KEEP THE KIDS INSIDE? JUVENILE CURFEWS AND URBAN GUN VIOLENCE

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Abstract—Gun violence is an important problem across the United States. However, the impact of government policies on gunfire has been difficult to test due to limited and low-quality data. This paper uses new, more accurate data on gunfire (generated by ShotSpotter audio sensors) to measure the effects of juvenile curfews in Washington, DC. Using variation in the hours of the DC curfew, we find that this policy increases gunfire incidents by 150% during marginal hours. In contrast, voluntarily reported crime measures (such as 911 calls) suggest that the curfew decreases gun violence, likely because of confounding effects on reporting rates.

I. Introduction

GUN violence is a chronic problem in the United States. Nationally in 2012, 11,622 people were killed by assault with a firearm.[1] Many more people are injured by guns each year: in 2011, 693,000 individuals were treated in emergency rooms for injuries due to assaults by firearms and similar mechanisms.[2] Gun violence takes a particularly large toll on young people. According to the CDC (2013), homicide accounts for 18% of deaths for males ages 15 to 24, more than for any other age group. For black males, homicide is the leading cause of death in that age group, accounting for roughly half of all deaths.

Cohen and Ludwig (2003) wrote that "policymakers who are concerned about America's problem with lethal violence must ask: how can we prevent young men from shooting one another?" This has been a difficult question to answer and continues to attract a great deal of academic and policy attention.[3] A primary challenge in studying gun violence is the selective underreporting of gunfire incidents. In this paper, we discuss the shortcomings of traditional, voluntarily reported crime measures and demonstrate the value of new data generated by audio sensor technology. As a case study, we use both types of data to measure the effect of juvenile curfew policies on urban gunfire.

Juvenile curfews are a popular but controversial policy that cities across the country use. They require young people to be home during the nighttime hours when crime is most prevalent. Their goal is to reduce criminal activity by keeping would-be offenders indoors, but these curfews might unintentionally reduce a deterrent effect that comes from having lots of people out on the streets. By incentivizing young people (and, by extension, their caregivers) to be at home, juvenile curfews remove potential bystanders and witnesses from public areas. Removing those people decreases the probability that any remaining offenders will get caught (because there are fewer witnesses who would call or assist the police), as well as the potential punishment (which would be higher if bystanders were injured). As Jane Jacobs wrote in 1961, "A well-used street is apt to be a safe street. A deserted street is apt to be unsafe." In addition, curfews change how police allocate their time. If curfew enforcement distracts police from more productive law enforcement activities, this too could reduce the deterrent effect. However, the net effect of juvenile curfews on public safety is unknown, and so the passage and enforcement of such policies continues unabated.

We use changes in curfew times in Washington, DC, to test the net effect of juvenile curfews on the number of gunfire incidents during marginal hours. DC's curfew time for anyone under age 17 is 11:00 p.m. on weeknights and midnight on weekends from September through June and midnight on all nights during July and August.[4] In other words, the weekday curfew time changes from midnight to 11:00 p.m. on September 1 and back to midnight on July 1, roughly following the school year. (We focus here on the September change because of concerns about data quality around the

Received for publication January 6, 2016. Revision accepted for publication July 26, 2017. Editor: Emir Kamenica.

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We thank Mark Anderson, Greg DeAngelo, David Eil, Bill Gale, Sara Heller, Judy Hellerstein, Mark Hoekstra, Sally Hudson, Jonathan Meer, Emily Owens, John Pepper, Ariell Reshef, Bernardo Silveira, Jeff Smith, Josh Teitelbaum, Carly Urban, and several anonymous reviewers for helpful comments. We also thank seminar participants at Georgetown Law, the University of Illinois at Urbana-Champaign, Montana State University, Purdue University, the University of Texas School of Law, UVA Batten, UVA Law, and Williams College, as well as conference participants at the Midwest Economics Association annual meeting, the IRP Summer Research Workshop, the Conference on Empirical Legal Studies, and the Southern Economics Association annual meeting.

A supplemental appendix is available online at http://www.mitpressjournals.org/doi/suppl/10.1162/rest_a_00720.

[1] These numbers do not include suicides. CDC report, "Deaths: Final Data for 2012," table 10.

[2] CDC: National Hospital Ambulatory Medical Care Survey: 2011 Emergency Department Summary Tables.

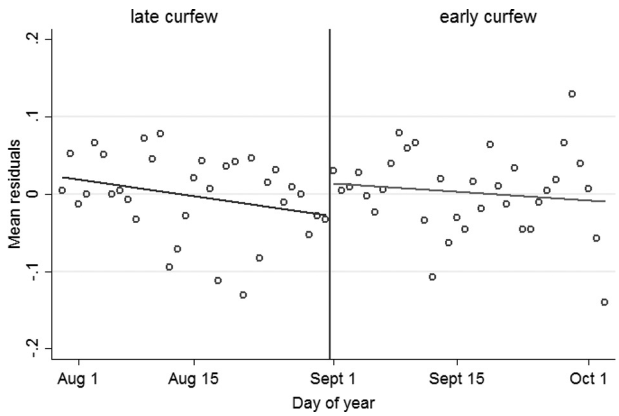
[3] Much of the literature on gun violence has focused on the effects of laws that restrict gun ownership or use (Cook, 1983; Lott & Mustard, 1997; Black & Nagin, 1998; Ludwig, 1998; Duggan, 2001; Marvell, 2001; Moody, 2001; Ayres & Donohue, 2003; Donohue, 2004; Mocan & Tekin, 2006; Duggan, Hjalmarsson, & Jacob, 2011; Cheng & Hoekstra, 2013; Manski & Pepper, 2015). Overall, there is mixed evidence that these laws improve public safety; results depend heavily on identification strategy and the data used.

[4] The Juvenile Curfew Act of 1995 states that individuals under age 17 cannot be "in a public place or on the premises of any establishment within the District of Columbia during curfew hours." Exceptions are made for several reasons, including if the juvenile is accompanied by a parent or guardian, is working, or is involved in an emergency. During most of the year, curfew hours are 11:00 p.m. on Sunday, Monday, Tuesday, Wednesday, and Thursday nights, until 6:00 a.m. the following morning. They are 12:01 a.m. until 6:00 a.m. on Saturday and Sunday (i.e., Friday and Saturday nights). During July and August, curfew hours are 12:01 a.m. to 6:00 a.m. every night. Juveniles who are caught violating curfew are taken to the nearest police station and released to the custody of their parents. They can also be sentenced to perform community service. Parents who violate the curfew law by allowing their child to be in public during curfew hours can be fined up to $500 per day. The curfew policy in Washington, DC, is very similar to policies in other cities across the country.

The Review of Economics and Statistics, October 2018, 100(4): 609–618

doi:10.1162/rest_a_00720

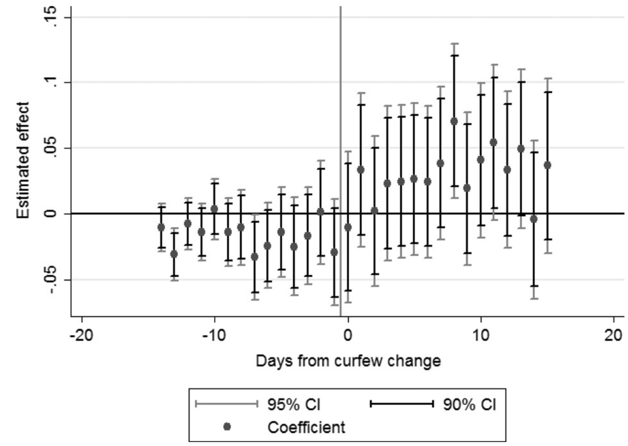
FIGURE 1.—EFFECT OF THE 11:00 P.M. CURFEW ON GUNFIRE INCIDENTS: DDD RESIDUALS



Points show differences in mean residuals, by day, from a regression including all of the control variables from equation (1) and a linear term controlling for the day of year. Analogous to a DDD estimator, we present the difference in mean gunshots during the 11:00 p.m. hour on weekdays and weekends, minus the difference in mean gunshots during the midnight hour on weekdays and weekends. The weekday curfew changes from midnight to 11:00 p.m. on September 1. Standard errors are clustered by day of year.

Source: Authors' calculations using ShotSpotter data from the Washington, DC, police department, 2006–2012. Geographic areas covered: police districts 3, 5, 6, 7; gunfire incidents are aggregated to the police service area (PSA).

FIGURE 2.—EFFECT OF THE 11:00 P.M. CURFEW ON GUNFIRE INCIDENTS BY DAY RELATIVE TO CURFEW CHANGE



The figure plots treatment variable coefficients and their 90% and 95% confidence intervals, by day, from the DDD analysis of the effect on gunfire during 11:00 p.m. on weekdays, relative to two sets of control hours: 11:00 p.m. on weekends and midnight. Day 0 is September 1, when the weekday curfew changes from midnight to 11:00 p.m. Standard errors are clustered by day of year.

Source: Authors' calculations using ShotSpotter data from the Washington, DC, police department. Sample: 11:00 p.m.–12:59 a.m., July 30 to October 4, 2006 to 2012. Geographic areas covered: police districts 3, 5, 6, 7; gunfire incidents are aggregated to the hour and PSA levels.

July Fourth holiday.[5]) With a nod to the concept of a witching hour,[6] we will henceforth refer to the treated hour, 11:00 to 11:59 p.m. on weekdays, as the "switching hour."

If curfews reduce crime, then when the curfew shifts to 11:00 p.m. rather than midnight, crime during the switching hour should go down. To identify the effect of the juvenile curfew, we use a triple-differences strategy to compare the change in the 11:00 p.m. hour on weekdays to changes during two sets of control hours: the 11:00 p.m. hour on weekends (which is always before curfew), and the midnight hour (which is always after curfew). Controlling for gunfire during these hours should isolate the effect of the curfew change from the effects of unrelated seasonal changes in gun violence.

We use the full universe of gunfire incidents detected by an audio sensor technology called ShotSpotter (described in more detail below) as our outcome measure. The main advantage of using ShotSpotter data in this context is that their accuracy is unaffected by the change in curfew time, so we avoid the potential confounding effect of a simultaneous change in reporting. We show that this confounding effect is important in this context.

Using ShotSpotter data, we estimate that the juvenile curfew in Washington, DC, increases the number of gunfire incidents. Figure 1 plots mean residuals from our main specification, which controls for a variety of time and spatial characteristics, as well as gunfire during the control hours described (11:00 p.m. on weekends and midnight on weekdays). There appears to be an increase in gunfire after September 1. Figure 2 is an event-study-style graph that plots the coefficients for our main specification for each day. It shows no change in the estimated effect before September 1 (which supports the identifying assumption that the treated and control hours exhibit parallel trends, as discussed in greater detail below), and an increase in the coefficients after the curfew switches to 11:00 p.m. on September 1. In our regression analysis, we estimate that the juvenile curfew causes 0.045 more gunfire incidents per police service area (equivalent to a police precinct) during the switching hour after September 1 than would have occurred without the curfew; this is a 150% increase relative to the late-curfew baseline. With 31 police service areas in our sample and five weekdays per week, this aggregates to seven additional gunfire incidents per week, citywide, during the switching hour alone.

Using voluntarily reported crime data such as 911 calls or reported crimes for this analysis would not allow us to study this effect. Like some other types of crime, gun violence is likely underreported in a highly selected manner. Particularly in inner-city communities that distrust the police, gunshots may not be reported unless the bullet hits someone and medical assistance is required (and even then, some individuals might avoid hospitals to avoid arrest or might have someone drive them to the hospital instead of calling 911 and drawing police to their neighborhood). Most policy interventions that aim to reduce gunfire probably also affect reporting rates. This makes it difficult to analyze policy effects on gunfire. Any empirical analysis based on traditional data—for instance, reported gunfire (via 911 calls) or gun-involved crime (via reported crime data)—will be biased, and often

[5] In particular, increased firecracker use leading up to July Fourth means that a large number of detected gunshots might be firecrackers that sound like gunfire, providing little information about public safety. We discuss this concern in more detail below.

[6] "The witching hour …was a special moment in the middle of the night when every child and every grown-up was in a deep deep sleep, and all the dark things came out from hiding and had the world all to themselves" (Dahl, 1982).

the direction of the bias will be unknown (Pepper, Petrie, & Sullivan, 2010).

In the past, these concerns have limited researchers to using homicide (which is reported with near-perfect accuracy) as an outcome measure. There are two problems with this. First, homicide is a relatively rare event that is becoming rarer as medical technology improves (saving more victims' lives), and this low incidence makes it difficult to detect policy effects. Second, homicide is not the only outcome of interest. Ideally, we would observe all instances where a gun threatened someone's safety. If we define gun violence in this way, a very small share of gun violence results in homicides.[7] ShotSpotter data from urban areas get closer to measuring all threatening uses of guns.

For comparison, we consider the effects of the curfew on reported crime and 911 calls, using geocoded data from DC's Metropolitan Police Department (MPD). The results are imprecise but generally suggest that the early curfew decreases gun violence. For instance, we estimate that the juvenile curfew reduces the number of 911 calls to report gunfire by 22%. This different (and we argue incorrect) conclusion is likely due to the simultaneous effect of the curfew on reporting behavior and highlights the problem with using voluntarily reported crime data for the study of gun violence.

A. *Background on Juvenile Curfews*

In general, violence-prevention policies can work one of two ways: by deterring violence[8] or by incapacitating would-be offenders.[9] If offenders have high discount rates and are unlikely to be deterred by future punishments (Becker, 1968), then limiting their opportunities to commit crime could be the most effective crime-prevention policy. With this in mind, cities across the United States have enacted and actively enforce juvenile curfews.

Juvenile curfews are common, but they are extremely controversial for several reasons. First, they give police officers discretion to stop any young-looking person who is out in public at night. Some worry this results in disproportionate targeting of racial minorities and contributes to tense relationships with law enforcement.[10] Second, they override the decisions of parents who would allow their kids to stay out late. Third, they may encourage kids at risk of abuse to return home to unsafe environments. Fourth, they divert police resources from other activities that may be more productive.

There is little previous work on the causal impacts of juvenile curfew policies. Kline (2012) studied the impact of juvenile curfews on juvenile and nonjuvenile arrest rates in cities across the country. He finds that juvenile arrests go down following the enactment of new curfew laws. He also finds evidence that arrest rates for older individuals decline, suggesting that juvenile curfews have spillover effects. However, arrest rates are a function of not only criminal behavior but police behavior and witnesses' behavior. Curfew laws likely affect all of these. Of particular concern, arrest rates might fall if witnesses and victims are less willing to cooperate with police as a result of heavy-handed curfew enforcement. Similarly, enforcing the curfew could distract police from solving criminal cases. Both effects could contribute to the reduction in arrests found in the study. The advantage of looking at arrest rates is that the age of the offender is known; the advantage of using ShotSpotter data is that we are able to isolate effects on actual criminal activity.

Another key difference between our paper and Kline (2012) is that our empirical strategies address slightly different but highly complementary questions: Kline considers the net effects of implementing new curfew policies, including any distrust they might generate. In contrast, we consider the effect of incentivizing local residents to go home during a marginal hour in a city with an existing curfew. This allows us to focus on the countervailing incapacitation and deterrent effects of curfews. However, our estimates do not capture these policies' effects on residents' trust of the police; such effects could worsen public safety further.

There is a slightly larger literature on other types of non-incarceration incapacitation policies. For instance, mandatory schooling keeps juveniles occupied when they might otherwise be unsupervised and likely to get into trouble. Anderson (2014) uses minimum dropout ages to measure the effect of mandatory school attendance on crime. He finds that minimum dropout age requirements decreased arrest rates for individuals aged 16 to 18 by 17%. Jacob and Lefgren (2003) also study the impact of school attendance on crime, using exogenous variation in teacher in-service days to estimate the causal impact of being in school on juvenile delinquency. They find that juvenile arrests for property crimes go down by 14% when school is in session, while juvenile arrests for violent crimes go up by 28%. This suggests that gathering juveniles in one place unintentionally increases interpersonal conflict that spills over into nonschool hours.[11]

We view our study as contributing to the academic literature in several ways. First, it measures the public safety impacts of juvenile curfews, a controversial but widely used

[7] In DC, only 0.5% of gunfire incidents result in a homicide (Carr & Doleac, 2016).

[8] Deterring crime requires changing the relative costs and benefits of committing a crime in such a way that would-be offenders rationally choose not to offend. Deterrence-based policies typically involve increasing the punishment or the probability of getting caught.

[9] Incapacitation is often thought of as synonymous with incarceration. In this paper, we follow the literature and refer to policies that operate by changing the relative costs and benefits of being in a particular location at a particular time as "incapacitation policies." The idea is that these policies reduce the opportunity to commit a crime rather than the relative costs and benefits of committing a crime per se. Mandatory schooling and summer jobs for teens are examples of policies that operate in this manner.

[10] A recent interview with several DC teenagers provides anecdotal evidence that this is a legitimate concern in the city. An excerpt: "Benn: And do you feel you're being protected and served by the police? Doné: No way. I feel more threatened by them than by anybody else. Benn: Would you all ever help a police officer to apprehend a criminal? Doné: No. Martina: Hell no." (Politico, 2015).

[11] Note that both of these studies again use arrest rates as outcome measures. Despite that measure's flaws, it is commonly used in crime studies because it includes information on offenders' ages.

crime-reduction policy about which there is little empirical evidence. Second, to our knowledge, this is the first study to use ShotSpotter data, or any other data generated by high-tech surveillance tools, to evaluate policy effects. It shows that using more accurate crime data based on sensors rather than human reporting can lead to qualitatively different conclusions about policy effects. Third, it considers the net effects of nonincarceration incapacitation policies on criminal behavior, thereby adding to a growing literature on this topic. Finally, it addresses gun violence, which is of particular interest in the United States but is generally very difficult to study due to the lack of reliable data.

The paper proceeds as follows: Section II describes the data; section III describes our empirical strategies; section IV describes our results and compares the gunfire effects with effects on reported crime and 911 calls; and section V discusses the results and concludes.

II. Data

A. *ShotSpotter*

ShotSpotter data have two key advantages over traditional reported crime data: they have accurate and precise time and location stamps, and they are not subject to selective underreporting that could bias empirical estimates. By using better data, we improve the precision of estimates by reducing measurement error; remove the selection bias resulting from variation in reporting rates over time, populations, and geographic areas; and eliminate the confounding effects of policies' simultaneous influences on reporting and crime.

We use ShotSpotter data from Washington, DC, from January 2006 through June 2013, aggregated to the level of police service areas (PSAs).[12] The technology was first implemented in police district 7 (Anacostia) in January 2006, then expanded to police districts 5 and 6 in March 2008, and to police district 3 in July 2008. These are the areas of DC that had the highest crime rates and so were expected to have the highest rates of gunfire. We therefore interpret our results as informative about the impacts of juvenile curfews in high-crime urban areas. While shots are detected outside of these targeted areas, we restrict our attention to districts 3, 5, 6, and 7, since the data from those areas are the most accurate.

ShotSpotter technology consists of audio sensors installed around the city. These sensors detect gunshots, and then triangulate the precise location of the sound. A machine-learning algorithm distinguishes the sound of gunfire from other loud noises, and human technicians verify those classifications.[13] Once verified, this information is relayed to law enforcement so that police officers can quickly respond to the scene.

There are some false positives or negatives in the data—that is, noises that are not gunshots but are recorded as gunshots, or gunshots that are missed—but in general, these mistakes will be randomly distributed and unaffected by the policy intervention we are studying. (The best evidence suggests the false-negative rate is very low—less than 1%. The false-positive rate is much more difficult to estimate, since the purpose of the technology is to detect gunfire that is not reported by others. See Carr & Doleac, 2016, for a review of current evidence on ShotSpotter's accuracy.) However, the false-positive rate will be higher when activity that sounds like gunfire is more likely. In particular, figure A.1 in the online appendix shows spikes in detected gunfire incidents around New Year's Eve and July Fourth. These spikes undoubtedly include some celebratory gunfire, but also false positives from fireworks and firecrackers. Of particular concern for our analysis, the use of firecrackers might increase leading up to the July Fourth holiday, as they tend to be more available in stores at that time of year. As expected, figure A.2 shows an increase in detected gunfire in the month before July Fourth. If this is largely due to local residents setting off firecrackers, it does not tell us anything meaningful about public safety. Of even greater concern, if juvenile curfews reduce this activity by incentivizing some of those firecracker users to go inside earlier, then it might appear that the curfew decreases gunfire when in fact it simply reduces firecracker use. (Alternatively, the curfew could increase firecracker use if this activity seems safer when streets are emptier.) This motivates us to exclude the July 1 curfew change from our main analysis, though we will show that our main estimate is similar if we include it.

Some readers might wonder if, by removing people from the streets, juvenile curfews reduce street noise, and if this improves the ability of the acoustic sensors to detect gunfire. We do not believe this is an issue here, for two reasons. First, the sensors are intentionally placed throughout the targeted coverage area in a way that ensures they are in reasonably close proximity to any potential gunshot. Concerns about reduced data quality outside of this targeted area, where sensors would be farther away, are the main reason we focus exclusively on the ShotSpotter-targeted police districts. Second, the decibel level of gunfire (166–170 decibels) is far louder than that even at a rock concert (110–120 decibels).[14] (Decibels are measured on a logarithmic scale, so a 10-decibel increase signifies a 10-fold increase in the sound intensity.) The firm Soundhawk measured street noise in several major U.S. cities, and its estimates ranged from 90 decibels on Wilshire Boulevard in Los Angeles, to 98 decibels at the intersection of Market and Geary Streets in San Francisco, to 104 in Times Square in New York City,[15] all levels that would not drown out the sound of gunfire. Sounds that are farther away will be less powerful, but since gunfire

[12] Each police district is composed of seven or eight PSAs; there are 31 PSAs in our sample.

[13] The sounds are classified as gunshots, construction, fireworks, car backfire, and so on. Only those classified as gunshot incidents are included in our data.

[14] Decibel ratings are from the Center for Hearing and Communication: http://chchearing.org/noise/common-environmental-noise-levels/.

[15] For more information, see http://elevatingsound.com/noise-levels-of-urban-america-why-the-city-soundscape-needs-to-be-transformed/.

TABLE 1.—SUMMARY STATISTICS

	Number	Mean	SD	Minimum	Maximum
Gunshots in Washington, DC					
Daily DC SST-detected incidents	483	7.795	5.659	0	38
Daily DC SST-detected incidents, 11:00 p.m.–midnight	483	0.969	1.442	0	10
Daily DC SST-detected incidents, midnight–1:00 a.m.	483	0.874	1.293	0	9
Gunshots at the PSA level (geographic unit of analysis)					
Daily PSA SST-detected incidents	11,799	0.319	0.813	0	11
Daily PSA SST-detected incidents, 11:00 p.m.–midnight	11,799	0.040	0.259	0	6
Daily PSA SST-detected incidents, midnight–1:00 a.m.	11,799	0.036	0.230	0	5
Crime at the PSA level (geographic unit of analysis)					
Daily PSA MPD reported crimes	4,278	1.791	1.552	0	15
Daily PSA MPD reported crimes, 11:00 p.m.–midnight	4,278	0.097	0.324	0	3
Daily PSA MPD reported homicides	4,278	0.006	0.079	0	1
Daily PSA MPD reported homicides, 11:00 p.m.–midnight	4,278	0.001	0.026	0	1
Daily PSA MPD reported gun-involved crimes	4,278	0.142	0.390	0	3
Daily PSA MPD reported gun-involved crimes, 11:00 p.m.–midnight	4,278	0.011	0.109	0	2
Daily PSA MPD reported violent crimes	4,278	0.448	0.696	0	4
Daily PSA MPD reported violent crimes, 11:00 p.m.–midnight	4,278	0.034	0.188	0	2
Daily PSA 911 calls	4,278	17.975	7.666	1	52
Daily PSA 911 calls, 11:00 p.m.–midnight	4,278	0.937	1.104	0	8
Daily PSA 911 calls for police	4,278	13.896	6.214	0	41
Daily PSA 911 calls for police, 11:00 p.m.–midnight	4,278	0.766	0.973	0	7
Daily PSA 911 calls reporting gunshot	4,278	0.142	0.423	0	4
Daily PSA 911 calls reporting gunshot, 11:00 p.m.–midnight	4,278	0.012	0.116	0	2

Geographic areas covered: police districts 3, 5, 6, 7. Sample: 11:00 p.m.–12:59 a.m., July 30–October 4. ShotSpotter data on gunshot incidents cover 2006-2012. Reported crime and 911 call data cover 2011–2012. Data source: Washington, DC, police department.

and other street noise occur on the street level, surrounded by ShotSpotter sensors, the effect of differences in relative distance will be negligible.[16] Given that the sound of gunfire is approximately 1 million times as powerful as street noise in Times Square (60 decibels difference = 10^6 times as powerful), we do not expect changes in street noise in DC to affect the rate at which ShotSpotter detects gunfire. However, ultimately there is no way for us to test this. We thus proceed on the assumption that juvenile curfews do not affect ShotSpotter's data quality.

One detail that is missing from ShotSpotter data (because there is no way for the sensors to detect it) is the context in which the gun was fired: Was it fired in anger, with the intent of harming someone? Was it fired recklessly, in a place where bystanders could be hurt? Or was it fired into a wall during target practice, or into the ground to test whether the gun worked? It would be helpful to be able to distinguish the first two circumstances from the last two, but unfortunately we cannot. In dense urban areas such as Washington, DC, discharging a weapon within city limits for any reason is illegal because it is presumed likely that someone could get hurt. We will discuss the ShotSpotter data as if every gunshot detected in DC is dangerous but acknowledge that some might not be. At the very least, every gunshot is a crime in this context.

ShotSpotter is currently active in over ninety cities in the United States; while considered proprietary in most locations, the data used in this paper are available from the MPD via public records request. The data include the date and time that the gunfire incident was detected, the latitude and longitude of the incident, and whether the incident consisted of a single gunshot or multiple gunshots. During the period of interest (July 30 through October 4, 2006–2012), there was an average of 7.8 gunfire incidents per day across the police districts where ShotSpotter was implemented. On average, 1.0 per day occurred during the 11:00 p.m. hour. Table 1 presents summary statistics.

B. *Reported Crime and 911 Calls*

For comparison, we repeat the main analysis using data on reported crime and 911 calls from the MPD. Our goal is to see how our conclusions about the curfew's effect would differ if we used traditional crime measures that are sensitive to changes in reporting. We therefore construct outcome measures that, without ShotSpotter data, would be the best available to study gun violence.

We use geocoded data on reported crime and 911 calls from 2011 and 2012, aggregated to the PSA level.[17]

The reported crime data include reports of homicide, sexual abuse, assault with a dangerous weapon, robbery, burglary, arson, motor vehicle theft, theft from an automobile, and other theft. We code the first four crime types as "violent crimes" and also consider homicide separately. The data include information on the weapon used, if any; we code any crime in which a gun is listed as the weapon as a "gun-involved crime." The time and location

[16] As distance from the source, r, increases, sound intensity decreases by $1/r^2$. This means that doubling the distance from the source drops sound intensity by only about 6 decibels. See http://www.sengpielaudio.com/calculator-squarelaw.htm for more.

[17] Due to a technical problem at the MPD, geocoded data on reported crime are not available for dates prior to January 2011. The MPD does not maintain 911 call data for more than three years, so these are also unavailable before January 2011.

stamps will generally be less precise than in the ShotSpotter data.[18]

The 911 call data include all calls for service, not necessarily for the police. The outcome measures of interest in this data set are all calls, calls for police, and calls to report gunshots. As for the reported crime data, the geocodes and time stamps will be less precise than in the ShotSpotter data.[19]

As above, we restrict our analysis to the areas covered by ShotSpotter (police districts 3, 5, 6, and 7). Summary statistics are in table 1.

C. *Other Data*

We use weather data (temperature and precipitation) from the National Oceanic and Atmospheric Administration (NOAA) as controls. Local data are available at the richest level (hourly and daily) based on measurements from the Reagan National Airport weather station, located just outside the city. We also collect information on DC public school calendars directly from the school district, to control for school year start and end dates.

III. Empirical Strategy

We exploit the September 1 curfew change from midnight to 11:00 p.m. as a natural experiment that allows a triple-differences analysis. Beginning on that date, the switching hour is treated by the curfew, but there is no curfew change in other hours.[20] The change in the curfew time roughly follows the academic year; school starts in late August in DC.[21] For this reason, we need to be careful to isolate the effects of the curfew time from that of unrelated seasonal changes in activity.

We use two sets of control hours to do this. The first is the 11:00 p.m. hour on weekends, and the second is the midnight hour. Both sets of control hours are subject to the same seasonal changes in activity but are not affected by the curfew change. Using a triple-differences (DDD) specification, we compare the difference in gunfire during the 11:00 p.m. hour (which is treated) to gunfire during the midnight hour (which is a control), across weekdays (treated) and weekends (control), testing for a differential effect during the switching hour. If the juvenile curfew is driving any observed change in gunfire, the curfew time change should affect the 11:00 p.m. hour on weekdays (the switching hour), netting out the effects on the control hours. (We will confirm below that the curfew does not shift crime to or from the midnight hour.) We estimate the following using ordinary least squares:

$$\begin{aligned} Gunshots_{h,d,p} &= \alpha + \beta_1 EarlyCurfew \times Weekday \times 11pmHour_h \\ &\quad + \beta_2 Time_d + \beta_3 Time \times EarlyCurfew \times Weekday \\ &\quad \times 11pmHour_h + \delta_1 School_d + \omega_d + \lambda_{dayofweek} \\ &\quad + \gamma_{year} + \rho_{PSA} + e_{d,p}, \end{aligned} \tag{1}$$

also including all single and pairwise combinations of the *EarlyCurfew*, *Weekday*, and *11pmHour* terms, as well as those terms interacted with *Time*.[22] In equation (1), h is the hour of observation, d is the day of observation, and p is the PSA. *Early Curfew* is an indicator for whether the weekday curfew time is 11:00 p.m. instead of midnight (i.e., whether the date is September 1 or later). Our baseline specification controls for a linear time trend based on the day of the year and allows the slope to vary before and after the curfew change, represented in equation (1) by the terms *Time* and $Time \times EarlyCurfew \times Weekday \times 11pmHour$; the estimates are similar if we instead control for quadratic or cubic functions of time (as discussed in section IV). *School* indicates whether the school year is in session and controls for any independent effect of school attendance on behavior. ω_d is a vector of weather variables, including temperature and precipitation. $\lambda_{dayofweek}$, γ_{year}, and ρ_{PSA} are fixed effects for day of the week, year, and PSA, respectively. Standard errors are clustered by day of the year to account for serial correlation across hours and spatial correlation across PSAs. The primary coefficient of interest is β_1; this tells us if the early curfew time has an effect on the level of gunfire during the switching hour, relative to the control hours. The outcome measure, *Gunshots*, is the number of gunshot incidents detected by the ShotSpotter sensors.[23] The sample is 11:00 p.m. to 12:59 a.m. on weekdays and weekend days.[24]

Recall that figure 1 plots residuals for the DDD specification (excluding the treatment variable and other interactions with *EarlyCurfew*). Figures A.3 and A.4 break this DDD approach into its two component difference-in-difference comparisons. These figures, respectively, plot residuals for the difference in gunfire between 11:00 p.m. on weekdays and 11:00 p.m. on weekends and between 11:00 p.m. and midnight on weekdays. We see an increase in gunfire after

[18] The geocodes are reported at the block level. The times are often estimates based on victims' and witnesses' recollections or the time the incident was reported, or both.

[19] The geocodes will often be the address where the caller is calling from rather than the location of the crime (this will be particularly problematic for reported gunshots, where the shots could have been fired blocks away from where they were heard). The time stamp is when the call was received.

[20] As discussed, we do not use the July 1 curfew time change in our main analysis due to the confounding effect of changes in firecracker use around the July Fourth holiday.

[21] School start dates vary from year to year, and do not perfectly coincide with the September 1 curfew change: August 28, 2006; August 27, 2007; August 25, 2008; August 24, 2009; August 23, 2010; August 22, 2011; and August 27, 2012.

[22] For a complete list of these control variables, see table A.2.

[23] We get very similar estimates if we use a Poisson model, and if we use a logit model with a binary outcome measure of whether any gunshots were detected.

[24] Our sample includes days within 33 days of September 1. In a previous version of this paper, we used a regression discontinuity design with an Imbens-Kalyanaraman optimal bandwidth of 33 days. We abandoned that empirical approach due to insufficient statistical power, but kept the 33-day cutoff to define the sample since we are unaware of a better way to define an optimal sample for a DDD analysis. Our estimates are very similar for a range of sample definitions from five to fifty days on either side of the curfew change; results available on request.

September 1 when comparing 11:00 p.m. on weekdays with the same hour on weekends. There appears to be a slight upward change in trend after September 1 when comparing 11:00 p.m. on weekdays with midnight on weekdays.

As mentioned in section II, we are concerned that data around the July 1 curfew change are heavily contaminated by false positives (firecrackers) and that that false-positive rate might be affected by the curfew (if the curfew affects firecracker use). We therefore focus our main analysis on the September 1 change, but will also show the effect when both curfew changes are included.

Our identifying assumption in this analysis is that absent the curfew change, the amount of gunfire during the switching hour would have evolved similarly to that in the control hours. (The control hours are not necessarily unaffected by seasonal changes; their purpose is to absorb any seasonal changes that would also affect the switching hour.) This is commonly referred to as the parallel trends assumption. Figure 2 supports this assumption: there is no apparent trend in gunfire during the switching hour, relative to the control hours, before September 1. We check that the trends during treated and control hours appear similar during the preperiod in two additional ways: graphing preperiod gunfire during the treated and control hours and formally testing for differences in trends during the preperiod by allowing the time trend to differ for treated and control hours and testing for whether any difference is statistically significant.

Figure A.5 shows residualized daily gunfire during the treated and control hours leading up to September 1. The graphs are noisy, but during this preperiod, the amount of gunfire during the switching hour appears to track that during the 11:00 p.m. weekend hour particularly well. During the midnight hour, the trends do not track quite as closely but still do not appear to diverge. More rigorously, table A.1 shows there is no significant difference between the preperiod trends in the 11:00 p.m. and midnight hours or the 11:00 p.m. weekday and 11:00 p.m. weekend hours. Combined with figure 2, this provides evidence that these control hours are good counterfactuals for the switching hour in the weeks leading up to the curfew time change and supports the assumption that they would continue to be good counterfactuals after the curfew change. We therefore proceed with the DDD analyses when using the ShotSpotter data.

We also test the parallel trends assumption for the reported crime and 911 call data. Because both reporting and actual crime affect these traditional crime measures, our parallel trends assumptions may be less likely to hold now than before. We consider a formal test of whether preperiod trends differ for these new outcome measures and table A.3 shows the results: there is no significant difference between the weekday and weekend trends, but there is a marginally significant difference between the 11:00 p.m. and midnight trends in reported crime. To be conservative, we will focus on difference-in-difference (DD) estimates using 11:00 p.m. on weekends as control hours for our comparison analyses.

IV. Results

Table 2 presents the results of the ShotSpotter data analysis. Column 1 shows our main result, which estimates the effect of the juvenile curfew on the number of gunfire incidents. We find that an earlier curfew increases the number of gunfire incidents by 0.045 during the switching hour, or 150% of the baseline. Note that while this estimate is statistically significant ($p < 0.05$) it is imprecise; the 95% confidence interval suggests gunfire incidents increase by 19% to 280% of the baseline, though even a 19% increase in gunfire is economically meaningful. Recall that observations are at the PSA level, and our data cover 31 PSAs across the city. The estimated effect size of 0.045 implies seven additional gunfire incidents per week during the switching hour alone.

The coefficients for the time trend (and interactions thereof) are omitted from table 2 (the full results of our DDD specification are in table A.2). We do not find an effect of the curfew change on the trend in gunfire: the coefficient on *Time* × *Early Curfew* × *Weekday* × *11:00 p.m. Hour* is near zero and statistically insignificant.

Column 2 of table 2 estimates the DDD effect when both the July and September curfew changes are included.[25] The data from June and July add a great deal of noise, so the standard error is larger, but the coefficient suggests that the curfew increases gunfire by 0.037 incidents, very close to our main estimate of 0.045.

For robustness, we conduct the DDD analysis using different functional forms of the time trend. Recall that our preferred function is linear; we consider quadratic and cubic functions as well. Estimates, presented in columns 3 and 4 of table 2, are nearly identical to our preferred estimate in column 1, though their statistical significance decreases, as including more terms reduces statistical power.

Next, we test the effect of the curfew change on gunfire during the other hours of the day. If our effects are being driven by the change in the deterrent effect due to the curfew change, we should see only a statistically significant increase during the switching hour. That is what we find. Figure 3 graphs the estimates by hour. Using a DD specification with only weekend hours as controls (because the concern is that all hours are treated, we do not want to select another hour on weekdays as a control), we see that estimates are generally small and all are statistically insignificant except for the positive estimate for the 11:00 p.m. (switching) hour. However, there is evidence that part of that increase is due to shifting from other times.[26] The coefficient for 10:00 p.m. is negative and about half the size[27] of our switching hour estimate; this suggests that some of the observed increase in gunfire is

[25] We exclude July 1–7, days that are extreme outliers due to holiday fireworks.

[26] The net effect over the full day is an increase of 0.016 incident; this aggregate effect is statistically insignificant but indicates that not all of the increase during the switching hour was due to the shifting of gunfire from other hours.

[27] The coefficient for the 10:00 p.m. hour is −0.025.

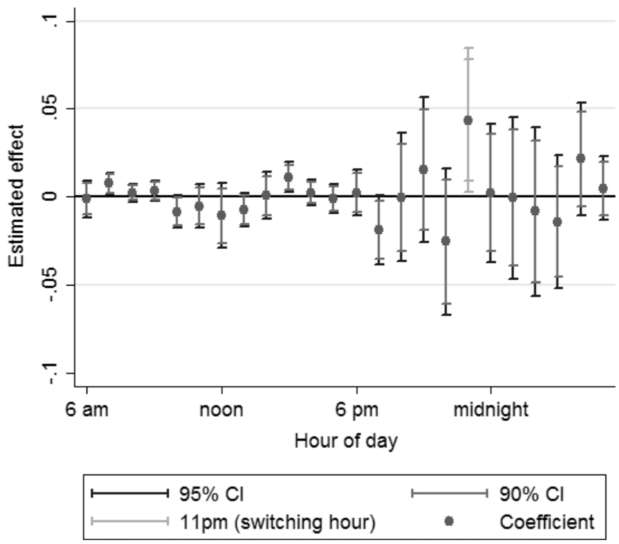
TABLE 2.—EFFECT OF THE 11:00 P.M. CURFEW ON GUNFIRE INCIDENTS

	Main Specification (1)	Include July Curfew Change (2)	Quadratic Time Trend (3)	Cubic Time Trend (4)
SST-detected gunshot incidents				
Early curfew × Weekday × 11:00 p.m. hour	0.045**	0.037	0.035	0.047
	(0.020)	(0.034)	(0.021)	(0.031)
Observations	22,914	40,186	22,914	22,914
Late curfew mean	0.030	0.034	0.030	0.030

*$p < .10$, **$p < .05$, and ***$p < .01$. Standard errors are clustered by day of year and are shown in parentheses. Results are from DDD regressions from equation (1), controlling for a linear time trend, unless otherwise noted. Outcome measure is the number of gunshot incidents. Analysis uses data from police districts 3, 5, 6, and 7, and gunfire incidents are aggregated to the hour and police service area (PSA) levels. Sample: 11:00 p.m.–12:59 a.m., July 30–October 4 (or May 30–October 2 when the July curfew change is included) for 2006 to 2012. All specifications include the following control variables: whether school is in session; year, day of week, and PSA fixed effects; precipitation; and temperature.

ShotSpotter data source: Washington, DC, police department. Weather data source: NOAA.

FIGURE 3.—EFFECT OF THE 11:00 P.M. CURFEW ON GUNFIRE INCIDENTS BY HOUR OF DAY



The figure plots treatment variable coefficients and their 90% and 95% confidence intervals from a difference-in-difference regression of gunfire incidents during the specified hour of the day, on all control variables from equation (1), and netting out gunfire during 11:00 p.m. on weekends to control for seasonal trends. Standard errors are clustered by day of year.

Source: Authors' calculations using ShotSpotter data from the Washington, DC, police department. Sample: 11:00 p.m.–12:59 a.m., July 30 to October 4, 2006–2012. Geographic areas covered: police districts 3, 5, 6, 7; gunfire incidents are aggregated to the hour and PSA levels.

due to people delaying gunfire until after the curfew time.[28] This is consistent with the hypothesis that people respond to the relative deterrent effects across these hours caused by the decrease in potential witnesses and bystanders. We see no effect in the midnight hour, which we use as a control hour in our DDD analysis. (This is important because if the curfew affected both hours—by shifting postcurfew gunfire from midnight to the switching hour—then midnight would not be a valid control and the DDD estimate would be biased upward.)

Finally, we estimate the effect of the curfew on reported crime and 911 calls for comparison. Recall that we are now using a DD specification with 11:00 p.m. on weekends as control hours. The results are presented in table 3. Since we are using a shorter time period, we lose a great deal of statistical power, but the ShotSpotter data estimates are similar in magnitude to those presented above. The DD specification estimates that the earlier curfew increases gunfire incidents by 154% of the baseline (close to the respective 150% estimate using the longer time period). In contrast, the reported crime and 911 call data suggest that the earlier curfew decreases gun violence.

The results are less precise than before and should be considered suggestive. However, it is striking that most of the coefficients are negative when using reported crime and 911 call data. Indeed, the outcomes most likely to be used to measure gun violence—gun-involved reported crimes and 911 calls to report gunfire—have negative coefficients. Using those traditional data sources would therefore lead to the opposite conclusion about the effectiveness of this policy. Our estimates suggest that the curfew reduces the number of gun-involved crimes by 107% and the number of 911 calls to report gunfire by 22%. These qualitatively different results are likely due to confounding changes in reporting rates. In general, these comparison results could be biased upward or downward; the direction of the bias is unclear a priori. The typical solution to this problem, using homicide as an outcome measure, does not help here: there were only three homicides during the switching hour—far too few to produce a meaningful estimate.

V. Discussion

In this paper, we use new gunshot incident data from ShotSpotter to measure the effects of a citywide juvenile curfew on gun violence. These data are not affected by the selective underreporting that plague traditional reported crime data. The resulting empirical estimates do not suffer from the biases that make empirical results throughout the literature difficult to interpret. This is crucial for determining the true impact of any policy on public safety.

The curfew policy in Washington, DC, was enacted in 1995 as an effort to improve public safety. Similar curfew laws are in effect across the United States, but they are

[28] The fact that not all gunfire is shifted from other hours is consistent with the finding in Doleac and Sanders (2015) that violent crime is not easily shifted across the day.

TABLE 3.—EFFECT OF THE 11:00 P.M. CURFEW ON OTHER MEASURES OF GUN VIOLENCE

		MPD Reported Crime				911 Calls for Service		
	ShotSpotter Incidents (1)	All (2)	Homicides (3)	Gun Involved (4)	Violent (5)	All (6)	For Police (7)	To Report Gunshots (8)
Early curfew × Weekday	0.0261	−0.0271	−0.0026	−0.0128	0.0197	0.1081	0.0709	−0.0029
	(0.0243)	(0.0445)	(0.0021)	(0.0168)	(0.0233)	(0.1784)	(0.1629)	(0.0146)
Observations	4154							
Late curfew mean	0.017	0.100	0.000	0.012	0.04	0.885	0.721	0.013

*$p < .10$, **$p < .05$, and ***$p < .01$. Standard errors, clustered by day of year, are shown in parentheses. Results are from difference-in-difference regressions of the curfew on various outcomes during the 11:00 p.m. hour on weekdays (the treated hour), netting out changes in the outcomes during 11:00 p.m. on weekends (the control hour). Analysis uses data from police districts 3, 5, 6, and 7, and all outcomes are aggregated to the hour and PSA levels. Sample: 11:00 p.m.–12:59 a.m., July 30–October 4, 2011–2012. All specifications include all control variables from equation (1) and a linear time trend. Column 1 shows effects on ShotSpotter incidents; columns 2–5 show effects on reported crime; columns 6–8 show effects on 911 calls. ShotSpotter, reported crime, and 911 call data source: Washington, DC, police department. Weather data source: NOAA.

controversial and in some cases have been ruled unconstitutional. While their goal is to improve public safety through an incapacitation effect, incentivizing local residents to go home and distracting cops from other duties could reduce a deterrent effect on street crime. The net effect of the policy is theoretically ambiguous. We show that in this city, the juvenile curfew law increases the number of gunfire incidents, by 0.045 additional incidents per PSA, during the switching hour. With 31 PSAs in our dataset, this aggregates up to 1.40 extra gunfire incidents per weekday, or 6.98 extra gunfire incidents per week, across police districts 3, 5, 6, and 7.

Our results suggest that curfew laws are not a cost-effective way to reduce gun violence. In fact, we find that curfews increase gun violence. This does not necessarily mean that juvenile curfews are not cost-effective more broadly. We cannot measure impacts on other types of crime, particularly minor offenses. It could be that curfews reduce those offenses enough to offset the increase in gun violence and the infringement on juveniles' rights and parents' choices. It is also possible that even if curfews do not reduce the number of gunshots, they might reduce the number of victims when there are fewer innocent bystanders in the area. Saving lives is, of course, a good thing. However, residents may not consider such an impact evidence of a real improvement in public safety, since the curfew makes it more dangerous to be outside. (Preventing victimization by telling people to hide in their homes is clearly not a solution.) Finally, juvenile curfews might increase domestic violence by incentivizing at-risk kids (and their caregivers) to be home at night. This is an important potential cost that should be considered.

Empirical evidence on this topic is particularly necessary in light of broader concerns about how to improve trust between law enforcement and city residents. Juvenile curfews are the type of policy that many worry worsens tensions between inner-city communities and the police. Ours is only the second rigorous examination of the effects of juvenile curfews on public safety. This is probably due in part to the difficulty of convincingly identifying impacts of a policy that could affect both criminal activity and reporting rates. Indeed, we show that we would get a qualitatively different result if we were using voluntarily reported crime data, such as 911 calls to report gunfire. ShotSpotter data on gunfire incidents provide a unique opportunity to isolate the effect on crime from the effect on reporting.

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