From Hotspots to Safe Spots: The Impacts of WiFi-Connected Community Centers on Youth Crime

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Abstract

Many juvenile crime reduction strategies rely on policing or intensive youth interventions. We analyze the crime impacts of a nationwide program targeted at youth that created digital spaces with high-speed Wi-Fi and internet-connected devices in over 1,250 community and recreation centers. The program significantly decreased juvenile offending and victimization in high-exposure cities. We find no evidence of impacts on adult crime, nor do find that crime was spatially displaced. This model of juvenile crime prevention appears highly cost-effective: the startup costs needed to attract youth to modernized, supervised community spaces are small compared to their potentially outsized social benefits.

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Juvenile crime generates lasting social and economic costs for offenders, victims, families, and communities. Many established strategies to prevent youth from committing crimes—ranging from heightened policing to high-touch interventions—require substantial funding, staff, time, and administration. Communities with the greatest demand for these services are often the least resourced to provide them.

In this paper, we examine the crime impacts of a scalable, community-oriented policy: creating spaces featuring high-speed WiFi and online devices in neighborhood locations such as community centers, gyms, and recreation centers (which we refer to generically as "community centers"). Many youth are chronically online: the average teenager spends over seven hours daily on entertainment screen usage (Rideout et al., 2022) and over an hour per day on homework that has become increasingly digital (Livingston, 2019; Auxier and Anderson, 2020). However, more than half of low-income youth cannot reliably connect online due to high costs, slow connection speeds, data caps, and limited bandwidth and devices shared with other household members (Katz and Rideout, 2021). Offering digital amenities in community centers may potentially draw at-risk youth away from high-risk environments and provide them opportunities for socializing, homework, and leisure in supervised spaces.

Specifically, we estimate the crime impacts of a program that established high-speed WiFi hubs in more than 1,250 community centers across the country. The program, known as "Lift Zones", was launched in September 2020 by a prominent internet service provider to create neighborhood spaces for youth to connect online and participate in remote schooling during the COVID-19 pandemic. Although community centers already served as neighborhood locations for recreation and other services, the addition of high-speed internet and devices to connect online reignited local interest and demand for these spaces. Lift Zones have reportedly connected over 6 million unique users to free WiFi (an average of 5,000 unique users per site),[1] with 52% of users reporting that they would not have had

[1]Usage estimates as self-reported by Comcast, the internet service provider which conceived and implemented the Lift Zones program. See: https://corporate.comcast.com/impact/digital-equity/lift-zones

the devices or technology to connect online at home (Toomey, Fitz and Gudger, 2023).

We estimate the impacts of Lift Zones on youth offending and victimization by linking data on Lift Zone locations and launch dates with monthly city-level crime data from the National Incident-Based Reporting System (NIBRS) between 2018 and 2022. We estimate the impact of city-level exposure to Lift Zones—determined primarily based on the geographic coverage of the program's founding internet service provider—using both two-way fixed effects ("TWFE") as well as synthetic difference-in-differences ("SDID") methods. We present estimates from both methods, and we show how they vary under different assumptions about treatment timing and how program exposure is defined.

Our results reveal that the implementation of the Lift Zone program led to large decreases in youth offending and victimization. Crime counts between high-exposure and no-exposure cities followed similar trends across the 30 pre-treatment months—including through the early months of the COVID-19 pandemic. The trends in the two groups began diverging precisely with the launch of the Lift Zones program. Our simplest specification—using difference-in-differences to compare cities with five or more Lift Zones to cities with no Lift Zones before and after the nationwide program launch—indicates that juvenile offending decreased by 17 percent in high-exposure cities relative to no-exposure cities. Youth victimization decreased by a smaller but still significant 9.1 percent. The estimates remain large and significant regardless of our specification choices. Our most conservative estimates, obtained from SDID allowing staggered adoption of Lift Zones across cities, indicate that youth offending decreased by 13.6 percent and that victimization decreased by 7.6 percent in high-exposure cities relative to no-exposure cities.

Our results remain consistent across many robustness and sensitivity tests. We find no significant effects from a placebo-in-time test using pre-treatment data and assigning treatment timing to different placebo months. Our results also do not appear to be driven by any single city or state. Effect sizes appear to decrease monotonically as we relax the definition of treatment from five Lift Zones to one Lift Zone, further bolstering the

interpretation that our estimates are driven specifically by exposure to Lift Zones.

Our investigation of mechanisms suggests a potentially important complementarity between Lift Zones and schools. First, we find that Lift Zones had no impact on non-juvenile crime. Second, the benefits of Lift Zone exposure vanish during the summer, when students are not in school. Third, results from heterogeneity analyses suggest that Lift Zones' effects were concentrated on weekdays during and immediately after school hours and that impacts were larger in cities where in-person learning was prioritized during the pandemic. Although the original purpose of Lift Zones was to facilitate access to remote learning, our results hint at potential synergies between Lift Zones and school attendance.

In terms of other dimensions of heterogeneity, we find larger effects in cities that experienced below-median uptake of federal COVID-era broadband subsidy programs—areas where barriers to home broadband adoption were more complex than just affordability. We also find suggestive evidence that Lift Zones had a larger impact on Black youth and did not affect the mix of crimes that were committed (e.g., violent, property, and drug crimes).

We also assess whether Lift Zones spatially displaced crime. Even if the program decreased crime in the aggregate, the possibility of concentrating youth crime near Lift Zones due to an influx of foot traffic (e.g., "concentration effects" as described by Jacob and Lefgren (2003)) could be undesirable for affected communities. Using spatial difference-in-differences methods comparing crime counts within a small radius of treated and untreated community centers in two cities with high Lift Zone exposure, we find no evidence that crime became concentrated near Lift Zone sites. We also find no evidence that the type of crimes committed near Lift Zones changed. This suggests that Lift Zones had minimal displacement and concentration effects on juvenile crime.

Our results indicate that Lift Zones were relatively cost-effective for preventing youth crime and victimization—both of which are associated with high social costs. The Lift

Zones model's use of existing community infrastructure and staff makes it relatively inexpensive to operate and scalable. Because the overwhelming majority of urban community centers do not require new broadband networks to be built, the actual cost of implementing a Lift Zone primarily involves the monthly cost of commercial-grade WiFi as well as an upfront investment in devices used to connect online. Community centers remain fully in charge of operation, staffing, and outreach. Although the Lift Zones model appears to generate large social value in terms of juvenile crime, its impacts on academic achievement are ambiguous and are outside the scope of this paper.[2]

This paper sits at a novel intersection between literatures on crime, place-based policies, and broadband access. One of the primary challenges of juvenile crime prevention is that youth exhibit severe present bias due to their ongoing physical, cognitive, and emotional development (Lee and McCrary, 2017; Steinberg and Scott, 2003). The literature offers several strategies for addressing this present bias. The first approach—and the one that our paper primarily speaks to—uses incapacitation effects to steer individuals away from criminal behavior by attracting individuals towards non-criminal alternatives, or even requiring participation in those alternatives. Leisure activities (Dahl and DellaVigna, 2009), school attendance (Jacob and Lefgren, 2003), and youth employment programs (Gelber, Isen and Kessler, 2016; Davis and Heller, 2020) provide salient examples. A second approach is to directly equip youth with skills to confront their present bias. Intensive school-based counseling programs rooted in cognitive behavioral therapy appear particularly effective (Heller et al., 2017), though even lower-touch efforts such as summer employment programs have been shown to confer conflict resolution skills which contribute to reducing youth crime (Modestino, 2019).

Crime rates are also inherently shaped by places. Environmental features of cities such as streetlights (Chalfin et al., 2021), green spaces (Bogar and Beyer, 2016), urban blight (Branas et al., 2018), and housing (Freedman and Owens, 2011) all influence the

[2]Lift Zones may provide access to learning technology and a safe environment to work and study in; alternatively, Lift Zones increase access to digital forms of leisure that could distract from learning.

spatial prevalence of crime. Police presence also decreases the nearby prevalence of crime (Mello, 2019; Chalfin and McCrary, 2018), though the social and political costs of such strategies have fluctuated immensely in recent years. Police presence in schools in the form of school resource officers also appears to reduce the rate of serious youth violence (Sorensen, Shen and Bushway, 2021), though student achievement appears to decline as a consequence (Weisburst, 2019).

Our focus on community centers offers a distinct contribution to this literature. We build on a small but growing body of work examining the causal effects of community organizations and programs on criminal behavior. Sharkey, Torrats-Espinosa and Takyar (2017) find that expanding nonprofit organizations focused on community well-being reduces violent and property crime. In concurrent work, Villa (2024) shows that closing youth clubs in London increases juvenile crime. Our paper extends this literature by examining how the introduction or enhancement of digital amenities in community centers influences juvenile criminal behavior.

Finally, our results shed light on the benefits of meeting community demand for connecting online with high-speed broadband, particularly in low-income areas. High-speed internet has transformed nearly every facet of modern daily life, particularly since the start of the COVID-19 pandemic. Despite this, approximately 23 percent of households as of 2023 lived without a home internet connection.[3] Low-income youth remain underconnected at disproportionate rates: even among those with home broadband connections, over half report that their service is too slow, one-third report service disruptions due to data limits, and one-quarter report unusable home speeds due to too many users connecting at once (Katz and Rideout, 2021). While previous causal work on low-income broadband access primarily focuses on labor market outcomes (Zuo, 2021; Hjort and Poulsen, 2019), our paper shows that low-income broadband access—available through community hubs—can also lead to large reductions in juvenile crime.

[3]Data from the NTIA Internet Use Survey, retrieved via the NTIA Data Explorer.

1 Lift Zones and Crime

1.1 Lift Zones and Bridging the Digital Divide

In September 2020, Comcast—one of the largest internet service providers in the country—launched the Lift Zones program, a multi-year initiative to establish WiFi-enabled community spaces across the United States. The program initially sought to provide supervised spaces for under-connected youth to participate in remote learning during the pandemic. The program solicited applications from nonprofit community centers, gyms, and small businesses with the primary goal of choosing locations that would attract and benefit low-income students. Lift Zone sites were required to meet certain criteria, including compliance with CDC and local COVID-19 safety guidelines, a verification check for insurance, and a covered indoor space.

Comcast provided participating community centers free commercial-grade WiFi, enabling top connection speeds while supporting many simultaneous users. If needed, sites were provided with up-to-date routing equipment, as well as complimentary access to educational and digital skills content. While some amenities varied across sites, all Lift Zone locations were required to provide safe, clean spaces for distance learning and homework. This included desks, chairs, electrical outlets for laptops, as well as devices such as tablets or computers to access the internet. Lift Zone sites were prohibited from charging fees to users outside of existing membership fees (e.g., YMCA memberships). Although youth were Lift Zones' target demographic, adults, seniors, and veterans increasingly used Lift Zones for remote work, applying for jobs, and building digital skills. By late 2022, Comcast had rolled out over 1,250 Lift Zones in cities across the country.

1.2 Lift Zones and Youth Crime

Lift Zones were originally launched to expand access to remote learning in under-connected communities, but what links them to juvenile crime? While community centers have tra-

ditionally served as multipurpose spaces, many have not adapted to youths' increasingly digital lifestyles. Homework and school resources have become increasingly digital, social media plays a central role in social interactions, and screen-based activities such as gaming and video streaming crowd out a significant amount of time previously spent on traditional forms of play (Aspen Institute, 2023). Although a growing number of community centers now provide public WiFi, few offer dedicated spaces to serve as digital hubs for their communities (Dayananda, 2017). Lift Zones, in contrast, create safe, communal spaces for youth to work, socialize, and play—all centered around robust digital amenities.

These digital hubs may affect youth crime through their effects on behavior and time use. Many models of criminal offending build on Becker's economic framework of crime (Becker, 1968). However, youth often deviate from this framework due to 1) an underdeveloped ability to fully process the severity or consequences of their actions (Steinberg and Scott, 2003), and 2) a susceptibility to present bias driven by ongoing cognitive development (Lee and McCrary, 2017). Because impulsivity and short-term decision-making play a significant role in juvenile crime, effective non-policing prevention strategies often include 1) incapacitation—creating structured, supervised environments that reduce opportunities for delinquency—and 2) equipping youth with decision-making skills to counteract their present bias (e.g., Heller et al. (2017)).

Lift Zones align with these strategies by providing supervised spaces that naturally attract youth through in-demand digital amenities. By creating an appealing alternative to unsupervised environments, Lift Zones may alter the relative appeal of crime (Chalfin and McCrary, 2017) and encourage youth to self-incapacitate in low-risk environments. In 2019, the average youth between the ages of 13 and 18 spent over seven hours daily on screen-based media purely for entertainment—including one hour watching online videos, an hour and a half of gaming, an hour on social media, and an hour for browsing websites and video chatting (Rideout et al., 2022). Additionally, teenagers spent one hour

daily on homework (Livingston, 2019), with six in ten students reporting that they used the internet at home to do homework every day (Auxier and Anderson, 2020). However, many low-income youth lack usable home broadband connections or devices to access these digital activities (Katz and Rideout, 2021), making Lift Zones an especially valuable resource. Among parents of Lift Zone users, 45 percent said that their children lacked a safe space to work and study at home, 44 percent said that their homes lacked devices to connect online, and 59 percent said that they lacked fast and reliable home internet (Toomey, Fitz and Gudger, 2023). These patterns suggest that Lift Zones may provide an attractive space for youth who were unable to engage in these activities at home, and who may have otherwise been involved in delinquent behavior.

It is also possible that Lift Zones might *increase* crime. Jacob and Lefgren (2003) show that clustering youth in a single location can lead to an increase in juvenile crime through "concentration effects", where the high density of youth creates opportunities for conflict. These effects could offset or even exceed the benefits of incapacitation, particularly in the early months of the pandemic when local lockdowns limited public gathering places. Additionally, even if Lift Zones decrease juvenile crime overall, the possible displacement of youth crime may potentially be undesirable.

2 Data

We compile monthly juvenile victimization and offender counts at the city level using data from the National Incident-Based Reporting System (NIBRS), managed by the Federal Bureau of Investigation (FBI). The NIBRS data provide detailed accounts of each reported crime, including victim and offender demographics, crime type, arrest type (if any), time of occurrence down to the hour, victim-offender relationship, and location type (e.g., residence, store, street, school, etc.). The data's key limitation is its limited national coverage: only 8,500 police agencies reported to NIBRS in 2019, representing just 45%

of the U.S. population—including a disproportionate number of small and rural agencies. While more cities have joined over time, most cities do not have much history in the NIBRS data.[4]

We utilize NIBRS data from 2018 through 2022 and include only cities that reported to NIBRS every month throughout this timeframe.[5] To maximize the coverage of crime-reporting agencies, especially those representing large cities, we restricted our analysis to incidents occurring since 2018. We focus on crime incidents involving a juvenile offender or victim between the ages of 8 and 17. We further restricted our sample to crime reporting agencies in cities with populations exceeding 50,000 residents.[6]

We link publicly available data on Lift Zones to the NIBRS data. Specifically, we obtained the names, addresses, and "go live" dates for every Lift Zone location, allowing us to establish the number of active Lift Zones within each city in any given month. In total, we tracked 1,408 total Lift Zones established between September 2021 and December 2022 across 466 total cities.

Figure 1 plots Lift Zone locations by city, with marker sizes representing the number of Lift Zones in each city. The cities indicated in blue represent the Lift Zone cities with NIBRS coverage from 2018 through 2022 used in our analysis. Roughly 60% of Lift Zone cities launched just one site, 25% launched between two and four sites, and 15% launched five or more sites. Roughly ten major cities—those with strong local ties to Comcast—launched thirty or more Lift Zones. The spatial distribution of Lift Zones aligns closely with Comcast's regional availability, as Lift Zones could not be supported outside of Comcast's broadband network. Table A.5 reports the average yearly crime counts in Lift Zone and control cities used in the analysis. For Lift Zone cities, the table also reports the total number of Lift Zones the city receives and the launch date of the first Lift Zone.

[4]Starting in 2021, the FBI will exclusively use the NIBRS for crime data reporting.

[5]We utilize Jacob Kaplan's concatenated NIBRS files prepared for researcher use (Kaplan, 2024).

[6]Each reporting agency is assigned a "population group" code indicating the size of the population covered by the agency, and whether the agency's jurisdiction is a city, a non-MSA county, or an MSA county. We excluded agencies that represent counties to minimize overlap of agencies and double-counting of crimes.

Lift Zones were often located in low-income neighborhoods with higher juvenile crime rates. As an illustrative example, we use crime data from Baltimore to plot a heat map of youth victimization against Lift Zone locations throughout the city in Figure 2.[7] Purple areas have low juvenile victimization rates, while yellow areas represent high victimization rates. Each red dot represents a Lift Zone location. Lift Zones tend to be located in higher-crime areas: nearly 50% of monthly juvenile victimizations occurred within a half-mile of one of Baltimore's 29 Lift Zones.

For certain analyses, we incorporated data on monthly takeup rates of two federally subsidized broadband internet programs, the Emergency Broadband Benefit Program (EBB) and the Affordable Connectivity Program (ACP).[8] The EBB provided a $30 monthly subsidy to low-income households for broadband internet service, while the ACP provided a $50 subsidy. Additionally, we linked district-level metrics for school learning modalities for the 2021-2022 school year, prepared by the CDC, to estimate the fraction of youth attending school in-person, remotely, or through a hybrid of the two.

3 Empirical Strategy

Our core analysis focuses on the impact of Lift Zones on city-level youth crime. We exploit city-level and monthly variation in the program's availability: Lift Zones were only made available in locations where Comcast offered broadband service, and the number of Lift Zones in each city was anecdotally tied to the strength of existing community partnerships with local Comcast branches.

We use several panel-based estimation approaches to estimate the causal impacts of Lift Zones on juvenile crime. Consider the following TWFE estimating equation, which

[7] Baltimore is one of the only cities throughout the U.S. for which geocoded juvenile crime data are available. It also received a large number of Lift Zones.

[8] Congress initiated the EBB as part of its COVID-19 response, and the ACP replaced it in 2022. When the ACP ended in April 2024, 23 million households were enrolled, and the program had cost $17 billion.

links youth crime outcomes to the number of Lift Zone sites in city i and month t:

$$y_{it} = \alpha + \rho(\text{LZ Sites})_{it} + \delta_i + \gamma_t + \epsilon_{it} \tag{1}$$

where we focus on two core outcomes: how many crimes involved a juvenile offender ("juvenile offending"), and how many crimes involved a juvenile victim ("juvenile victimization"). We express both outcomes in log form. City and year-month fixed effects round out the TWFE regression. The coefficient of interest, ρ, aims to capture the causal effect of an additional Lift Zone site. However, because city Lift Zone counts change over time, Equation (1) is difficult to interpret due to the staggered nature of treatment timing and the non-binary, non-absorbing nature of the treatment variable. Although tools exist for both issues individually (Callaway and Sant'Anna, 2021; Borusyak, Jaravel and Spiess, 2023; Sun and Abraham, 2021; Callaway, Goodman-Bacon and Sant'Anna, 2021; D'Haultfœuille, Hoderlein and Sasaki, 2023; de Chaisemartin and D'Haultfoeuille, 2022), methods that incorporate both issues simultaneously are still developing and may ultimately require strong assumptions to produce readily interpretable estimates. Therefore, we present results using several specifications with more transparent assumptions. At baseline, we compare high-exposure cities with five or more Lift Zones (approximately one-quarter of Lift Zone cities in our NIBRS sample) to cities without Lift Zones in a difference-in-differences approach assuming uniform treatment across cities at the initial launch of the Lift Zones program in September 2020. We exclude cities with one to four Lift Zones from this analysis but provide robustness checks illustrating how our results change when relaxing this restriction. We estimate the following baseline difference-in-differences equation:

$$y_{it} = \alpha + \rho(\text{High LZ Exposure})_{it} + \delta_i + \gamma_t + \epsilon_{it} \tag{2}$$

where $(\text{High LZ Exposure})_{it}$ equals one in high-exposure cities after September 2020 and

zero otherwise. Standard errors are clustered at the city level. We note that we are limited in our ability to incorporate covariates because of the monthly frequency of our data; to our knowledge, virtually no publicly available data source provides monthly, city-level covariates with national coverage.

Table 1 presents summary statistics comparing no-exposure, any-exposure, and high-exposure cities in our NIBRS analysis sample. Lift Zones were launched in cities that were more populous and less affluent on average. One-third of these cities—which were even more populous and less affluent—received five or more Lift Zones. While differences exist, our identifying assumption requires that high-exposure and no-exposure cities would have trended similarly in the absence of Lift Zones. We provide evidence in support of this assumption by examining how youth crime evolved between these two groups before September 2020. Additionally, given the level differences in crime counts between high- and no-exposure cities, we transform crime counts into logs to assess proportional changes in crime. In robustness tables, we show that our results consistent to our main specification when we use crime counts and love crime rates.

Next, we relax the uniform timing assumption, acknowledging that some treated cities did not receive Lift Zones immediately in September 2020.[9] Many recent methods offer various approaches to estimate Equation (2) in the presence of heterogeneous treatment effects between cities or over time (Callaway and Sant'Anna, 2021; Borusyak, Jaravel and Spiess, 2023; Sun and Abraham, 2021; Callaway, Goodman-Bacon and Sant'Anna, 2021; D'Haultfœuille, Hoderlein and Sasaki, 2023; de Chaisemartin and D'Haultfoeuille, 2022), while accounting for problematic comparisons between cities treated at different times (Goodman-Bacon, 2021). Given the length of our pre-period and the convincing pre-treatment parallel trends that we preview in Figure 3, we present results using the imputation-based estimator from Borusyak, Jaravel and Spiess (2023) which provides efficiency benefits when pre-treatment parallel trends can be convincingly established over

[9]Figure A.1 shows that sites "ramp up" quickly in high-exposure cities; these cities typically reach an average of 5 Lift Zones within the first two months of the program launching locally.

a longer period of time (Roth et al., 2023).

Beyond TWFE approaches, we also present results obtained using synthetic difference-in-differences (SDID), a method that borrows strengths from both synthetic control methods (SCM) and difference-in-difference methods to construct a comparison group that more compellingly resembles the treatment group. SDID compares outcomes between treated cities and a weighted average of control cities, with weights algorithmically determined to align the pre-treatment trajectory of youth crime in the "synthetic" group of control cities with the pre-treatment trajectory in treated cities. SDID also weights time periods, placing greater weight on pre-treatment periods that are more similar to post-treatment periods. The inclusion of time weights adds a layer of robustness by allowing the validity of SDID to rely on the effectiveness of either the weighting of control cities or the weighting of time periods (Arkhangelsky et al., 2021). The validity of SCM-based methods crucially relies on the strength of pre-treatment fit across many pre-treatment periods to minimize bias from overfitting (Ben-Michael, Feller and Rothstein, 2021).

SDID improves upon difference-in-differences by allowing control cities to be individually weighted to improve pre-treatment fit—improving the case for parallel trends when the number of pre-treatment periods is large (Abadie, Diamond and Hainmueller, 2010; Ben-Michael, Feller and Rothstein, 2021). SDID improves upon traditional synthetic controls by accommodating multiple treatment units and allowing crimes in treatment and synthetic control cities to differ by a constant—a parallel trends assumption that is less restrictive than SCM's stronger requirement for matching pre-treatment outcomes in levels. Finally, SDID's incorporation of time weights adds a double robustness property not found in either difference-in-differences or traditional synthetic controls.

Notationally, we observe a panel for $i = 1, ..., N$ grantees, N_{tr} of which comprise the treatment group and N_{co} of which comprise the comparison group. The panel extends over the months $t = 1, ..., T$, with $T_{pre} = 32$ pre-treatment months. Treatment status W_{it} equals zero for all grantees before September 2020 and equals one for treated grantees

after T_{pre}. SDID calculates city-specific weights $\hat{\omega}_i^{sdid}$ and pre-treatment month weights $\hat{\lambda}_{ti}^{sdid}$ to solve the following optimization problem:[10,11]

$$\left(\hat{\tau}^{sdid}, \hat{\mu}, \hat{\alpha}, \hat{\beta}\right) = \underset{\tau,\mu,\alpha,\beta}{\operatorname{argmin}} \left\{ \sum_{i=1}^{N} \sum_{t=1}^{T} \left(Y_{it} - \mu - \alpha_i - \beta_t - W_{it}\tau\right)^2 \hat{\omega}_i^{sdid} \hat{\lambda}_t^{sdid} \right\} \tag{3}$$

We draw special attention to the fixed effects α_i and β_t which do not appear in the estimation of standard synthetic controls and allow SDID to rely on a weaker parallel trends assumption instead of exact matching in levels. City weights $\hat{\omega}_i^{sdid}$ are chosen to minimize pre-treatment differences in outcomes between the treatment group and the weighted comparison group, accounting for a constant level difference between the two groups. Time weights $\hat{\lambda}_{ti}^{sdid}$ are correspondingly calculated using control units and are chosen to minimize the difference between the weighted average of pre-period outcomes with the simple average of post-period outcomes, also accounting for a constant level difference between pre- and post-periods.

We then incorporate recent methodology developments to account for staggered adoption (Clarke et al., 2023). The approach intuitively follows methods in the TWFE staggered adoption literature which separately estimate treatment effects for individual treatment cohorts before aggregating them together. For each group of treated cities that received their first Lift Zones in month a, we calculate an SDID estimate $\hat{\tau}_a^{sdid}$ and weight all A estimates together as follows:

$$\widehat{ATT} = \sum_{a \in A} \frac{T_{post}^{a}}{T_{post}} \times \hat{\tau}_a^{sdid} \tag{4}$$

[10] Arkhangelsky et al. (2021) suggest a variety of approaches to inference. With multiple treated units, the authors provide variations of the clustered bootstrap (Efron, 1979) and jackknife applied to SDID which both produce conservative confidence intervals. We report standard errors from the bootstrap approach using 50 repetitions which generally produces more conservative confidence intervals than the jackknife.

[11] Additionally, SDID incorporates a regularization parameter which increases the dispersion of the weights and ensures a unique solution. We use the default regularization parameter proposed by Arkhangelsky et al. (2021) based on the size of a typical one-period change for comparison units before the event.

where the weight on each cohort a is determined by the number of post-periods available (i.e. cohorts with fewer post-treatment data points are weighted less than cohorts with more). Inference is conducted via a multi-period extension of the bootstrap procedure for standard non-staggered SDID.

4 Results

4.1 Main Results

Figure 3 plots how youth crime evolved in high-exposure and no-exposure cities, before and after the launch of Lift Zones. The y-axis depicts the log of juvenile offending/victimization, minus each series' respective pre-treatment mean to facilitate comparison. In both graphs, the pre-treatment de-meaned outcomes follow each other closely, including the sudden drop in crime during the COVID-19 lockdowns in early 2020. Starting in September 2020, youth crimes in high-exposure cities fell relative to youth crimes in no-exposure cities. The gap remains consistent over time, except during the summer when the two series appear to momentarily re-converge. The size of the effect appears larger for crimes involving youth offenders than for crimes involving youth victims. The duration of parallel pre-trends as well as the precise timing of when the two series split provide preliminary but compelling evidence in support of a causal relationship between Lift Zones and juvenile crime.

Figure 4 provides event study representations of our estimates. We show and compare estimates for both SDID and DID. For SDID, the point estimate represents the outcome difference between the treated group and the synthetic control. Standard errors were manually calculated via a bootstrap resampling procedure described by Clarke et al. (2023). For DID, we replaced the treatment variable with a series of interaction terms between our treatment indicator and individual indicators for month-years, leaving out the month immediately preceding the launch of the Lift Zone program. The results appear

similar across both specifications. We do not observe any clear pre-trends, and both SDID and DID suggest a noticeable negative effect on juvenile offending and victimization.

Table 2 presents our regression estimates when aggregating treatment effects across the post-period.[12] Column (1) provides the baseline difference-in-differences estimate from Equation (2). Column (2) presents results using the imputation-based estimator from Borusyak, Jaravel and Spiess (2023) when allowing treatment timing to vary across cities. Column (3) presents results from SDID with uniform timing, and column (4) presents the staggered SDID estimate. Column (5) presents the continuous TWFE estimate with staggered timing introduced in Equation (1) which seeks to capture the causal impact of each additional Lift Zone on youth crime. Panel A presents results for juvenile offending and Panel B focuses on victimization.

Our baseline difference-in-differences estimates in Column (1) indicate that monthly crimes committed by juvenile offenders decreased significantly by 17 percent in cities with five or more Lift Zones compared to cities with no Lift Zones. Juvenile victimization decreased by a smaller but still significant 9.1 percent. These results remain virtually unchanged in Column (2) at 17.1 percent and 10 percent after incorporating staggered timing.

With SDID in Column (3), the effect on offending increases to 18.2 percent, and the effect on victimization decreases to 6.8 percent. Figure A.2 plots the distribution of unit weights and time weights, showing that no single city or time period receives a disproportionately large weight. When incorporating staggered treatment into SDID in Column (4), the effect on juvenile offending decreases to 13.6 percent and the effect on victimization increases to 7.6 percent. In Column (5), we provide suggestive evidence that each additional Lift Zone leads to a 1.1 percent decrease in juvenile offending and a 0.7 percent decrease in victimization, though we caution that multiple sources of bias from staggered

[12] Table A.2 provides the equivalent table when the outcome is expressed in levels. We also report the main results when the outcome is log city crime rates, $\log(\frac{\text{Crime Count}_{it}}{\text{City Population}_{y}})$, in Table A.3. Our results are consistent in terms of the interpretation of the findings across the different outcome definitions.

adoption and dynamic treatment may affect this specification.

At baseline, treated cities experienced an average of 96.5 crimes per month involving a youth offender and 80.6 crimes involving a youth victim. Our most conservative estimates imply that Lift Zones in high-exposure cities averted 13.1 monthly crimes with a youth offender (157 annual crimes) and 5.5 monthly crimes with a youth victim (66 annual crimes). We consider two approaches to approximating the per-site impact on youth crime. First, the average high-exposure city received 12 Lift Zones, suggesting that the average Lift Zone averted 13.1 annual crimes involving youth offenders and 5.5 annual crimes involving youth victims. Next, we revisit the continuous TWFE estimate in Column (5), which similarly implies that each site averted 12.7 annual crimes with a youth offender and 6.8 annual crimes with a youth victim.[13] We further caveat that NIBRS only includes crimes reported by law enforcement agencies, so our estimates do not capture any additional effect on unreported crimes.

In Table 3, we show how our staggered TWFE and SDID estimates change as the definition of treatment expands to include fewer than five Lift Zones. Each successive estimate decreases the required number of Lift Zones for inclusion in the treatment group, increasing the number of treated cities. The left-most column indicates that 21 cities in the NIBRS sample received five or more Lift Zones; the right-most column shows that 61 cities received one or more Lift Zones. From left to right, the point estimates for both staggered TWFE and SDID gradually decrease as we relax the threshold for inclusion in the treatment group. This trend suggests that greater exposure to Lift Zones leads to a larger causal decrease in youth crime. The TWFE estimates remain large and significant at the 5 percent level for both offending and victimization, even when allowing the treatment group to include cities receiving two or more Lift Zones. The dropoff is more notable for SDID, where the point estimates remain consistently negative but are only significant when the treatment group includes cities with four or more Lift Zones.

[13] Using youth offending, we calculate these by multiplying our estimate, -0.011, by the baseline monthly count of 96.5. We multiply the monthly counts by 12 to convert to annual counts.

Table 4 presents estimates from a placebo-in-time test. This test uses pre-treatment data to estimate TWFE and SDID effects while assigning placebo treatment dates in the pre-treatment period and estimating treatment effects using just pre-treatment data. For SDID in particular where an examination of parallel pre-trends is not possible, the placebo-in-time test provides a falsification test with a very similar premise: the presence of significant effects in the pre-treatment period casts doubt on the validity of our SDID estimates. Each column in Table 4 presents the TWFE and SDID estimates for a various placebo treatment dates, spread out in 3-month intervals. The right-most column presents the actual TWFE and SDID estimates with uniform treatment timing, to directly compare against the placebo estimates. We find that none of the placebo estimates are significant, providing further support for a causal interpretation of our estimates.

Table 5 reports estimates for crimes without juvenile involvement. We observe precisely-estimated null effects across all specifications, indicating that non-juvenile crime remained similar in high-exposure cities compared to no-exposure cities after the launch of Lift Zones. This provides even further evidence for a causal interpretation of our results: any potential confounding changes in policy or local conditions would therefore need to precisely affect youth crime after September 2020, without affecting adult crime.

We also address potential concerns that our results are driven by individual cities. For example, several cities received an outlier number of Lift zones (e.g., Philadelphia with 78 Lift Zones and Minneapolis with 76). we provide estimates omitting individual treated cities, one at a time. Figures A.7 and A.9 plot SDID and TWFE estimates, respectively, for each city that we leave out. The estimates are not affected by the exclusion of any treated city, suggesting that the inclusion of specific cities does not appear to exert a disproportionate influence over our results. Figures A.6 and A.8 present similar results, omitting an entire state at a time. Again, the omission of individual states does not affect the magnitude and significance of our results.

Taken together, our results indicate that Lift Zones caused large reductions in youth

offending and victimization. We briefly outline a back-of-the-envelope cost-benefit analysis for efforts to scale this approach beyond Lift Zones. Relative to the economic cost of juvenile crime, the cost of a commercial-grade internet subscription and the upfront investment in new technology is likely small, particularly since centers themselves remain responsible for long-run variable costs involving staffing and outreach. Fully subsidizing Comcast's most premium business internet plan would cost $4,200 annually ($350 monthly) per site. An upper-end estimate of $30,000 would cover upgrading routing equipment, purchasing 50 devices to connect online, and modernizing spaces to integrate these devices (e.g., workspaces, computer labs, digital play areas, etc.). This amounts to a three-year investment of $42,600 per site.

Our estimates suggest that each additional Lift Zone averts approximately 13 annual crimes involving a youth offender. Heller et al. (2017) estimates that the cost of youth crime—including savings to victims, savings to governments, and impacts on human capital accumulation—ranges from $6,000 to $39,000. Even at the lower end of this range, a Lift Zone could recoup its entire up-front fixed cost in social value by preventing just five youth crimes. Over three years, the lower bound benefit from Lift Zones' impact on averting juvenile crime is $234,000 compared to a cost of $42,600, a 5.5-to-1 benefit-to-cost ratio. This ratio does not factor in the potential long-run costs of youth victimization above and beyond standard victimization cost estimates, which would likely further increase our lower bound estimate. At the middle of the youth crime cost range ($16,500 per averted crime), the benefit-to-cost ratio increases to 15-to-1.

4.2 Heterogeneity and Mechanisms

We use the rich detail of the NIBRS data to explore effect heterogeneity and the mechanisms underlying Lift Zones' impacts on youth crime. First, Lift Zones appear to complement school attendance. In Table 5, we find that Lift Zones only affected juvenile crime without affecting non-juvenile crime. We also find that although Lift Zones generally re-

mained in operation during the summer, a closer examination of Figure 3 suggests that Lift Zones' effects on youth crime disappear during these months, when youth are not attending school during summer break.

Figure 5 provides further evidence for this relationship through heterogeneity analysis comparing subgroups of cities, crimes, and youth characteristics.[14] While our results generally lack the power to conclude statistical differences between subgroups, we highlight several notable differences in point estimates. First, the impact of Lift Zones appears larger during school and after-school hours than in the evenings or on weekends. Second, we compare cities where students received in-person learning throughout the 2021-2022 school year to cities where any hybrid or remote learning occurred. Lift Zones appear more effective in cities where in-person schooling occurred throughout the year. This suggests that the effects were not solely driven by access to remote schooling; rather, Lift Zones appear to complement school attendance by offering a space for students to use outside of school hours.

We also find evidence that home broadband access appears to play an important role. First, point estimates are larger for black youth than non-black youth, aligning with racial differences in home broadband adoption. Second, effects were larger in cities where low-income households were less likely to take up federally subsidized broadband internet programs. Given that these subsidies were universally available to low-income households, low take-up indicates that other barriers were impeding home broadband adoption. The larger impact in these areas potentially reflects youths' greater reliance on Lift Zones to connect online.

Figure 5 provides several additional insights with respect to heterogeneity. First, we find no evidence that Lift Zones affected the mix of crimes youth were involved in (i.e., violent vs. property vs. or drug crimes). Additionally, we find no evidence that Lift

[14]Figure A.3 provides the corresponding estimates using staggered TWFE, and Figures A.4 and A.5 present the corresponding estimates for youth victimization, which appear nearly identical in terms of the interpretation of the results.

Zones affected the frequency of crimes occurring within the home (where 40 percent of all crimes occur). Instead, the effect appears driven by crimes occurring in other locations.

5 Did Lift Zones change the spatial distribution of crime?

Lift Zones could also influence the spatial distribution of juvenile crime within a city by altering where youth spend their time. An increase in the number of youth in a specific area might lead to higher crime rates in that area (i.e. "concentration effects" (Jacob and Lefgren, 2003)). Even if Lift Zones reduce overall youth crime, the potential spatial redistribution of youth and crime could negatively impact certain areas due to concentration effects.

We explore this possibility using geocoded crime data from the Baltimore and Chicago Police Departments. Since September 2020, Comcast has launched 29 Lift Zones in Baltimore and 30 in Chicago. We use publicly available locations of community centers in these cities that were not made Lift Zones to identify counterfactual Lift Zones. We calculate crime counts occurring within a small distance of every Lift Zone and counterfactual Lift Zone. These data come from city victimization databases and include the precise location of crimes, victim age, and other victim and crime information.[15]

Table A.4 provides juvenile crime statistics for Baltimore and Chicago, including localized crime counts around Lift Zones and counterfactual Lift Zones. On average, one juvenile victimization occurs each month within a quarter-mile radius of each Lift Zone. This suggests that 16% of all city-wide juvenile victimization occurs within a quarter-mile of the 29 Baltimore Lift Zone.[16] This increases to nearly 50% of all juvenile victimizations within a half-mile radius. Counterfactual Lift Zones appear relatively similar. In Chicago, there are five times more juvenile victims than in Baltimore. Nearly 6% of all

[15]Unfortunately, these data do not contain juvenile offense data.

[16]Multiplying 0.823 crimes within a quarter-mile of each Lift Zone by 29 Lift Zones yields 23.9 crimes, which is roughly 16% of the 146 total monthly crimes which occurs citywide.

youth victimization occurs within a quarter-mile of Chicago's 30 Lift Zones, a number which increases to 18% within a half-mile radius.

Our baseline specification compares crime counts within a quarter-mile of Lift Zone locations and counterfactual locations, before and after the Lift Zone becomes operationalized. The limited quarter-mile radius mitigates the possibility of double counting victims in areas where Lift Zones and counterfactual Lift Zones are located near each other. Because our analysis relies on count data with a large number of zeros, we use Poisson regressions to estimate the localized treatment effect as follows:[17]

$$Y_{ict} = exp(\beta \text{LiftZone}_{ict} + \psi_i + \gamma_{ct}) \tag{5}$$

where Y_{ict} is the count of the number of juvenile victims around community center i in city c in month-year t. LiftZone_{ict} is an indicator for whether a community center i in city c is a Lift Zone as of month-year t. We include a community center fixed effect ψ_i and a city-month-year fixed effect ϕ_{ct}. The city-month-year fixed effect restricts comparisons of Lift Zones and counterfactual Lift Zones to the same city and time period. For ease of interpretation, we calculate and report the average marginal effect of a Lift Zone on juvenile victimization instead of Poisson coefficients.

Figure 6 plots the dynamic effect of Lift Zones on the localized effect of juvenile victimization. The analysis uses a quarter-mile radius around Lift Zones and counterfactual Lift Zones and shows estimates for overall crime and by crime type. In all three figures, we find limited evidence of nonparallel pre-trends. We also see little evidence that launching a Lift Zone has any immediate or delayed effect on juvenile victimization. While occasional negative and positive bumps occur in the latter months, the pooled DID estimate reported in Table 6 indicates that overall juvenile victimization decreases after a Lift Zone is launched. There is also little evidence that implementing a Lift Zone has any differential effect on localized violent and non-violent victimization.

[17] see Wooldridge (2023) for treatment on nonlinear DID, with examples using Poisson regression.

To test the sensitivity of our results to the chosen radius, we present the estimates for different radii in Table 6. Column (1) presents estimates using a quarter-mile radius, while columns (2) and (3) present estimates using a half-mile and 1-mile radius. Columns (1) and (2) appear fairly similar. While all point estimates are statistically insignificant across all three specifications, we note that Column (3)—looking within a 1-mile radius—differs in terms of sign and magnitude. One possibility is that a larger radius increases the likelihood that crime is double-counted within multiple Lift Zone or counterfactual Lift Zone boundaries, biasing our results. Focusing on the point estimates in columns (1) and (2), we find negative effects for overall and violent juvenile victimization and a positive point estimate for non-violent crime. These signs are the opposite of what we would expect in the presence of meaningful concentration effects. Overall, our results suggest that Lift Zones led to minimal displacement and concentration effects.

6 Conclusion

Youth crime is costly for victims, offenders, and the communities they live in. This paper examines a novel approach to youth crime prevention through the modernization of supervised community spaces and equipping them with in-demand digital amenities. We analyze the nationwide Lift Zones program, which established WiFi hubs in over 1,250 such spaces during the height of the COVID-19 pandemic. Our estimates suggest that Lift Zones significantly decreased youth offending and victimization in cities where they were highly prevalent. We find evidence that Lift Zones appear complementary to schools and that low community broadband adoption enhances the importance of these spaces. We also do not find that Lift Zones concentrated crime around these spaces due to the influx of youth traffic.

Economically, Lift Zones are inexpensive to establish and operate as they rely heavily on existing community infrastructure; however, their value-add of high-speed, free

broadband in a communal and recreational space significantly enhances the appeal of these spaces. Although most youth spend a large and growing amount of time online, many low-income youth face barriers to online access including slow home broadband speeds, frequent service disruptions, and a lack of devices to connect from. Combining free broadband access with supervised community spaces decreased juvenile crime at a fraction of the financial cost and complexity of other youth crime prevention strategies. Perhaps surprisingly, crime reduction was not originally an intended outcome of the Lift Zones program. Instead, the program's core mission to bridge local disparities in broadband access appears to have generated large public spillovers with significant implications for community well-being and safety.

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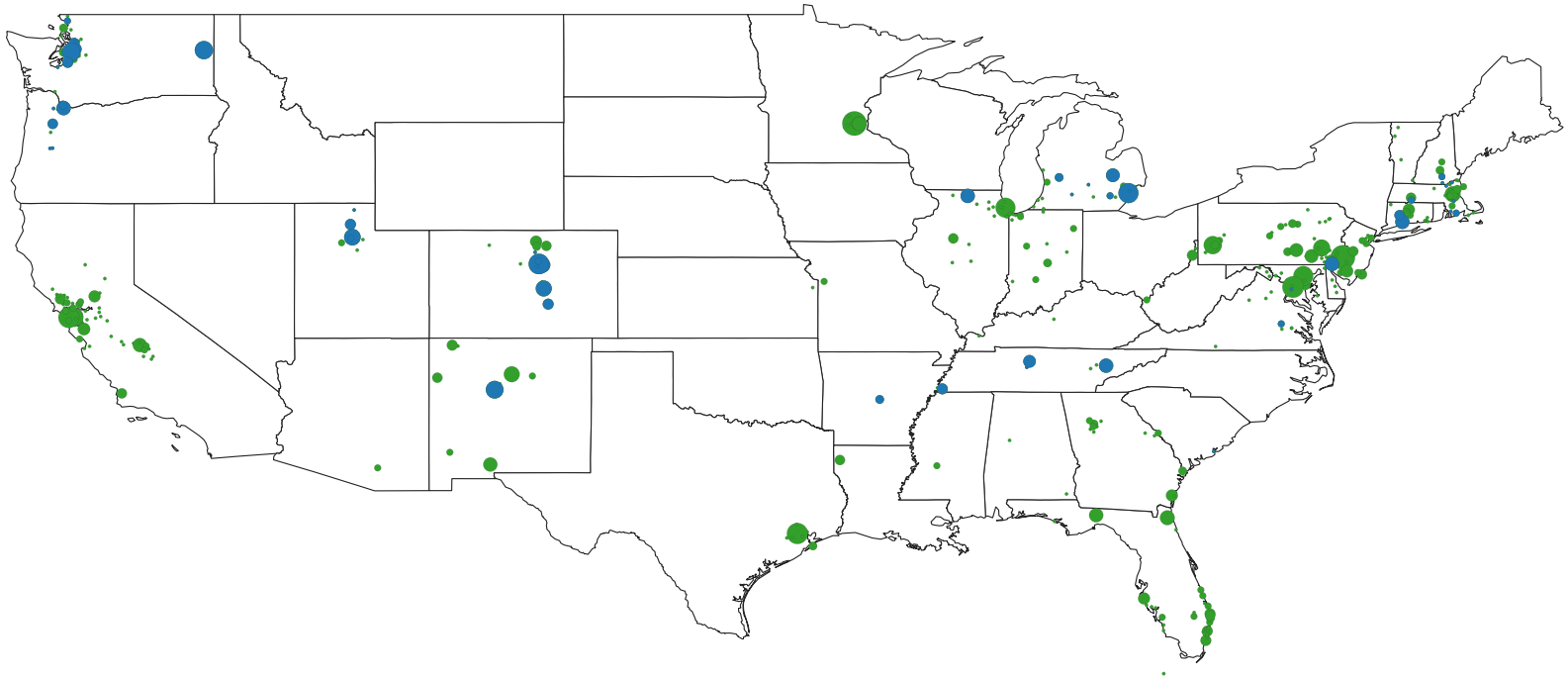
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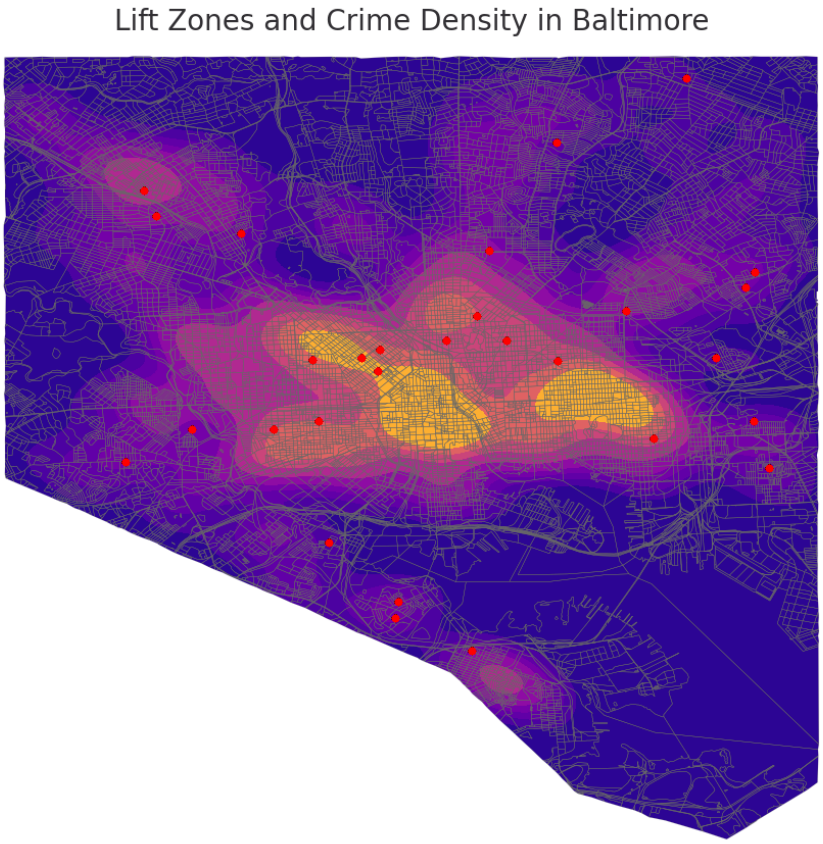
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Figure 1: Spatial Distribution of Lift Zones in Cities across the U.S.



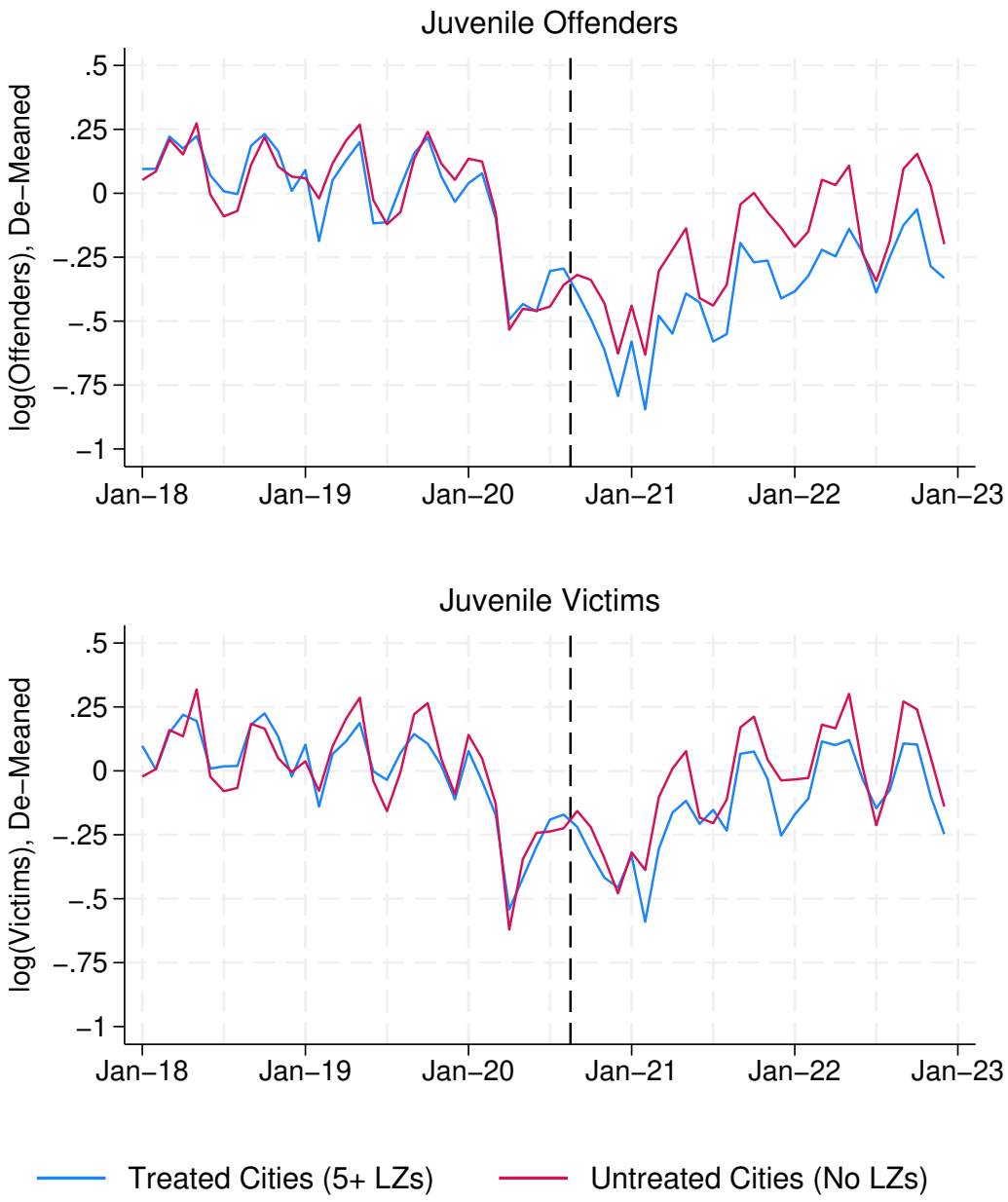
Note: This figure plots cities where Lift Zones were launched. The size of each marker is scaled based on the number of Lift Zones in each city. Most cities represented on this map received just one Lift Zone (63%), whereas the most concentrated cities (Philadelphia and Minneapolis) received over seventy. Data were compiled from public Comcast records of Lift Zone addresses. Cities marked in blue are those that reported to NIBRS in every year from 2018 to 2022 and are used in our analysis.

Figure 2: Juvenile Crime Density and Lift Zone Locations in Baltimore



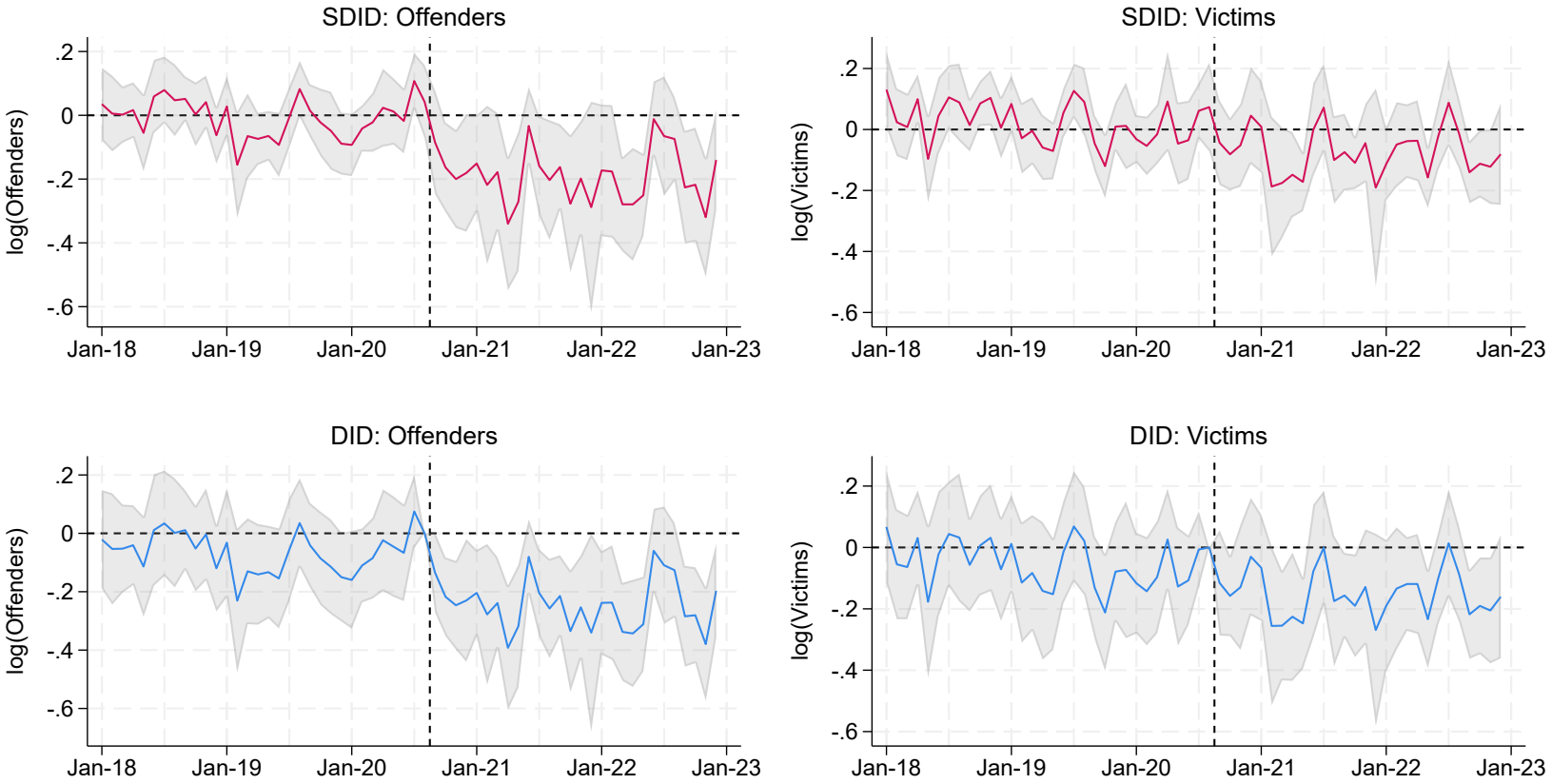
Note: This figure depicts the co-location of Lift Zones and juvenile victimization, using data from Baltimore. The yellow areas represent locations with the most juvenile victimization; the red dots represent Lift Zone locations.

Figure 3: Juvenile Offenders and Victims, by Treatment Status



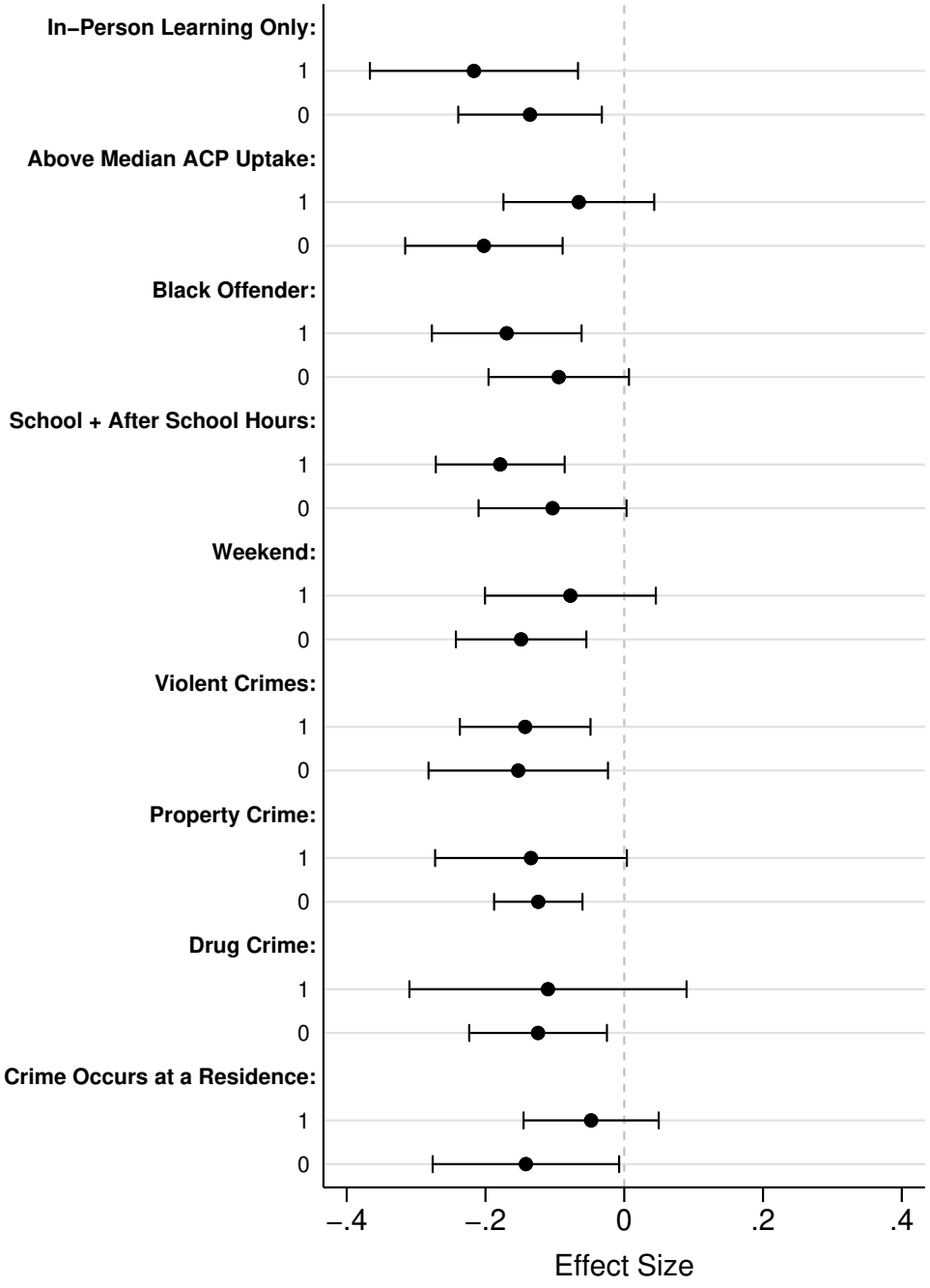
Note: This figure plots the log of monthly juvenile offender and victim counts, separate for high-exposure cities with at least 5 Lift Zones and no-exposure cities that never received Lift Zones. Both series were de-meaned by subtracting the series-specific pre-treatment mean from every month to enhance the visual inspection of pre-treatment trends.

Figure 4: Juvenile Offenders and Victims, by Treatment Status



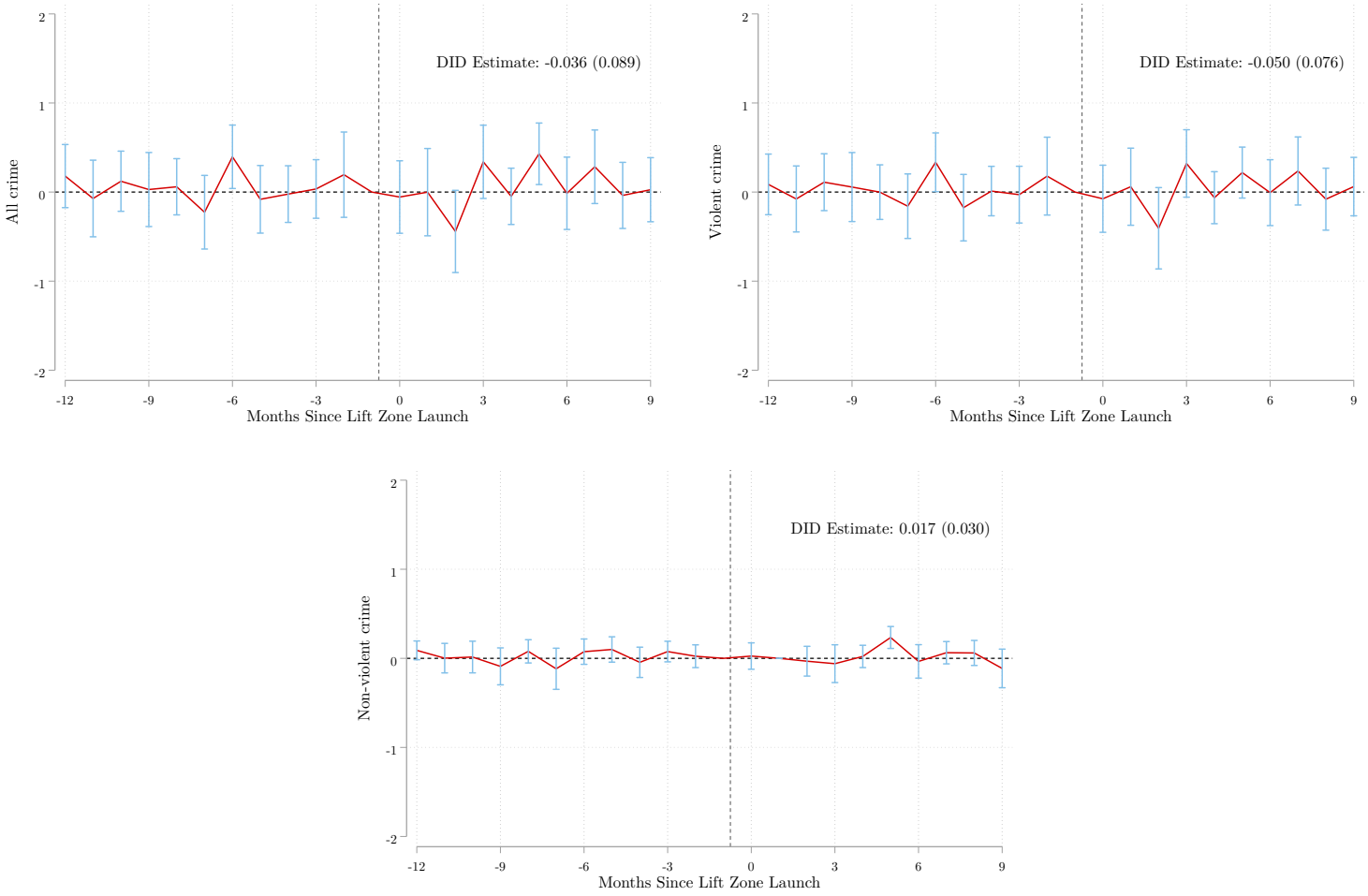
Note: This figure compares event study plots for both synthetic difference-in-differences (see Clarke et al. (2023) for an explanation of the manual bootstrapping procedure used to construct confidence intervals) and standard difference-in-differences, assigning a uniform treatment date to the official launch of Lift Zones. Separate graphs are provided for both juvenile offenders and victims as outcomes.

Figure 5: Heterogeneity in Offender Treatment Effects By Subgroup



Note: This figure plots staggered SDID estimates for youth offenders, restricting the sample as indicated on the graph. For example, "Black Offender: 1" represents the staggered SDID estimate when restricting only to incidents with black youth offenders. While most of the subcategories in the figure are self-explanatory, the "In-person Learning Only" is an indicator of whether all students in a city exclusively received in-person learning. The "School + After School Hours" category includes crimes that occurred on weekdays between the hours of 8 am and 7 pm. The "Crime Occurs at a Residence" category is comprised of crimes that took place in someone's home.

Figure 6: Localized Treatment Effects Within 0.25 Miles of Lift Zone



Note: We calculate and report the average marginal effect of a Lift Zone on the localized effect of juvenile victimization around a Lift Zone. The dependent variable is the count of the number of juvenile victims within a quarter-mile radius around each Lift Zone or counterfactual Lift Zone. Counterfactual Lift Zone locations are identified using publicly available community center locations in Baltimore and Chicago. All estimates are relative to $t = -1$, which is indicated by a vertical line. The first month a community center becomes a Lift Zone is indicated by $t = 0$. We plot 95% confidence intervals. Standard errors are clustered at the community center level.

Table 1: City-Level Summary Stats, by Lift Zone Exposure

	No Lift Zones		Any Lift Zones		5+ Lift Zones	
	Mean	SD	Mean	SD	Mean	SD
Population	127,415	126,974	186,048	185,053	336,095	250,054
% Black	0.128	0.137	0.145	0.178	0.204	0.236
% HS or Less	0.355	0.103	0.369	0.118	0.368	0.111
% in Poverty	0.151	0.072	0.173	0.073	0.200	0.082
Median Income	60,231	20,611	57,610	17,151	53,424	18,743
Median Rent	994	255	1,074	247	1,029	268
Median Home Price	202,397	100,609	246,193	117,419	234,105	154,866
EPOP Ratio	0.627	0.057	0.620	0.065	0.604	0.079
Annual Juvenile Offenders	2,140	2,156	2,727	3,302	4,972	4,744
Annual Non-Juvenile Offenders	39,731	47,593	75,618	96,933	156,040	129,400
Annual Juvenile Victims	1,586	1,820	2,426	3,002	4,551	4,250
Annual Non-Juvenile Victims	28,989	38,802	57,794	76,107	120,943	101,498
Number of Cities	145		61		21	

Note: This table presents summary statistics for cities represented in the National Incident-Based Reporting System (NIBRS). Cities were divided into three categories: cities where no Lift Zones were launched during our study period, cities where at least one Lift Zone was launched, and cities where at least five Lift Zones were launched. Note that the second and third groups overlap. Covariates were obtained from 2014-2018 5-year American Community Survey (ACS) estimates compiled by the National Historical Geographic Information System (Manson et al., 2023). Four control cities in the NIBRS data could not be linked to NHGIS data. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Impacts of Lift Zones on Juvenile Offenders and Victims

	(1)	(2)	(3)	(4)	(5)
A. Juvenile Offenses					
Estimate	-0.170***	-0.171***	-0.182***	-0.136***	-0.011**
	(0.052)	(0.039)	(0.055)	(0.046)	(0.004)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	96.5	96.5	96.5	96.5	96.5
N	9,960	9,960	9,960	9,960	9,960
	(1)	(2)	(3)	(4)	(5)
B. Juvenile Victimization					
Estimate	-0.091**	-0.100***	-0.068**	-0.076***	-0.007**
	(0.037)	(0.028)	(0.033)	(0.024)	(0.003)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	80.6	80.6	80.6	80.6	80.6
N	9,960	9,960	9,960	9,960	9,960

Note: This table presents two–way fixed effects (TWFE) and synthetic difference–in–differences (SDID) estimates for the impact of Lift Zones on the log of city–level juvenile crime (ages 8–17). Each regression compares a treatment group of cities receiving 5 or more Lift Zones to a group of control cities that never received Lift Zones. We present estimates using both uniform treatment timing (beginning with the national launch of Lift Zones in September 2020) and staggered timing (accounting for localities which did not adopt Lift Zones immediately). The staggered TWFE estimate uses the Borusyak, Jaravel and Spiess (2023) imputation–based estimator. The continuous TWFE estimates in column (5) represent the impact of an additional Lift Zone on monthly juvenile crime, which are calculated by allowing the treatment variable to vary based on the number of Lift Zones present in the city in any given month. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: Impacts of Lift Zones on Juvenile Offenders and Victims

	Definition of Treated Cities				
	5+ LZs	4+ LZs	3+ LZs	2+ LZs	1+ LZs
A. Juvenile Offenses					
Staggered TWFE	-0.171***	-0.162***	-0.128***	-0.093***	-0.074**
	(0.039)	(0.037)	(0.035)	(0.032)	(0.030)
Staggered SDID	-0.136***	-0.122***	-0.088*	-0.045	-0.035
	(0.046)	(0.044)	(0.048)	(0.044)	(0.032)
N	9,960	10,080	10,320	10,860	12,360
Baseline (Monthly Counts)	96.5	90.4	86.3	73.5	51.9
# Treated Cities	21	23	27	36	61
# Control Cities	145	145	145	145	145
	Definition of Treated Cities				
	5+ LZs	4+ LZs	3+ LZs	2+ LZs	1+ LZs
B. Juvenile Victimization					
Staggered TWFE	-0.100***	-0.091***	-0.061**	-0.054**	-0.017
	(0.028)	(0.028)	(0.028)	(0.026)	(0.023)
Staggered SDID	-0.076***	-0.069**	-0.036	-0.027	0.001
	(0.024)	(0.032)	(0.034)	(0.029)	(0.026)
N	9,960	10,080	10,320	10,860	12,360
Baseline (Monthly Counts)	80.6	75.7	71.9	61.0	42.4
# Treated Cities	21	23	27	36	61
# Control Cities	145	145	145	145	145

Note: This table presents staggered TWFE and SDID estimates when redefining "treated" cities based on the number of Lift Zones received during the study time frame. The left–most column provides the baseline result, which assigns treatment status based on whether cities received 5 or more Lift Zones (and removing cities with 1–4 Lift Zones from the estimation sample). The next four columns present estimates when treatment assignment is expanded to cities that received fewer Lift Zones. The number of treated and control cities under each definition is provided in the bottom panel. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 4: Impacts of Lift Zones on Juvenile Offenders and Victims

	Placebo Treatment Date				Actual
	2019m7	2019m10	2020m1	2020m4	2020m9
A. Juvenile Offenders					
TWFE Estimates	0.009	-0.009	0.016	0.061	-0.170***
	(0.036)	(0.036)	(0.042)	(0.049)	(0.052)
SDID Estimates	0.017	-0.007	0.022	0.046	-0.182***
	(0.040)	(0.036)	(0.043)	(0.042)	(0.055)
	Placebo Treatment Date				Actual
	2019m7	2019m10	2020m1	2020m4	2020m9
B. Juvenile Victims					
TWFE Estimates	-0.029	-0.048	-0.024	0.012	-0.091***
	(0.039)	(0.042)	(0.053)	(0.070)	(0.037)
SDID Estimates	0.001	-0.027	-0.037	-0.023	-0.068***
	(0.041)	(0.040)	(0.048)	(0.061)	(0.033)
N	5,312	5,312	5,312	5,312	9,960

Note: This table presents placebo–in–time TWFE and SDID estimates, which assigns a placebo (uniform) treatment date and restricts the estimation sample to time periods prior to the actual Lift Zone launch date (September 2020). The right–most column presents the comparable SDID estimate using a uniform treatment date set to September 2020. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Impacts of Lift Zones on Non-Juvenile Offenders and Victims

	(1)	(2)	(3)	(4)	(5)
A. Non-Juvenile Offenses					
Estimate	0.018	0.026	0.029	0.012	0.002
	(0.041)	(0.033)	(0.027)	(0.030)	(0.003)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	2,561	2,561	2,561	2,561	2,561
N	9,960	9,960	9,960	9,960	9,960
	(1)	(2)	(3)	(4)	(5)
B. Non-Juvenile Victimization					
Estimate	0.012	0.026	0.024	0.016	0.002
	(0.043)	(0.034)	(0.031)	(0.033)	(0.004)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	1,965	1,965	1,965	1,965	1,965
N	9,960	9,960	9,960	9,960	9,960

Note: This table presents two–way fixed effects (TWFE) and synthetic difference–in–differences (SDID) estimates for the impact of Lift Zones on the log of city–level non–juvenile crime (18 or older). Each regression compares a treatment group of cities receiving 5 or more Lift Zones versus a group of control cities that never received Lift Zones. We present estimates using both uniform treatment timing (beginning with the national launch of Lift Zones in September 2020) and staggered timing (accounting for localities which did not adopt Lift Zones immediately). The staggered TWFE estimate uses the Borusyak, Jaravel and Spiess (2023) imputation-based estimator. The continuous TWFE estimates in column (5) represent the impact of an additional Lift Zone on monthly juvenile crime, which are calculated by allowing the treatment variable to vary based on the number of Lift Zones present in the city in any given month. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

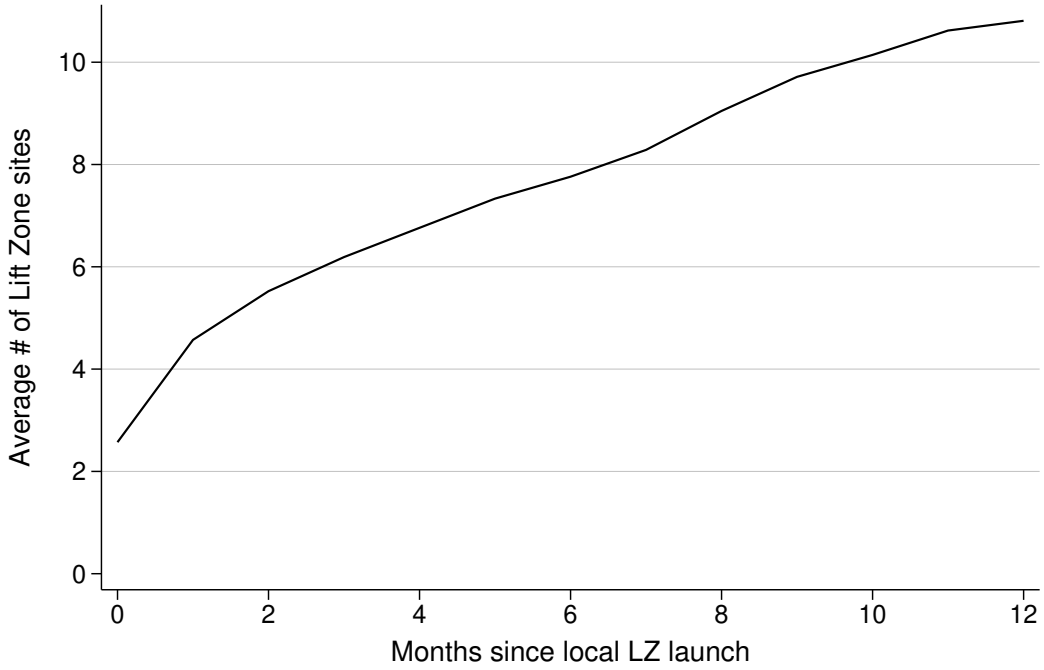
Table 6: Effect of Lift Zones on Localized Juvenile Victimization

	$\leq$ .25 mile (1)	$\leq$ .5 mile (2)	$\leq$ 1 mile (3)
A. All			
Lift Zone	-0.036	-0.026	0.219
	(0.089)	(0.158)	(0.329)
N	15,264	15,264	15,264
Pre-treatment mean	1.047	3.316	11.112
B. Violent crimes			
Lift Zone	-0.050	-0.054	0.085
	(0.076)	(0.135)	(0.295)
N	15,264	15,264	15,264
Pre-treatment mean	0.935	2.951	9.860
C. Non-violent crimes			
Lift Zone	0.017	0.036	0.124
	(0.030)	(0.055)	(0.096)
N	14,284	14,284	14,284
Pre-treatment mean	0.119	0.383	1.302

Note: This table reports the average marginal effect of a Lift Zones on localized juvenile victimization after estimating equation 5. Column (1) presents the estimates using a 0.25-mile radius around each community center. Columns (2) and (3) present the estimates when using a 0.5- or 1-mile radius. Standard errors clustered at the community center level are presented in parentheses. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

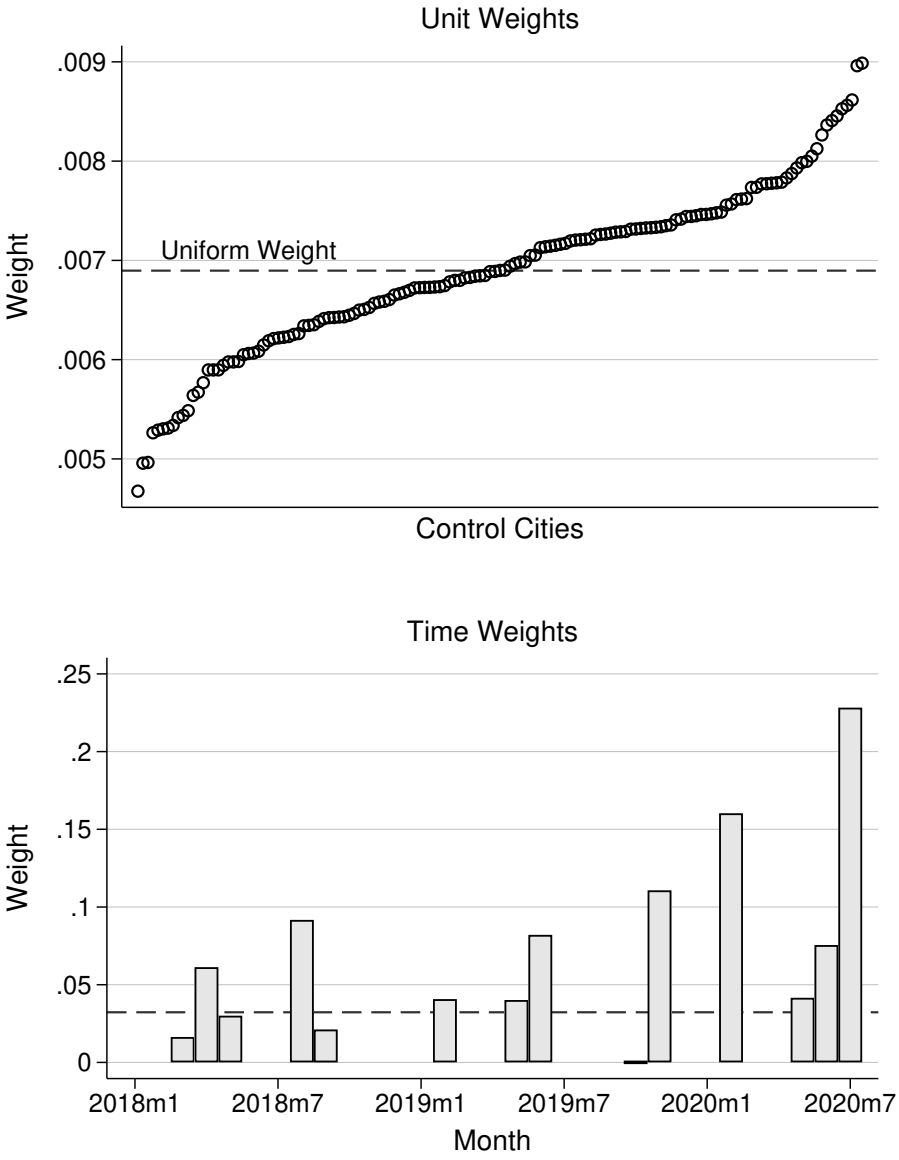
A Appendix Tables and Figures

Figure A.1: Trajectory of Lift Zone implementation in treated cities



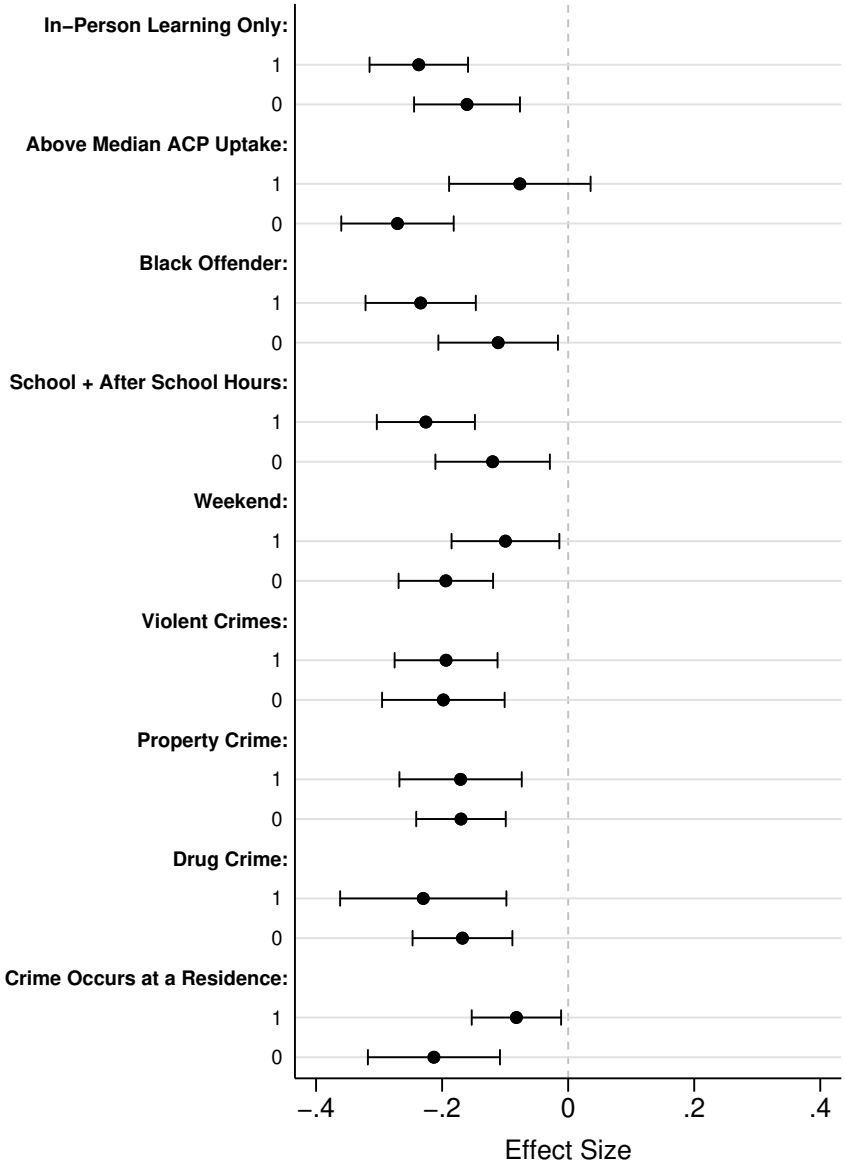
Note: This figure depicts how quickly Lift Zones "ramped up" in treatment cities that ultimately received 5 or more sites by the end of the study time period. The line depicts the average number of Lift Zones, relative to the number of months elapsed since the arrival of each city's first Lift Zone(s).

Figure A.2: SDID unit and time weights



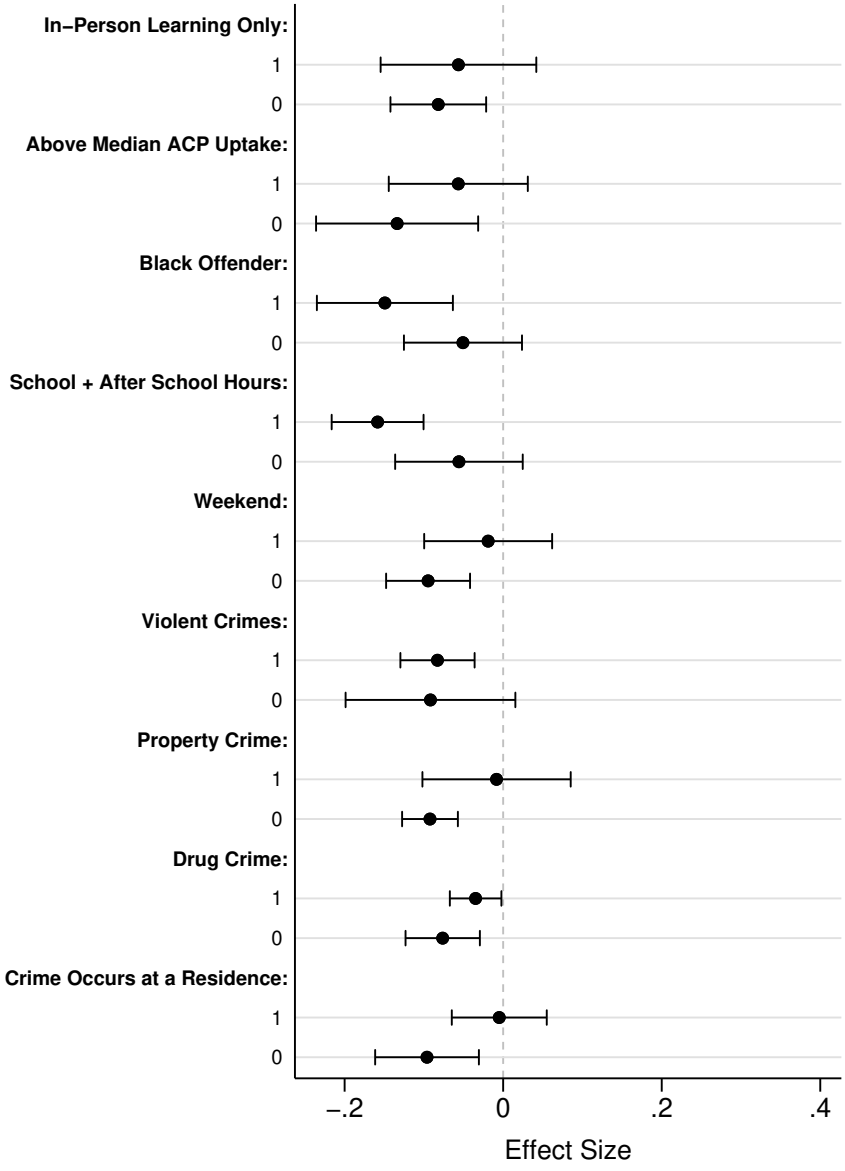
Note: This figure plots SDID unit weights assigned to each of the 145 control cities, as well as time weights assigned to each pre-treatment month. A reference line is drawn showing how much weight each city (month) would be given under uniform weighting. This figure specifically plots weights assuming a uniform treatment date; for the staggered implementation a separate set of weights is calculated for each treatment cohort.

Figure A.3: Heterogeneity in Offender Treatment Effects By Subgroup, using Staggered TWFE



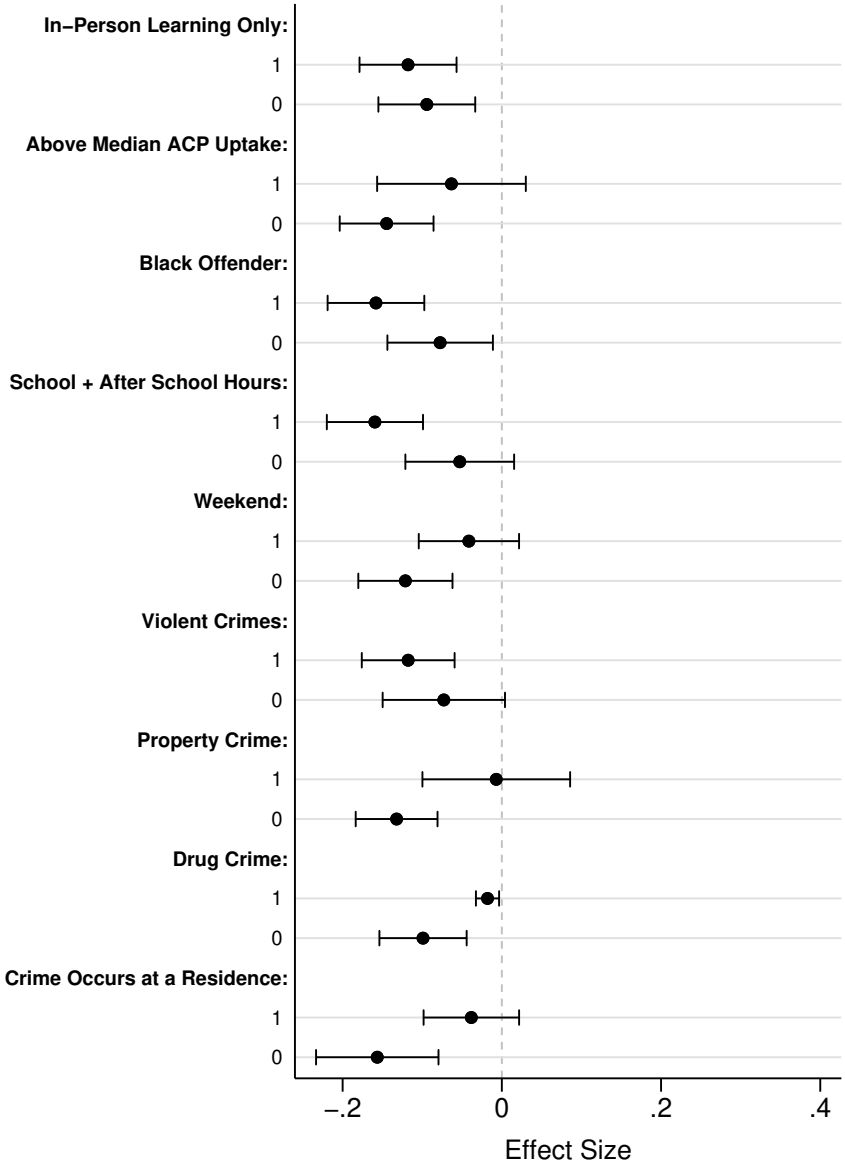
Note: This figure plots staggered TWFE estimates for youth offenders, restricting the sample as indicated on the graph. For example, "Black Offender: 1" represents the staggered TWFE estimate when restricting only to incidents with black youth offenders. While most of the subcategories in the figure are self-explanatory, the "In-person Learning Only" is an indicator of whether all students in a city exclusively received in-person learning. The "School + After School Hours" category includes crimes that occurred on weekdays between the hours of 8 am and 7 pm. The "Crime Occurs at a Residence" category is comprised of crimes that took place in someone's home.

Figure A.4: Heterogeneity in Victim Treatment Effects By Subgroup, using Staggered SDID



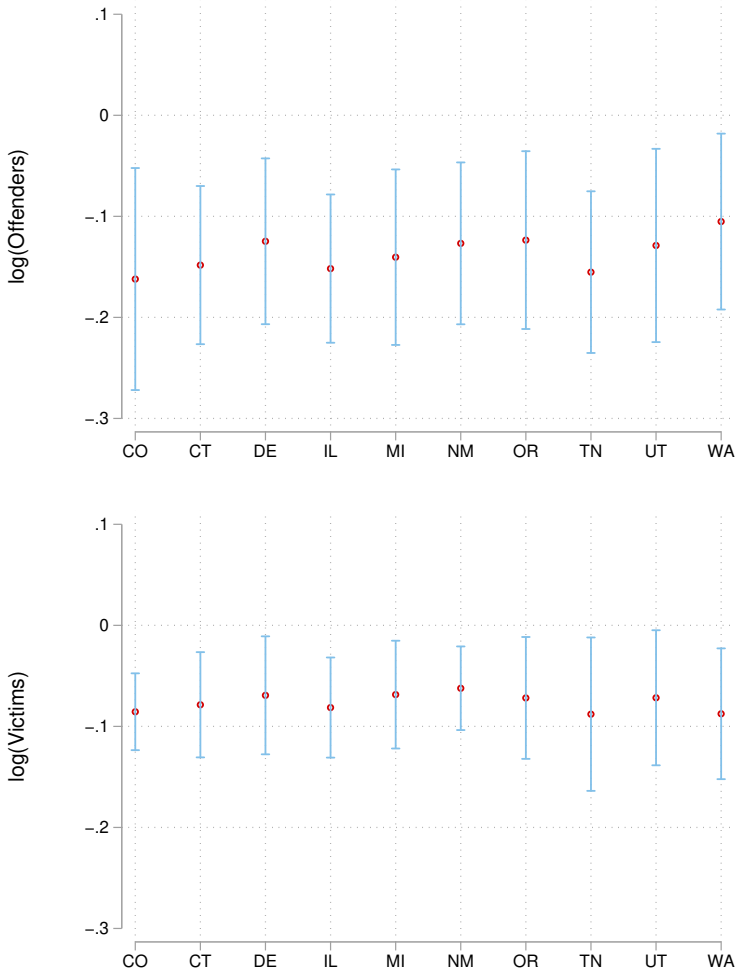
Note: This figure plots staggered SDID estimates for youth victims, restricting the sample as indicated on the graph. For example, "Black Offender: 1" represents the staggered SDID estimate when restricting only to incidents with black youth victims. While most of the subcategories in the figure are self-explanatory, the "In-person Learning Only" is an indicator of whether all students in a city exclusively received in-person learning. The "School + After School Hours" category includes crimes that occurred on weekdays between the hours of 8 am and 7 pm. The "Crime Occurs at a Residence" category is comprised of crimes that took place in someone's home.

Figure A.5: Heterogeneity in Victim Treatment Effects By Subgroup, using Staggered TWFE



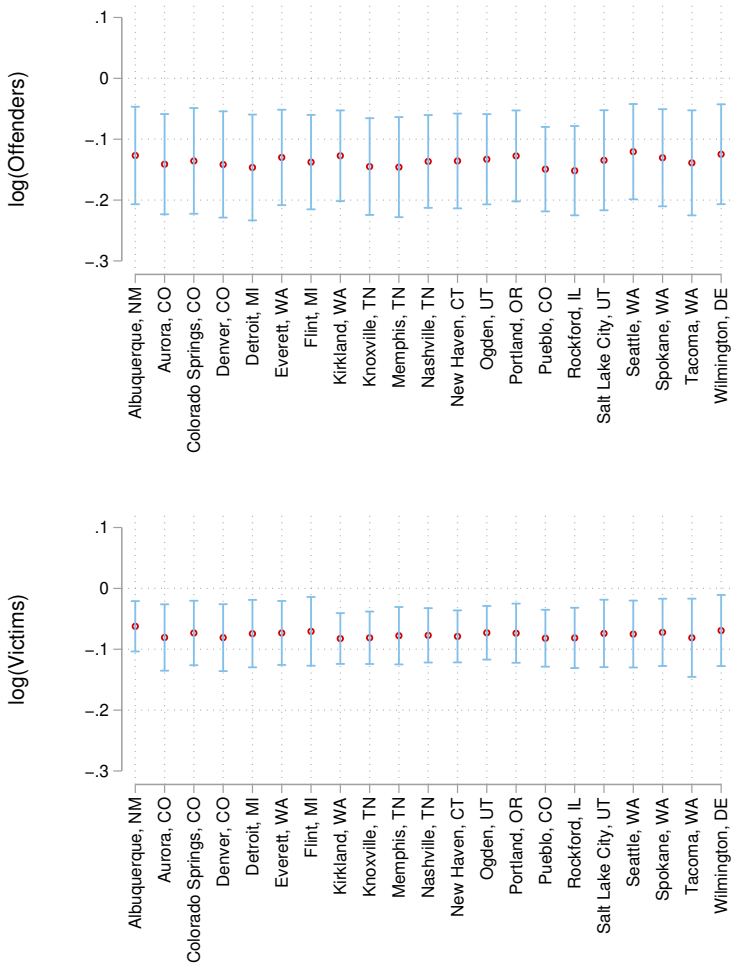
Note: This figure plots staggered TWFE estimates for youth victims, restricting the sample as indicated on the graph. For example, "Black Offender: 1" represents the staggered TWFE estimate when restricting only to incidents with black youth victims. While most of the subcategories in the figure are self-explanatory, the "In-person Learning Only" is an indicator of whether all students in a city exclusively received in-person learning. The "School + After School Hours" category includes crimes that occurred on weekdays between the hours of 8 am and 7 pm. The "Crime Occurs at a Residence" category is comprised of crimes that took place in someone's home.

Figure A.6: SDID Estimates Leaving Out One Treated State



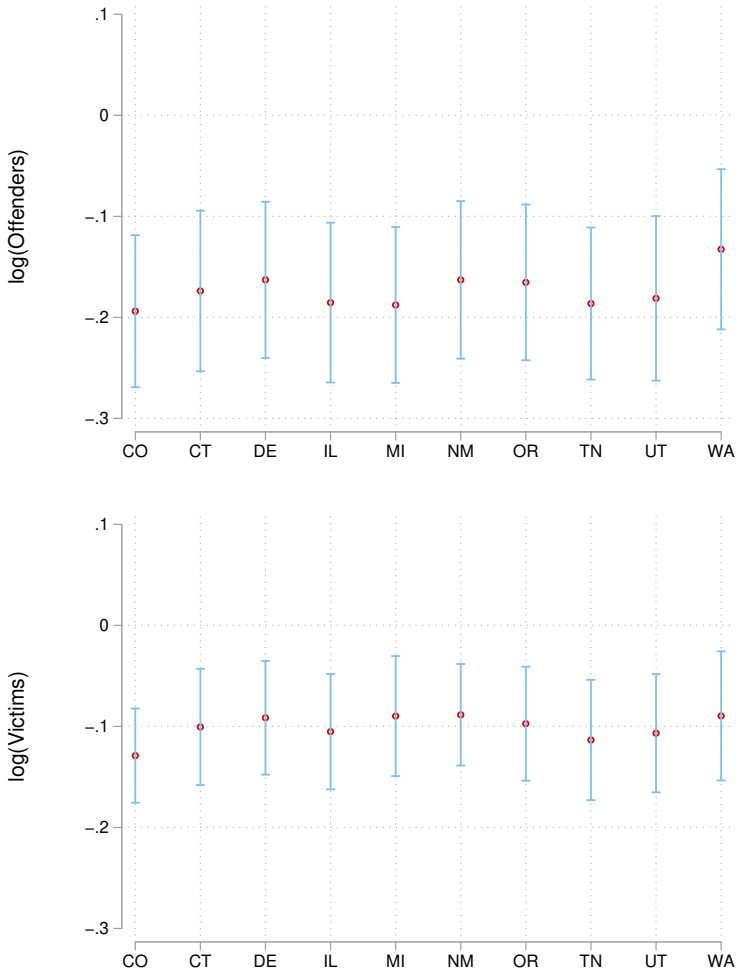
Note: This figure plots staggered SDID estimates for youth offenders and victims leaving out one treated state. The left-out state is indicated on the x-axis.

Figure A.7: SDID Estimates Leaving Out One Treated City



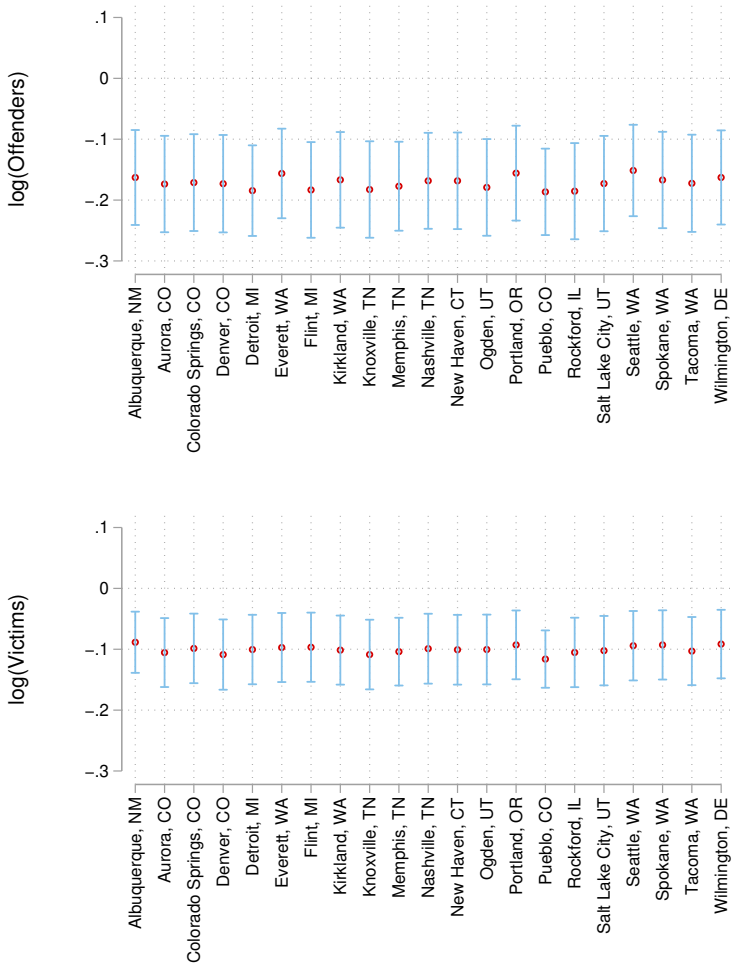
Note: This figure plots staggered SDID estimates for youth offenders and victims leaving out one treated city. The left-out city is indicated on the x-axis.

Figure A.8: TWFE Estimates Leaving Out One Treated State



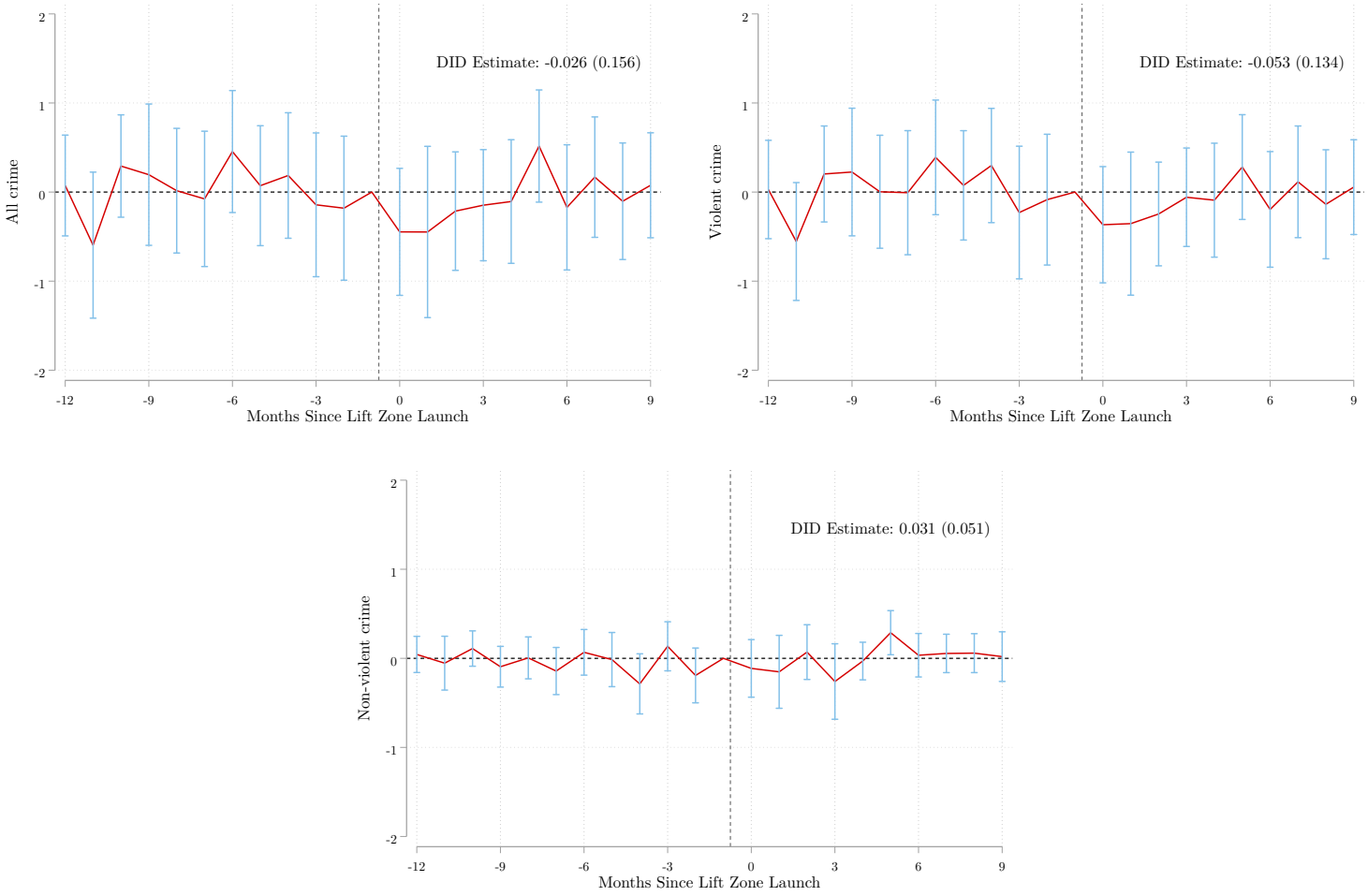
Note: This figure plots staggered TWFE estimates for youth offenders and victims leaving out one treated state. The left-out state is indicated on the x-axis.

Figure A.9: TWFE Estimates Leaving Out One Treated City



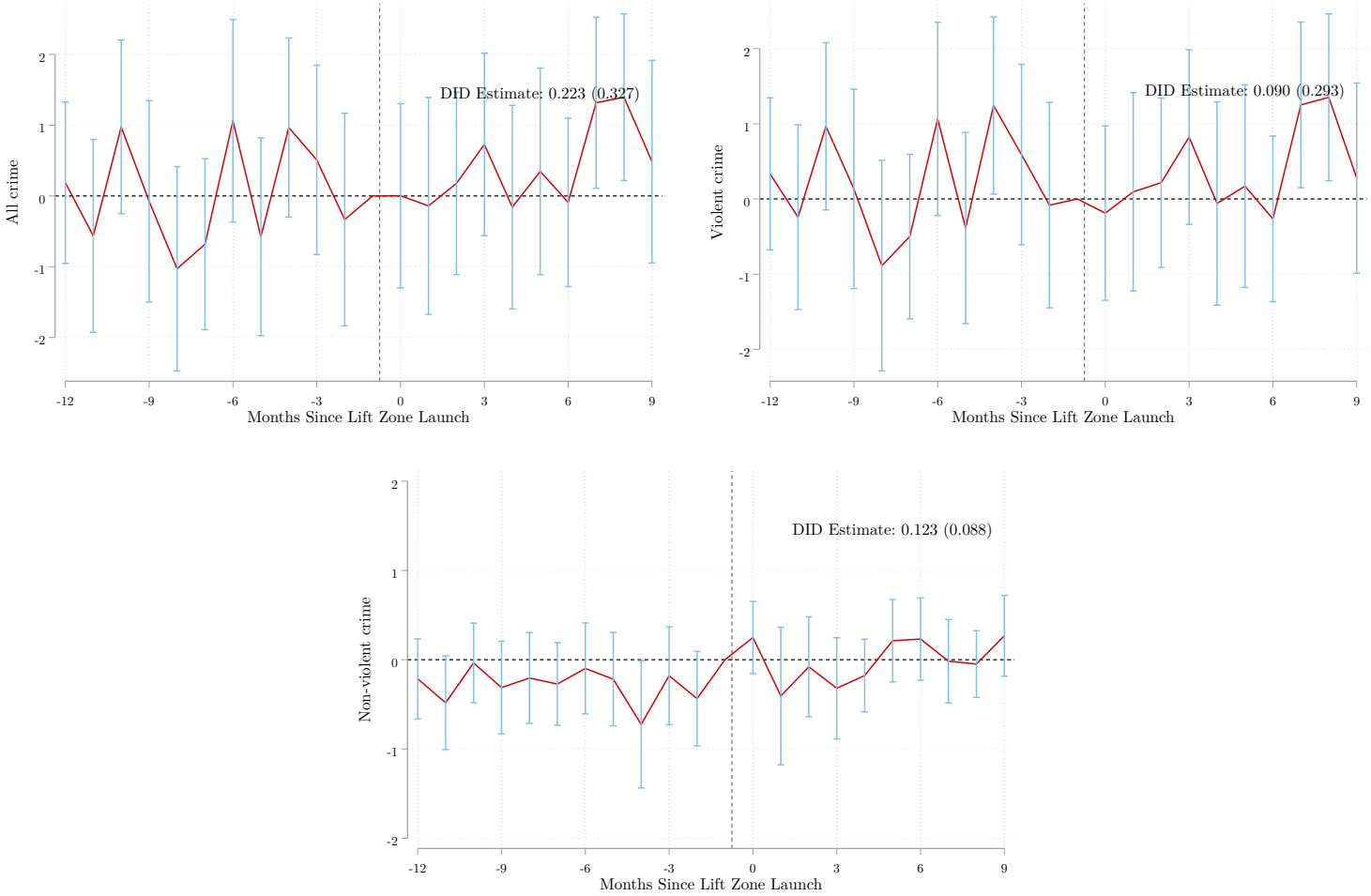
Note: This figure plots staggered TWFE estimates for youth offenders and victims leaving out one treated city. The left-out city is indicated on the x-axis.

Figure A.10: Localized Treatment Effects Within 0.5 Mile of Lift Zone



Note: We calculate and report the average marginal effect of a Lift Zone on the localized effect of juvenile victimization around a Lift Zone. The dependent variable is the count of the number of juvenile victims within a half-mile radius around each Lift Zone or counterfactual Lift Zone. Counterfactual Lift Zone locations are identified using publicly available community center locations in Baltimore and Chicago. All estimates are relative to $t = -1$, which is indicated by a vertical line. The first month a community center becomes a Lift Zone is indicated by $t = 0$. We plot 95% confidence intervals. Standard errors are clustered at the community center level.

Figure A.11: Localized Treatment Effects Within 1 Mile of Lift Zone



Note: We calculate and report the average marginal effect of a Lift Zone on the localized effect of juvenile victimization around a Lift Zone. The dependent variable is the count of the number of juvenile victims within a one-mile radius around each Lift Zone or counterfactual Lift Zone. Counterfactual Lift Zone locations are identified using publicly available community center locations in Baltimore and Chicago. All estimates are relative to $t = -1$, which is indicated by a vertical line. The first month a community center becomes a Lift Zone is indicated by $t = 0$. We plot 95% confidence intervals. Standard errors are clustered at the community center level.

Table A.1:
Monthly Juvenile Victimization Summary Statistics

		Near LZs			Near counterfactual LZs		
	Whole city	$\leq$ .25 mile	$\leq$ .50 mile	$\leq$ 1 mile	$\leq$ .25 mile	$\leq$ .50 mile	$\leq$ 1 mile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Baltimore							
All	146.396	0.823	2.419	7.912	0.607	2.102	7.278
	(44.803)	(1.194)	(2.357)	(6.274)	(1.099)	(2.372)	(6.078)
Violent crime	127.896	0.741	2.164	7.027	0.526	1.839	6.370
	(37.878)	(1.127)	(2.182)	(5.614)	(0.984)	(2.127)	(5.394)
Non-violent crime	18.500	0.082	0.255	0.885	0.081	0.263	0.909
	(9.942)	(0.308)	(0.561)	(1.272)	(0.335)	(0.620)	(1.304)
B. Chicago							
All	742.312	1.390	4.575	15.086	0.962	3.373	11.867
	(282.613)	(1.766)	(3.702)	(9.947)	(1.495)	(3.598)	(9.956)
Violent crime	643.014	1.251	4.084	13.413	0.827	2.911	10.213
	(240.216)	(1.634)	(3.422)	(9.082)	(1.349)	(3.296)	(9.105)
Non-violent crime	99.299	0.139	0.491	1.673	0.135	0.462	1.654
	(48.202)	(0.443)	(0.832)	(1.744)	(0.476)	(0.888)	(1.939)

Note: This table presents summary statistics for juvenile victimization in Chicago and Baltimore as reported by data provided by Baltimore and Chicago Police Departments. Panel A provides statistics for Baltimore, and Panel B provides statistics for Chicago. Column (1) provides the number of monthly juvenile victims and city-level counts of juvenile victims. Columns (2)-(4) report the number of crimes within a 0.25-, a 0.5-, or a 1-mile radius of Lift Zones. Columns (5)-(7) do the same for counterfactual Lift Zones. Standard errors are provided in parentheses.

Table A.2: Impacts of Lift Zones on Juvenile Offenders and Victims in Levels

	(1)	(2)	(3)	(4)	(5)
A. Juvenile Offenses					
Estimate	-23.111***	-20.366***	-10.964***	-8.481**	-1.093***
	(6.711)	(3.306)	(3.227)	(3.319)	(0.299)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	96.5	96.5	96.5	96.5	96.5
N	9,960	9,960	9,960	9,960	9,960
	(1)	(2)	(3)	(4)	(5)
B. Juvenile Victimization					
Estimate	-9.244***	-7.206***	-2.489	-1.662	-0.355
	(2.665)	(1.735)	(1.888)	(1.776)	(0.281)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	80.6	80.6	80.6	80.6	80.6
N	9,960	9,960	9,960	9,960	9,960

Note: This table presents two-way fixed effects (TWFE) and synthetic difference-in-differences (SDID) estimates for the impact of Lift Zones on the city-level juvenile crime (ages 8-17) in *levels*, in contrast to Table 2 which presents our primary results in logs. Each regression compares a treatment group of cities receiving 5 or more Lift Zones versus a group of control cities that never received Lift Zones. We present estimates using both uniform treatment timing (beginning with the national launch of Lift Zones in September 2020) and staggered timing (accounting for localities which did not adopt Lift Zones immediately). The staggered TWFE estimate uses the Borusyak, Jaravel and Spiess (2023) imputation-based estimator. The continuous TWFE estimates in column (5) represent the impact of an additional Lift Zone on monthly juvenile crime, which are calculated by allowing the treatment variable to vary based on the number of Lift Zones present in the city in any given month. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.3: Impacts of Lift Zones on Juvenile Offenders and Victims using Log(Crime Rates)

	(1)	(2)	(3)	(4)	(5)
A. Juvenile Offenses					
Estimate	-0.167***	-0.168***	-0.176***	-0.132***	-0.010**
	(0.055)	(0.042)	(0.055)	(0.052)	(0.005)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	96.5	96.5	96.5	96.5	96.5
N	9,960	9,960	9,960	9,960	9,960
	(1)	(2)	(3)	(4)	(5)
B. Juvenile Victimization					
Estimate	-0.088**	-0.096***	-0.064*	-0.068***	-0.006*
	(0.038)	(0.030)	(0.038)	(0.030)	(0.003)
Method	TWFE	TWFE	SDID	SDID	Continuous
Timing	Uniform	Staggered	Uniform	Staggered	Staggered
Baseline (Monthly Counts)	80.6	80.6	80.6	80.6	80.6
N	9,960	9,960	9,960	9,960	9,960

Note: This table presents two–way fixed effects (TWFE) and synthetic difference–in–differences (SDID) estimates for the impact of Lift Zones on the log of city–level juvenile crime rates (ages 8–17). Yearly population counts for each city are obtained from SEER population estimates, 2018-2022 (National Cancer Institute, 2025). Each regression compares a treatment group of cities receiving 5 or more Lift Zones to a group of control cities that never received Lift Zones. We present estimates using both uniform treatment timing (beginning with the national launch of Lift Zones in September 2020) and staggered timing (accounting for localities which did not adopt Lift Zones immediately). The staggered TWFE estimate uses the Borusyak, Jaravel and Spiess (2023) imputation–based estimator. The continuous TWFE estimates in column (5) represent the impact of an additional Lift Zone on monthly log juvenile crime rates, which are calculated by allowing the treatment variable to vary based on the number of Lift Zones present in the city in any given month. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A.4:
Monthly Juvenile Victimization Summary Statistics

		Near LZs			Near counterfactual LZs		
	Whole city	$\leq$ .25 mile	$\leq$ .50 mile	$\leq$ 1 mile	$\leq$ .25 mile	$\leq$ .50 mile	$\leq$ 1 mile
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Baltimore							
All	146.396	0.823	2.419	7.912	0.607	2.102	7.278
	(44.803)	(1.194)	(2.357)	(6.274)	(1.099)	(2.372)	(6.078)
Violent crime	127.896	0.741	2.164	7.027	0.526	1.839	6.370
	(37.878)	(1.127)	(2.182)	(5.614)	(0.984)	(2.127)	(5.394)
Non-violent crime	18.500	0.082	0.255	0.885	0.081	0.263	0.909
	(9.942)	(0.308)	(0.561)	(1.272)	(0.335)	(0.620)	(1.304)
B. Chicago							
All	742.312	1.390	4.575	15.086	0.962	3.373	11.867
	(282.613)	(1.766)	(3.702)	(9.947)	(1.495)	(3.598)	(9.956)
Violent crime	643.014	1.251	4.084	13.413	0.827	2.911	10.213
	(240.216)	(1.634)	(3.422)	(9.082)	(1.349)	(3.296)	(9.105)
Non-violent crime	99.299	0.139	0.491	1.673	0.135	0.462	1.654
	(48.202)	(0.443)	(0.832)	(1.744)	(0.476)	(0.888)	(1.939)

Note: This table presents summary statistics for juvenile victimization in Chicago and Baltimore as reported by data provided by Baltimore and Chicago Police Departments. Panel A provides statistics for Baltimore, and Panel B provides statistics for Chicago. Column (1) provides the number of monthly juvenile victims and city-level counts of juvenile victims. Columns (2)-(4) report the number of crimes within a 0.25-, a 0.5-, or a 1-mile radius of Lift Zones. Columns (5)-(7) do the same for counterfactual Lift Zones. Standard errors are provided in parentheses.

Table A.5: Average Yearly Crime Counts and Number of Lift Zones for Cities in the Sample

	Adult		Juvenile			
City	Offenders	Victims	Offenders	Victims	# of LZ	First LZ Date
A. Lift Zone Cities						
Denver, CO	60692	46323	1434	1550	33	2020m10
Detroit, MI	72123	60871	1617	2700	30	2020m11
Spokane, WA	23170	18895	974	850	20	2020m11
Seattle, WA	65376	52475	839	815	19	2020m12
Albuquerque, NM	53250	39142	1128	1266	18	2021m1
Salt Lake City, UT	25863	18575	582	492	15	2021m2
Colorado Springs, CO	28045	21114	1151	836	14	2021m1
Aurora, CO	26670	21049	1478	1228	12	2021m1
New Haven, CT	12130	9462	400	435	10	2021m3
Portland, OR	55846	43684	541	635	10	2020m12
Knoxville, TN	16089	11080	722	485	10	2021m4
Wilmington, DE	6882	5355	531	272	9	2020m11
Rockford, IL	11570	9272	874	575	9	2021m6
Flint, MI	4950	4547	205	243	8	2021m2
Nashville, TN	58356	42540	2998	1905	7	2020m12
Kirkland, WA	3054	2484	81	75	6	2021m10
Pueblo, CO	9577	7428	200	323	5	2021m1
Memphis, TN	85624	65486	3958	3350	5	2020m12
Ogden, UT	6875	4637	446	398	5	2021m2
Everett, WA	7300	5187	142	145	5	2021m1
Tacoma, WA	21927	18357	581	535	5	2020m11
Waterbury, CT	7660	5493	254	294	4	2021m4
Salem, OR	12672	7654	284	282	4	2020m12
Little Rock, AR	25858	21016	1009	1106	3	2021m2
Arvada, CO	6282	4568	295	214	3	2021m1
Grand Rapids, MI	14857	11588	1091	689	3	2021m4
Vancouver, WA	13240	10620	531	475	3	2021m1
Commerce City, CO	3947	2611	236	178	2	2020m10
Thornton, CO	8184	5706	566	402	2	2021m10
New Bedford, MA	5984	4838	392	344	2	2021m5
Springfield, MA	10659	8827	599	571	2	2021m4
Ann Arbor, MI	3790	3038	170	123	2	2021m6
Manchester, NH	7044	5330	401	359	2	2021m10
West Valley City, UT	9316	6598	436	413	2	2021m7

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City	Adult		Juvenile		Number of	Date of First
	Offenders	Victims	Offenders	Victims	Lift Zone	Lift Zone
Richmond, VA	16426	12156	401	334	2	2022m9
Bellingham, WA	6892	4215	152	114	2	2021m6
North Little Rock, AR	5228	3402	193	172	1	2021m6
Broomfield, CO	2898	1975	165	100	1	2021m9
Englewood, CO	3230	2506	169	95	1	2021m10
Longmont, CO	4548	3337	300	250	1	2022m3
Brockton, MA	4644	3945	210	225	1	2021m2
Fall River, MA	3569	2834	208	232	1	2021m4
Haverhill, MA	2103	1745	169	154	1	2021m6
Lowell, MA	4331	3669	215	187	1	2021m5
Battle Creek, MI	4424	3352	253	243	1	2021m11
Lansing, MI	7623	6040	331	299	1	2021m2
Warren, MI	6055	4347	332	255	1	2021m8
Nashua, NH	3134	2145	203	183	1	2021m12
Eugene, OR	10979	7912	286	291	1	2021m1
Hillsboro, OR	4248	2797	150	150	1	2021m2
Springfield, OR	3537	2278	180	138	1	2021m9
Charleston, SC	7119	4569	296	196	1	2022m2
Franklin, TN	2137	1135	145	80	1	2020m12
Layton, UT	3282	2135	382	267	1	2021m10
Logan, UT	1922	1145	260	153	1	2021m10
Alexandria, VA	5872	4491	349	199	1	2020m12
Federal Way, WA	6717	4898	171	206	1	2021m9
Lacey, WA	2564	1792	142	103	1	2021m6
Lakewood, WA	4934	3706	177	143	1	2021m4
Marysville, WA	2615	1628	134	116	1	2021m6
Renton, WA	6648	5082	148	149	1	2021m9
B. Control Cities						
Bentonville, AR	1986	1296	160	115	0	.
Conway, AR	5206	3393	271	225	0	.
Fayetteville, AR	7795	6061	294	223	0	.
Fort Smith, AR	9440	6885	456	344	0	.
Jonesboro, AR	6835	4740	395	286	0	.
Rogers, AR	3571	2102	274	183	0	.
Gilbert, AZ	8193	5062	612	456	0	.

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City	Adult		Juvenile		Number of	Date of First
	Offenders	Victims	Offenders	Victims	Lift Zone	Lift Zone
Yuma, AZ	4291	2733	327	197	0	.
Castle Rock, CO	1953	1275	124	77	0	.
Grand Junction, CO	4703	2931	247	173	0	.
Parker, CO	1812	1237	123	89	0	.
Westminster, CO	6583	4915	333	240	0	.
Danbury, CT	2303	1686	135	116	0	.
Norwalk, CT	2851	2268	123	104	0	.
Stamford, CT	4110	3263	189	185	0	.
West Haven, CT	2206	1643	56	63	0	.
Honolulu, HI	44412	36154	947	1233	0	.
Ankeny, IA	1864	1161	153	111	0	.
Cedar Rapids, IA	7583	5209	564	236	0	.
Davenport, IA	8256	5734	559	356	0	.
Dubuque, IA	3012	2074	409	241	0	.
Iowa City, IA	2882	2090	154	93	0	.
Sioux City, IA	6116	4036	514	283	0	.
Waterloo, IA	4143	2793	305	167	0	.
Boise, ID	9140	5963	580	401	0	.
Couer D'Alene, ID	2158	1313	184	127	0	.
Idaho Falls, ID	2543	1620	204	159	0	.
Meridian, ID	3203	1837	402	196	0	.
Nampa, ID	4534	2564	433	273	0	.
Pocatello, ID	2829	1884	330	197	0	.
Lenexa, KS	2072	1252	112	61	0	.
Shawnee, KS	2233	1634	134	104	0	.
Lexington, KY	17498	12281	622	506	0	.
Owensboro, KY	4446	2816	382	197	0	.
Chicopee, MA	2716	2182	151	165	0	.
Plymouth, MA	1708	1261	124	90	0	.
Worcester, MA	8848	7279	299	336	0	.
Canton, MI	2640	1817	169	119	0	.
Clinton Township, MI	3866	3150	182	183	0	.
Dearborn, MI	4172	2698	221	175	0	.
Dearborn Heights, MI	2423	1968	134	129	0	.
Kalamazoo, MI	8899	6840	624	390	0	.
Livonia, MI	3390	1966	173	121	0	.

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City	Adult		Juvenile		Number of	Date of First
	Offenders	Victims	Offenders	Victims	Lift Zone	Lift Zone
Saint Clair Shores, MI	1484	1122	98	82	0	.
Southfield, MI	3629	2985	109	113	0	.
Sterling Heights, MI	3518	2665	227	208	0	.
Taylor, MI	2960	2152	135	124	0	.
Troy, MI	2211	1459	106	58	0	.
Waterford, MI	1792	1499	126	106	0	.
Westland, MI	3556	2708	182	162	0	.
Wyoming, MI	3502	2354	283	230	0	.
Joplin, MO	6082	4041	159	152	0	.
Kansas City, MO	40797	32451	961	1327	0	.
St. Peters, MO	2277	1092	162	85	0	.
Billings, MT	9053	6062	415	367	0	.
Great Falls, MT	4803	3091	279	178	0	.
Missoula, MT	5188	3478	285	187	0	.
Bismarck, ND	5495	3087	419	247	0	.
Fargo, ND	9616	6400	501	350	0	.
Grand Forks, ND	3156	2263	233	147	0	.
Grand Island, NE	3415	2230	503	282	0	.
Akron, OH	15991	12612	823	677	0	.
Canton, OH	8203	6664	481	396	0	.
Cincinnati, OH	23752	19893	1483	799	0	.
Cleveland, OH	42421	36916	1262	1813	0	.
Columbus, OH	61989	52022	3047	2577	0	.
Dayton, OH	12578	10134	687	476	0	.
Toledo, OH	23621	18757	1494	1067	0	.
Edmond, OK	3188	2080	220	140	0	.
Norman, OK	6359	4945	315	274	0	.
Stillwater, OK	2273	1550	136	87	0	.
Bend, OR	3453	2334	162	124	0	.
Gresham, OR	5574	4346	122	143	0	.
Medford, OR	6823	3930	184	155	0	.
Cranston, RI	2203	1565	93	83	0	.
Pawtucket, RI	3524	2896	203	147	0	.
Providence, RI	10279	8872	250	294	0	.
Warwick, RI	2311	1462	121	78	0	.
Columbia, SC	9588	7140	322	236	0	.

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City	Adult		Juvenile		Number of	Date of First
	Offenders	Victims	Offenders	Victims	Lift Zone	Lift Zone
Greenville, SC	6102	3348	328	167	0	.
Mount Pleasant, SC	2495	1508	185	106	0	.
North Charleston, SC	13531	9752	793	559	0	.
Rapid City, SD	6252	4011	649	313	0	.
Sioux Falls, SD	11610	7535	758	427	0	.
Bartlett, TN	2401	1423	163	128	0	.
Chattanooga, TN	20213	13844	698	624	0	.
Clarksville, TN	10761	6558	620	401	0	.
Collierville, TN	1509	764	137	75	0	.
Jackson, TN	6700	4614	365	255	0	.
Johnson City, TN	4813	2744	215	133	0	.
Kingsport, TN	5405	3470	281	188	0	.
Murfreesboro, TN	9911	6578	407	357	0	.
Abilene, TX	7514	5434	640	476	0	.
Amarillo, TX	16876	12971	1042	816	0	.
Arlington, TX	24672	18052	1952	1472	0	.
Flower Mound, TX	1252	827	113	48	0	.
Fort Worth, TX	45492	33753	2419	1759	0	.
Frisco, TX	5347	3211	463	258	0	.
Georgetown, TX	1945	1306	178	83	0	.
Grand Prairie, TX	8501	5120	433	226	0	.
Harlingen, TX	4923	4072	297	196	0	.
Lewisville, TX	5203	3311	297	181	0	.
Longview, TX	5043	3046	248	159	0	.
Lubbock, TX	23676	17983	1625	1043	0	.
Mckinney, TX	4446	3186	337	196	0	.
Pearland, TX	4066	2367	309	166	0	.
Pflugerville, TX	1932	1163	152	54	0	.
Plano, TX	9899	6097	534	339	0	.
Rowlett, TX	1950	1500	207	145	0	.
San Angelo, TX	7024	5151	441	286	0	.
Temple, TX	4216	2601	219	182	0	.
Tyler, TX	6434	4234	320	216	0	.
Victoria, TX	4186	2817	231	136	0	.
Wylie, TX	914	512	119	59	0	.
South Jordan, UT	2544	1761	181	228	0	.

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City	Adult		Juvenile		Number of	Date of First
	Offenders	Victims	Offenders	Victims	Lift Zone	Lift Zone
St. George, UT	3487	1999	338	263	0	.
West Jordan, UT	5744	3881	450	288	0	.
Chesapeake, VA	12787	8919	845	609	0	.
Hampton, VA	8120	5599	414	290	0	.
Harrisonburg, VA	2140	1309	95	58	0	.
Leesburg, VA	1796	1145	231	187	0	.
Lynchburg, VA	4612	3200	298	196	0	.
Newport News, VA	11264	8011	641	442	0	.
Norfolk, VA	16863	13088	583	468	0	.
Portsmouth, VA	8954	7059	331	290	0	.
Roanoke, VA	10245	6883	439	338	0	.
Suffolk, VA	5002	3726	405	292	0	.
Virginia Beach, VA	17218	12147	1176	578	0	.
Bellevue, WA	6754	4931	167	122	0	.
Kennewick, WA	5439	3733	380	304	0	.
Kent, WA	9852	7369	329	325	0	.
Olympia, WA	3719	2657	105	100	0	.
Pasco, WA	3398	2350	259	203	0	.
Richland, WA	3022	2154	221	149	0	.
Spokane Valley, WA	7150	5147	283	228	0	.
Yakima, WA	6121	4587	332	326	0	.
Appleton, WI	3280	2009	351	215	0	.
Eau Claire, WI	3826	2140	282	169	0	.
Green Bay, WI	4758	2838	643	344	0	.
Janesville, WI	3314	1906	278	156	0	.
La Crosse, WI	3446	1980	227	115	0	.
Madison, WI	11448	8256	532	295	0	.
Milwaukee, WI	41726	32791	1605	1360	0	.
Racine, WI	3127	2248	271	206	0	.
West Allis, WI	3968	2204	295	160	0	.