The Impact of Juvenile Curfew Laws on Arrests of Youth and Adults*

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Abstract: Youth curfew ordinances are a widely touted, yet little studied, policy tool available to local police departments. This paper evaluates the effectiveness of curfew ordinances by comparing the arrest behavior of various age-groups within a city before and after curfew enactment. The evidence suggests that curfews are effective at reducing both violent and property crimes committed by juveniles below the statutory curfew age. Arrests of adults and youth above the curfew age also appear to decrease in the wake of curfew enactment, however these effects are smaller and statistically insignificant.

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> "[Youth curfews] help keep our children out of harm's way. They give parents a tool to impart discipline, respect, and rules at an awkward and difficult time in children's lives."
>
> - Bill Clinton (1996)

> "Can you tell the difference between a 19-year-old (who may be exempt from a curfew) and a 17-year-old (who may not be)? A law that gives the police the right – indeed, requires them – to stop people on the basis of their perceived age is an invitation to trouble."
>
> - Geoffrey Canada (1996), president of Rheedlen Centers for Children and Families

Youth curfews have become a popular tool for combating juvenile delinquency. A survey by Ruefle and Reynolds (1996a) found that 146 of the 200 American cities with population above 100,000 had curfew laws on the books, with 110 having enacted or revised their ordinances between 1990 and 1995. A subsequent study by the U.S. Conference of Mayors (1997) found that 80% of the 347 cities with population over 30,000 had youth curfews.

Despite their popularity with local governments, existing studies find little evidence to support the notion that curfews are effective at reducing crime (Males and Macallair, 1999; McDowell et al., 2000; Reynolds et al., 2000).[1] This paper reassesses this conclusion, extending the literature by combining newly collected data on local ordinances with an event study research design comparing the arrest behavior of various age-groups within a city before and after curfew enactment. A key contribution is the separate assessment of the impact of curfew ordinances, which normally only

apply to youth under the age of 16 or 17, on arrests of youth subject to the curfew law and those above the city's statutory maximum curfew age.

Analyzing both sets of impacts is important because curfew policies can be thought of as constituting two treatments, each applying to a different set of age-groups. The first treatment, the *statutory* treatment, is that of being subject to a curfew citation, fine, temporary detention, or whatever punishment is statutorily prescribed for curfew violations by minors. This treatment only affects those youth under the statutory curfew age. The second treatment, the *statistical discrimination* treatment, is that of being subject to lower standards of probable cause as a result of one's perceived youth. Police are unlikely to be able to distinguish ex-ante between young people just above and below the curfew age. Thus, for adjacent age-groups curfews should raise the probability of being stopped or searched by an amount that depends very little on one's actual age. The possibility of this second effect is frequently cited by the American Civil Liberties Union (ACLU) as an argument for reversing such ordinances on the grounds that they constitute violations of fundamental civil liberties.

Constitutional issues aside, both treatments should be of interest to economists. The statutory treatment represents the deterrent effect of the curfew's statutory sanctions. Identifying this effect tells us how much crime could be reduced by raising penalties or increasing enforcement of curfew ordinances.[2] It also implicitly provides an estimate of an important margin in the economics of crime: the substitutability of criminal activity across time.[3] Indeed, if, as in basic economic models (Becker, 1968), crime is a purposive activity, then curfews should only reduce delinquency if

the technology used to produce this behavior is imperfectly substitutable between curfew and non-curfew hours.

The statistical discrimination treatment tells us the impact of weakening Fourth Amendment protections against unreasonable search and seizure. Estimates of this margin are important not only for those interested in evaluating the costs and benefits of the age discrimination implicit in youth curfews, but those involved in recent debates over racial profiling and national security. Furthermore, these estimates are closely tied to the elasticity of criminal behavior with respect to the probability of detection, a key parameter in Becker's classic model.

Under the assumption that police cannot distinguish between adjacent ages ex-ante, comparisons of the response of age-groups just below the curfew age to those just above will estimate the statutory treatment effect. Similarly, statistical discrimination effects can be estimated by comparing the response of age-groups just above the curfew age to those several years older. The presence of non-trivial statutory or statistical discrimination effects may also induce a second order effect on the crime rates of adults due to cross-age interactions. These effects on adults may be identified provided curfew laws are not enacted in response to trends in adult crime, a condition which appears to be satisfied in the data.

To preview the results, I find strong evidence of persistent statutory treatment effects on criminal behavior. Arrests for Part I offenses appear to fall by around 10% in the three years following curfew enactment. Though the results are imprecise, there do appear to be spillover effects on young adults and adults over age 25,

with both groups exhibiting roughly equivalent *declines* in arrests in response to enactment. Hence, the data are less supportive of statistical discrimination effects than the hypothesis that criminal propensity is positively dependent across age-groups.

Section II provides background on curfew legislation, Section III describes the econometric methodology, Section IV describes the data, Section V provides results, Section VI concludes.

II A Brief Introduction to Youth Curfew Laws

Juvenile curfews are local ordinances proscribing minors, generally within a specified age range, from occupying public areas and streets during particular times. These policies are not new. The first youth curfew was enacted in Omaha, Nebraska in 1880 (Hemmens and Bennett, 1999). In 1884, President Harrison gave a speech endorsing curfews as "the most important municipal regulation for the protection of the children of American homes, from the vices of the street" (Note, 1958). By 1957, 57 of the 109 cities with 1950 population over 100,000 reported having curfews (Note, 1958).

Although many cities have long had curfew statutes, the most recent period of sustained enforcement came in the early 1990s when violent crime and victimization of juveniles began to rise and cities learned to craft curfew legislation in a manner amenable to the courts. Prior to this period, many cities had been unable to defend

their curfews from legal challenges that they violate civil rights, especially the First, Fourth, and Fourteenth Amendments of the Constitution.

Many cities, fearful of challenges by the ACLU and others, either allowed enforcement of their laws to lapse or in some cases actually repealed existing ordinances. It is against this backdrop that Dallas, Texas passed a new curfew ordinance in 1991 that would become a model for many other American cities. The Dallas curfew was narrowly tailored to apply to youth of specified ages, at specific times, and had a number of exemptions to the law including for youth accompanied by an adult, responding to an emergency, and traveling to or from school, work, or a religious service. Furthermore, the parameters of the curfew were designed to deal with the specific needs of the city. The city had collected data showing, among other things, that juvenile delinquency increased proportionally with age between the ages of 10 and 16, that the time during which murders by juveniles were most frequently committed was between 10 p.m. and 1 a.m., that these murders most often occurred in apartments, parking lots, and streets and highways, that aggravated assaults by juveniles were most likely to occur between 11 p.m. and 1 a.m., and that 31 percent of robberies occurred on public streets and highways (Department of Justice, 1996).

The ACLU challenged Dallas's law shortly after it was introduced, causing a judge to issue an injunction against its immediate enforcement. In Qutb v. Strauss (11 F. 3d 488, 1993) the U.S. Court of Appeals for the Fifth Circuit upheld the law, arguing that the city demonstrated that the ordinance was sufficiently narrowly tailored and that it met a compelling state interest, the two conditions necessary for passing the "strict scrutiny" test of constitutional infringement. An appeal was made to the

Supreme Court which refused to hear the case, thus setting a precedent for the design of youth ordinances. Even before the Fifth Circuit upheld the law, local governments were paying close attention to the construction of the Dallas curfew. Cities such as Miami, FL, El Paso, TX, and San Antonio, TX explicitly modeled their programs on Dallas's. By 1996, President Clinton was publicly touting youth curfews as an effective policy for combating juvenile delinquency and the Department of Justice and U.S. Conference of Mayors were issuing briefings to local governments on best practices for curfew creation (U.S. Department of Justice, 1996; U.S. Conference of Mayors, 1997).

The Dallas Model

Although the specifics of curfew implementation vary by city, it is worth delving into the details of the Dallas program so that we might understand the issues involved. The Dallas curfew applied to all youth under the age of 17 and proscribed them from being in public places during the hours of 11 pm – 6 a.m. on weekdays and 12 a.m. – 6 a.m. on weekends. Before the curfew was implemented on May 1, 1994, the Dallas Police Department put out public service announcements in English and Spanish on the radio and in poster form to announce that the ordinance would soon be enforced. They also held a well covered press conference explaining details of the law. Furthermore, a week before the curfew was implemented police handed out warning fliers to youth in public during curfew hours.

Once the curfew actually went into effect, police had substantial discretion over

how they would implement the ordinance. Police could give youths in violation of the curfew a verbal warning, take them home, issue a ticket with a fine as high as $500, or take them into custody. A youth detention facility was staffed by the city for holding curfew violators. If a child was found in repeated violation of the curfew, police had the authority to fine the child's parents up to $500. Furthermore, businesses could also be fined for allowing minors to remain on their premises during curfew hours. In conjunction with these penalties were a series of youth programs including a midnight basketball program and a youth education program. Other cities such as New Orleans went further than this and sent minors picked up on curfew violations to a detention center staffed by psychologists, medical professionals, and clergy to provide counseling for violators.

In the first 3 months of the Dallas program no arrests were made for curfew violations, but hundreds of warnings and citations were handed out to youth and 8 tickets were written to adults for permitting violations. In an interim review of the program, the Dallas police department found that juvenile victimization during curfew hours had dropped 17.7% from 1,950 during the period from May to July 1993, to 1,604 during the same period in 1994. Considering that no youths were actually arrested for curfew violations during this period, this result may seem surprising. However, as made clear above, arrests are only one tool made available to police officers by the curfew. The greatest treatment induced by the curfew may be the weakened standards of probable cause. As one Dallas police officer put it:

"There's no way I'm going to stop every kid I see... I come down on them when I suspect they're into something else, like breaking into a car or vandalizing. When

I stop them for those offenses, the curfew gives me an extra tool of enforcement. If they're not guilty of the offense I suspected them of, an underage (16 or under) person can still be hit with the curfew." (Bell, 1994)

But clearly, even if the youth is not underage, curfew laws provide police with a legal justification for stopping and questioning people who appear to be young. This has the potential to lead to the arrests of many more young people for serious crimes than would otherwise have been apprehended by the police.

III Methodology

Recent studies rely on variation in the date of adoption of city curfew laws to identify treatment effects on criminal behavior (Males and Macallair, 1999; McDowall et al., 2000). These studies may easily be confounded if curfew laws are enacted in response to city specific trends in arrests. I examine these issues using an "event study" research design capable of testing for such trends and recovering any dynamics of the impact of curfew enactment.[4]

Consider the following econometric model of arrests:

$$R_{cy} = \sum_t \beta_t D_{cy}^t + \psi_y + \theta_c + \varepsilon_{cy} \tag{1}$$

where R_{cy} is the log of the number of arrests of individuals in some age-group of interest in city c in calendar year y, ψ_y is a year effect, θ_c is a city effect, and ε_{cy} is an

error term which may exhibit arbitrary dependence within city but is uncorrelated with the other right hand side variables.

The D_{cy}^{t} are a series of "event time" dummies that equal one when curfew enactment is t periods away in a city. Formally, we may write:

$$D_{cy}^{t} \equiv I\left[y - e_c = t\right]$$

where $I\left[.\right]$ is an indicator function for the expression in brackets being true and e_c is the year a curfew is enacted in city c.

Thus, the β_t coefficients represent the time path of arrests relative to the date of curfew enactment for cities subject to the curfew conditional on the three unobserved variance components ψ_y, θ_c, and ε_{cy}. If curfews are randomly assigned the following restriction should hold:

$$\beta_t = 0 \quad \forall t < 0$$

In words, this condition states that curfew enactment is not, on average, preceded by trends in city specific arrests.

The results in this paper are obtained by estimating (1) by ordinary least squares, including a set of event time dummies along with dummies for the city and year fixed effects. The reader familiar with such models will recognize that not all of the β's can be identified as the D_{cy}^{t}'s are perfectly collinear in the presence of the city effects. For this reason I normalize $\beta_{-1} = 0$, so that all post-enactment coefficients can be

thought of as treatment effects. I also impose the following endpoint restrictions:

$$\beta_t = \begin{cases} \overline{\beta} & \text{if } t \geq 6 \\ \underline{\beta} & \text{if } t \leq -6 \end{cases}$$

which simply state that any dynamics wear off after six years.[5] This restriction helps to reduce some of the collinearity between the year and event time dummies. Because the sample is unbalanced in event time, these endpoint coefficients give unequal weight to cities enacting curfews early or late in the sample. For this reason I focus the analysis on the event time coefficients falling within a five year window which are identified off of a nearly balanced panel of cities. Hypothesis testing is conducted using robust standard errors clustered at the city level.[6]

IV Data

The data on curfew enactment were collected from a variety of sources. I start with the lists given in Ruefle and Reynolds (1996a, 1996b), who surveyed the universe of cities with a 1992 population of 100,000 or more. They queried each city's police department as to whether a youth curfew had been enacted, the hours of the curfew, whether the curfew was newly enacted or revised, and which age-groups were subject to the curfew. Comparisons of the data with city codes and newspaper stories indicated that some of the information was inaccurate. Many cities had enacted curfews prior to the dates listed in the survey and some had not enacted curfews at all. Furthermore, some of the information on which age-groups the curfew applied

to was incorrect.

To deal with these data quality problems I acquired municipal codes from all 92 cities with a 1990 population greater than 180,000.[7] These codes generally contained a history of revisions and a description of exactly which age-groups were covered by the curfew ordinance. In some cases the revision history was not listed and I contacted city clerks directly to inquire about previous ordinances and prior versions of the code. To be sure that the code did not refer to a rewritten version of an old ordinance, I searched the ProQuest and Lexis-Nexis periodical indices for newspaper stories detailing the process of curfew enactment in each city for 5 years before and after each of the suspected dates of enactment. Using this information I compiled a legislative history of each city's curfew law.

After cross-referencing the legislative history, the list from Ruefle and Reynolds, and newspaper articles, I arrived at an estimation sample of 54 cities that began enforcing a curfew law during the sample period.[8] Table I shows the final list of cities retained in the analysis and their associated curfew information. There is no obvious pattern as to which cities did (or did not) enact curfews. Cities of all sizes and in all regions made use of such ordinances.

I use the FBI's Unified Criminal Reporting (UCR) files to obtain detailed information on arrests by age, city, gender, and type of offense for the years 1980-2004. Some cities do not report information for all offenses in all years, and some have values in some years which are clearly erroneous.[9] I drop only the most serious cases of erroneous data from my analysis. To maximize power, I pool the arrest data on

men and women.

Arrests are not a perfect measure of youth criminal behavior, since they also reflect the behavior of police. However, detailed age data are not available in UCR offense reports and other work indicates that arrest data provide fairly accurate representations of underlying criminal activity.[10] To deal with issues of police discretion I focus on serious felonies which are unlikely to be reclassified as curfew violations. Thus, we should expect that if curfew laws change the behavior of police, they should allow them to more easily apprehend and arrest minors below the curfew age (to the extent that such minors are capable of being distinguished from their peers), biasing estimates of the statutory treatment effect up towards zero.

Table II shows summary statistics for various categories of arrest by age-group. The "Violent Crimes" variable is an unweighted sum of the following offenses: murder, manslaughter, rape, robbery, and aggravated assault. The Property Crimes variable is the sum of the remaining FBI Part I offenses: burglary, larceny, motor vehicle theft, and arson. Burglary and larceny constitute a large fraction of the property crime index, while assaults make up the bulk of the violent crime index. To reduce the influence of assaults on the Violent Crimes Index, I also show results for "Severe Violent Crimes" which I define as the sum of Part I offenses other than aggravated assault. I also report results for the sum arrests for Violent and Property offenses which I refer to as "total Part I arrests." In all cases, very few observations are lost when taking the log.

V Results

As an initial check on whether the scheme for classifying curfew enactment dates described in the Appendix was succesful, I begin by examining the impact of curfew enactment on arrests for curfew and loitering violations. These data are of lower quality than those on arrests for Type I offenses, with roughly half of the cities in my sample reporting no such arrests in any given year. This is consistent with the UCR's hierarchical classification scheme which requires that arrests for such violations that also yield evidence of more serious infractions (such as arson or burglarly) be reported as due to the more serious offense. However, all municipalities eventually report at least one such arrest during the sample period.

Figure I plots the estimated β_t coefficients from a regression of the form given in (1) where the dependent variable is the log of one plus the number of arrests of youth in age-groups subject to the curfew.[11] The bands around the point estimates are 90% cluster-robust confidence intervals. There is no pre-treatment trend in the coefficients but a rather dramatic increase in arrests for curfew violations at the date of enactment which intensifies over the following years. The sharp timing of this result suggests that the data classification scheme described in the Appendix provides relatively accurate information about the timing of curfew enactment.

Turning now to the more serious criminal outcomes of interest, the panels of Figure II plot the estimated β_t coefficients from regressions of the form given in (1) for three different age-groups: a) youth subject to the curfew, b) young adults above

the curfew age but less than age 25, c) adults over age 25. The dependent variable is the log of total arrests for Part I offenses.

The story told by the figures is rather striking. Prior to enactment there is no trend in the arrests of youth subject to curfew laws or adults over age 25. A slight upward trend in arrests appears to be present among young adults above the curfew age. Evidently, curfew laws are enacted in response to trends in the behavior of older teens rather than city-wide crime rates.

Though the estimates are imprecise, it appears that curfews generate large reductions in the number of arrests of youth below the curfew age. Arrests drop by nearly 15 percent in the year after enactment and then appear to revert slowly after that to a new steady state level 10% below baseline. Note from Table II that, in the average city, a permanent ten percent reduction corresponds to roughly 135 youth arrests per year. Although the results are somewhat less reliable because of what appears to be an upward pre-enactment trend, young adults above the curfew age also appear to exhibit small decreases in arrests, casting doubt on the statistical discrimination hypothesis. Small decreases also appear to be present among adults over age 25 for whom no such trend was found. These results suggest that curfews actually reduce criminal activity among adults, either due to cross-age interactions or stepped up social programming efforts which might accompany curfew enactment.

To remove the potential influence of social programming efforts like midnight basketball, Figure III reports event study coefficients where the dependent variable is the log difference between arrests of the oldest age-group in a city subject to

the curfew law and the youngest age-group exempt from the curfew. Since these two groups differ in age by a single year, it is unlikely that local authorities can distinguish between them ex-ante. Hence, any impacts on their relative arrest rates ought to identify a pure statutory treatment effect. Once again we see from the Figure little evidence of a pre-treatment trend and a sharp decrease in the relative arrest rates of youth subject to the curfew in the years following enactment. The magnitude of this effect provides little evidence that social programming or other treatments accompanying curfew enactment are driving the arrest reductions found in Figure II.

Quantitative Estimates

Although the general pattern of the figures is clear, the individual β_t coefficients are quite imprecise. It is useful to provide more formal tests of the null hypotheses that curfews have no effect on arrests of the various age-group samples considered thus far. In order to gain statistical power I test hypotheses about averages of the β_t coefficients over various time intervals. The results are disaggregated by crime category.

Table III provides estimates of the total impact of curfew enactment on youth below the statutory curfew age. The results of the table are in keeping with the pattern suggested by Figure II. The estimated average reduction in crime due to curfew enactment in the three years starting with the year of enactment is 11% and statistically distinguishable from zero. The average effect over the six years starting with the year of enactment is also 11% and distinguishable from zero. Few

discernable differences in effect patterns are present across the three subcategories of offenses. Impacts on violent crime appear to dissipate faster than other categories but the standard errors for this subcategory are quite large.

Table IV provides estimates of statutory treatment effects. As in Figure III, enactment appears to be associated with declines in the relative arrests of youth just below curfew age of roughly 9% in the three years starting with the year of enactment, with similar patterns across offense categories.[12] These effects appear to begin to dissipate after 3 years, except for severe crimes which show no sign of a rebound. The modest differences between the statutory impacts and the overall impacts on youth suggests little role for social programming or statistical discrimination effects.

Since Figure II indicates that young adults just above the curfew age and adults age 25+ respond in a similar fashion to curfew enactment I pool the two categories together in estimating spillover effects on exempt age-groups. Table V provides estimated impacts on arrests of all individuals above the curfew age. The point estimates suggest curfew enactment led to a modest reduction in arrests among groups exempt from the curfew, with the strongest response present among violent crimes. However these impacts are statistically insignificant. Since the point estimates are negative, however, they suggest little role for statistical discrimination effects but rather the possibility of positive cross-age dependence in criminal activity.[13]

As a final check on this interpretation of the estimated spillover effects, I examine whether curfew enactment is associated with changes in the number of police officers per capita as an increase in police resources could yield reductions in crime across

all age-groups even if statistical discrimination effects were present. Figure IV plots event study coefficients where the dependent variable is the log of the number of police officers per capita as reported in the Uniform Crime Reports police employee data. The estimates are quite precise and centered around zero, indicating no impact of curfew enactment on the number of police officers.

VI Conclusion

Curfews appear to have important effects on the criminal behavior of youth. The arrest data suggest that being subject to a curfew reduces the arrests of juveniles below the curfew age by approximately 10% in the five years following enactment. Arrests of adults also appear to fall in response to enactment though the intensity and timing of the effect appears to be similar across exempt age-groups, suggesting that statistical discrimination has little to do with any spillover effects. However, the precision of the estimated effects on older age-groups is poor and I cannot rule out small (and potentially important) discrimination effects.

It is interesting to note that these findings are in keeping with the perceptions of those subject to curfew policies. As Adams (2003) notes,"Public opinion shows overwhelming support for curfews... the primary basis for [this] support is the conviction that curfews reduce crime and make the streets safer."Though this analysis cannot uncover the exact mechanism through which curfews affect crime, the large statutory results suggest youth crime is imperfectly substitutable across time and that temporal targeting of law enforcement policies may be effective.

An alternative rationalization of the evidence is that parents play an important role in the enforcement of curfews over and above that of police. If municipal curfews act as focal points in the establishment of household policies, a curfew with modest fines (and arrests) could lead to large changes in the behavior of youth. The potential role of parents in self-enforcement of curfews is an important area for future research.

Finally, though curfews appear to be effective at reducing juvenile arrests, it is important to bear in mind that we have little data on the costs of such programs, either directly in terms of dollars spent enforcing such ordinances, or indirectly in terms of the opportunity costs of policing. Additional evidence on these issues is necessary to inform policy decisions.

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Notes

[1]See Fried (2001) and Adams (2003) for reviews of the literature.

[2]As such, the paper falls into the well developed economics literature on optimal fines. See Becker (1968), Stigler (1970), Polinsky and Shavell (1984) for theory and Bar-Ilan and Sacerdote (2004) for an example of recent empirical work.

[3]See Jacob et al. (2007) for an examination of the substitutability of criminal activity across days as opposed to within a day.

[4]See Jacobson et al. (1993) and McCrary (2007) for related examples of event study designs.

[5]See McCrary (2006) for another example of such endpoint restrictions. Nearly identical results ensue if I fully saturate the model in event time.

[6]See Bertrand et al. (2001) for an examination of the performance of cluster-robust variance estimators in samples of the size considered here.

[7]This number was chosen for convenience. It became increasingly difficult to obtain reliable contact information, newspaper stories, and records as city size decreased.

[8]The details of the method used to select cities and assign enactment dates are explained in the Appendix.

[9]San Antonio for instance, lists 727 arrests of 17-year olds for violent crime in 1992, but only 7 in 1993.

[10]See Cook and Laub (1998). In results not shown I have examined whether curfew enactment is associated with a change in the clearance rate of Type I offenses. I was unable to find a significant impact though the standard errors of the analysis were relatively large.

[11]The results from this analysis are somewhat sensitive to functional form assumptions. An equivalent analysis in levels or via Poisson regression techniques yields poor pre-treatment balance in trends.

[12]Note that the statutory treatment effects are computed on a sample of youth above and below the curfew age who, in each city, differ in age by a single year. Hence, Figure III does not correspond to the difference between Figures IIa and IIb which analyze samples involving coarser age categories.

[13]An alternative interpretation of the spillover effects, suggested by a referee, is that curfews, by deterring youth from illegal activity, may free officers to more actively police adults, thereby generating a reduction in adult criminal activity.

Appendix: Data Construction

The data on curfew implementation were constructed from a variety of sources with the aim of addressing three primary sets of concerns. The first concern was that the assigned dates of curfew enactment would be too late, having been preceded by another enactment a few years prior. Such errors should not only add to the noise of the estimated treatment effects, but bias them towards zero while biasing pre-treatment trends away from zero. For this reason I created a detailed legislative paper trail for the curfew policies in each city and an algorithm for categorizing what had been found. The second concern was that the age-groups to which the curfew were to apply might be wrong or might have changed over time. I went to great lengths to make sure that the age-groups being compared actually received different treatments, searching legislation and newspaper stories for signs of a change. Again, errors in the assignment should yield treatment effects biased towards zero. Finally, I was concerned that in some cases the passing of curfew ordinances might not be associated with enforcement. There is no perfect way to measure enforcement and I settled upon a simple expedient which was to interpret certain disagreements between the municipal code and survey responses to questions regarding the presence of curfew laws as evidence of lax enforcement.

Sample Selection and Dates of Enactment

Several sources of information were used to assign dates of curfew enactment to cities. The first is a 1996 survey of police departments with a 1992 population over 100,000

performed by Ruefle and Reynolds (R&R). R&R called every such police department inquiring if a curfew law was in place, when the curfew was originally enacted, and when, if ever, it had been revised or re-enacted. While R&R's survey is most likely a good measure of which cities had curfews in 1996, its reliability as a measure of when, prior to 1996, a city's curfews were in effect is unknown.[14] Police departments have a limited institutional memory and it is unclear how they interpreted questions about the date that a city's curfew was "originally" enacted.[15]

To complement the data found in R&R, I obtained a current copy of the municipal code for every city with a 1990 population over 180,000. These municipal codes generally contain the initial date of enactment and dates of revision of the sections relating to the curfew. However, in some cases portions of the code have been totally repealed only to be replaced by similar language. In such cases the "initial" date of enactment listed on the code is actually the date of enactment of the most recent curfew language. To avoid this problem, I made heavy use of city municipal clerks, asking them to search for prior ordinances related to juvenile curfew laws. My search was aided by information from newspaper articles which were obtained by searching ProQuest and Lexis-Nexis for articles including the name of the city and the word "curfew." These articles frequently mentioned the history of curfew legislation in the cities in question and the details of their implementation. When an earlier instance of a curfew was found I obtained a copy of the earlier ordinance and in turn searched it for references to earlier laws.

Having assembled a large paper trail for each city, I used the following algorithm to determine the date of enactment. First, I checked for agreement between R&R

and other sources over the basic question of whether a city had ever passed a curfew law. Of the 92 cities with 1990 population over 180,000, 77 were listed by R&R as having curfew ordinances. For each of the 15 cities listed as not having curfews, I searched newspaper articles and city codes for mention of a curfew law. I found that four of these cities had passed curfews although two of those came after the time of R&R's survey. The two cities with curfews enacted after 1996 were included in the sample of enacters, while the remaining two were dropped under the assumption that a failure to report a curfew to R&R signaled a lack of enforcement.[16]

Next, for the cities that had enacted a curfew according to R&R, I compared the date of enactment found in the city code to the date of enactment in R&R. Because my data spans the years 1980-2004, I first subcategorized the R&R enactment dates by whether they fell into the sample period. According to R&R, 55 of the 77 cities with curfews had enactments or revisions during the sample period. Investigation of the city codes of the 22 that were listed as enacting curfews prior to 1980 indicated that 15 of them in fact had revisions during the sample period that were not listed in R&R. Nevertheless, I dropped all 22 cities from my analysis under the assumption that the failure on the part of the police department to recall recent revisions/re-enactments was an indicator of lax enforcement.

For cities that were listed in R&R as enacting a curfew during the sample period, I used the following rule to assign enactment dates. If the city code listed a date in the sample period that was identical or prior to R&R I used the earliest such date. If the R&R date was prior to the earliest code date in the sample period, I called the city clerk to search for earlier ordinances. In most cases I was able to find prior

ordinances with enactment dates identical or prior to those found in R&R. Of the 55 cities that R&R listed as enacting curfews during the sample period, the assignment rule yielded a date that agreed with R&R in 40 cases. Of the 15 remaining cases 12 yielded enactment dates prior to those found in R&R while the remaining 3 had enactment dates after those found in R&R.

Inspection of newspaper articles indicated that 3 cities (Baltimore, Miami, and Dallas) had injunctions issued against their curfew ordinances preventing them from being enforced at the time of enactment. All of these cities eventually won their court battles and thus I changed the date of enactment to reflect the post-injunction date of enforcement. A fourth city, the District of Columbia, also faced an injunction against its curfew ordinance, which was subsequently declared unconstitutional by the courts. Since DC attempted to enact curfew laws several times, each time suffering a defeat in court, I drop it from the sample.

Finally, two cities had exceptional circumstances that warranted dropping them from the sample. The first, Hialeah, according to the city attorney, never enforced a curfew, but was forced to legally adopt one as part of a measure passed by Dade County. The second, Las Vegas, was dropped because its only curfew enacted during the sample period applied to "high school students" without specifying a particular age range and only covered a narrow geographic area.

The final sample contained 54 cities enacting curfews (55 enacters – 3 exceptions + 2 enacters not listed in R&R). For each city, I rounded the date of enactment to the next year if the curfew was enacted in December or November.[17] The following

table summarizes the major decisions leading to cities being dropped:

Summary of Sample Selection Criteria

Cities with population >=180,000	92
Cities with curfews enacted during sample period but not listed in R&R	-2
Cities with old curfew laws not revised during sample period	-7
Cities with old curfew laws revised during sample period but not reported to R&R	-15
Exceptional cities (DC, Hialeah, Las Vegas)	-3
Cities with no curfew laws	-11
Final Sample Size	54

Maximum Curfew Age

In all cases the maximum statutory curfew age was taken from the city code or relevant ordinance. Among the 54 cities listed in R&R as enacting curfews during the sample period, 46 had information identical to that found in their city code/ordinances. Two of the eight cities with different age information had different enactment dates. The remaining six discrepancies appear to be mistakes on the part of R&R.

Table I: Curfew Data by City

City	State	Population in 1990	Year Curfew Enacted	Statutory Curfew Age
Akron	OH	223,019	1990	17
Albuquerque	NM	384,736	1994	16
Anaheim	CA	266,406	1990	16
Anchorage	AK	226,338	1989	15
Atlanta	GA	394,017	1991	16
Austin	TX	465,577	1992	16
Baltimore	MD	736,014	1995	16
Baton Rouge	LA	219,531	1995	16
Birmingham	AL	265,852	1996	16
Buffalo	NY	328,123	1994	16
Charlotte	NC	396,003	1985	15
Cincinnati	OH	364,040	1994	17
Cleveland	OH	505,616	1993	17
Colorado Springs	CO	281,140	1992	17
Corpus Christi	TX	257,453	1991	16
Dallas	TX	1,006,831	1994	16
Denver	CO	467,610	1994	17
Detroit	MI	1,027,974	1987	17
El Paso	TX	515,342	1992	16
Fort Worth	TX	447,619	1994	16
Fresno	CA	354,202	1990	17
Garland	TX	180,635	1994	16
Glendale	CA	180,038	1989	17
Houston	TX	1,630,672	1992	17
Jackson	MS	196,594	1992	17
Jacksonville	FL	635,230	1991	17
Jersey City	NJ	228,537	1987	16
Kansas City	MO	435,141	1991	17
Lexington-Fayette	KY	225,366	1995	17
Long Beach	CA	429,433	1988	17
Los Angeles	CA	3,485,398	1988	17
Louisville	KY	269,157	1997	17
Lubbock	TX	186,281	1994	16
Madison	WI	191,262	1992	16
Mesa	AZ	288,091	1991	17
Miami	FL	358,548	1996	16
Mobile	AL	196,278	2002	17
New Orleans	LA	496,938	1994	16
Newark	NJ	275,221	1993	17
Norfolk	VA	261,229	1993	17
Oklahoma City	OK	444,730	1994	17
Phoenix	AZ	983,403	1993	17
Richmond	VA	203,056	1992	17
Sacramento	CA	369,365	1995	17
San Antonio	TX	935,927	1991	16
San Diego	CA	1,110,549	1994	17
San Jose	CA	782,225	1994	17
Shreveport	LA	198,528	1992	16
St. Paul	MN	272,235	1990	17
Tampa	FL	280,015	1994	16
Toledo	OH	332,943	1993	17
Tulsa	OK	367,193	1995	17
Virginia Beach	VA	393,069	1989	17
Wichita	KS	304,011	1992	17

Table II: Summary Statistics (Mean and Standard Deviation)[1]

	Age-Group[2]				
Outcome	Youth Below Curfew Age	Young Adults Above Curfew Age	Adults Age 25+	Oldest Age Group Subject to Curfew	Youngest Age Group Exempt from Curfew
Log Arrests for Type I Offenses	7.210 (0.777)	7.419 (0.743)	7.794 (0.866)	5.773 (0.754)	5.681 (0.771)
Log Arrests for Violent Crime	5.203 (1.057)	6.040 (0.962)	6.472 (1.066)	3.990 (1.051)	4.054 (1.041)
Log Arrests for Property Crime	7.006 (0.821)	7.064 (0.748)	7.436 (0.801)	5.528 (0.764)	5.393 (0.774)
Log Arrests for Severe Violent Crimes	4.353 (1.175)	5.165 (1.001)	5.239 (1.024)	3.235 (1.150)	3.322 (1.116)

[1] First number in each box is the mean of the variable, number in parentheses is standard deviation.

[2] Actual ages contained in each age-group vary by city. "Youth Below Curfew Age" refers to all ages less than or equal to city-specific statutory curfew age, "Young Adults Above Curfew Age" refers to all ages above city-specific statutory curfew age and less than age 25. "Oldest Age Group Subject to Curfew" refers to youth with age exactly equal to statutory curfew age. "Youngest Age Group Exempt from Curfew" refers to youth with age a single year above statutory curfew age.

Table III: Total Impact on Youth Below Curfew Age

Outcome	Contemporaneous	Short Run	Long Run	Average
Total Crimes	-0.037 (0.027)	-0.109 (0.042)	-0.112 (0.069)	-0.110 (0.049)
Violent Crimes	-0.049 (0.040)	-0.097 (0.063)	-0.042 (0.113)	-0.070 (0.080)
Property Crimes	-0.040 (0.029)	-0.114 (0.041)	-0.131 (0.071)	-0.123 (0.050)
Severe Violent Crimes	-0.065 (0.053)	-0.131 (0.065)	-0.123 (0.122)	-0.127 (0.084)

Estimates taken from specification of form given in equation (1) with dependent variable log of arrests of youth in age-groups subject to curfew.

Numbers in parentheses are standard errors clustered at the city level.

Short run effects refer to the average of the coefficients in periods t=0,1, and 2.

Long run effects refer to the average of the coefficients in periods t=3,4, and 5.

Average effect refers to the average of coefficients in periods 0 through 5.

Table IV: Statutory Treatment Effects

Outcome	Contemporaneous	Short Run	Long Run	Average
Total Crimes	-0.069 (0.030)	-0.085 (0.033)	-0.035 (0.058)	-0.060 (0.041)
Violent Crimes	-0.058 (0.066)	-0.084 (0.063)	0.000 (0.106)	-0.042 (0.077)
Property Crimes	-0.079 (0.030)	-0.100 (0.034)	-0.052 (0.058)	-0.076 (0.041)
Severe Violent Crimes	-0.093 (0.073)	-0.084 (0.066)	-0.116 (0.102)	-0.100 (0.077)

Estimates taken from specification of form given in equation (1) with dependent variable log ratio of arrests of youth with single digit age equal to statutory curfew age to youth one year above curfew age.

Numbers in parentheses are standard errors clustered at the city level.

Short run effects refer to the average of the coefficients in periods t=0,1, and 2.

Long run effects refer to the average of the coefficients in periods t=3,4, and 5.

Average effect refers to the average of coefficients in periods 0 through 5.

Table V: Treatment Effects on Arrests Above Curfew Age

Outcome	Contemporaneous	Short Run	Long Run	Average
Total Crimes	0.003	-0.040	-0.007	-0.023
	(0.019)	(0.040)	(0.059)	(0.047)
Violent Crimes	-0.020	-0.076	-0.037	-0.056
	(0.032)	(0.052)	(0.071)	(0.057)
Property Crimes	0.014	-0.008	0.039	0.016
	(0.018)	(0.037)	(0.065)	(0.048)
Severe Violent Crimes	0.020	-0.006	-0.008	-0.007
	(0.029)	(0.041)	(0.060)	(0.048)

Estimates taken from specification of form given in equation (1) with dependent variable log of arrests of adults age 35+.

Numbers in parentheses are standard errors clustered at the city level.

Short run effects refer to the average of the coefficients in periods t=0,1, and 2.

Long run effects refer to the average of the coefficients in periods t=3,4, and 5.

Average effect refers to the average of coefficients in periods 0 through 5.

Figure I: Arrests of Youth Below Curfew Age for Curfew and Loitering Violations

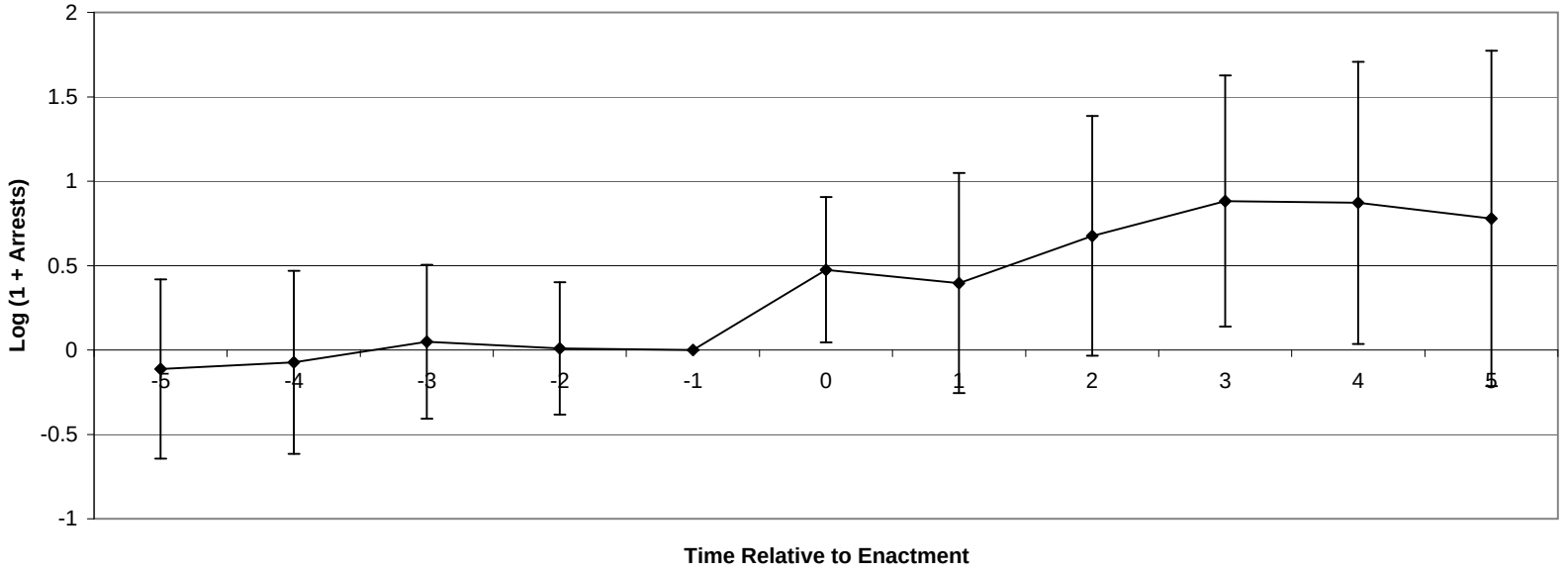


Figure IIa: Youth Below Curfew Age

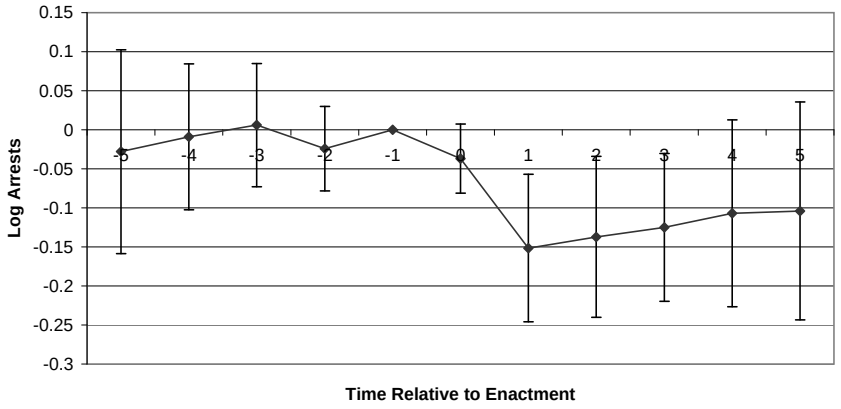


Figure IIb: Young Adults Above Curfew Age

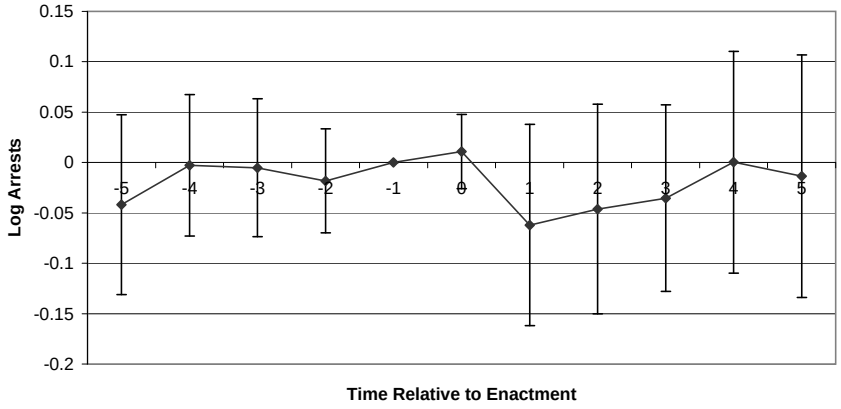


Figure IIc: Adults Age 25+

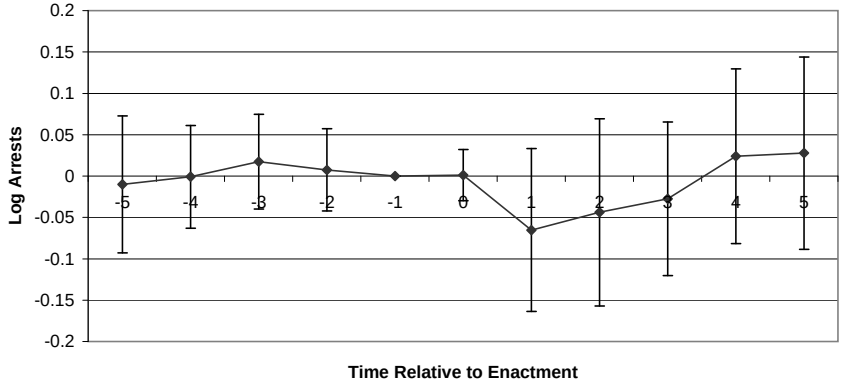
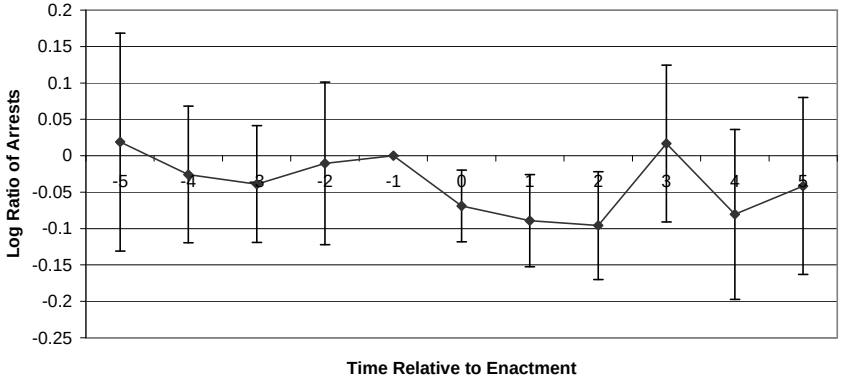


Figure III: Ratio of Arrests of Youth Just Above and Below Curfew Age



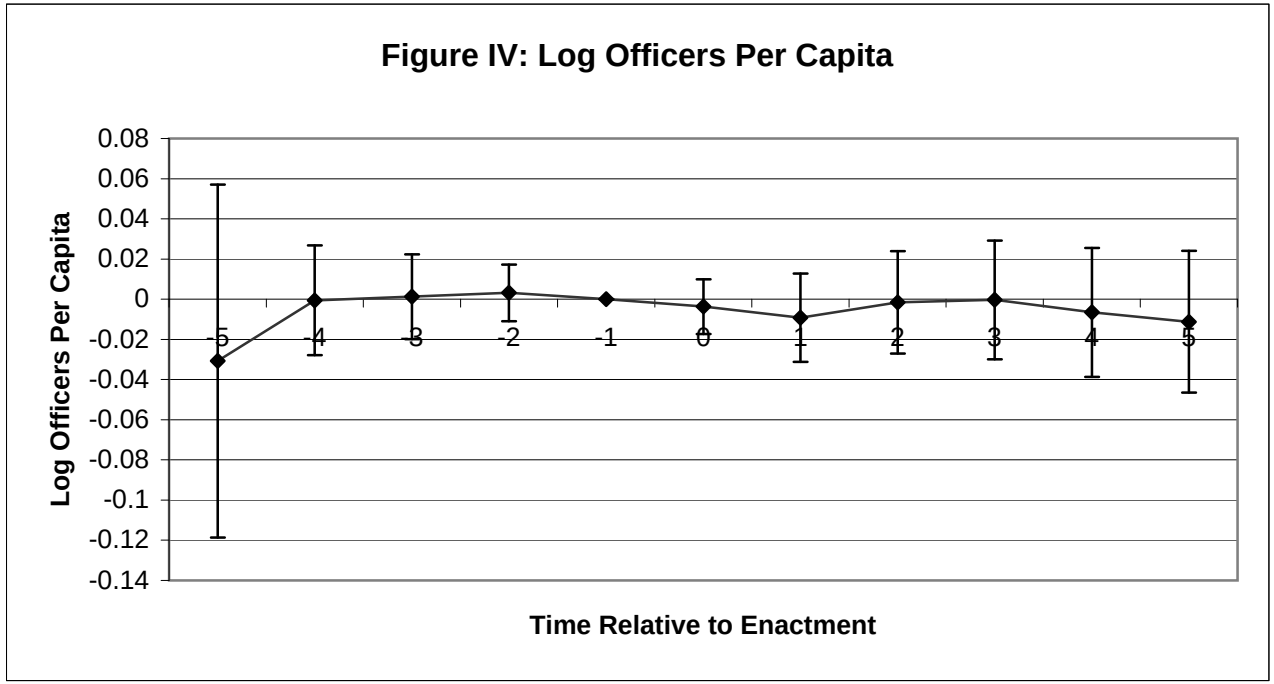


Figure IV: Log Officers Per Capita
0.08
0.06
0.04
0.02
0
-0.02
-0.04
-0.06
-0.08
-0.1
-0.12
-0.14
Log Officers Per Capita
-5
-4
-3
-2
-1
0
1
2
3
4
5
Time Relative to Enactment