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This study used panel data from a sample of cities and counties to examine the effects of curfew laws on youth crime rates. The analysis estimated the impact of new and revised laws on juvenile homicide victimizations (1976 to 1995) and on juvenile arrests for a variety of offenses (1985 to 1996). The results show statistically significant decreases in burglary, larceny, and simple assault arrests after revised laws, but only in the county data. Homicide rates did not change in either counties or cities, and new laws were not followed by reductions in crime. Any preventive effects of curfews appear to be small.

During the 1990s, juvenile curfew laws became a popular strategy for preventing youth crime. Critics of the laws question their constitutionality (for example, Hemmens and Bennett 1999), but local surveys show that they enjoy wide support among adults (Crowell 1996). Perhaps reflecting their appeal to voters, both major candidates endorsed curfews during the 1996 presidential campaign. The Clinton administration has continued to promote the laws as a partial solution to youth crime problems.

A survey by Ruefle and Reynolds (1996) documents the rapid rate of curfew law adoptions. Between 1990 and 1995, 60 percent of the 200 largest American cities enacted a new curfew statute or revised an existing one. By 1995, more than three-quarters of these cities had a curfew ordinance in effect. The U.S. Conference of Mayors (1998) obtained similar results in a survey that included smaller jurisdictions.

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Despite their favor among adults, the success of curfew laws in controlling crime is largely unknown. Several police departments report that juvenile offending rates decreased—sometimes dramatically—after they began enforcing curfew ordinances (Crowell 1996; Office of Juvenile Justice and Delinquency Prevention [OJJDP] 1996). Yet these claims rest on uncertain comparison groups, and few evaluations have considered more than a single area.

This article uses panel data from a sample of major American cities to examine the impact of curfew laws on offenses and victimizations involving young people. In the next section of the article, we consider the logic of juvenile curfews and review previous evaluations of their influence on crime. We then describe our research design and present our results. We do not find strong evidence that the curfew laws reduce juvenile offending or victimization rates. In the final section, we consider the implications of these results for policy and suggest directions for additional research.

BACKGROUND

Curfew laws rest on a simple premise: Controlling the hours when young people may be in public will limit their opportunities to commit offenses or suffer victimization. If juvenile crimes often take place in group settings and among persons of similar ages, curfews should reduce contacts between potential victims and offenders. Sherman, Gottfredson, MacKenzie, Eck, Reuter, and Bushway (1998) argue that this separation of victims and offenders is a basic feature of problem-oriented policing.

Besides separating criminals from victims, curfew laws may strengthen the control that parents can exert over their children's activities. By restricting the times when other juveniles are on the streets, curfews make it easier for parents to limit their own children's hours (Ruefle and Reynolds 1996).

Unlike most crime control strategies, curfew laws also could have independent effects on rates of both offending and victimization. By discouraging idle youths from gathering in groups conducive to crime, curfews might help reduce offending rates (Wilson 1995). By limiting the times that young people may be in public places, the laws could lower their exposure to risky settings.

Still, their logical appeal aside, there are reasons to believe that curfew laws might have little influence on youth crime. Most important, curfews depend on enforcement for their effectiveness. Because of competing demands on police resources, the laws may have no more than a modest impact on the behavior of most juveniles.

In addition, curfews apply to only a few hours of the day. Although several cities have ordinances that cover the times when young people are in school, curfews generally begin in the late evening and end in the early morning. Juvenile violence arrests peak in the afternoon, however, immediately after school ends. Arrest rates then decrease through the rest of the day, settling at low levels through the periods when most curfew laws are in effect (Sickmund, Snyder, and Poe-Yamagata 1997).

Although one can construct plausible arguments that curfew laws either will or will not influence juvenile crime, surprisingly little research exists to help resolve the issue. In one of the few evaluations of curfews, Hunt and Weiner (1977) used a short time series to study the impact of a Detroit ordinance. They found that juvenile arrest rates decreased during curfew hours, but correspondingly increased during other periods. The net impact of the law was therefore zero.

In a more comprehensive analysis, Males and Macallair (1999) used a sample of California counties to examine the relationship between curfew arrests and arrests of young people for other offenses. Applying bivariate correlation analysis to panel data for 1980 through 1997, they found that changes in curfew law enforcement were unrelated to changes in youth crime rates.

Fritsch, Caeti, and Taylor (1999) found more support for the efficacy of curfew laws in their evaluation of an antigang program in Dallas. Comparing one year of data before and after the program began, they concluded that gang activity decreased in police beats that received aggressive curfew enforcement. In contrast, using time series data from Dallas and three other Texas cities, Adams (1997) found no consistent evidence that the laws reduced general rates of juvenile offenses.

Although the existing evaluations have produced mixed results, they suggest that curfew laws are ineffective in reducing crime. These studies analyzed small samples, however, and they examined a limited selection of laws. Additional research on the statutes would be desirable before drawing strong conclusions about their outcomes.

RESEARCH DESIGN

Sample

This article evaluates the impact of curfew laws on juvenile crimes using panel data from all 57 U.S. cities with a 1980 population of 250,000 or more. As we later explain in more detail, our basic model estimates the average change in juvenile crime rates following a new or revised law. We therefore

compare crime rates during periods in which new or revised curfew laws were in effect with rates in other periods.

Reflecting the spread of curfews, 28 of the cities in our study introduced new laws, and 14 revised existing statutes. This pattern of adoptions and revisions provides a large amount of variation for the analysis, making it easier to estimate the effects of the laws.

Although the details of the laws vary from city to city, their basic provisions are highly similar. The oldest age subject to the laws ranges from 15 to 17, with more than 60 percent of the cities specifying 17 as the upper limit. Most curfews begin between 10:00 PM and midnight, and most end between 4:00 and 6:00 in the morning.

The laws differ more substantially in minor respects. Some curfews begin later on weekend nights than on weekdays. Some begin later during summer months than during the school year. Several cities specify earlier curfews for younger children than for older ones, and several impose special rules for entertainment districts. A few cities have curfews that cover both daytime and nighttime hours.

Given the high degree of similarity in the laws, we treat them as identical in our analysis. Still, daytime curfews cover more hours than do those that operate only at night. We therefore also separately analyze the impact of the daytime ordinances.

Data

Our study uses two bodies of data. The first is annual juvenile arrest totals from the Federal Bureau of Investigation's (FBI) Uniform Crime Reporting Program (UCR). The arrest data include persons age 17 and younger, and cover a variety of offenses. They span the 12-year period between 1985 and 1996.

Arrests are a less direct measure of offending than are reported crimes, and UCR arrest counts often suffer from missing observations. Still, arrests are the only measure of offending that allow disaggregation by age. The available evidence also suggests that arrest totals are reasonably accurate indicators of age-specific participation in crime (see Cook and Laub 1998 for a recent discussion of the issues).

Besides allowing estimates of juvenile crime rates, the arrest data cover categories of crime that the UCR offense reports do not include. Many of these (e.g., simple assaults) are minor offenses that one might expect curfew laws to influence.

The relatively short length of the study period reduces the problems posed by jurisdictions that did not submit complete arrest counts to the FBI for all

years. Nevertheless, five counties are omitted from the arrest analysis because of large numbers of missing cases.

Our second body of data is vital statistics counts of homicide victims age 17 or younger, obtained from tapes distributed by the National Center for Health Statistics. The homicide counts allow us to study the effects of curfew laws on one type of juvenile victimization. Examining victimizations less serious than homicide also would be desirable, but the relevant data do not exist for most areas. Because we could obtain the homicide counts for a longer period than the arrests, this analysis covers the 20 years between 1976 and 1995.

Although the curfew laws in our study are all city ordinances, the arrest and victimization data are from counties. Our analysis therefore estimates the effects of the city laws on the juvenile offending and victimization rates of the counties that contain them.

On average, the cities in our sample include 65 percent of their county populations, with a range from 28 percent to 100 percent of the total. Using county data to evaluate the impact of the laws thus introduces measurement error into the analysis. As a partial solution to this problem, this article also includes a separate analysis of the 13 cities that make up at least 90 percent of their county's population. Of the 13 cities, 10 are exactly conterminous with their county, and so contain all of its population.

The city-county subsample avoids the difficulties in using county data to make inferences about city laws. Still, the smaller number of areas produces larger standard errors for the estimates. This then reduces the chances of detecting any impact of the laws.

Models

Our basic model is a pooled cross-section and time-series analysis. The dependent variable is the juvenile arrest or victimization rate in a given area and year. A binary indicator for the presence or absence of a curfew law is the major independent variable.

A revised curfew statute may signal the beginning of an increased enforcement effort. Our analysis therefore separately examines the impact of revisions and of new laws. We took the dates at which cities adopted or revised their laws from Ruefle and Reynolds (1995; see also Maguire and Pastore 1997). To ensure that the annual crime data cover the same periods as do the laws, the analysis lags the beginning date of each law by one year.

An important issue in evaluating the impact of curfew laws is the degree to which cities enforce their statutes. A law is unlikely to affect youth crimes if the police ignore its existence. Besides analyzing the difference in crime rates

TABLE 1A: Means and Standard Deviations, Juvenile Arrest Rates per 100,000 Juveniles, 52 Counties, 1985 to 1996

	Mean	*Standard Deviation*
Homicide	8.21	9.54
Rape	12.69	10.63
Robbery	102.74	84.31
Aggravated assault	134.15	92.95
Burglary	212.78	95.82
Larceny	760.84	356.32
Motor vehicle theft	203.85	172.66
Simple assault	273.94	193.53
Vandalism	180.46	123.89
Weapon offenses	92.95	57.55
Curfew violations		
Law throughout period	496.33	771.19
After new law	156.88	225.74
After revised law	389.17	657.61

between periods with and without curfew laws, we therefore also estimate models that substitute curfew arrest rates for the binary indicators. This analysis addresses the question of whether the impact of curfew laws varies with the rigor of their enforcement.

It is notable that the cities and counties in our sample did make many curfew arrests. Table 1A presents the means and standard deviations for curfew and other juvenile arrest rates for all counties in the arrest analysis. Table 1B presents these statistics for the city-counties. Both Tables 1A and 1B show that areas arrested more juveniles for curfew violations than for most other offenses. In some cases, curfew arrest rates were higher than for any other crime. Areas more frequently arrested curfew violators when statutes were in effect throughout the study period, and after revised ordinances. Although arrest rates were lower in areas that enacted new laws, the average levels clearly show more than token efforts at enforcement.

Analytic Methods

Our analysis uses different estimation methods for the two bodies of data. Few counties reported zero juvenile arrests for any crime during any year in the study period. Ordinary least squares should therefore provide reasonable estimates of changes in arrests following the curfew laws. For the arrest analysis, we compute rates per 100,000 juveniles for each crime. We then logarithmically transform the rates to reduce skews.

TABLE 1B: Means and Standard Deviations, Juvenile Arrest Rates per 100,000 Juveniles, 12 City-Counties, 1985 to 1996

	Mean	*Standard Deviation*
Homicide	14.50	15.10
Rape	17.90	15.07
Robbery	168.51	126.22
Aggravated assault	178.63	109.23
Burglary	209.61	116.33
Larceny	636.36	325.13
Motor vehicle theft	309.67	263.31
Simple assault	294.23	212.48
Vandalism	206.88	164.41
Weapon offenses	117.37	65.58
Curfew Violations		
Law throughout period	728.16	1057.75
After new law	163.71	255.35
After revised law	1029.74	1171.57

Panel unit root tests (Levin and Lin 1992, 1993; Kao 1999) showed that the arrest time series are nonstationary in their means. The series lack constant means, and an analysis of the data in their original levels would risk finding spurious relationships. We therefore use first-differences of all variables to ensure stationarity. Due to this transformation, our analysis relies on within-city variation to estimate the effects of the laws. After the log-differencing, the curfew coefficients multiplied by 100 measure the percentage change in the relevant arrest rate following a new or revised ordinance.

The pooled arrest analysis employs a fixed-effects model (Hsaio 1986), with binary indicator variables for each area and each year. Because we difference the other variables, the area dummies measure the strength of stochastic trends in the county arrest rates. The year dummies allow for a limited amount of cross-sectional dependence. This dependence would arise, for example, if national events identically increase or decrease all county arrest rates in a given year.

In contrast to arrests, many counties recorded zero juvenile homicide victimizations during one or more years in the study period. Normal theory methods, such as ordinary least squares, are not appropriate in this situation. As an alternative, we employ panel Poisson models for count data (see, for example, Cameron and Trivedi 1998).

The estimates that we report rely on Liang and Zeger's (1986; see also Diggle, Liang, and Zeger 1994) Generalized Estimating Equation (GEE) method. GEE uses quasi-likelihood to model the marginal expectation of the dependent variable separately from within-unit (within-county) autocorrelation. It provides sta-

tistically consistent estimates of the coefficients and their standard errors, but it requires only modest assumptions about temporal dependence.[1]

Our GEE models use a negative binomial distribution to allow for extra-Poisson variation (overdispersion) in the homicide rates across counties. They include the log of county population age 19 or younger as an offset to control for size-related variations in exposure to homicide risk. As in the arrest analysis, we also include separate indicator variables for each year.

The GEE analysis assumes an autoregressive correlation structure for the errors. Still, as is often the case in practice (see Liang and Zeger 1986), the results do not vary greatly with other error specifications. To compute the percentage change in the homicide rate after a curfew, one may exponentiate the coefficient for the law, subtract 1, and multiply by 100.

Besides the curfew law measures and the binary indicators for areas and years, the analyses use several covariates.[2] First, the models include the logarithm of each county's total population, which helps weight the data for differences in population size. Second, the models include the logarithm of each county's real per capita personal income, which provides a general measure of economic well-being. Third, the models include the logarithm of the ratio of each county's infant mortality rate to the infant mortality rate in the United States. This measures poverty levels (as opposed to general economic conditions) relative to the rest of the nation.[3]

Finally, in the arrest analysis only, we include the logarithm of the total (juvenile and adult) arrest rate for cocaine possession and sale. Blumstein (1995) argues that juvenile violence increased sharply in the late 1980s and early 1990s with the expansion of crack cocaine markets. Our analysis spans this period. If left uncontrolled, the rise and fall of crack sales could provide an alternative explanation for an apparent effect of the laws.

Our use of the cocaine arrest measure follows Baumer, Lauritsen, Rosenfeld, and Wright (1998), who employed it as an indicator of crack use in their study of city crime rates. They found that cocaine arrests were highly correlated with other indicators of drug availability, including emergency room visits and arrestee urine test results.[4]

Additional covariates would have been desirable, but few useful annual indicators exist at the county level. It is worth stressing, however, that differencing the arrest rates removes the effects of factors that are constant within counties over time. The arrest analysis thus controls for any omitted covariates that were approximately stable within counties during the study period. Because the arrest analysis covers only 12 years, stability in omitted covariates is a reasonable possibility.

The standard errors in all models are White-Huber estimates (see Davidson and MacKinnon 1993). These are robust against heteroscedasticity in the

error terms, a likely outcome with pooled data. Still, conventional model-based standard errors only rarely led to different inferences than did their robust counterparts.

RESULTS

Estimates of the impact of curfew laws on juvenile arrest rates appear in Table 2 (all 52 counties with complete data) and Table 3 (the 12 city-counties with complete data). For purposes of brevity, and to ease interpretation, Tables 2 and 3 include only the coefficients for the curfew law indicators.

We initially estimated separate models for new laws and for revisions. We discovered, however, that the results from these separate equations were almost identical to the estimates from combined models that included both variables. Because it simplifies our presentation, the tables report the combined model estimates.

Table 2 provides limited evidence that curfews are effective in preventing some types of crimes. In particular, juvenile arrests for burglaries, larcenies, and simple assaults show statistically significant decreases after counties revised existing curfew laws. In each case, the size of the decrease is about 14 percent.

The point estimates for the effects of revised curfews on these three offenses are of roughly the same magnitude in the city-county subsample (Table 3) as in the full sample. Here, however, only the decrease in larcenies is statistically significant. In addition, inconsistent with the hypothesis that curfews prevent crimes, city-county homicide arrests significantly increase after the beginning of new laws.

Despite the significant decreases for some crimes, arrests for most of the offenses in Tables 2 and 3 do not change in the postintervention period. All of the large decreases also are for revised laws; for no crime is a new curfew followed by a reduction in arrests.

Table 4 presents the estimates for the effects of the curfew laws on homicide victimization rates. As in Tables 2 and 3, Table 4 reports only the coefficients for the laws. All of the estimates in Table 4 are statistically insignificant, both for the county and city-county samples. Curfew laws thus appear to be irrelevant to juvenile homicide victimizations.

To examine the impact of enforcement, Table 5 (all counties) and Table 6 (city-counties) replace the indicator variables for the laws with changes in logged curfew arrest rates. As in the previous arrest analysis, all outcome variables are also first-differences of logarithmically transformed rates. The estimates hold controlling for the same covariates as in the earlier models,

TABLE 2: Estimates of the Impact of Juvenile Curfew Laws on Arrests, 52 Counties, 1985 to 1996

	Coefficient	*Standard Error*	t
Homicide			
New law	.2455	.1493	1.64
Revised law	−.0843	.1841	−0.46
Rape			
New law	.0755	.1224	0.62
Revised law	−.1446	.1382	−1.05
Robbery			
New law	.0072	.0490	0.15
Revised law	−.1089	.0908	−1.20
Aggravated assault			
New law	.0604	.0471	1.28
Revised law	−.1039	.1104	−0.94
Burglary			
New law	.0380	.0516	0.74
Revised law	−.1378	.0554	−2.49*
Larceny			
New law	−.0559	.0458	−1.22
Revised law	−.1413	.0425	−3.32*
Motor vehicle theft			
New law	−.0538	.0688	−0.78
Revised law	−.1536	.0894	−1.72
Simple assault			
New law	−.0189	.0580	−0.33
Revised law	−.1517	.0650	−2.33*
Vandalism			
New law	.0999	.0531	1.88
Revised law	−.1042	.0873	−1.19
Weapon offenses			
New law	.0017	.0527	0.03
Revised law	−.0099	.0796	−0.12

NOTE: These are ordinary least squares estimates on log-differenced data. The full models included dummy variables for each county and year and used log differences of total county population, the infant mortality ratio, the cocaine arrest rate, and real per capita personal income as covariates. The standard errors are heteroscedasticity consistent.
*$p < .05$, two-tailed.

and for dummy variables representing each county and year. Again as in the other analyses, Tables 5 and 6 include only the coefficients for the effects of the curfew laws.

We noted earlier that the sample counties reported high curfew arrest rates. Although this outcome is consistent with rigorous enforcement efforts, it is worth stressing that arrests may not fully capture the intensity with which a city implements its statute. Police officers may concentrate on dispersing

TABLE 3: Estimates of the Impact of Juvenile Curfew Laws on Arrests, 12 City-Counties, 1985 to 1996

	Coefficient	*Standard Error*	t
Homicide			
New law	.3619	.1577	2.29*
Revised law	.3869	.2828	1.37
Rape			
New law	−.1856	.2937	−0.63
Revised law	−.4460	.2398	−1.86
Robbery			
New law	.0499	.1273	0.39
Revised law	−.1914	.2352	−0.81
Aggravated assault			
New law	.1047	.1947	0.54
Revised law	.1641	.1651	0.99
Burglary			
New law	−.0366	.1756	−0.21
Revised law	−.1663	.1474	−1.13
Larceny			
New law	−.1980	.1798	−1.10
Revised law	−.2073	.0879	−2.36*
Motor vehicle theft			
New law	.1463	.1054	1.39
Revised law	−.1056	.2318	−0.46
Simple assault			
New law	−.0481	.2716	−0.18
Revised law	−.1091	.0896	−1.22
Vandalism			
New law	.2002	.2365	0.85
Revised law	−.2033	.2926	-0.69
Weapon offenses			
New law	−.1822	.1498	−1.22
Revised law	.1648	.1516	1.09

NOTE: These are ordinary least squares estimates on log-differenced data. The full models included dummy variables for each county and year and used log differences of total county population, the infant mortality ratio, the cocaine arrest rate, and real per capita personal income as covariates. The standard errors are heteroscedasticity consistent.
*$p < .05$, two-tailed.

groups of young people, or on warning juveniles who are violating the law. As a result, cities may very effectively enforce a curfew even with few arrests. In addition, some cities rely on short but intense periods of police activity, during which they arrest many violators. Although such efforts may not produce a large volume of arrests over a year, they might still deter juveniles from venturing out during curfew hours.

TABLE 4: Estimates of the Impact of Juvenile Curfew Laws on Juvenile Homicide Victimizations, 1976 to 1995

	Coefficient	*Standard Error*	t
57 counties			
New law	−.0030	.0849	−0.03
Revised law	.0458	.0761	0.60
12 city-counties			
New law	−.0416	.1168	−0.36
Revised law	−.1065	.0655	−1.63

NOTE: These are Generalized Estimating Equation estimates for negative binomial data. The full models included dummy variables for each year and used the logarithms of total county population, the infant mortality ratio, and real per capita personal income as covariates. The estimation assumes an autoregressive error structure, and employs the logarithm of juvenile population as an offset. The standard errors are heteroscedasticity consistent.

TABLE 5: Estimates of the Impact of Juvenile Curfew Arrests on Other Arrests, 52 Counties, 1985 to 1996

Curfew Arrest	*Coefficient*	*Standard Error*	t
Homicide	.0574	.0684	0.84
Rape	−.0745	.0859	−0.87
Robbery	.0107	.0393	0.27
Aggravated assault	−.0033	.0311	−0.11
Burglary	−.0426	.0276	−1.54
Larceny	.0127	.0174	0.73
Motor vehicle theft	.0281	.0424	0.66
Simple assault	.0394	.0213	1.85
Vandalism	.0370	.0368	1.00
Weapon offenses	−.0017	.0296	−0.06

NOTE: These are ordinary least squares estimates on log-differenced data. The full models included dummy variables for each county and year and used log differences of total county population, the infant mortality ratio, the cocaine arrest rate, and real per capita personal income as covariates. The standard errors are heteroscedasticity consistent.

These issues aside, the results in Tables 5 and 6 do not provide evidence that higher levels of curfew enforcement reduce arrests for other crimes. All coefficients for the effects of curfew arrests are small. The only significant change is an increase in city-county simple assaults, which obviously goes against a preventive effect.

In a final analysis, we separated the daytime curfew laws from the others. We then estimated models like those in Tables 2, 3, and 4, but with separate dummies for the daytime laws and the laws that apply only at night. Results

TABLE 6: Estimates of the Impact of Juvenile Curfew Arrests on Other Arrests, 12 City-Counties, 1985 to 1996

Curfew Arrest	*Coefficient*	*Standard Error*	t
Homicide	.0457	.1153	0.40
Rape	.2235	.1402	1.59
Robbery	−.0102	.1106	−0.09
Aggravated assault	.0058	.0778	0.07
Burglary	.0014	.0586	0.02
Larceny	.0124	.0383	0.32
Motor vehicle theft	.0969	.0668	1.45
Simple assault	.0984	.0484	2.03*
Vandalism	.1506	.1179	1.28
Weapon offenses	−.0262	.0444	−0.59

NOTE: These are ordinary least squares estimates on log-differenced data. The full models included dummy variables for each county and year and used log differences of total county population, the infant mortality ratio, the cocaine arrest rate, and real per capita personal income as covariates. The standard errors are heteroscedasticity consistent.
*$p < .05$, two-tailed.

from these models revealed no differences between the daytime and other laws.[5]

Although we do not include the estimates for the control variables in the tables, it is worth noting that the signs of their coefficients generally conform to our expectations. The effects of the poverty and cocaine arrest indicators are positive in most equations, whereas the influence of per capita income is usually negative. The sign for total population varies across the models. Standard errors of the four covariates are large, however, and their effects are rarely statistically significant. Inferences about the curfew laws also do not change when we add or delete any of the covariates.[6]

DISCUSSION AND CONCLUSION

Our analysis provides, at best, extremely weak support for the hypothesis that curfews reduce juvenile crime rates. Of the offense and victimization measures, only burglary, larceny, and simple assault arrests significantly decreased after cities adopted curfew statutes. These decreases occurred only for revised laws, and only the reductions in larceny appeared in both the county and city-county samples.

Conclusions about the impact of curfew statutes therefore depend on how much weight one attaches to the burglary, larceny, and simple assault results

from the county data. Juvenile curfews might plausibly influence these offenses, and the insignificance of the estimates for burglaries and simple assaults in the city-county subsample could be due to its smaller size. The analysis would then support the idea that curfews have at least modest effects on some crimes.

Any influence of the curfews appeared only for revised statutes, however, and new laws were ineffective in reducing offending or victimization. This raises the possibility that more vigorous enforcement after the laws were revised accounts for the results. Yet, the analysis using curfew arrests did not find that increases in enforcement produced lower rates of other offenses. In addition, a statistically significant increase in city-county homicide arrests balanced the three significant decreases in the county data. One might therefore dismiss the apparent effects of the revised statutes as the product of chance. Under this interpretation, curfew laws have no effect on crime at all.

However one explains the results, our study has several limitations that deserve emphasis. Most obviously (and as already noted), the analysis used arrests as a measure of age-specific offending, and it examined only the most serious type of victimization. Curfew laws may affect victimizations other than homicides, and arrest statistics almost certainly contain high levels of measurement error. These factors make it difficult to separate the impact of the laws from other sources of variation.

The analysis also covered a relatively brief period of time. This short time span produces additional error in estimating the preintervention and postintervention means, further reducing the chances of discovering a nonzero impact.

Finally, and perhaps most important, the study estimated only the average outcomes of the curfew laws across the sample. This average impact may hide major variations between cities in the effects of the laws. For example, many cities have adopted curfew statutes as part of a larger effort to reduce juvenile crime (OJJDP 1996). Because of differences in strategy, some cities might register substantial decreases in crime even when the average population change is zero.

More generally, the details of where and how the police enforce curfews could influence their effects. Future research that focuses on individual cities and on variations in policy could help resolve questions about the conditions under which curfews are most likely to be successful.

Despite these qualifications, our results do not encourage the idea that curfews help prevent juvenile crime. Any impacts of the laws were small, and they applied only to a few offenses. If curfew statutes do reduce juvenile offending and victimization rates, their influence may not be as large or as general as policy makers have hoped.

NOTES

1. We also estimated conditional fixed-effects negative binomial models (Cameron and Trivedi 1998, pp. 280-284). The results were substantively identical to those from the GEE analysis.

2. In the arrest analysis, we use first-differences of the covariates. In the homicide victimization analysis, we use the covariates in their original levels.

3. Markides and McFarland (1982), among others, provide a discussion of infant mortality rates as an indicator of poverty. See Loftin and Parker (1985) and McDowall (1986) for uses of the measure in criminological research.

4. Baumer, Lauritsen, Rosenfeld, and Wright (1998) report a correlation of .77 between their cocaine arrest measure and positive urine test results from the Drug Use Forecasting Program (DUF). They also report correlations of .72 between their measure and emergency room cocaine mentions, and of .67 between their measure and medical examiner cocaine mentions. We found a correlation of .51 between our cocaine measure and DUF data for 1988 to 1996, using the 21 sample counties that contain a city in the DUF program. Although this is lower than in Baumer et al. (1998), it is reasonable given the mismatch between city DUF data and county arrest data.

5. To conserve space, we do not report the estimates from this analysis. We will send interested readers the complete set of results on request.

6. Again, we would be happy to provide full estimates for models with all covariates on request.

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