that in-person schooling is associated with a 32 to 69 percent increase in juvenile violence.[12]

A new working paper by Jones and Karger (2023) collects data on pre-pandemic school start dates from Public Holidays and, using a regression discontinuity design, finds that in-person schooling increases the number of total reported crime involving 10-17-year-olds by 47 percent and arrests of 10-17-year-olds by 41 percent. They find that their increase in crime is driven by drug crimes, assaults, and non-violent crimes related to intimidation and weapons law violations.

While the above studies focus on contemporaneous crime effects of in-person schooling by exploiting quasi-experiments that close schools for all students over a short period of time, Anderson (2014) exploits a different natural experiment with a potentially quite different (though policy relevant) local average treatment effect (LATE). Specifically, Anderson (2014) examines the effect of raising the state minimum legal dropout age (MLDA) to 18 on juvenile arrests. Using a triple-differences approach (using younger teens ages 13-15 as a control group), Anderson (2014) finds that an 18-year-old MLDA is associated with a 23 percent reduction in male juvenile (ages 16 to 18) violent crime arrests and a 10 percent reduction in property crime arrests. Anderson (2014) concludes that the incapacitation and human capital effects of additional schooling dominate any concentration effects. He attributes the differences in findings from Jacob and Lefgren (2003) and Luallen (2006) to a very different LATE.[13] Anderson (2014) also finds no evidence that criminal activity is displaced to school.

2.5 Contributions

In this paper, we make several contributions to the above literature. First, we introduce a new measure of local school calendars — measured at the county-by-day level prior to, during, and following the COVID-19 pandemic — using anonymized smartphone data from SafeGraph. These data will allow us to capture localized variation in in-person schooling across both public and private schools and cover a wider set of jurisdictions than previous studies have been able to examine, yielding policy estimates with a greater degree of external validity.

Second, we exploit unique, large changes in school calendars generated by the COVID-19 pandemic to identify their effects on juvenile violence and link these effects to pre-pandemic school

[12] Moreover, in heterogeneity analyses, Luallen (2006) finds that these effects are largely driven by juveniles in highly urban school districts who have prior criminal records.
[13] The LATE identified by Anderson (2014) is the effect of inducing a teenage student on the margin of dropout to remain in high school for several months to two years.

calendars to establish external validity of the experiment. We also use the richness of non-schooling-related social mobility data from SafeGraph (as well as data on COVID-19-related health, lockdown, and economic shocks) to disentangle the effects of in-person schooling policies from other contemporaneous COVD-19-related shocks.

Third, we introduce new on crime victimization and hospitalizations investigate whether the changes in reported juvenile violence are a result of changes in monitoring or reporting or changes in violent behavior. From a policy prescription perspective, understanding the potential role of increased monitoring/reporting is critical.

Finally, we explore the heterogeneity in the estimated effects of in-person schooling on juvenile arrests by the location of arrest, time of arrest, and race. We also descriptively investigate the channels of a concentration effect and a peer quality effect in explaining our findings. This represents the first attempt to empirically explore the likely channels at work.

3. Data

3.1 Arrest and Criminal Incident Data

We use two datasets to measure reported juvenile crime: the Uniform Crime Reports (UCR) and the National Incident-Based Reporting System (NIBRS), each provided by the Federal Bureau of Investigation (FBI). Each data source has advantages and disadvantages and complements the other, as described below.

The UCR includes arrest data from over 18,000 law enforcement agencies (LEAs) across the United States, covering over 98 percent of the nation's population. Kaplan (2022) provides LEA-by-month arrest data that include information on offense type (i.e., violent crime), as well as the age and sex of the arrestee.[14] The focus of our analysis is on arrests involving 13-to-18-year-old arrestees or offenders ("juveniles"), who are of middle school age or high school age. We also examine arrests involving young adult arrestees ages 19-24, a control group that is less likely to be directly impacted by K-12 school closings.

From national seasonality analysis, we generate a count of violent crime arrests involving juveniles (and young adults) from January 1, 2015 through December 31, 2020. For our regression analyses that uses SafeGraph point-of-interest data, which are available for the 2019-2020 period), we generate a law enforcement agency (LEA)-by-year-by-month-by-age count of arrests.

[14] These data are available at: https://www.openicpsr.org/openicpsr/project/118281/version/V6/view.

There are well-known data quality concerns with UCR arrest data. For instance, because reporting to the UCR is voluntary, LEAs may choose to report arrests for only some months, some years, only in December (rather than in each month), or not at all. Given that we are interested in monthly variation in juvenile arrests to explore seasonality patterns and the relationship between K-12 foot traffic and juvenile arrests, we restrict our analysis sample to agencies that reported arrests each month (a balanced panel).[15]

We also experiment with alternate analysis samples, including (1) sensitivity checks that used data from LEAs that reported information for at least three, four, and five years, (2) restricting the analyses to LEAs that serve large cities (populations greater than 25,000), which are often measured with less error (Nolan III 2004), and (3) conducting county-by-year-by-month (rather than agency-by-year-by-month) analyses. The pattern of findings, in each case, is similar to those using our main analysis sample, as discussed below.

Panel I of Appendix Table 1 shows weighted means of juvenile and young adult arrests for violent offenses over the period during which we study seasonality effects of juvenile crime (2015-2020) and over the period for which we have both K-12 foot traffic and UCR arrest data (2019-2020). We also show means for arrests for individual violent offenses (aggravated assault, burglary, murder, rape) as well as for simple assaults.

The UCR has important advantages, notably its wide coverage across the nation (including in large populous cities,) that allows us to estimate externally valid policy parameters.[16] However, there are several disadvantages worthy of note as well. First, while arrests may be a reasonable proxy for crime — as has been documented when both data on criminal incidents and arrests are available (Lochner and Moretti 2004) — changes in arrests may also capture changes in crime reporting or policing practices. Moreover, the UCR do not include detailed information the day (i.e., weekday versus weekend), time (i.e., school hours versus non-school hours), and location (i.e., on versus off-of school property).

To complement our UCR-based analysis, we turn to the NIBRS. The NIBRS includes data on criminal incidents, whether such incidents lead to an arrest. In 2019, the NIBRS included over 8,400 LEAs, covering approximately 146.5 million persons, or about 51 percent of all LEAs that reported data to the UCR.[17] From these data, we generate an age-specific LEA-by-day count of

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[15] This sample represents 37.5 percent of all reporting agencies in the UCR during the 2015-2020 period.

[16] The balanced panel requirement allows us to identify 1,903 counties. Relaxing this assumption results in a qualitatively similar pattern of findings for our difference-in-differences analyses.

[17] Appendix Figure 1 shows the geographic coverage of the counties that were included in our NIBRS sample.

juvenile violent offenses and young adult violent offenses for the period January 1, 2019 through December 31, 2021, a time period where we have both NIBRS and foot traffic data available.

Despite the more modest geographic coverage of the NIBRS relative to the UCR, these data have several attractive features that we leverage. First, these data are available through 2021, while the UCR concluded its data collection in 2020.[18] For our SafeGraph-based regression analysis, this will allow us to exploit added variation in school calendars generated by schools that remained closed in Fall 2020 but reopened in Spring 2021 or Fall 2021. Second, by providing information on reported criminal incidents rather than (only) arrests, the NIBRS data allow us to, at least in part, disentangle policing effort (which may affect arrest rates) from criminal behavior. In addition, the NIBRS data includes information on the precise date of the offense, which will allow us to (1) exploit daily variation in in-person school instruction and crime, and (2) examine the effects of in-school instruction on criminal incidents that occur on weekdays versus weekends. Moreover, information on the location where criminal incidents occur will allow us to explore crime that occur on school property versus in off-campus locations (where crime may be displaced). Information on the time the offense occurred will also permit us to isolate whether the crime was committed during likely school hours. Finally, the NIBRS also provides information on the race and ethnicity of the offender (by age), which will allow an exploration of heterogeneity along this dimension. Given the salience of race in the pipeline to prison literature, this dimension may be especially important.

To minimize measurement error, our primary analysis sample for the NIBRS focuses on a balanced panel of LEAs that report criminal incidents in all 36 months between January 1, 2019 and December 31, 2021.[19] Panel II of Appendix Table 1 shows weighted means of the dependent variables examined in our NIBRS sample for juveniles and young adults.

3.2 Crime Victimization and Hospitalization Data

One important concern with both the UCR and NIBRS data is that both data sources only include information on reported criminal incidents. Given that in-person schooling may affect the likelihood of reporting criminal offenses, we are interested in whether the effects we are observe are driven by changes in behavior as compared to changes in the reporting of violent offenses to law enforcement. For this purpose, we turn to self-reported data from the National Crime Victimization

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[18] In January 2021, all agencies began transitioning into reporting to the NIBRS instead of the UCR. Thus, the last year where we have data for the UCR is 2020.
[19] In our sample, we have a total of 5,236 NIBRS agencies.

Survey (NCVS) and medical claims data from the National Electronic Injury Surveillance System (NEISS) for the period January 2015 to December 2020

The NCVS data are collected through a survey administered by the Bureau of Justice Statistics and are designed to be nationally representative sample of crime victims in the United States. The NCVS samples approximately 240,000 persons in 150,000 households and includes information on the frequency of and details surrounding criminal victimization. Our focus is on violent crime offenses, examining assaults (aggravated or simple assault) and other more costly violent crime (rape, sexual assault, robbery, or assault) victimization that involves juvenile victims ages 13-18 as well as those ages 19-24. To compare our findings from crime victimization data to our NIBRS-based findings, we will use NIBRS data on the age of crime *victims* reported by police for a direct comparison.

The NEISS data are collected by the U.S. Consumer Product Safety Commission, alongside with the Centers for Disease Control and Prevention. The NEISS data provide data on emergency department visits from approximately 100 hospitals and are designed to be representative of emergency departments across the United States. We measure the monthly count of hospitalizations for assaults for juvenile victims ages 13-18 and young adult victims ages 19-24. Again, we compare findings to the NIBRS on juvenile assault *victims*.

While the publicly available NCVS and NEISS data do not include geographic identifiers, there is information available on the month in which the offense or hospitalization occurred. Therefore, we can evaluate seasonality in reported violent offenses involving juveniles and young adults over the 2015-2020 period. The means of violent crime victimization rates and violence-related hospitalizations are reported in Panels III and IV of Appendix Table 1.

3.3 In-Person Schooling Measure

One important paper is our use of a novel measure of school calendars, spanning the period prior to, during, and following the COVID-19 pandemic. Having local data on in-person schooling over the 2020-2021 period is critical given the large, localized shocks to in-person schooling over this period. While school calendar data obtained from Public Holidays provides valuable administrative information on academic calendars, there are several limitations of these data that we attempt to complement with our use of smartphone data. First, the data from Public Holidays do not include information on within-school calendar year school closures that might be generated by four-day school week policies, full-year schooling, or teacher-in service days. Second, Public

Holidays data do not measure in-person schooling during the COVID-19 pandemic, including distinguishing between full in-person instruction, hybrid learning, or full online schooling. Moreover, these data do not allow us to measure the intensity of enrollments (which can be proxied by relative smartphone "pings") to capture heterogeneous concentration effects by school size. Finally, Public Holidays data are limited to public school districts and do not cover private schools.

Given the lack of consistently collected local information on school district calendars before, during, and following the pandemic, we construct a proxy for in-person elementary and secondary school instruction using Safegraph Inc's Point of Interest (POI) data. Recently, Goldhaber et al. (2024) evaluated a variety of other centralized databases on school calendars and closures and find SafeGraph based proxies outperform other attempts to scrape or collect school calendars when compared against administrative records from Michigan. These data provide location-specific information collected from over 45 million anonymized smartphone devices and include data on daily "pings" at over four million POIs in the United States, identified using North American Industry Classification System (NAICS) six-digit codes. Using the NAICS code 611110 (Elementary and Secondary Education), we construct a county-by-year-by-day measure of smartphone pings into K-12 over the period January 1, 2019 through December 31, 2021.

In Figure 1, we provide some evidence in support of the validity of our SafeGraph measure in the year prior to the pandemic by collecting Fall 2019 data on school district opening dates from Public Holidays[20] (Jones and Karger 2023) over the period July 15, 2019 to November 15, 2019 (the period during which Public Holidays data are available) and estimating the following event-study regression equation:

$$\mathrm{FT}_{\mathrm{cd}} = \beta_0 + \sum_{j=-6, j\neq -1}^{6} \beta_j D_{cd}^{j} + \gamma_c + \rho_d + \boldsymbol{X}'_{cd}\beta_1 + \varepsilon_{cd} \qquad (1)$$

where the $\mathrm{FT}_{\mathrm{cd}}$ is the inverse hyperbolic sine transformed number of cellphone pings per 100,000 at K-12 schools in county *c* in day *d*, γ_c is a county fixed effects, ϱ_d is a daily fixed effect, $\mathbf{X}_{cd}$ is vector of county-level controls (monthly unemployment rate, daily precipitation, and daily average temperature), $\beta_j D_{cd}^{j}$ is a set of dummy variables denoting j weeks from a Fall 2019 school start date, and β_j is a set of coefficients on those indicators.

[20] These data are available at: https://publicholidays.com/us/school-holidays/

We define the period *j=0* as the week prior to the official school start date, when (1) teachers commonly return campus for teacher in-service days and setting up offices before classes resume, and (2) pre-season school sporting events begin intensive practices.[21] The period *j=1* is when student classes officially begin, where we expect a larger increase in smartphone presence.

In panel (a), we show estimates of β_j from the regression on the full sample. We show that in the weeks leading up to a Fall 2019 school reopening, K-12 school foot traffic in treatment and control counties was similar. In the week before the start of classes ($j = 0$), there is a sharp uptick in school foot traffic relative of approximately 22 percent, likely reflecting students pre-class orientation or pre-academic year sports programs, or teachers returning to school for training or preparations. Beginning with the official school start date ($j = 1$), there is a further, substantial jump in school foot traffic of 73 to 82 percent relative to weeks prior.

In panel (b) of Figure 1, we restrict the analysis sample to weekdays only. The pattern is quite similar to that reported in panel (a). The findings continue to show that relative to two weeks prior to the start of classes, K-12 school foot traffic rose by approximately 122 percent when classes began. Together, the findings in Figure 1 suggest that our foot traffic measure is a credible proxy for school openings and believe it to be useful.

For our analysis examining both temporal and geographic variation in school calendars during the pandemic, we generate a relative foot traffic measure, *Relative K-12 FT*, constructed as the ratio of the K-12 foot traffic on a given week (or day) to the average weekday foot traffic in January and February 2020, the pre-pandemic period. For example, a value would of 50 for *Relative K-12 FT* would mean that K-12 school foot traffic was at 50 percent of its level in the immediate pre-pandemic period. For ease of interpretation in our regressions, we rescale our fK-12 oot traffic measure so that our estimate will represent the effect of moving from 5th percentile to 95th percentile of school foot traffic, which is equivalent to moving from (likely) fully closed schools to fully in-person schooling.

Figure 2 shows county-level variation in K-12 school foot traffic in the Fall of 2020, restricting our policy variation to those counties in which UCR data are non-missing in our analysis sample. We note substantial variation in this measure both across and within states. For instance, K-12 school foot traffic approached pre-pandemic levels in many jurisdictions in Midwest and

[21] See, for example, NBC Chicago (2023), Los Angeles Unified (2023), and Kentucky Department of Education (2023) for a handful of examples of local school districts in which teacher in-service days and pre-season school sporting practice commences in the weeks prior to the official start of the school year.

Mountain West, while in the Northeast and on the West Coast, K-12 schools remained largely closed or involved more hybrid education (i.e., in-person schooling a few days per week and online instruction on other days). Moreover, within states, there was often important variation across counties. For instance, in the state of Texas, many counties in the panhandle saw foot traffic that returned to pre-pandemic levels in the Fall of 2020, while in other large Texas counties (i.e., Bexar, Dallas, and Harris) remained online part or all of the time. This pandemic era variation in K-12 foot traffic will be exploited as part of our identification strategy, described below.

4. Empirical Strategy

4.1 Seasonality in Juvenile Violence, Pre-Pandemic (2015-2019) and Pandemic Years (2020)

Our analyses begin by providing descriptive evidence on monthly seasonality in juvenile crime. We begin by using UCR and NIBRS data from 2015-2019 (and then in the pandemic year of 2020) to explore pre-pandemic seasonality in juvenile violent crime. We estimate a Poisson regression of the following form:

$$E(V_{icmy}|\boldsymbol{X_{cmy}}) = exp\left[\beta_0 + \boldsymbol{\beta_m} + \boldsymbol{\tau_y} + \boldsymbol{\gamma_i} + \ln\left(days_{my} * pop_{cmy}\right) + \boldsymbol{X'_{cmy}\beta_1}\right] \qquad (2)$$

where V_{icmy} is the count of juvenile violent crime (arrests in the UCR and incidents in the NIBRS) reported by LEA *i* located in county *c* in month *m* in year *y*, γ_i is an LEA fixed effect, τ_t is a year fixed effect, $\boldsymbol{X_{cmy}}$ is a vector of county-level, time-varying macroeconomic controls (unemployment rate and median income) and weather controls (temperature and precipitation), and $\ln\left(days_{my} * pop\right)$ is our exposure variable, capturing the product of the number of days per month and the population served by the LEA. Our key parameters of interest, $\boldsymbol{\beta_m}$, are the coefficients on month indicators, capturing seasonality in juvenile violence. Standard errors will be clustered at the LEA-level.

We will estimate equation (2) separately for the 2015-2019 (pre-pandemic) and 2020 (pandemic) periods given changes in the national school calendar that coincided with the onset of the pandemic in the U.S. in March 2020. In addition, we will estimate equation (2) for young adults (ages 19-24) as a quasi-placebo test. While young adult violence could be affected by K-12 schooling through effects on younger peers (or siblings) or through school employment effects, the effects are expected to be much smaller.

In addition, to test for whether any seasonality effects could be driven by changes in reporting of offenses, we estimate similar regressions using the NCVS and the NEISS hospitalization data for 2015-2019, as well as during the 2020 pandemic year.

4.2 Cross-County Variation in K-12 Foot Traffic

Next, we explore whether summer seasonality in juvenile violence differs across the academic calendar in the pre-pandemic year across jurisdictions with August-to-May vs. September-to-June academic calendars. If our hypothesis that in-person schooling increases contemporaneous juvenile violence is true, we would expect that juvenile violence would fall most in June in school districts where summer break begins in May, while it would remain relatively high in June in districts where summer break does not begin until later June. A similar pattern should be observed in August, with juvenile violence rising in districts that begin school in August, but being delayed in districts that begin school later.

Our focus on the pre-pandemic year of 2019, a year for which we have SafeGraph data. We then measure the county-by month distribution of June K-12 smartphone pings (on non-weekend days) relative to January of the same year (when schools are in session nationwide) to capture the timing of schools beginning their summer vacation. *Relative June K-12 FT* is a set of indicator variables measuring terciles of the distribution of relative June foot traffic. The jurisdictions in the bottom tercile can be thought of as jurisdictions with schools that began their summer break in late May or early June; whereas the jurisdictions in the top tercile can be thought of as jurisdictions with schools that began their summer break in late June.

Panel (a) of Appendix Figure 2 shows the distribution of June K-12 foot traffic and panel (b) shows the distribution of August K-12 foot traffic. We find that those jurisdictions with high relative K-12 foot traffic in June have low relative foot traffic in August, indicative of a September-to-June, while districts with a low relative K-12 foot traffic in June have high relative foot traffic in August, indicative of an August-to-May schedule. The origins of regional differences in school starting times are a matter of some conjecture, with some explaining this difference by differences in local agriculture seasons (growing and harvesting of local crops) while others relate it to historical patterns of urbanicity and disutility for humidity (Gold 2002; Pedersen 2012; Weiss and Brown 2003).

To empirically test the above hypothesis — whereby juvenile violence falls first in June in counties with districts that begin summer break in May — we estimate the following Poisson regression:

$$E(V_{icmy}|\boldsymbol{X_{cmy}}) = exp\begin{bmatrix} \beta_0 + \boldsymbol{\beta_m} + \ \boldsymbol{\beta_m} * Relative\, June\, K12\, FT_c + \ \boldsymbol{\tau_y} + \boldsymbol{\gamma_i} \\ + \ln\left(days_{my} * pop_{cmy}\right) + \boldsymbol{X'_{cmy}\beta_1} \end{bmatrix} \tag{3}$$

And test whether the interactive effect of month dummies and relative June foot traffic reflects seasonality patterns in June and August consistent with geographic differences in school starting (and ending) times. Again, we estimate equation (3) for juveniles and young adults, with the hypothesis that young adult violence should be largely unaffected by K-12 school foot traffic, expect through employment and younger sibling-related spillovers.

4.2 Within-County Variation in K-12 Foot Traffic

Our "difference-in-differences" analyses spans the years 2019-2021 (2019-2021 using the NIBRS and 2019-2020 using the UCR, given the latter's end in 2020) and exploits within-county variation in school foot traffic to identify the effects of in-person schooling on juvenile violence.[22] We estimate the following Poisson regression:

$$E(V_{ict}|\boldsymbol{X_{ct}}, \boldsymbol{Z_{ct}}) = exp\begin{bmatrix} \beta_0 + \beta_1 K12\, FT_{ct} + \ \boldsymbol{\tau_t} + \boldsymbol{\gamma_i} + \ \ln(days_t * pop_{ct}) \\ + \boldsymbol{X'_{ct}\beta_2} + \boldsymbol{Z'_{ct}\beta_3} \end{bmatrix} \tag{4}$$

where t denotes time (year-by-month-by-day in the NIBRS, year-by-month in the UCR), $K12\ FT_{st}$ measures K-12 school foot traffic in county c at time t, the vector $\mathbf{Z}_{ct}$ includes measures of COVID-19 shocks (to disentangle the effects of non-schooling related COVID-19 shocks, such as lockdowns and economic downturns, from those of K-12 school closings) including restaurant and bar foot traffic and COVID-19 deaths, and the vector $\mathbf{X}_{ct}$ captures economic shocks, including during the pandemic (e.g., unemployment rate, per capita income). Then, β_1 is our key parameter of interest, the effect of K-12 school foot traffic on juvenile violence. For ease of interpretation, $K12\ FT_{st}$ is rescaled so that β_1 can be interpreted as the effect of moving from the 5th to the 95th percentile of relative school foot traffic. This approximates the difference between schools were

[22] Because the UCR data stopped being reported in 2020, our analysis using UCR focuses on 2019 and 2020.

most likely to be fully closed (5th percentile) as compared to when full in-person instruction was ongoing (95th percentile).[23] Note that the estimated treatment effect in equation (4) will not be identified via nationwide seasonality effects, but largely by local variation in school calendars, in part generated from school openings and closings in the wake of the COVID-19 pandemic.

To ensure that β_1 is capturing the effect of in-person schooling rather than generic COVID-19 shocks, we take three approaches. As noted above, we are careful to control for other measures of lockdown-induced or voluntary social mobility (e.g., restaurant and bar foot traffic), COVID-19 health (e.g., COVID-19 deaths), and COVID-19 economic shocks. In addition, we estimate equation (4) for young adults who should be less affected by K-12 foot traffic; if there were a comparably-sized effect, that would suggest our measure of in-person schooling is simply capturing a generic COVID-19 shock. Finally, we also narrow the ages of treatment and control teens for a comparison of more similar individuals who should be relatively more (16 to 17-year-olds) and less (19 to 20-year-olds) likely to be affected by in-person schooling.

Furthermore, to explore the common trends assumption underlying our "difference-in-differences" approach, we undertake a number of strategies. First, we add controls for state-by-month (or state-by-month-by-day) fixed effects to control for unobserved shocks that commonly affect local jurisdictions, essentially forcing geographically proximate control groups. Next, we pool juveniles and young adults ages and estimate a "difference-in-difference-in-differences" (DDD) Poisson model that estimates the effect of K-12 school foot traffic on juvenile as compared to young adult violence:

$$E(V_{ict}|\boldsymbol{X_{ct}},\boldsymbol{Z_{ct}}) = exp\begin{bmatrix}(1+a)\beta_0 + \beta_1(1+a)K12\ FT_{ct} + (1+\mathrm{a})\boldsymbol{\tau_t} + (1+a)\boldsymbol{\gamma_i} \\ +\ (1+\mathrm{a})\ln\left(days_t * pop_{ct}\right) \\ +(1+a)\boldsymbol{X}'_{\boldsymbol{ct}}\boldsymbol{\beta_2} + (1+a)\boldsymbol{Z}'_{\boldsymbol{ct}}\boldsymbol{\beta_3}\end{bmatrix} \quad (5)$$

where *a* is an indicator variable set equal to 0 for young adults ages 19-24 and is set equal to 1 for juveniles ages 13-18. Thus, equation (5) is a Poisson model and the key parameter of interest is the differential effect of K-12 foot traffic on juveniles relative to young adults. We will also experiment with augmenting equation (5) with a full set of LEA-by-year-by-month fixed effects, $\gamma_i * \tau_t$, which will

[23] We also allow for non-linearities in the effect of K-12 school foot traffic by estimating models with indicator variables taking on the value of 1 if the relative foot traffic is between 25 and 50, 50 and 100, and greater than 100.

control for any unmeasured city-specific time effects that commonly affect juveniles and young adults.

Then, following Hansen, Sabia and Schaller (2022) and Schmidheiny and Siegloch (2019)[24], we test for common pre-treatment trends in an event-study framework:

$$E(V_{ict}|\boldsymbol{X_{ct}}, \boldsymbol{Z_{ct}}) = exp\begin{bmatrix}(1+a)\beta_0 + \sum^{J}{}_{j\neq -1}(1+a)\delta_j\, D^j_{ct} + \ (1+\text{a})\boldsymbol{\tau_t} + (1+a)\boldsymbol{\gamma_i} \\ +\ (1+\text{a})\ln(days_t * pop_{ct}) \\ +(1+a)\boldsymbol{X'_{ct}\beta_2} + (1+a)\boldsymbol{Z'_{ct}\beta_3}\end{bmatrix} \quad (6)$$

where *j* denotes event time and D^j_{ct} is a set of variables that measure the difference between county-level K-12 school foot traffic in month-by-year *t* and *t-1* occurred *j* months from *t*. We explore whether pre-treatment trends in juvenile vs. young adult violence differ in the period prior to a change in foot traffic (*j* = *-1*). If estimates of δ_{ja}= 0 for j < -1, this would be consistent with the common trends assumption.

Next, to expunge potential bias due to heterogeneous and dynamic treatment effects, we use the dynamic difference-in-differences estimator recommended by de Chaisemartin and D'Haultfoeuille (2020; 2022). For this estimator, we bin foot traffic measures in 4 categories: 100 percent or higher (full reopening), 50 to 99 percent (high in-person hybrid schooling), 25 to 49 percent (high online hybrid learning), and 0 to 24 percent (very high online, likely full-online schooling). Our counterfactuals are restricted to "stayers" whose treatment status (bin category) did not change and had the same baseline treatment. An important advantage of this estimator is that it permits the value of the treatment variable to both increase and decrease. We then estimate event-studies based on this estimator as a further test of our identification assumption.

Finally, we explore heterogeneity in the effects of K-12 school closings on criminal incidents in the NIBRS by whether the criminal incident took place during the school year, during likely school hours (vs. before or after school), on (vs. off) of school property, and by race and gender. In addition, we attempt to disentangle a quantity versus quality of teenage peer interactions by exploring whether treatment effects differ by pre-treatment (1) school size and peer density, and (2) the strength and comprehensiveness of anti-bullying laws, which have been shown to affect the quality of peer interactions (Sabia and Bass 2017; Rees et al. 2022).

[24] See also Rees, Sabia, and Margolit (2021).

5. Results

5.1 National Seasonality Effects in Crime: 2015-2019

In panel (a) of Figure 3, we show pre-pandemic seasonal trends of violent crime arrests for juveniles and young adults using the UCR. For juveniles, we find a pattern of arrests for violent offenses that follows the academic calendar. During the 2015-2019 period, juvenile arrest rates rise between the months of January and May, ranging from 245 to 268 violent crime arrests per 100,000 persons. After peaking in May, the violent crime arrest rate falls by 9 to 12 percent in June, July, and to a lesser extent August before rising sharply in the fall (peaking in November) and plummeting by approximately 9 percent in December. This pattern follows the typical K-12 calendar in the U.S., where schools are closed for summer vacation and December holidays (i.e., Christmas and Channukah) breaks.[25]

In sharp contrast to juveniles, violent crime arrests for young adults *peak* in the summer months (May to August) before continuously plummeting during the fall and through December. Given that young adults are less likely to be affected by K-12 in-person instruction, the difference in seasonality of arrests between juveniles and young adults is consistent with the hypothesis that in-person K-12 schooling is a driver of juvenile violent crime arrests.

Columns (1) and (2) of Table 1 show estimated seasonality effects in juvenile violent crime that include controls for macroeconomic conditions and weather for juveniles (column 1) and young adults (column 2), respectively. Along the same lines, panels (b) and (c) of Figure 3 show 95% confidence intervals on the month dummy coefficients for juveniles and young adults, respectively. The pattern of findings in these figures continue to show a decline in summertime violence for juveniles, but not young adults.

Figure 4 presents pre-pandemic seasonality in violent crime incidents involving juvenile offenders using NIBRS data. Importantly, because these are incident-based data, reported incidents without arrests are included, which serves as a descriptive test for whether our seasonality effects can be explained by changes in police effort to detect and arrest juvenile offenders. The seasonality pattern show similar summer and December decline in juvenile violence incident, while incident

[25] In Appendix Figure 3, when we disaggregate violent crime to aggravated assault, rape, murder, and robbery. We find a large decline during the summer for aggravated assault, but not for other violent crime offense. Moreover, we find similar seasonality effects for simple assault arrests.

involving young adult remains relatively the same. We document that the number of juvenile violent crime offenders decrease by 7 to 13 percent during the summer months.

The NIBRS provides information on both the age of the offender and victim. There is a high degree of correlation between age of victim and offender. For example, from 2015-2019, 45 percent of juvenile violent crime offenders were alleged to have victimized a juvenile. Similarly, among juvenile victims of violent offenses, 37 percent were reported to involve a juvenile offender. In Appendix Figure 4, we show seasonality in violent crime incidents involving juvenile victims. The pattern of findings is qualitatively similar to the findings observed in Figure 4.

In Figure 5, we explore violence involving juvenile victims using data from the NCVS (panels a and b) and NIECS (panel c), respectively. These data allow us to explore whether the primary mechanism to explain the relationship between in-person schooling and juvenile violent crime arrests is enhanced reporting. For example, violence that takes place at or near school may be more likely to be reported by school faculty and staff. The pattern of findings from the NCVS on violent crime victimization (panel a) and assault victimization (panel b), which includes victimization not reported to law enforcement, continue to show that juvenile violence falls during summer months and the December break. Additionally, we find that juvenile hospitalizations for violence fall during the summer months and in December. This pattern of findings is consistent with increased juvenile violent behavior rather than simply increased reporting of juvenile violence by school faculty and staff. Moreover, as in the UCR and NIBRS, the summer decline in violence in the NCVS and NEICS appears concentrated among juveniles rather than young adults, though young adults experience a more modest increase in reported victimization (but not hospitalizations) in the fall.

In Appendix Table 2, we present the findings from equation (2) using crime victimization and hospitalization data. Estimates from the odd-numbered columns show that during the summer, youth violent crime and assault victimization decreased by 21 to 53 percent and 27 to 77 percent, respectively; moreover, the estimates in column (5) reflect a 25 to 34 percent reduction in juvenile assault-related hospitalizations during the summer. In sharp contrast, we find no such decline in hospitalizations during the summer months for young adults. Together, the results from these descriptive exercises suggest that our findings are not driven by changes in the probability of detection or reporting when school is open, but rather by increases in violent behavior.

5.2 Exploiting Cross-County Variation in Timing of Pre-Pandemic School Calendar

The above national seasonality trends mask geographic heterogeneity in the timing of K-12 in-person schooling, as described in Appendix Figure 2. Panel (a) of Figure 6 illustrates that juvenile violence experiences a more pronounced decline in June in counties where schools end in-person instruction in late May or early June (20.6 percent) compared to those ending in late June (3.1 percent). With respect to August, we find that juvenile violence rises relatively sooner in jurisdictions that end summer break in August as compared to September. This pattern of findings is consistent with Jones and Karger (2023) and suggests that juvenile violent crime is positively related to in-person schooling. Importantly, the pattern of summer month coefficients in panel (b) show that county-level relative June K-12 school foot traffic is not related to violent crime arrests involving adults. These findings align with differences in local school districts' school calendars affecting juvenile violent crime.

5.3 The COVID-19 Pandemic Year of 2020

The arrival of the COVID-19 pandemic dramatically changed in-person K-12 schooling. Figure 7 uses data from the UCR to document seasonality trends during the pandemic year of 2020. In sharp contrast to the 2015-2019 period, we find that the onset of the U.S. COVID-19 pandemic in March 2020, when there was a near-nationwide school shutdown, was associated with a 10 percent decline in juvenile violent crime arrests (relative to January 2020). The decline was even greater in April 2020 and remained low (150 to 170 arrests per 100,000) between May and December 2020. In contrast, young adult arrests were relatively unchanged from February 2020 through October 2020 (relative to January 2020), falling only during holiday months of November and December. This pattern of findings, also reported in column (3) of Table 1, suggests that the seasonality of juvenile violent crime arrests changed drastically between 2015-2019 and 2020, consistent with changes in full in-person schooling during the pandemic year. The fact that such changes were not observed for young adults (column 4 of Table 1) suggests that these results likely do not reflect other correlated COVID-19 shocks (e.g., lockdowns, non-essential business closures, voluntary social distancing), though we explore this issue further in the difference-in-differences models discussed below.[26]

[26] In Appendix Figure 5, we show seasonality patterns in the NIBRS for the 2020 pandemic year. We find that during April to December of 2020, the number of reported juvenile violent crime offenders decreased by 21.3 to 32.3 percent (relative to January 2020).

Appendix Figure 6 shows pandemic year seasonality in juvenile hospitalizations for violence. This pattern of findings is consistent with what we observe for arrests and suggests that our findings are likely not entirely explained by changes in monitoring/reporting of behavior.

In our final descriptive analysis, presented in Figure 8, we examine violent criminal incidents reported to police from the NIBRS, by day and time of the incident. In the top-left panel, we observe that on weekdays during the school year, juvenile violent crime incidents peak around 3pm, coinciding with the typical school dismissal time. Notably, this 3pm peak is not observed during (2) the summer (top-right panel), (3) weekends (bottom two panels), or (4) for young adults (indicated by the blue dashed line). Moreover, we do not find any evidence of such patterns from March 2020 to June 2020, a period when schools were mostly or partially shut down (panel b). These differences in the timing of when youth violence peaks suggest that in-person schooling may have a negative impact on youth violent behavior.

5.3 UCR-Based Arrest Estimates, 2019-2020

Table 2 presents estimates of equation (4) from the 2019-2020 UCR. Controlling for agency and year-by-month fixed effects, we find that moving from fully closed to fully opened K-12 schools is associated with a 39.8 percent ($e^{0.335}$-1) increase in violent crime arrests among those ages 13-18 (panel I, column 1). The addition of pandemic controls for COVID-19 deaths, restaurant and bar foot traffic (column 2), macroeconomic controls for the unemployment rate and per-capita income (column 3), temperature and precipitation (column 4), and either census division-by-month-by year (column 4) or state-by-month-by-year fixed effects (column 5) do not principally change this finding. The findings in our most saturated specification (column 5) show that moving from fully closed to fully reopened K-12 schooling is associated with a 26.0 percent ($e^{0.231}$-1)*100 increase in juvenile violent crime arrests (panel I, column 5).

Could these findings be explained by COVID-19 shocks that affected juveniles and young adults? The findings in Appendix Table 3 show that after controlling for COVID-19 shocks (column 2), there is no statistically significant or economically meaningful effect of K-12 in-person schooling on young adult violent crime arrests, consistent with a causal interpretation of the estimates in panel I. In panel II of Table 2, we present estimates from the "triple-differences" model described in equation (5). Notably, this specification also includes controls for age-by-census division-by-year-by-month fixed effects in column (5) and age-by-state-by-year-by-month fixed effects in column (6). Moreover, in column (7), we experiment with agency-by-year-by-month fixed effects to control for any unmeasured city-specific time effects that commonly affect juveniles and young adults. The findings from our DDD model shows that the full opening of K-12 schools is associated with a 21.2 to 28.0 percent increase in juvenile as compared to young adult violent crime arrests.

In panel III, we tighten the age bounds of our treatment and control groups, focusing on 16-to-17-year-olds who are affected by K-12 school foot traffic and 19-to-20-year-olds who are less affected. This comparison will net out any unmeasured differences in juvenile violence between 13-15-year-olds and 21-to-24-year-olds, who may be different along many dimensions. Our results from our preferred specification continue to show an increase in juvenile violence by 21.2 percent. Together, the results in Table 2 provide strong evidence for a concentration and/or negative peer interaction effect dominating any incapacitation effect for juvenile violent crime.

To descriptively assess the validity of the parallel trends assumption, we present findings from two event study analyses. In panel (a) of Figure 9, we present results from equation (6), which estimates an event study in the spirit of Schmedheiny and Siegloch (2019) using a continuous treatment. We find no evidence of differential pre-treatment trends in juvenile violent crime arrests in treatment and control jurisdictions prior to changes in K-12 foot traffic. Moreover, we document a substantial, immediate, and persistent increase in juvenile violence immediately after the change in K-12 foot traffic (that approximates likely full in-person schooling), which is consistent with the magnitude we report in Table 2. Finally, in Appendix Figure 7, we that event study examining arrest rates of 16-17-year-olds versus 19-20-year-olds produces a similar pattern of findings.

In panel (b), we re-estimate our event study using deChaisemartin and D'Haultfoeuille (dCDH) estimates. An advantage of the dCDH estimator is that it expunges potential bias from heterogeneous and dynamic treatment effects by relying on counterfactuals whose K-12 school foot traffic is unchanged over the sample period. A further advantage is that the estimator allows the continuous treatment to increase and decrease in value rather than be "all absorbing" in nature. There are some limitations, however, notably, the treatment must be "binned" rather than allowed to be fully continuous. We bin our treatment into the following categories: 0 to 25 percent, 25 to 50 precent, 50 to 100 percent, and 100 or more percent. Second, because the dCDH approach does not allow estimation of non-linear (i.e., Poisson) models or allow for interaction terms (i.e., a DDD-type model), we redefine the dependent variable as the difference in the arrest rate (arrests per 100,000 persons) between juveniles and young adults and use ordinary least squares (OLS) as our regression strategy.[27] The results from an event study based on the dCDH estimator (panel b) is qualitatively similar to that generated in panel (a). The findings are also similar to a Poisson-based DDD event study using the same bins required of the dCDH estimator (panel c).

In panel I of Table 3, we explore arrests for individual violent crime offenses (columns 2-5) and extend our analysis to an exploration of arrests for simple assaults.[28] The results suggest that increases in arrests for violent crime offenses are largely driven by aggravated assaults (26.9 percent) and robbery (30.7 percent). We also find that moving from fully closed to fully reopened schooling is associated with a statistically significant 52.5 percent increase in simple assaults (column 6). In panel II of Table 3, we show that our estimates are robust to the inclusion of agency-by-year-by-month fixed effects.

Table 4 explores non-linearities in the effects of K-12 school foot traffic, which allows us to explore potential effects of hybrid schooling. The results show that fully reopening schools (K-12 school foot traffic ≥ 100% of January-February 2020 K-12 school foot traffic) is associated with the largest increases in juvenile violent crime arrests. Relative to K-12 foot traffic < 25% of January-February 2020 (likely fully or nearly-fully closed schools), a likely full K-12 opening is associated with 19.7 percent increase in violent crime arrests (column 1), a 21.5 percent increase in aggravated assaults (column 2), and 37.7 percent increase in simple assaults (column 6).

Nonetheless, there is also evidence that partially opened K-12 schools (i.e., hybrid education) is associated with increases in juvenile violent crime arrest, particularly if K-12 school foot traffic exceeds at least 50 percent of what it was in the pre-pandemic (January-February 2020) period, though the effects are uniformly smaller than for full reopenings. The pattern of findings in Table 4 are largely consistent with a concentration effect.

5.4 NIBRS-Based Criminal Incident Estimates, 2019-2021

To supplement our UCR estimates, we now shift our focus to daily data from the NIBRS between January 1, 2019 and December 31, 2021. We focus on DDD estimates from the NIBRS and these are presented in Tables 5-7 and in Figure 10.[29]

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[27] Our main estimate is qualitatively similar when we estimate our main equation using the rate as our outcome variable (Appendix Table 4).

[28] While simple assault is part of the violent crime defined by the UCR, we also examine simple assault since it is an offense related and similar to aggravated assault.

[29] Due to the low number of murder when we examine agency-by-day panel, we omitted murder from our NIBRS estimation.

In Table 5, we find moving from fully closed to fully in-person K-12 schooling is associated with a 17.8 percent increase in violent crime, primarily driven by increases in aggravated assault (28.0 percent). We also find a very large (over 90 percent) increase in simple assaults.[30] Event-study analyses, shown in Figure 9, are consistent with the parallel trends assumption in the pre-treatment period and suggest an immediate, economically important, and persistent increase in juvenile violent crime arrests following a move to fully in-person K-12 schooling.

5.5 Heterogeneity in Treatment Effects

The remaining tables focus on heterogeneity in treatment effects. First, we explore heterogeneity of the effects of in-person schooling on violent crime arrests that occur (1) on the weekend versus weekday, as well as (2) on school versus off of school property, and (3) during school hours vs non-school hours.

Our results provide little evidence that K-12 school foot traffic affects juvenile (relative to young adult) violent crime arrests on weekends (panel I), as expected. The largest effects are concentrated for violent crime incidents that occur on school campuses during a weekday (panel II) and during school hours (7am to 5pm) during a weekday (panel IV).[31] These findings are consistent with the notion that in-person schooling increases violent behavior.

In Table 7, we examine the heterogeneous treatment effects by race of the offender (White vs. Black) and by age. With the exception of simple assaults, we find that K-12 school foot traffic has a somewhat larger effect on violent criminal incidents among Whites as compared to Blacks, but the effect sizes for Blacks are substantial, suggesting a broad impact across races.

In-person schooling-induced juvenile violence is larger for younger teens ages 13-16 than older teens ages 17-18. This finding could suggest that the concentration effect is moderated by

[30] We note two differences in the estimates obtained when using the NIBRS as compared to the UCR. First, in the case of rape, we now find a statistically significant effect in our NIBRS analysis. However, the point estimate is of similar magnitude, but simply greater precision in the NIBRS. Second, we do not detect an effect of K-12 school foot traffic on robbery-related arrests in in the NIBRS, while we do in the UCR. A possible explanation is that the differences in agencies included in our sample may contribute to this disparity. In auxiliary analyses, we find that if we restrict our UCR sample to agencies reporting crime data in both the UCR and NIBRS, our robbery results become null, aligning more closely with our NIBRS estimate. This suggests that our findings on robbery are likely driven by larger law enforcement agencies and regions in UCR but not NIBRS, which are represented in the UCR, but not the NIBRS.

[31] For example, we find that fully in-person schooling is associated with a 83.5 percent increase in weekday, on-premises juvenile violent crime incidents (column 1, panel II) and a 38.9 increase in weekday, during-school-hours violent crime incidents (column 1, panel IV). Only for simple assaults is there some evidence of spillovers to violent incidents off of school property or off-school hours, which could suggest that schooling incidents precipitate more minor violent interactions.

maturity and school learning or, alternatively, that the onset of hormones exacerbates the negative effects of in-school peer interactions.

5.5. Peer Concentration Effect and Negative Peer Interaction Effect

Finally, we attempt to descriptively explore two potential in-school mechanisms at work: a peer concentration effect and a negative peer interaction effect.[32] First, in column (1), we draw data from the Common Core of Data in the pre-treatment period and explore whether estimated treatment effects differ across counties with larger (above median) vs. smaller (below median) pre-treatment (2017-2018) school sizes.[33] In column (2), we interact cross-county pre-treatment differences in schooling characteristics related to resource availability, including county per-capita income, school expenditures per pupil, and racial composition of the school districts to ensure that school size is not simply proxying for available schooling resources.[34] Our results suggest that the estimated effect of K-12 foot traffic on juvenile violence is driven by counties with relatively larger high school sizes. Controlling for cross-sectional pre-treatment schooling differences has little effect on the estimated treatment effect.

In columns (3) and (4), we use an alternate measure of student density, the median pupil-teacher ratio. Again, the findings point to greater juvenile violence where there is a higher density of peers, which can increase student interactions and the likelihood of violent altercations. Together, the findings in columns (1)-(4) are consistent with a concentration effect.

In the remaining columns of Table 8, we exploit cross-sectional variation in the intensity of state anti-bullying laws.[35] In columns (5) and (6), we define "strong" anti-bullying laws based on the Department of Education (DOE)'s rating of each state's anti-bullying law. The DOE assigns a score of 0 to 2 to each anti-bullying law component, measuring the overall expansiveness of each provision, and creates an aggregate "intensity rating" based on these scores (see Sabia and Bass 2017 for further discussion on this score). A higher DOE rating implies that a state mandates strong, comprehensive anti-bullying policies. Evidence from Rees et al. (2022) suggests that ABL adoption, particularly with strong, comprehensive statutes, is associated with a reduction in bullying

[32] To do this, we interact the treatment effect with pre-treatment average school size in 2017-2018. The median school size in that time period was 390 students. To increase geographic coverage and maximize county-level variation in pre-treatment school characteristics, we use data from the UCR for this analysis.
[33] The data on school size come from NCES CCD and are available at: https://nces.ed.gov/ccd/ccddata.asp.
[34] Our estimates remain qualitatively similar when we use national median.
[35] The data on state anti-bullying laws come from Sabia and Bass (2017) and Rees et al. (2022).

victimization and Sabia and Bass (2017) suggest these benefits extend to inducing a reduction in physical violence on school campuses.

The findings in columns (5) and (6) suggest that jurisdictions where there have been stricter anti-bullying policies implemented — where the quality of the peer environment has likely improved — have experienced much smaller "concentration effects" of in-person schooling on juvenile violence.

In columns (7) and (8), we use an alternate measure of ant-bullying law strength, defining "strong" anti-bullying laws based on the comprehensiveness and strength of the statute based on five bullying policies, including investigatory procedures, public reporting, and graduated sanctions for perpetrators of bullying.[36] The findings continue to show that improvements in the quality of the peer quality environment appears to mitigate the effect of in-person schooling on juvenile violence.

Finally, in Appendix Table 5, we include controls for both the strength of pre-treatment anti-bullying laws and school size (or pupil-teacher ratio). These descriptive results suggest both peer concentration and peer quality pathways.

5.6 Property Crime Comparison

The results presented above show strong evidence that in-person schooling leads to an increase in juvenile violent crime arrests. How does this finding compare to property crime arrests? The mechanisms to explain such effects may be quite different than for juvenile violence, where aggression among teens (especially male teens) may be an important motivator for such offenses. Moreover, there may be little scope for property crime in school given some limitations on expensive property carried on school property by teens. Thus, if juvenile property crime is motivated by income-generating purposes, there may be greater opportunities for larger payoff crimes outside of school.

In Figures 10-12 we explore seasonality in property crime arrests in the pre-pandemic period (Figure 10), explore heterogeneity in summer property crime arrests by June relative foot traffic in 2019 (Figure 11), and show event-study analyses of the effects of K-12 school foot traffic on property crime arrests using dCDH estimates (Figure 12). The pattern of findings across multiple sources of variation in in-person schooling provides no evidence that in-person schooling is causally

[36] The five requirements are that school districts must (i) provide written records of bullying and how each incident was resolved; (ii) implement strict investigatory procedures for bullying incidents; (iii) implement graduated sanctions for bullying; (iv) offer training to teachers, staff, and parents; and (v) clearly define the behaviors that constitute bullying. See Sabia and Bass (2017) for further discussion on each policy.

related to property crime arrests. The result also points to increased peer aggression due to concentration and negative peer interaction effects to explain our findings.

6. Conclusions

Investments in K-12 schooling generate important longer-run private and social benefits, including higher earnings, improvements in health, and lubricating the wheels of a well-functioning democracy. However, education policymakers are increasingly aware of social costs associated with negative peer interactions in school, including bullying victimization, as well as the unintended consequences of school discipline policies that may generate so-called pipelines to prison. Thus, nuanced education policy seeks to achieve the largest long-term economic benefits of schooling while minimizing short-run contemporaneous costs of negative school-based interactions.

This study explores the impact of in-person schooling on juvenile violence and in so doing, exploits a novel natural experiment generated by the COVID-19 pandemic with respect to school closings and reopenings. We introduce a novel proxy for in-person schooling by using SafeGraph data on foot traffic at Using data from the UCR and NIBRS covering the period prior to, during, and in the aftermath of COVID-19, and a difference-in-differences approach, we find that moving from fully closed to fully reopened schools generates an 18 to 28 increase in juvenile violent crime. Descriptive evidence from hospitalization and victimization data on juvenile violence suggests that these findings are not driven by an increase in reporting of criminal behavior. Moreover, the lack of evidence that in-person schooling affects reported property crime also points away from increased monitoring as a likely channel to explain our school violence results. Event-study analyses, including those based on TWFE and dCDH estimators, point to evidence consistent with parallel pre-treatment trends and support for the hypothesis that in-person schooling drives increases in juvenile violence.

An examination of heterogeneity in in-person schooling effects show that the effects we observe are largest for criminal offenses that occur on school property during school hours and appear to be driven largely by assaults. We further find that juvenile violent crime effects are largest for school districts with larger pupil-teacher ratios, suggesting that the "concentration effect" is important. However, we also find that the effects of in-person schooling on juvenile violence are mediated by states with strong, comprehensive anti-bullying laws, which have been documented to improve the quality of student peer interactions (Rees et al. 2022).

We also find increases in violent crime increases outside of school hours which suggests the interactions in school can also spill into time outside of school. This suggests the estimates identified in Jacob and Lefrgren (2003) and Jones and Karger (2023) maybe lower bounds. Their identification strategies, respectively based on the school in-service days and the start of the school year will be weighted to identify the short run impacts.

We generate a back-of-the-envelope estimate of the monthly contemporaneous annual crime cost of juvenile violence, using offense specific marginal effects reported in Tables 2 and 5, along with arrest estimates reported by the Federal Bureau of Investigation, and per-offense crime costs reported by McCollister et al. (2010). Together, these estimates suggest that full in-person schooling (relative to fully closed schools) generates contemporaneous juvenile violent crime costs of approximately $233 million per month.[37] These findings suggest that cost-effective policies that minimize the juvenile violence effects of negative peer interactions are likely to maximize the longer run social benefits of schooling.

[37] To generate this cost estimate, we first gather offense-specific Part I violent crimes committed in 2019 using the FBI's Crime in the United States report (available from: https://ucr.fbi.gov/crime-in-the-u.s/2019/crime-in-the-u.s.-2019/topic-pages/tables/table-1). We then use the UCR's Arrests by Age, Sex, and Race files from 2019 to calculate the offense-specific share of violent crimes committed by juveniles. To generate the offense-specific estimate of the number of crimes committed by juveniles, we calculate the product of total crime counts and the share of violent committed by juveniles. Using our findings from Table 2 (Panel II, column 5) and Table 5 (column 1), we estimate the number of additional violent crimes that were generated from full in-person schooling for each offense. Then, we use the per crime cost of $327,280 for rape, $57,510.85 for robbery, and $145,469 for assaults (in 2022USD) from McCollister et al. (2010) to estimate the total additional crime for each offense.

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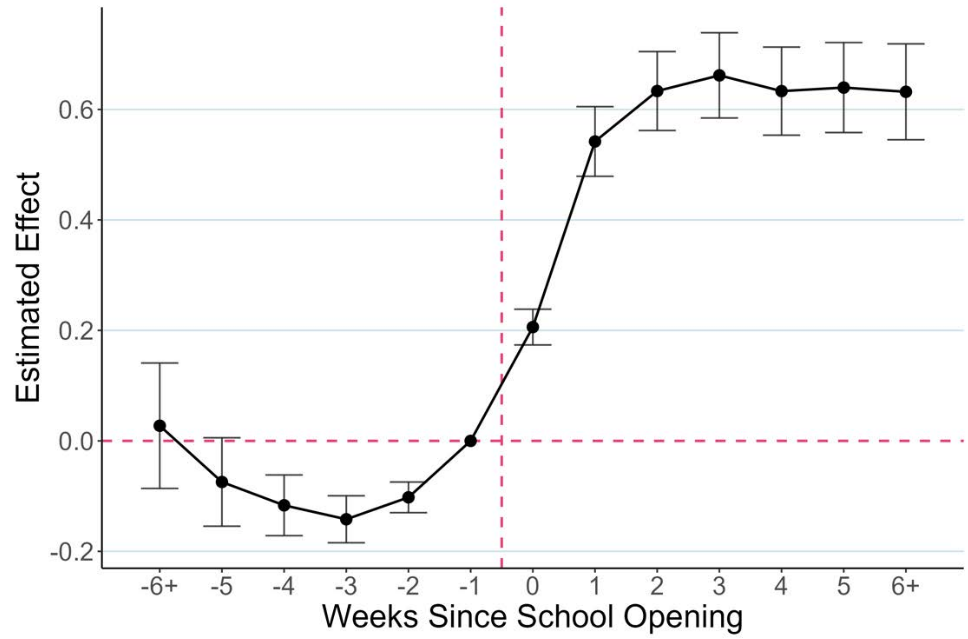
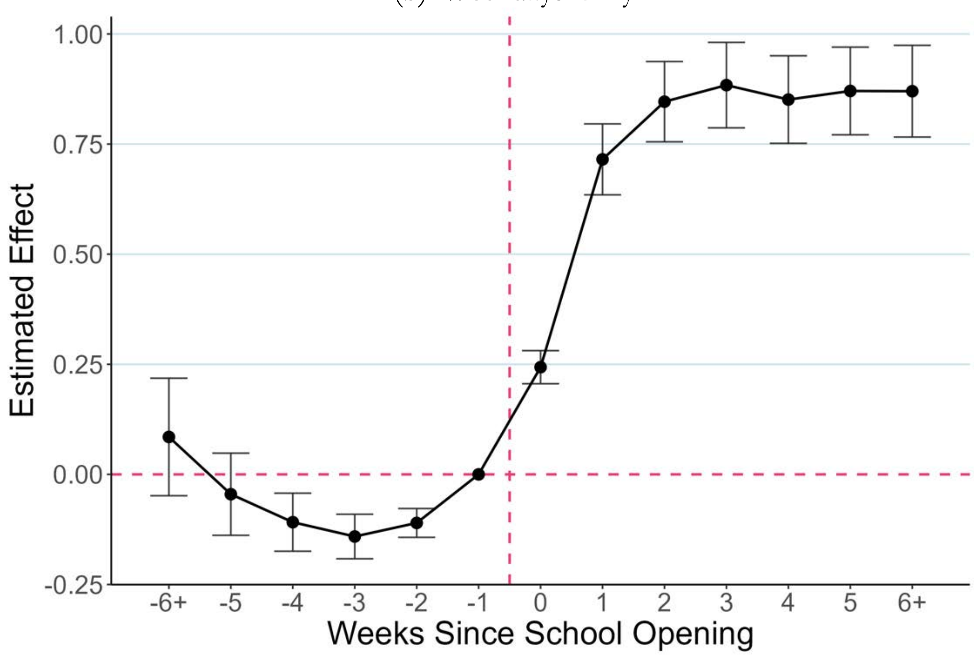
Figure 1. First Stage Estimate, Schooling Opening on Fall 2019 Foot Traffic, July 15-November 15, 2019

(a) Weekends and Weekdays

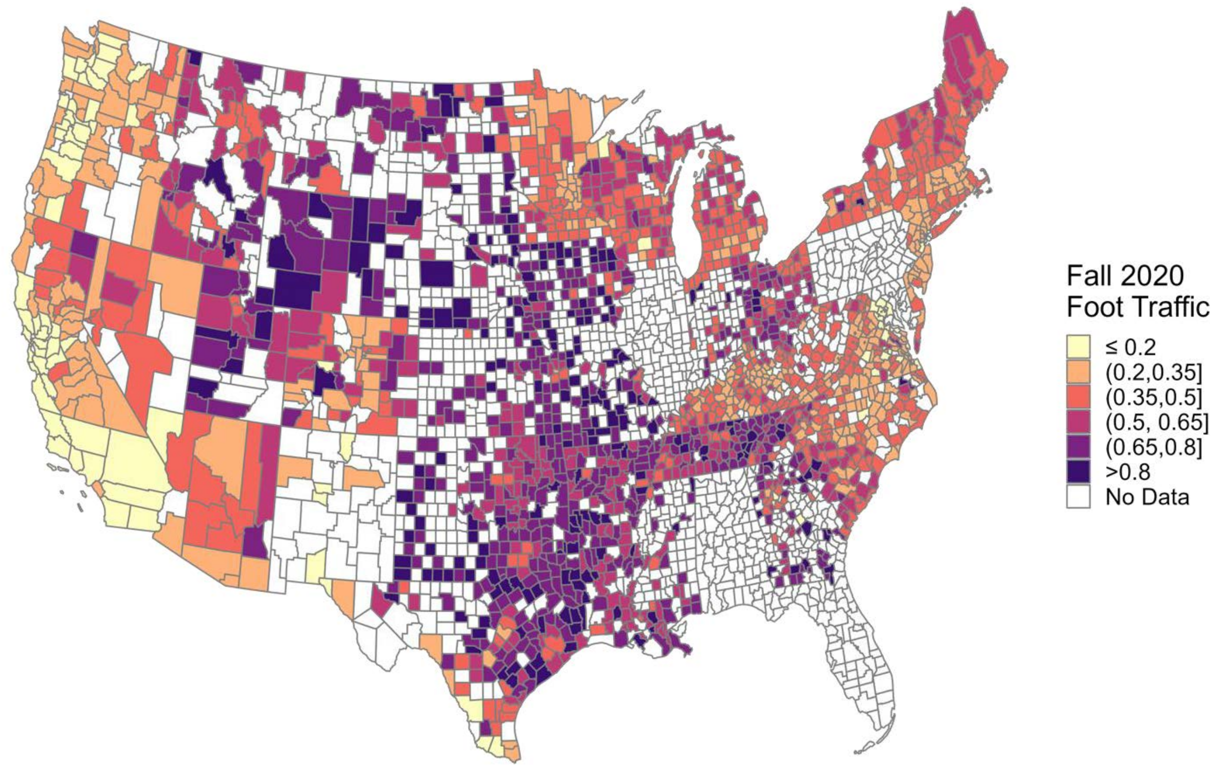


(b) Weekdays Only



Notes: Event-study estimates are generated from two-way fixed effects regressions using 2019 K-12 foot traffic data and school calendar data from publicholidays.com. Vertical bars represent 95% CIs generated using SEs clustered at the county-level. All estimates include controls for county and year fixed effects. Controls include the monthly unemployment rate, daily average temperature, and daily total precipitation.

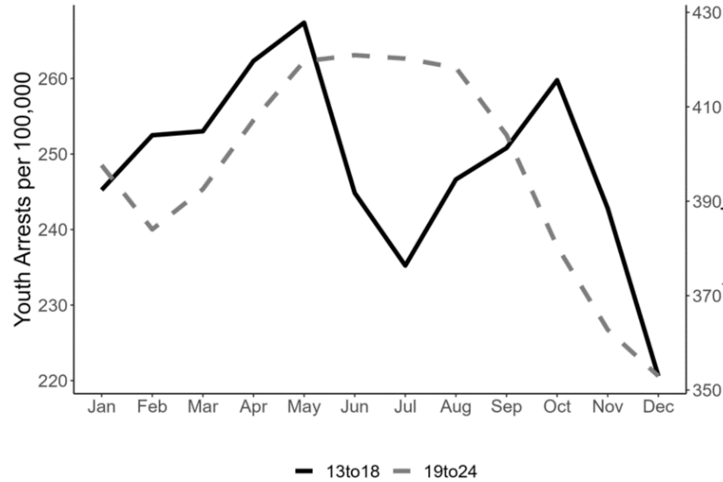
Figure 2. County-Level Variation in Fall 2020 Foot Traffic, Sample UCR-Reporting Counties



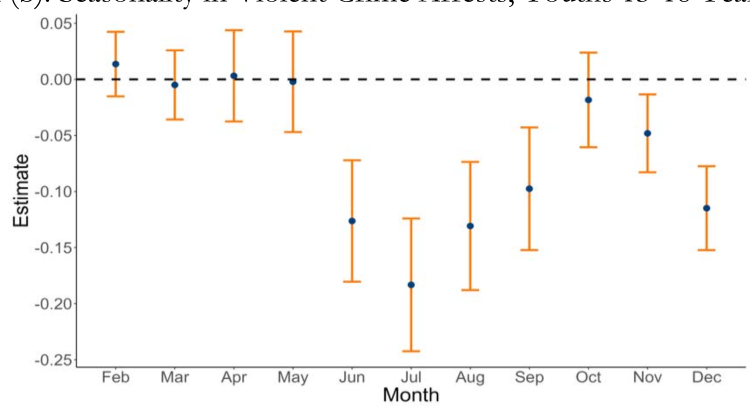
Notes: The map shows county-level K-12 foot traffic relative to the county's pre-pandemic (Jan 2020 and Feb 2020) K-12 foot traffic value. Only counties where we have both the Safegraph data and UCR data are shown. The sample UCR counties are defined as counties that report crimes in all 24 months between 2019 and 2020.

Figure 3. Seasonality in Violent Crime Arrests, UCR 2015-2019

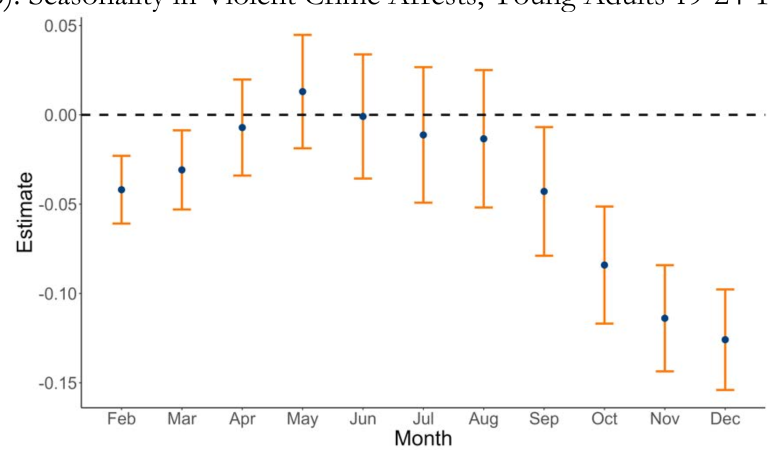
Panel (a): Seasonality Trend



Panel (b): Seasonality in Violent Crime Arrests, Youths 13-18 Years-Old



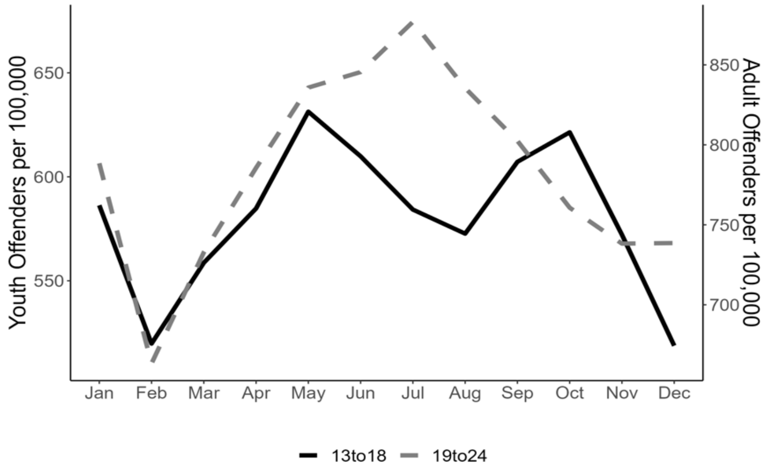
Panel (b): Seasonality in Violent Crime Arrests, Young Adults 19-24 Years-Old



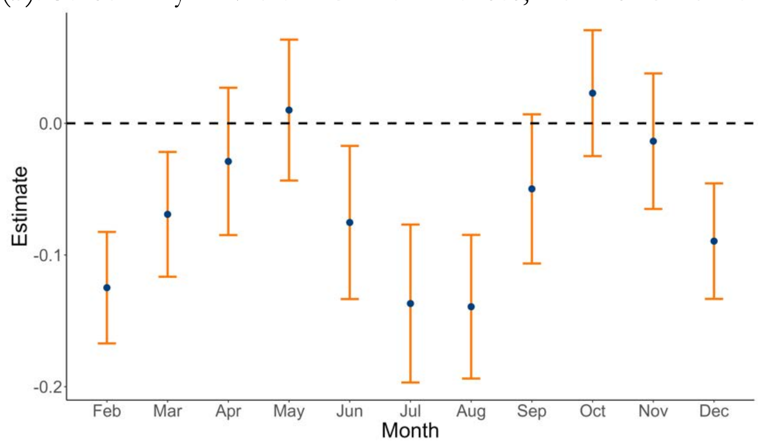
Notes: Panel (a) plots agency population weighted means of violent crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable, and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency and year fixed effects. Sample is restricted to agencies that report data in all 60 months. Standard errors are clustered at the agency-level.

Figure 4. Seasonality in Violent Criminal Incidents, NIBRS 2015-2019

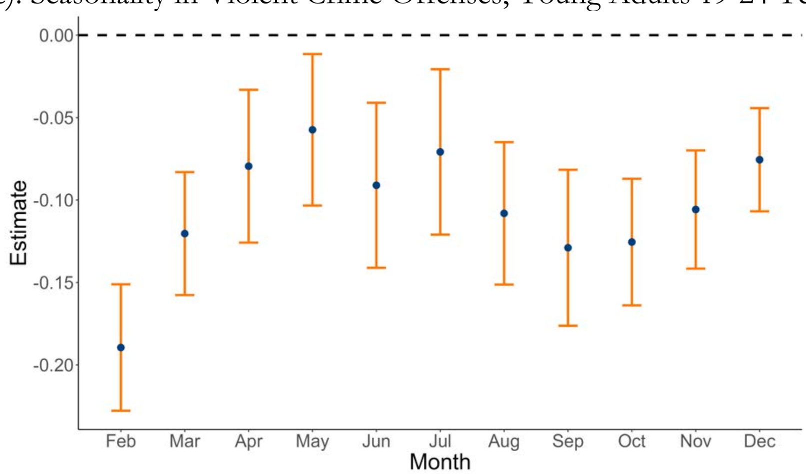
Panel (a): Seasonality Trend



Panel (b): Seasonality in Violent Crime Offenses, Youths 13-18 Years-Old



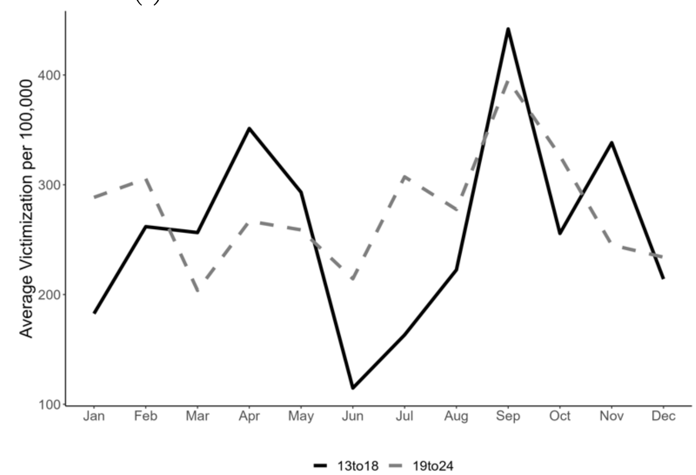
Panel (c): Seasonality in Violent Crime Offenses, Young Adults 19-24 Years-Old



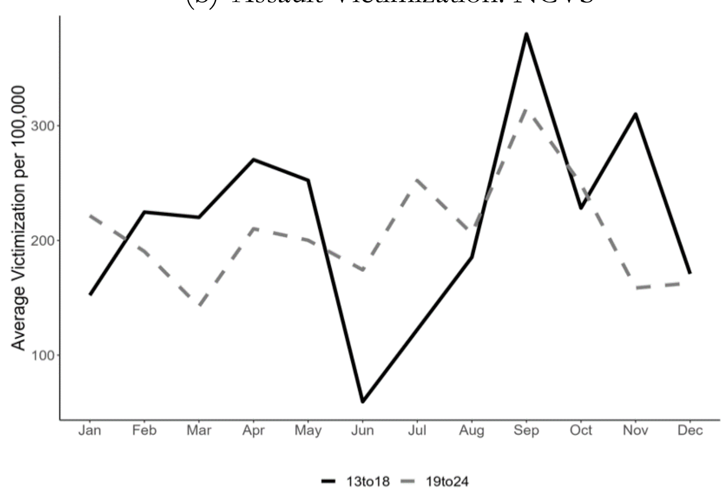
Notes: Panel (a) plots agency population weighted means of violent crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable,and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency, day of the week, and year fixed effects. Sample is restricted to agencies that report data in all 60 months. Standard errors are clustered at the agency-level.

Figure 5. Monthly Trend in Violent Crime, Hospitalization & Victimization, NEISS, and NCVS 2015-2019

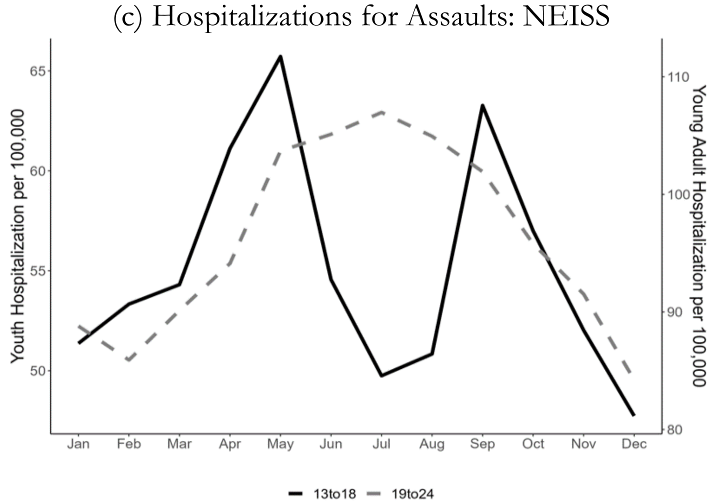
(a) Violent Crime Victimization: NCVS



(b) Assault Victimization: NCVS



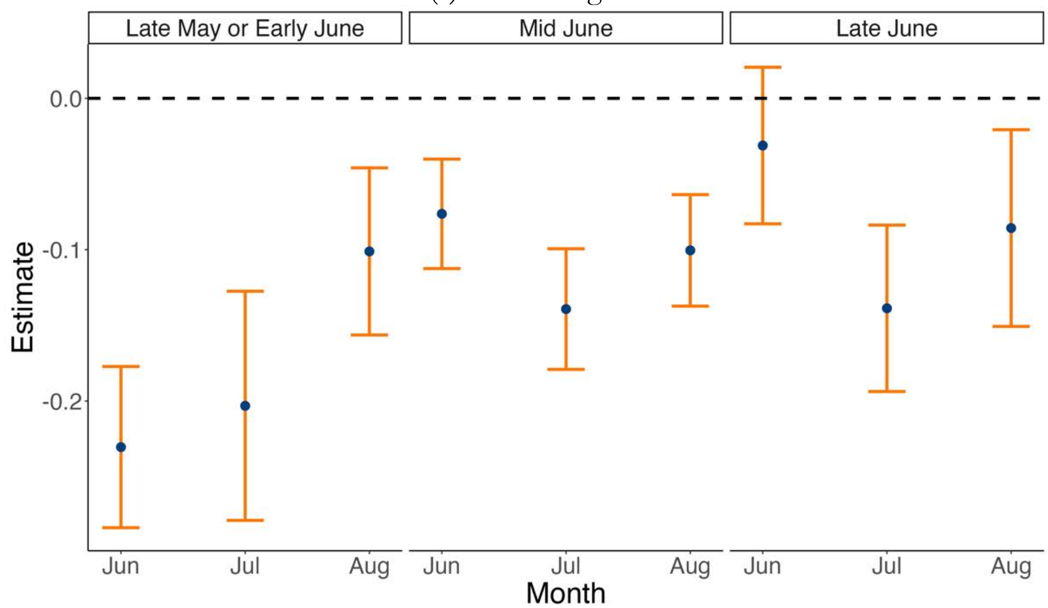
(c) Hospitalizations for Assaults: NEISS



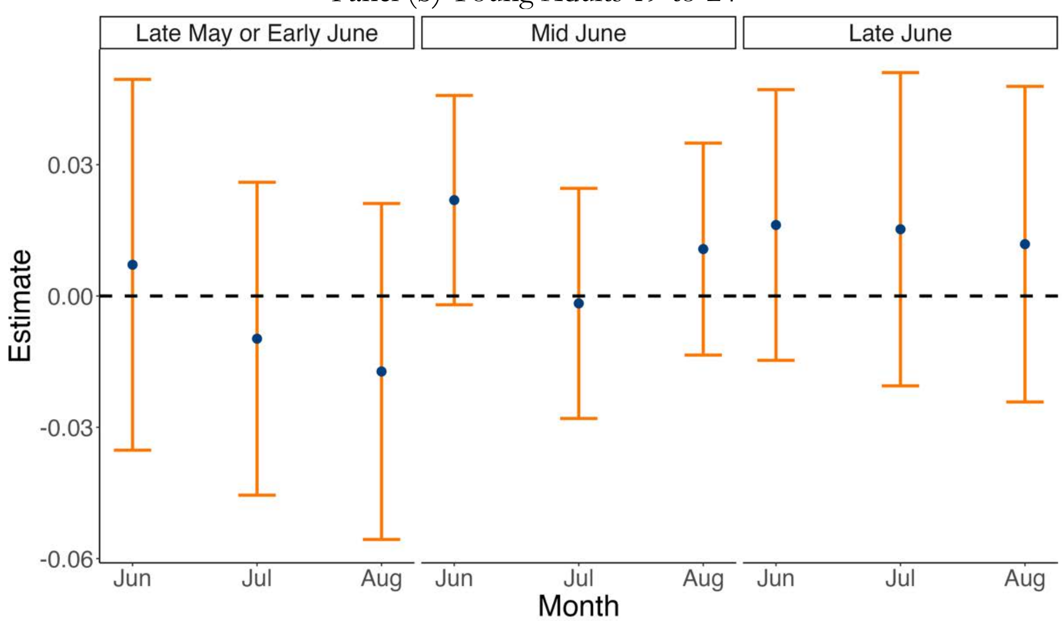
Notes: Figures are generated using data from the 2015-2019 NCVS (panels a and b) and NEISS (panel c). Weighted monthly trends are generated using NCVS and NEISS-provided sample weights.

Figure 6. Summer Effects of Arrests, UCR 2015-2019, by Foot Traffic in June (School Closing Month)

Panel (a) Youths Ages 13-18

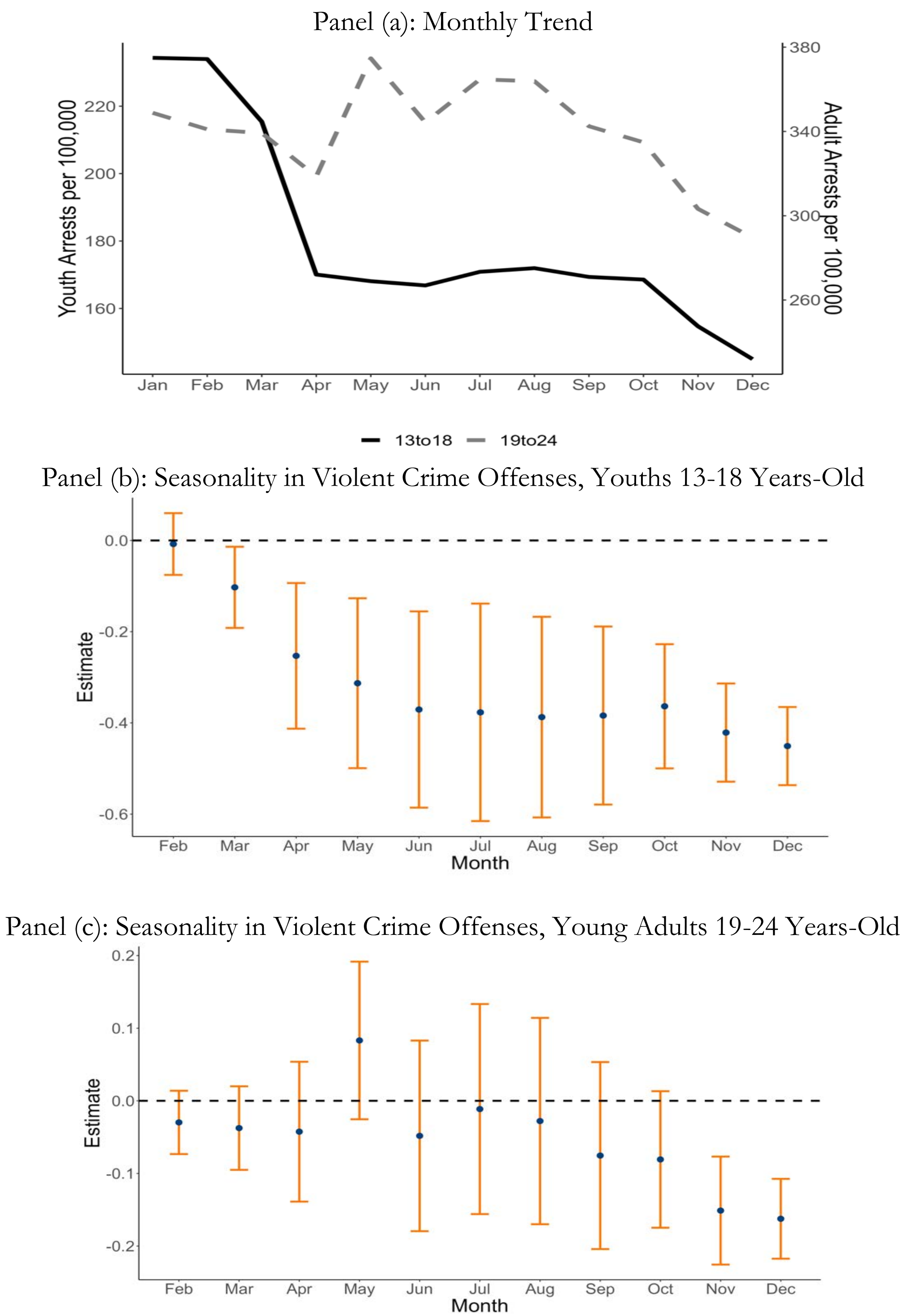


Panel (b) Young Adults 19-to-24



Notes: Estimates are generated using 2015 to 2019 agency-by-month data from the UCR. Only agencies that report crime in all 60 months are used. Each Poisson regression is estimated via weighted Poisson using population-day as an exposure variable. Vertical bars represent 95% CIs generated with standard errors clustered at the agency-level. All estimates include controls for agency and year fixed effects., monthly unemployment rate, monthly average temperature, and monthly total precipitation. Late May or Early June is defined as counties in the lowest tercile foot traffic in June 2019. Mid June is defined as counties in the middle tercile K-12 foot traffic in June 2019. Late June is defined as counties in the lowest tercile foot traffic in June 2019.

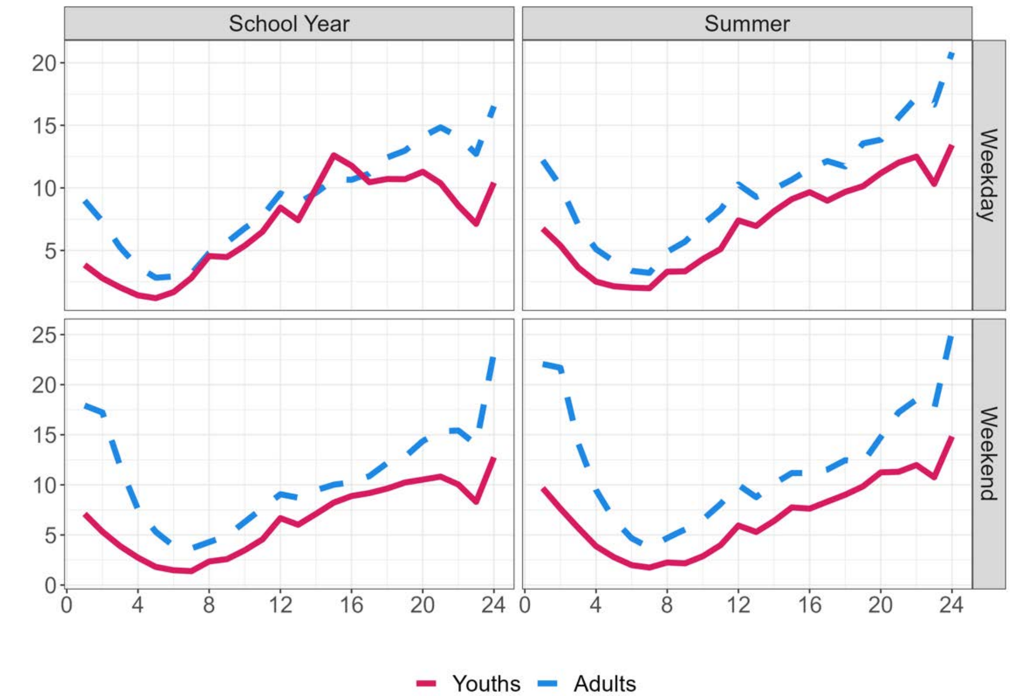
Figure 7. Monthly Trend in Violent Arrests, UCR 2020



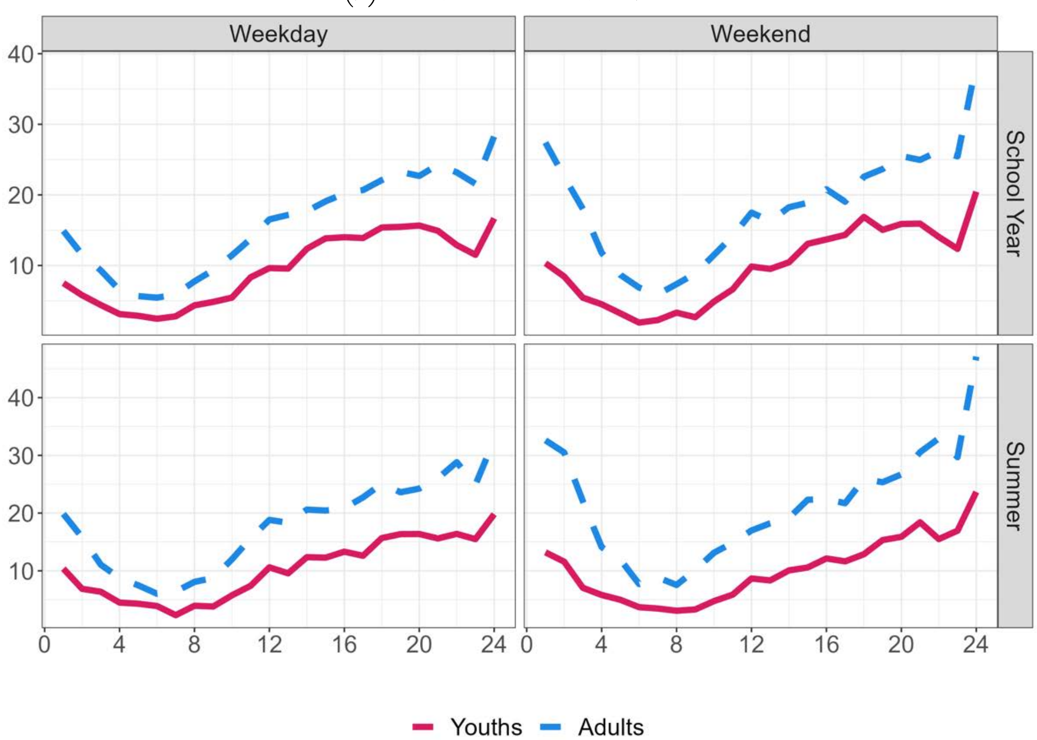
Notes: Panel (a) plots agency population weighted means of violent crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable, and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency, day and year fixed effects. Sample is restricted to agencies that report data in all 12 months. Standard errors are clustered at the agency-level.

Figure 8. Hourly Trend in Violent Crime Rates, NIBRS 2015-2020

Panel (a) January 2015- December 2019

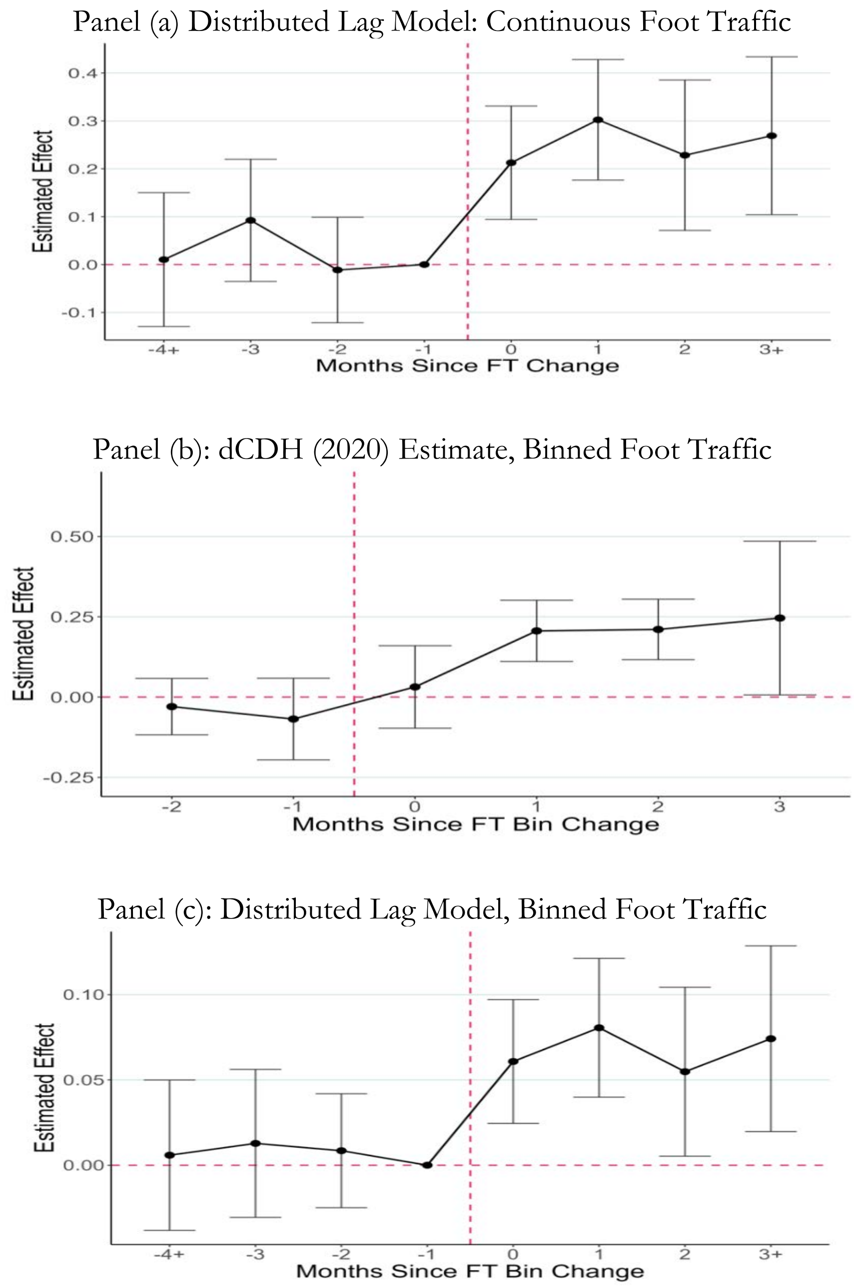


Panel (b) March 2020-December 2020



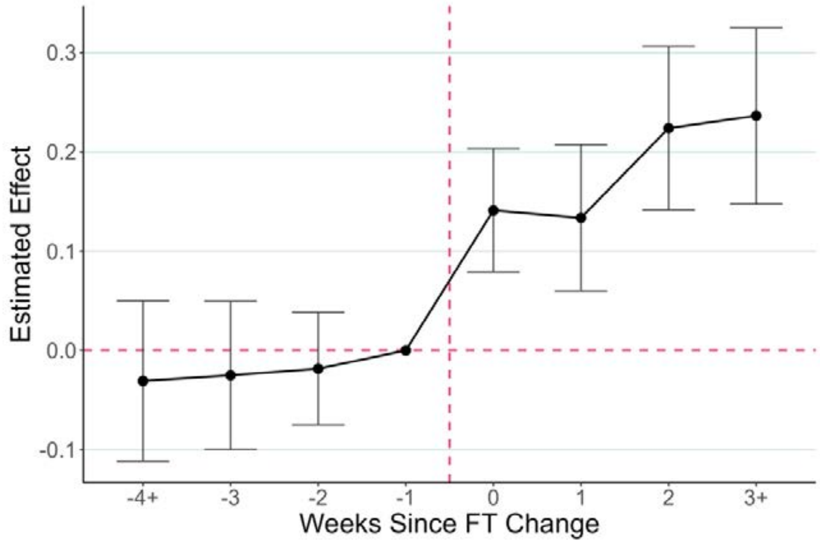
Notes: Figures are generated using data from the 2015-2019 NIBRS (panel a) and 2020 NIBRS (panel b). The hourly trends are weighted using the agency population.

Figure 9. Event-Study Analyses of K-12 School Foot Traffic and Juvenile vs Young Adult Violent Crime Arrests, UCR, 2019-2020



Notes: Fully interacted difference-in-difference-in-differences estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Regressions in panels (a) and (c) are estimated via weighted Poisson using population-day as an exposure variable. Regressions in panel (b) is estimated via dCDH (2020) estimator using rate of violent crime as the outcome variable. Bar lines represent 95% CIs generated using SEs clustered at the county-level. All estimates include controls for agency and month-by-year fixed effects. Controls include the monthly unemployment rate, yearly per capita income, monthly COVID-19 deaths, monthly foot traffic into restaurant or bars, monthly average temperature, and monthly total precipitation.

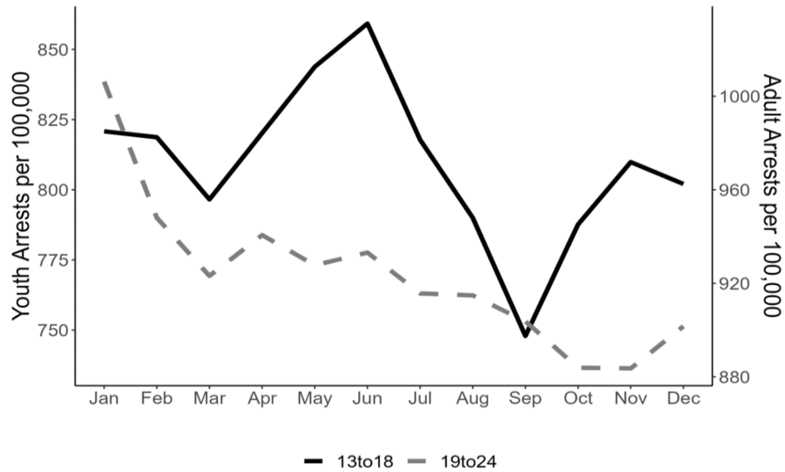
Figure 9. Event-Study Analyses of K-12 School Foot Traffic and Juvenile vs Young Adult Violent Crime Arrests, NIBRS, 2019-2021



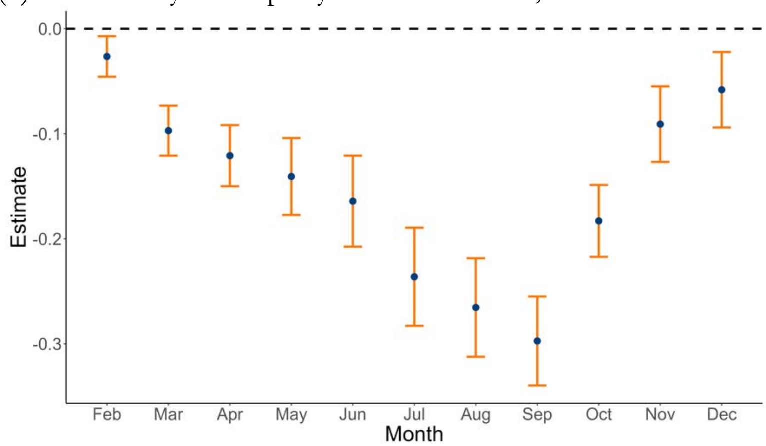
Notes: Fully interacted difference-in-difference-in-differences estimates are generated using 2019 to 2021 agency-by-day data from the NIBRS. Only agencies that report crime in all 36 months are used. Regression is estimated via weighted Poisson using population-day as an exposure variable. Bar lines represent 95% CIs generated using SEs clustered at the county-level. All estimates include controls for agency and day fixed effects. Controls include the monthly unemployment rate, yearly per capita income, daily COVID-19 deaths, daily foot traffic into restaurant or bars, daily average temperature, and daily total precipitation.

Figure 10. Seasonality in Property Crime Arrests, UCR 2015-2019

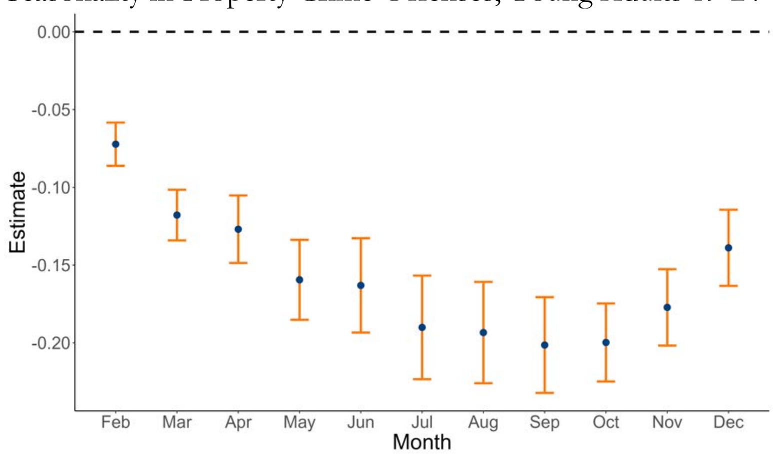
Panel (a): Seasonality Trend



Panel (b): Seasonality in Property Crime Offenses, Youths 13-18 Years-Old



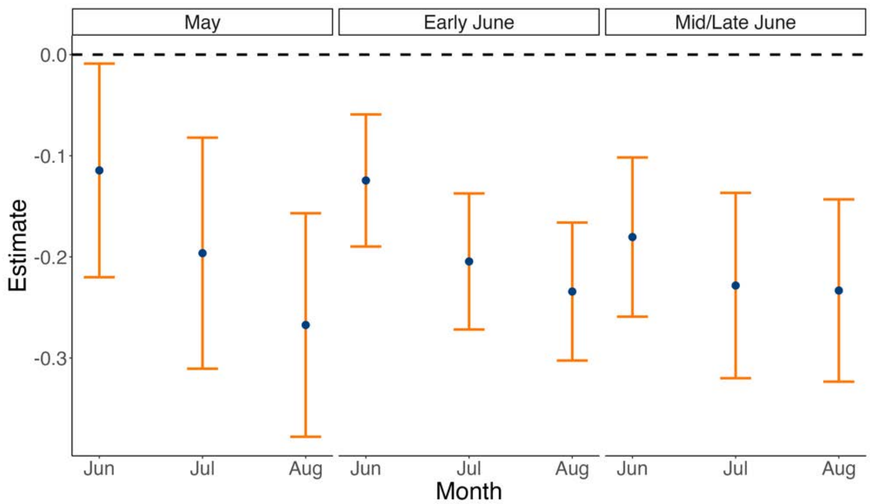
Panel (c): Seasonality in Property Crime Offenses, Young Adults 19-24 Years-Old



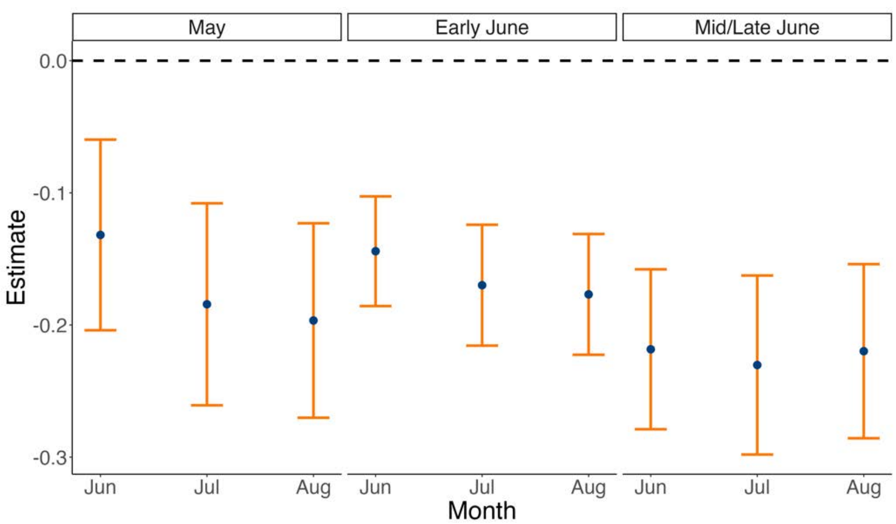
Notes: Panel (a) plots agency population weighted means of property crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable, and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency and year fixed effects. Sample is restricted to agencies that report data in all 60 months. Standard errors are clustered at the agency-level.

Figure 11. Summer Effects of Property Crime Arrests, UCR 2015-2019, by Foot Traffic in June (School Closing Month)

Panel (a) Youths Ages 13-18

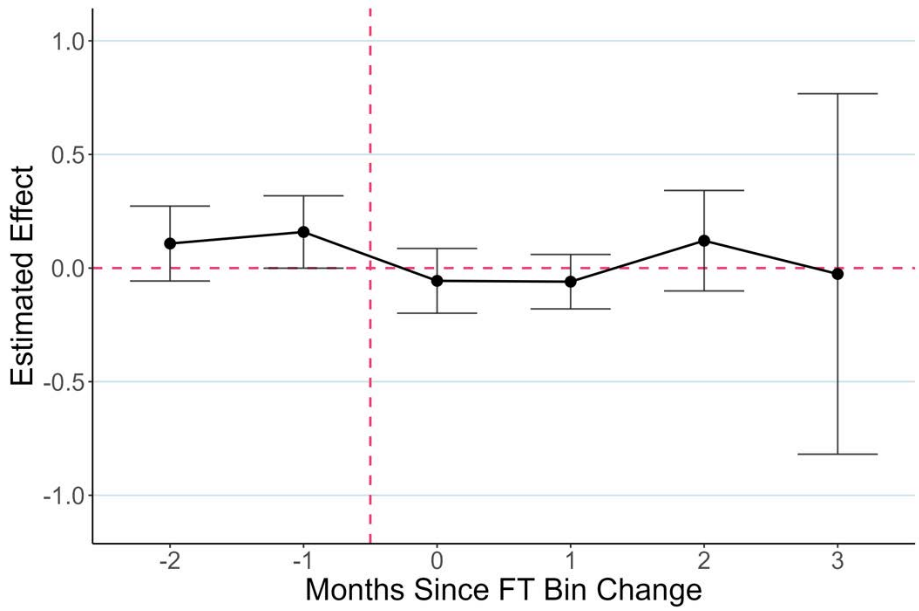


Panel (b) Young Adults 19-24



Notes: Estimates are generated using 2015 to 2019 agency-by-month data from the UCR. Only agencies that report crime in all 60 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. Bar lines represent 95% CIs generated using SEs clustered at the agency-level. All estimates include controls for agency and year fixed effects. Controls include the monthly unemployment rate, monthly average temperature, and monthly total precipitation. Late May is defined as counties in the lowest tercile foot traffic in June 2019. Early June is defined as counties in the middle tercile K-12 foot traffic in June 2019. Mid to Late June is defined as counties in the lowest tercile foot traffic in June 2019.

Figure 12. dCDH Event Study Analyses of K-12 School Foot Traffic and Juvenile vs Young Adult Property Crime Arrests, UCR, 2019-2020



Notes: Fully interacted difference-in-difference-in-differences estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Regression is estimated via dCDH (2020) estimator using rate of violent crime as the outcome variable. Bar lines represent 95% CIs generated using SEs clustered at the county-level. All estimates include controls for agency and month-by-year fixed effects. Controls include the monthly unemployment rate, yearly per capita income, monthly COVID-19 deaths, monthly foot traffic into restaurant or bars, monthly average temperature, and monthly total precipitation.

Table 1. Poisson Estimates of Monthly Seasonality Effects on Juvenile and Young Adult Violence, Pre-Pandemic (2015-2019) and Pandemic (2020) Periods, UCR

	2015-2019		2020	
	13-to-18	19-to-24	13-to-18	19-to-24
	(1)	(2)	(3)	(4)
February	0.014	-0.042***	-0.008	-0.030
	(0.015)	(0.010)	(0.035)	(0.022)
March	-0.005	-0.031***	-0.103**	-0.038
	(0.016)	(0.011)	(0.045)	(0.029)
April	0.003	-0.007	-0.253***	-0.042
	(0.021)	(0.014)	(0.081)	(0.049)
May	-0.002	0.013	-0.313***	0.083
	(0.023)	(0.016)	(0.095)	(0.055)
June	-0.126***	-0.001	-0.371***	-0.048
	(0.028)	(0.018)	(0.110)	(0.067)
July	-0.183***	-0.011	-0.377***	-0.011
	(0.030)	(0.019)	(0.122)	(0.074)
August	-0.131***	-0.013	-0.387***	-0.028
	(0.029)	(0.020)	(0.112)	(0.072)
September	-0.098***	-0.043**	-0.384***	-0.075
	(0.028)	(0.018)	(0.100)	(0.066)
October	-0.018	-0.084***	-0.364***	-0.081*
	(0.022)	(0.017)	(0.069)	(0.048)
November	-0.048***	-0.114***	-0.421***	-0.151***
	(0.018)	(0.015)	(0.055)	(0.038)
December	-0.115***	-0.126***	-0.451***	-0.162***
	(0.019)	(0.014)	(0.044)	(0.028)
N	644,520	644,520	155,832	155,832

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Estimates are generated using 2015 to 2020 agency-by-month data from the UCR. Only agencies that report crime in all 60 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the agency-level are reported inside the parenthesis. All estimates include controls for monthly unemployment rate, average temperature and total precipitation; and agency fixed effects. In addition, columns 1 and 2 include controls for yearly per capita income and year fixed effects. Columns 3 and 4 include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars

Table 2. Poisson Estimates of the Effect of School Foot Traffic on Violent Crime Arrests, UCR 2019-2020

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel I: DD Estimate, Ages 13-18						
K12FT	0.335***	0.280***	0.265***	0.270***	0.221***	0.231***	
	(0.050)	(0.046)	(0.046)	(0.048)	(0.047)	(0.055)	n/a
N	133,512	133,512	133,512	133,512	133,512	133,512	
	Panel II: DDD Estimate: Ages 13-18 vs Ages 19-24						
K12FT	0.239***	0.234***	0.238***	0.244***	0.192***	0.247***	0.248***
	(0.046)	(0.047)	(0.050)	(0.051)	(0.054)	(0.062)	(0.062)
N	267,024	267,024	267,024	267,024	267,024	267,024	267,024
	Panel III: DDD Estimate: Ages 16-17 vs Ages 19-20						
K12FT	0.195***	0.179**	0.188**	0.192**	0.189**	0.153	0.210**
	(0.070)	(0.071)	(0.074)	(0.075)	(0.084)	(0.099)	(0.095)
N	267,024	267,024	267,024	267,024	267,024	267,024	267,024
Controls:							
Agency & Year-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 & Dining Foot Traffic Controls	No	Yes	Yes	Yes	Yes	Yes	Yes
Macroeconomic Controls	No	No	Yes	Yes	Yes	Yes	Yes
Weather Controls	No	No	No	Yes	Yes	Yes	Yes
Census-Division Specific Year-by-Month FE	No	No	No	No	Yes	No	No
State-by-Year-by-Month FE	No	No	No	No	No	Yes	No
Agency-by-Year-by-Month FE	No	No	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Estimates are generated using 2019 and 2020 agency-by-month data from the UCR. In Panel I, we report the difference-in-differences estimate, and in Panel II, we report the fully interacted difference-in-difference-in-differences estimate. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for agency and month-by-year fixed effects. COVID-19 controls include monthly COVID-19 deaths and foot traffic into restaurant or bars. Macroeconomic controls include monthly unemployment rate and yearly per capita income. Weather controls include monthly average temperature and total precipitation.

Table 3. DDD Poisson Estimates of the Effect of School Foot Traffic on Arrests for Violent Arrests, UCR 2019-2020

	(1) Violent	(2) Aggravated Assault	(3) Rape	(4) Murder	(5) Robbery	(6) Simple Assault
	Panel I: DDD Model					
K12FT	0.244***	0.236***	0.137	0.146	0.268**	0.422***
	(0.051)	(0.057)	(0.173)	(0.329)	(0.105)	(0.041)
N	267,024	267,024	267,024	267,024	267,024	267,024
	Panel II: DDD Model Controlling for Agency-by-Year-by-Month FE					
K12FT	0.248***	0.248***	-0.072	-0.153	0.327**	0.405***
	(0.062)	(0.069)	(0.297)	(0.440)	(0.142)	(0.048)
N	267,024	267,024	267,024	267,024	267,024	267,024

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Fully Interacted DDD Estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects.

Table 4. Exploring Non-Linearities in the Effect of K-12 School Foot Traffic on Juvenile vs Young Adult Violent Crime Arrests, UCR 2019-2020

	(1) Violent	(2) Aggravated Assault	(3) Rape	(4) Murder	(5) Robbery	(6) Simple Assault
K12FT ≥100%	0.180***	0.195***	0.149	-0.002	0.159	0.320***
	(0.051)	(0.060)	(0.292)	(0.167)	(0.112)	(0.045)
50% < K12FT ≤100%	0.153***	0.176***	-0.081	-0.021	0.130*	0.256***
	(0.038)	(0.044)	(0.176)	(0.125)	(0.075)	(0.035)
25% < K12FT ≤50%	0.055**	0.046	0.016	-0.112	0.085*	0.093***
	(0.026)	(0.030)	(0.123)	(0.101)	(0.052)	(0.031)
N	267,024	267,024	267,024	267,024	267,024	267,024

***Statistically significant at 1% level **at 5% level *at 10% level

* P-val < 0.1; ** P-val < 0.05; *** P-val < 0.01
Notes: Fully Interacted DDD Estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects.

Table 5. DDD Poisson Estimates Effect of School Foot Traffic on Weekday Violent Criminal Incidents Among Juveniles versus Young Adults, NIBRS, 2019-2021

	(1)	(2)	(3)	(4)	(5)
	Violent	Aggravated Assault	Rape	Robbery	Simple Assault
K12FT	0.164***	0.247***	0.195***	-0.099	0.688***
	(0.035)	(0.040)	(0.062)	(0.074)	(0.025)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Fully Interacted DDD Estimates are generated using 2019 to 2021 agency-by-month data from the NIBRS. Only agencies that report crime in all 36 months are used. The sample is restricted to crime occurring during the weekdays. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for daily COVID-19 deaths, foot traffic into restaurant or bars, average temperature, and total precipitation; monthly unemployment rate; yearly per capita income; and agency and month-by-year fixed effects.

Table 6. Exploring Heterogeneity in Estimated Treatment Effect, by Day of Week of Offense and Location of Offense, NIBRS 2019-2021

	(1) Violent	(2) Aggravated Assault	(3) Rape	(4) Robbery	(5) Simple Assault
	Panel I: Weekend				
K12FT	0.100	0.006	0.055	0.324	0.105
	(0.122)	(0.153)	(0.273)	(0.313)	(0.069)
N	3,316,222	3,316,222	3,316,222	3,316,222	3,316,222
	Panel II: On Campus Weekday				
K12FT	0.607**	0.873**	0.352	0.377	0.604***
	(0.295)	(0.443)	(0.523)	(0.632)	(0.165)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel III: Off Campus Weekday				
K12FT	0.018	0.035	0.072	-0.149**	0.184***
	(0.034)	(0.039)	(0.063)	(0.075)	(0.023)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel IV: School Hours Weekday				
K12FT	0.329***	0.453***	0.345***	-0.063	0.978***
	(0.048)	(0.060)	(0.086)	(0.109)	(0.034)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel V: Non-School Hours Weekday				
K12FT	-0.017	0.020	-0.035	-0.137*	0.191***
	(0.042)	(0.052)	(0.093)	(0.082)	(0.030)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Estimates are generated using 2019 to 2021 agency-by-day data from the NIBRS. Only agencies that report crime in all 36 months are used. In panel I, the sample is restricted to crime occurring during the weekends. In panels II to V, the sample is restricted to crime occurring during the weekdays. School hour is defined as 7am to 5pm. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for daily COVID-19 deaths, foot traffic into restaurant or bars, average temperature, and total precipitation; monthly unemployment rate; yearly per capita income; and agency and month-by-year fixed effects.

Table 7. Exploring Heterogeneity in Estimated Treatment Effect, by Race and Age of Offender, NIBRS 2019-2021

	(1) Violent	(2) Aggravated Assault	(3) Rape	(4) Robbery	(5) Simple Assault
	Panel I: White Offenders				
K12FT	0.231***	0.298***	0.177**	-0.032	0.625***
	(0.039)	(0.050)	(0.072)	(0.114)	(0.027)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel II: Black Offenders				
K12FT	0.063	0.173***	0.175	-0.154	0.801***
	(0.054)	(0.058)	(0.134)	(0.094)	(0.039)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel III: 13-16 Year-Olds				
K12FT	0.271***	0.411***	0.255***	-0.115	0.897***
	(0.047)	(0.052)	(0.074)	(0.097)	(0.030)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028
	Panel IV: 17-18 Year-Olds				
K12FT	0.062	0.068	0.205**	-0.133	0.288***
	(0.039)	(0.050)	(0.084)	(0.081)	(0.026)
N	8,333,028	8,333,028	8,333,028	8,333,028	8,333,028

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Estimates are generated using 2019 to 2021 agency-by-day data from the NIBRS. Only agencies that report crime in all 36 months are used. The sample is restricted to crime occurring during the weekdays. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for daily COVID-19 deaths, foot traffic into restaurant or bars, average temperature, and total precipitation; monthly unemployment rate; yearly per capita income; and agency and month-by-year fixed effects.

Table 8. Heterogeneity in Estimated Treatment Effects, by School Characteristics & Anti-Bullying Laws, UCR 2019-2020

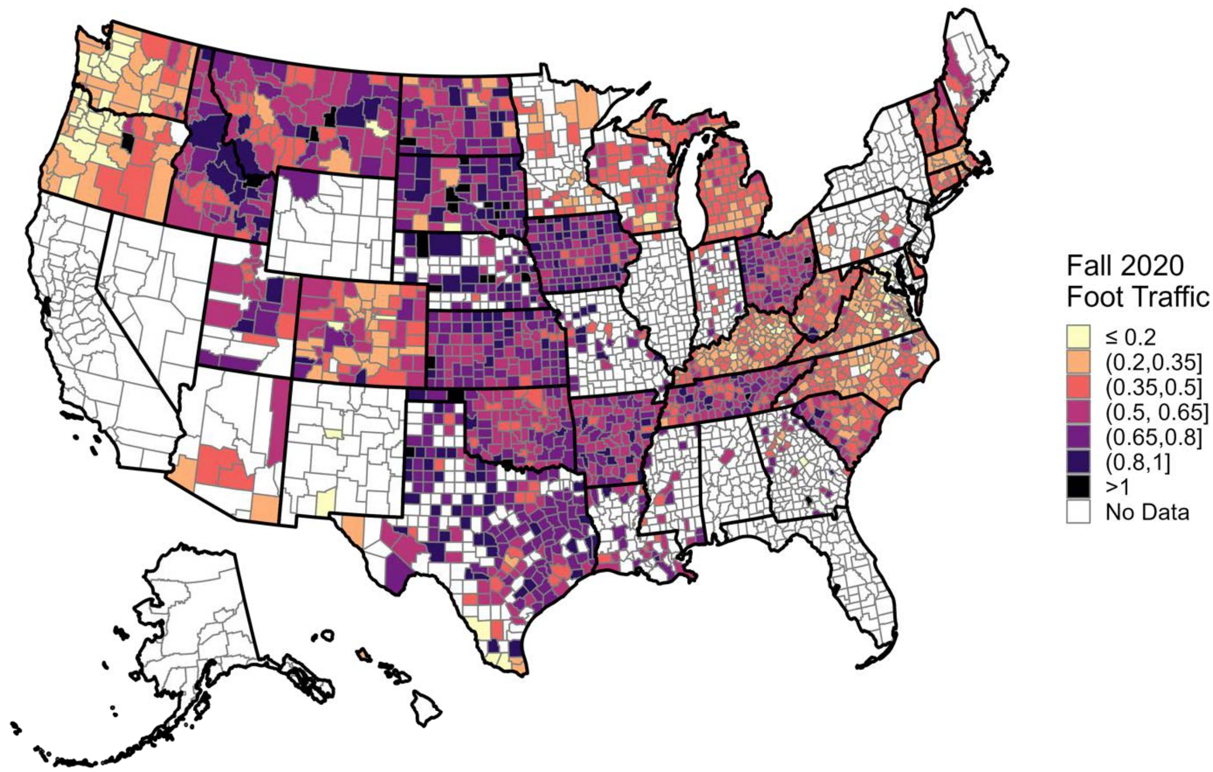
	School Size				Anti-Bullying Laws			
	Average Enrollment		Student per Teacher		DOE Rating		# of Laws	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
K12FT * < Median School Size	0.149*	0.155	0.133	0.127				
	(0.090)	(0.124)	(0.093)	(0.120)				
K12FT * ≥ Median School Size	0.324***	0.352***	0.283***	0.291**				
	(0.060)	(0.108)	(0.060)	(0.113)				
K12FT * Weak ABL					0.329***	0.375***	0.265***	0.276***
					(0.065)	(0.113)	(0.056)	(0.101)
K12FT * Strong ABL					0.039	0.059	0.047	0.036
					(0.087)	(0.121)	(0.168)	(0.198)
Interaction of Income, Expenditure & Race with Treatment	No	Yes	No	Yes	No	Yes	No	Yes
N	267,024	267,024	267,024	267,024	267,024	267,024	267,024	267,024

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Fully interacted DDD estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects. The data on school characteristics are from the NCES CCD, and the data on anti-bulling laws are from Sabia and Bass (2017) and Rees et al. (2023). School characteristics are at the county-level and anti-bullying laws are at the state-level. The median average enrollment is 390 and the median student per teacher ratio is 14.5. In columns 5 and 6, strong ABL is defined as states with DOE rating greater than 18 (the median DOE rating). In columns 7 and 8, strong ABL is defined as any states with three or more of the following components: written records and reporting, investigations, consequences, training and transparency, and legal definitions (Sabia and Bass 2017; Rees et al. 2023). In columns 2, 4, 6, 8 we control for the interaction of our treatment variable with 2017-2018 per capita income, expenditure per pupil, and the share of racial minorities. The data on per capita income comes from the BEA and the data on expenditure and racial composition comes from NCES CCD.

Appendix Tables and Figures

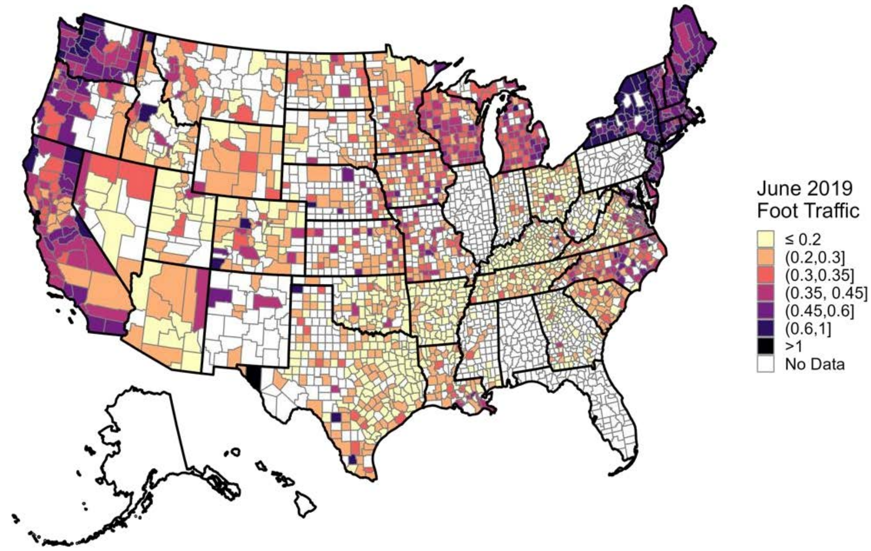
Appendix Figure 1. Variation in Fall 2020 Foot Traffic, NIBRS Counties



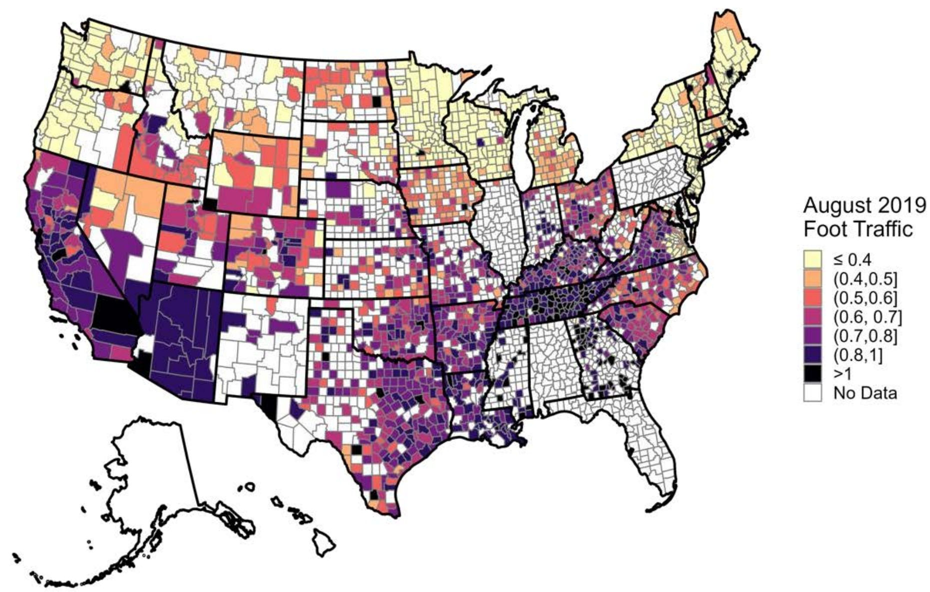
Notes: The map shows county-level K-12 foot traffic relative to the county's pre-pandemic (Jan 2020 and Feb 2020) K-12 foot traffic value. Only counties where we have both the Safegraph data and NIBRS data are shown. The sample NIBRS counties are defined as counties that report crimes in all 36 months between 2019 and 2021.

Appendix Figure 2. County-Level Variation in June and August 2019 Foot Traffic, Sample UCR-Reporting Counties

Panel (a) June 2019

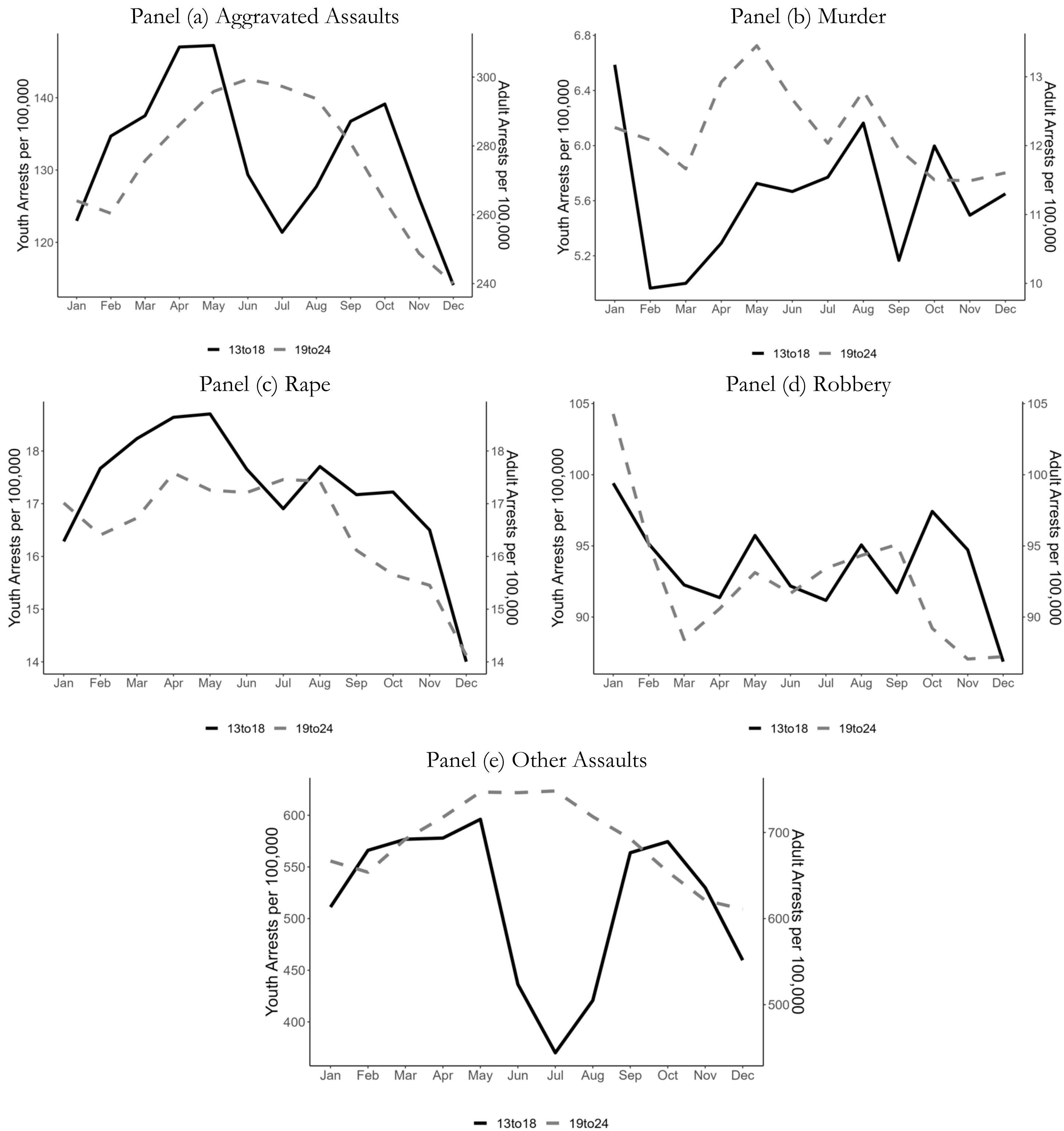


Panel (b) August 2019



Notes: The map shows county-level K-12 foot traffic relative to the county's pre-pandemic (Jan 2020 and Feb 2020) K-12 foot traffic value. Only counties where we have both the Safegraph data and UCR data are shown. The sample UCR counties are defined as counties that report crimes in all 24 months between 2019 and 2020.

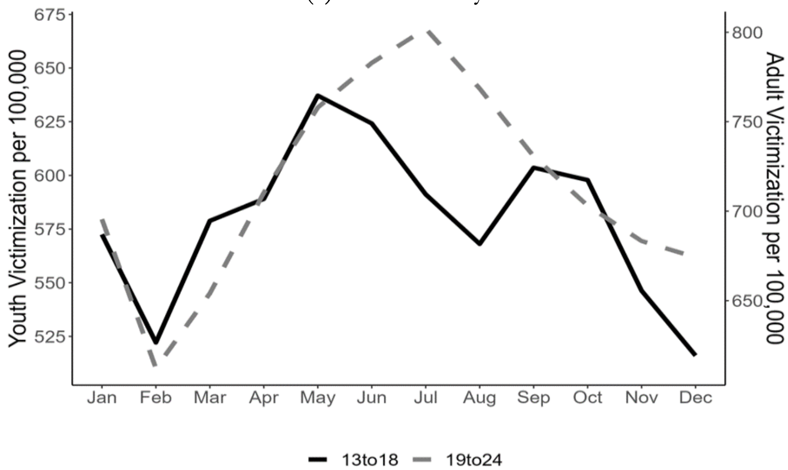
Appendix Figure 3. Monthly Trend in Specific Violent Crime & Other Assaults, UCR 2015-2019



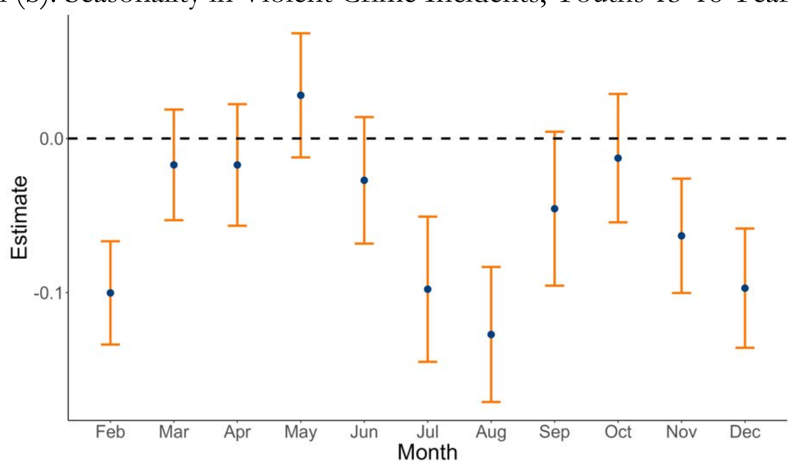
Notes: Population weighted means of violent crime arrests are reported using data from the Uniform Crime Reports. Sample is restricted to agencies that report data in all 60 months.

Appendix Figure 4. Seasonality in Violent Victimizations, NIBRS 2015-2019

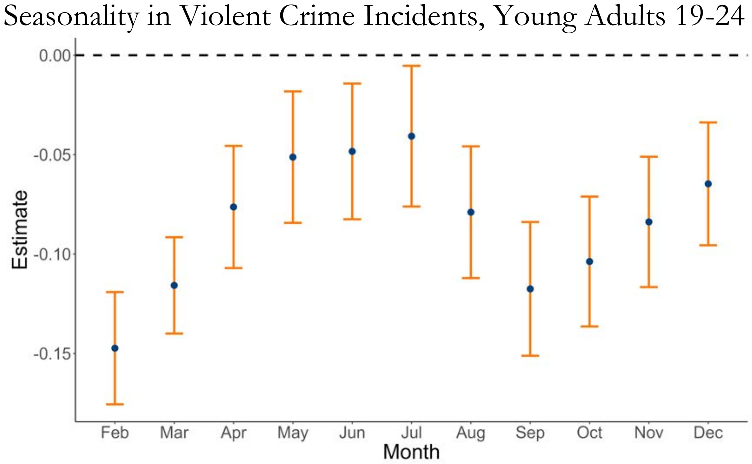
Panel (a): Seasonality Trend



Panel (b): Seasonality in Violent Crime Incidents, Youths 13-18 Years-Old



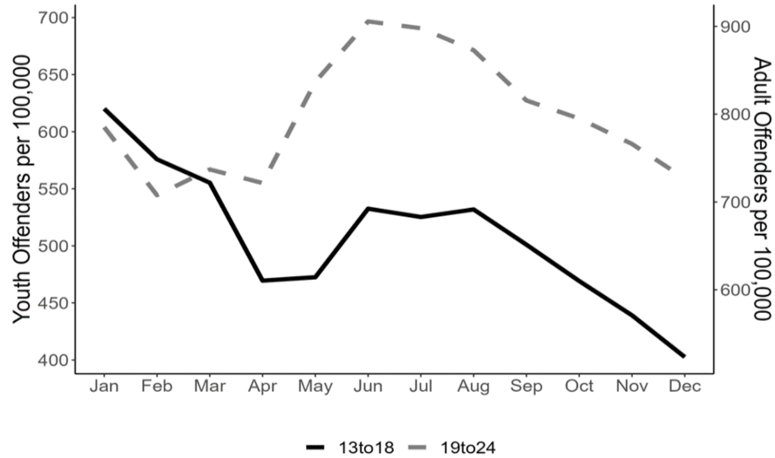
Panel (c): Seasonality in Violent Crime Incidents, Young Adults 19-24 Years-Old



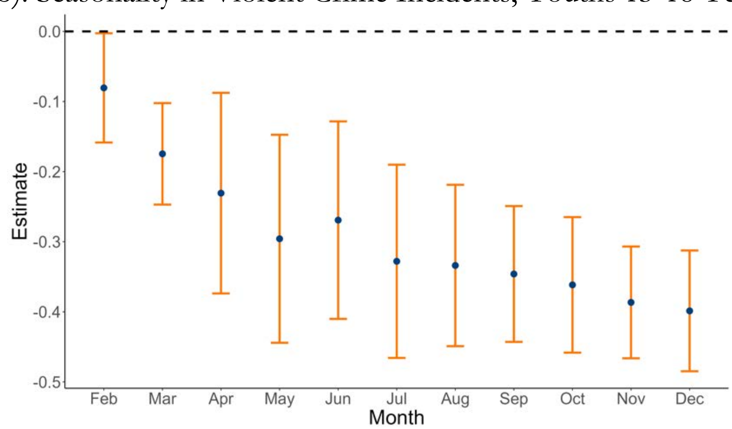
Notes: Panel (a) plots agency population weighted means of violent crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable, and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency, day of the week, and year fixed effects. Sample is restricted to agencies that report data in all 60 months. Standard errors are clustered at the agency-level.

Appendix Figure 5. Seasonality Trends in Violent Offenses, NIBRS 2020

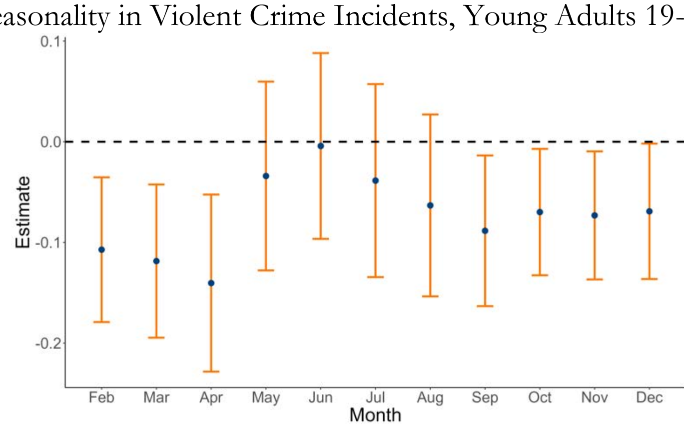
Panel (a): Seasonality Trend



Panel (b): Seasonality in Violent Crime Incidents, Youths 13-18 Years-Old

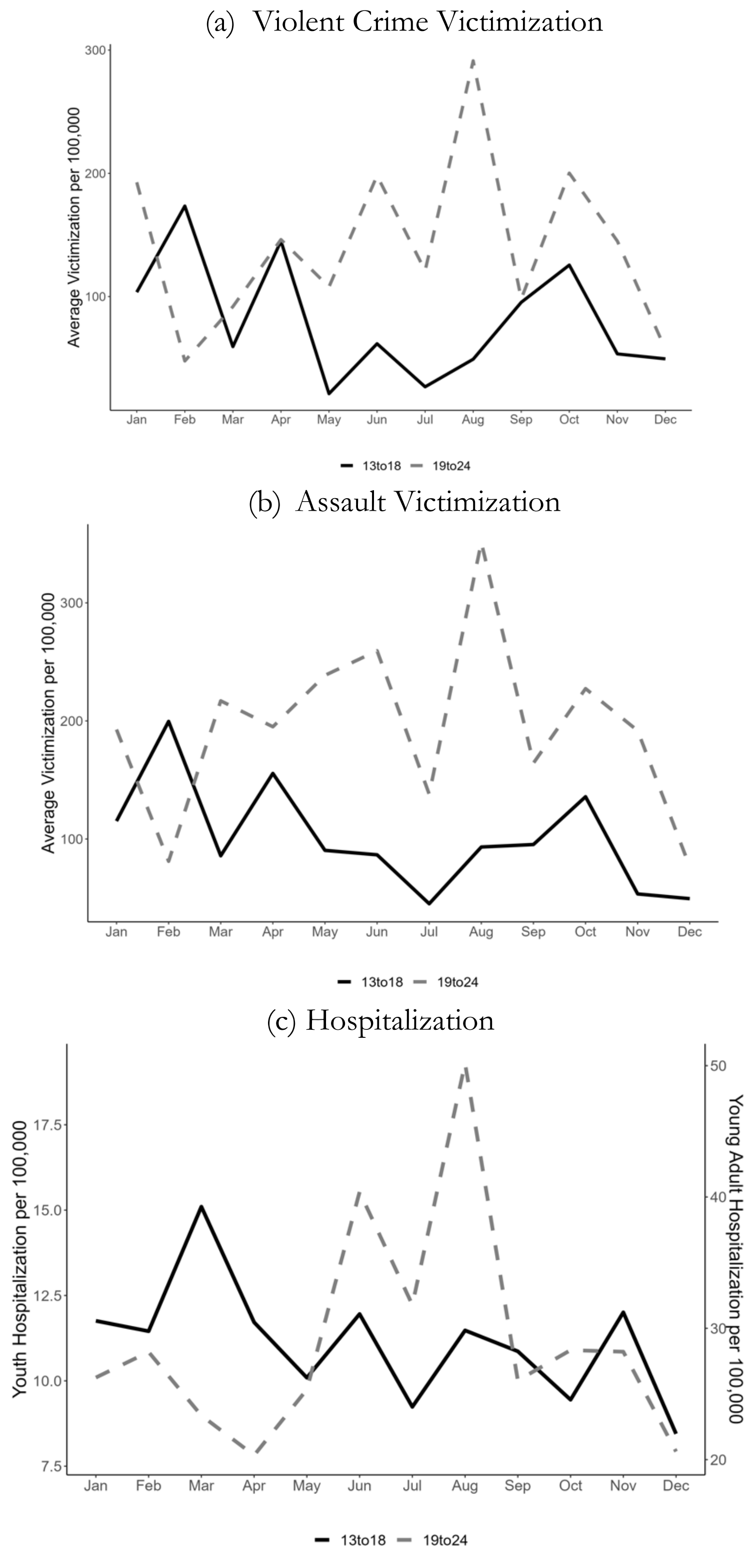


Panel (c): Seasonality in Violent Crime Incidents, Young Adults 19-24 Years-Old



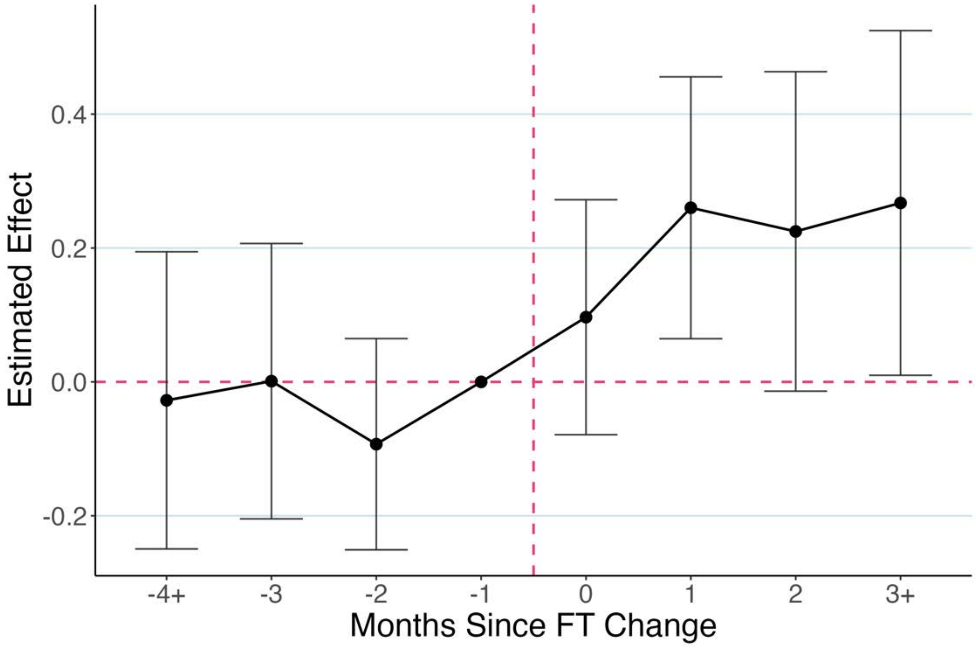
Notes: Panel (a) plots agency population weighted means of violent crime arrests. Panels (b) and (c) plot monthly 95% confidence intervals via Poisson regression using agency population*days in month as the exposure variable, and controlling macroeconomic conditions (unemployment rate and per capita income), weather (average temperature and precipitation), and agency, day of the week, and year fixed effects. Sample is restricted to agencies that report data in all 12 months. Standard errors are clustered at the agency-level.

Appendix Figure 6. Monthly Trend in Violent Crime Hospitalization & Victimization, NEISS & NCVS 2020



Notes: Figures are generated using data from the 2015-2019 NCVS (panels a and b) and NEISS (panel c). Weighted monthly trend are generated by sample weights provided by the NCVS and NEISS surveys, respectively.

Appendix Figure 7. Event-Study Analyses of K-12 School Foot Traffic and 16-17-Year-Old vs 19-20-Year-Old Violent Crime Arrests, UCR, 2019-2020



Notes: Fully interacted difference-in-difference-in-differences estimates are generated using 2019 to 2020 agency-by-month data from the UCR. Only agencies that report crime in all 25 months are used. Regression is estimated via weighted Poisson using population-day as an exposure variable. Bar lines represent 95% CIs generated using SEs clustered at the county-level. All estimates include controls for agency and month-by-year fixed effects. Controls include monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects.

Appendix Table 1. Descriptive Statistics, Arrests and Incidents

	Youths Ages 13-18	Young Adults Ages 19-24
	Panel I: UCR 2015-2019, Arrests	
Violent Crime	250.66	405.17
	(426.6)	(559.66)
Aggravated Assault	132.94	279.38
	(303.76)	(441.66)
Rape	17.12	16.71
	(106.41)	(104.91)
Murder	5.57	12.47
	(46.51)	(69.36)
Robbery	95.03	96.61
	(236.13)	(235.13)
Simple Assault	521.96	708.69
	(784.47)	(883.98)
	Panel II: UCR 2019-2020, Arrests	
Violent Crime	210.21	355.11
	(376.24)	(494.29)
Aggravated Assault	112.90	257.66
	(268.38)	(404.16)
Rape	14.97	14.94
	(95.71)	(97.11)
Murder	6.09	12.51
	(57.91)	(79.3)
Robbery	76.26	70.00
	(206.21)	(181.84)
Simple Assault	398.48	587.06
	(704.31)	(774.87)
	Panel III: NIBRS 2019-2021, Incidents	
Violent Crime	538.68	765.7
	(3433.64)	(3615.07)
Aggravated Assault	249.00	448.89
	(2536.86)	(2887.98)
Rape	94.66	76.68
	(1521.71)	(1278.51)
Robbery	191.37	231.36
	(1689.01)	(1580.96)
Simple Assault	1127.12	1323.77
	(5710)	(5243.4)

Appendix Table 1, Continued

	Youths Ages 13-18	Young Adults Ages 19-24
	Panel IV: NCVS & NEISS 2015-2019	
Violent Crime Victimization	244.327	262.782
	(145.115)	(109.177)
Assault Victimization	203.616	197.464
	(135.643)	(101.024)
Hospitalization	55.07	96.15
	(8.534)	(13.539)

Notes: Weighted means are shown. In panels I to III, we use agency population as the sample weight, and in panel IV, we use NEISS and NCVS provided weights.

Appendix Table 2. Summer Seasonality Effects on Youth Victimization and Hospitalization: 2015-2019 NCVS and NEISS

	Victimization				Hospitalization	
	Violent Crime		Assault		Assault	
	(1)	(2)	(3)	(4)	(5)	(6)
June	-0.762**	-0.302	-1.451***	-0.171	-0.291***	-0.020
	(0.369)	(0.311)	(0.490)	(0.360)	(0.057)	(0.051)
July	-0.530	-0.022	-0.662*	0.132	-0.416***	-0.051
	(0.374)	(0.307)	(0.345)	(0.347)	(0.049)	(0.067)
August	-0.240	-0.023	-0.319	-0.004	-0.366***	-0.049
	(0.418)	(0.249)	(0.426)	(0.275)	(0.056)	(0.046)
Sample	Youths	Adults	Youths	Adults	Youths	Adults

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: OLS estimates are generated using 2015 to 2019 NCVS (columns 1 to 4) and NEISS (columns 5 and 6). Each regression is estimated via weighted Poisson using population-day as an exposure variable. Newey-West SEs using are reported inside the parenthesis. All estimates include controls for agency and day fixed effects. Controls include the monthly unemployment rate, yearly per capita income, monthly COVID-19 deaths, monthly foot traffic into restaurant or bars, monthly average temperature, and monthly total precipitation.

Appendix Table 3. DD Poisson Estimates of the Effect of School Foot Traffic on Violent Crime Arrests, Ages 19-24, UCR 2019-2020

	(1)	(2)	(3)	(4)	(5)	(6)
K-12 School Foot Traffic	0.096***	0.046	0.026	0.026	0.013	-0.016
	(0.037)	(0.034)	(0.032)	(0.033)	(0.031)	(0.041)
N	133,512	133,512	133,512	133,512	133,512	133,512
Controls:						
Agency & Year-by-Month FE	Yes	Yes	Yes	Yes	Yes	Yes
COVID-19 & Dining Foot Traffic Controls	No	Yes	Yes	Yes	Yes	Yes
Macroeconomic Controls	No	No	Yes	Yes	Yes	Yes
Weather Controls	No	No	No	Yes	Yes	Yes
Census-Division Specific Year-by-Month FE	No	No	No	No	Yes	No
State-by-Year-by-Month FE	No	No	No	No	No	Yes

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: DD Estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for agency and month-by-year fixed effects. COVID-19 controls include monthly COVID-19 deaths and foot traffic into restaurant or bars. Macroeconomic controls include monthly unemployment rate and yearly per capita income. Weather controls include monthly average temperature and total precipitation.

Appendix Table 4. Sensitivity of UCR DDD Estimates to OLS, UCR 2019-2020

	(1)	(2)	(3)	(4)	(5)	(6)
	Violent	Aggravated Assault	Rape	Murder	Robbery	Simple Assault
FT * Youths 13-to-18	34.69***	17.08*	-0.21	0.47	17.36***	108.27***
	(12.58)	(9.29)	(2.82)	(2.30)	(6.64)	(20.66)
Mean of DV	210.21	112.90	14.97	6.09	76.26	398.48
N	267,024	267,024	267,024	267,024	267,024	267,024

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: DDD Estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted OLS using population-day as the weight. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects.

Appendix Table 5. Heterogeneity in Estimated Treatment Effects, by School Characteristics & Anti-Bullying Laws, UCR 2019-2020

	(1)	(2)	(3)	(4)
K12FT * < Median School Size * Weak ABL	0.295**	0.169	0.216	0.170
	(0.146)	(0.126)	(0.131)	(0.122)
K12FT * ≥ Median School Size * Weak ABL	0.415***	0.352***	0.402***	0.311***
	(0.116)	(0.112)	(0.124)	(0.114)
K12FT * < Median School Size * Strong ABL	-0.002	-0.121	-0.048	-0.245
	(0.141)	(0.249)	(0.141)	(0.217)
K12FT * ≥ Median School Size * Strong ABL	0.150	0.128	0.069	0.083
	(0.134)	(0.203)	(0.131)	(0.203)
School Size Definition	Enrollment	Student per Teacher	Enrollment	Student per Teacher
Strong ABL Definition	DOE Rating	# of Laws	DOE Rating	# of Laws
N	267,024	267,024	267,024	267,024

***Statistically significant at 1% level **at 5% level *at 10% level

Notes: Fully interacted DDD estimates are generated using 2019 and 2020 agency-by-month data from the UCR. Only agencies that report crime in all 24 months are used. Each regression is estimated via weighted Poisson using population-day as an exposure variable. SEs clustered at the county-level are reported inside the parenthesis. All estimates include controls for monthly COVID-19 deaths, foot traffic into restaurant or bars, unemployment rate, average temperature and total precipitation; yearly per capita income; and agency and month-by-year fixed effects. The data on school characteristics are from the NCES CCD, and the data on anti-bulling laws are from Sabia and Bass (2017) and Rees et al. (2023). School characteristics are at the county-level and anti-bullying laws are at the state-level. The median average enrollment is 390 and the median student per teacher ratio is 14.5. In columns 1 and 3, strong ABL is defined as states with DOE rating greater than 18 (the median DOE rating). In columns 2 and 4, strong ABL is defined as any states with three or more of the following components: written records and reporting, investigations, consequences, training and transparency, and legal definitions (Sabia and Bass 2017; Rees et al. 2023). We also control for the interaction of our treatment variable with 2017-2018 per capita income, expenditure per pupil, and the share of racial minorities. The data on per capita income comes from the BEA and the data on expenditure and racial composition comes from NCES CCD.