ARE IDLE HANDS THE DEVIL'S WORKSHOP?

INCAPACITATION, CONCENTRATION AND JUVENILE CRIME *

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Abstract

This paper examines the short-term effect of school on juvenile crime. To do so, we bring together daily measures of criminal activity and detailed school calendar information from 27 jurisdictions across the country. We find that the level of property crime committed by juveniles decreases by 15 percent on days when school is in session, but that the level of violent crime *increases* by nearly 20 percent on such days. These results do not appear to be driven by inflated reporting of crime on school days or substitution of crime across days. Our findings provide evidence for both incapacitation and concentration models of schooling—when juveniles are not provided with constructive activities, they are more likely to engage in certain anti-social behaviors; at the same time, the increase in interactions associated with school attendance leads to more interpersonal conflict and violence. These results underscore the social nature of violent crime. Furthermore, they suggest that youth programs—particularly those with no educational component such as midnight basketball or summer concerts—may entail important tradeoffs in terms of their effects on juvenile crime.

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ABSTRACT

This paper examines the short-term effect of school on juvenile crime. To do so, we bring together daily measures of criminal activity and detailed school calendar information from 27 jurisdictions across the country. We find that the level of property crime committed by juveniles decreases by 15 percent on days when school is in session, but that the level of violent crime increases by nearly 20 percent on such days. These results do not appear to be driven by inflated reporting of crime on school days or substitution of crime across days. Our findings provide evidence for both incapacitation and concentration models of schooling—when juveniles are not provided with constructive activities, they are more likely to engage in certain anti-social behaviors; at the same time, the increase in interactions associated with school attendance leads to more interpersonal conflict and violence. These results underscore the social nature of violent crime. Furthermore, they suggest that youth programs—particularly those with no educational component such as midnight basketball or summer concerts—may entail important tradeoffs in terms of their effects on juvenile crime.

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1. Introduction

Juvenile crime touches millions of people in the United States each year, imposing substantial costs on society. In 1997, law enforcement officials arrested 2.8 million people under the age of 18, accounting for one in five of all arrests that year. Homicide is the second leading cause of death for youth ages 15 to 24; and juveniles are twice as likely as adults to be victims of serious violent crime and three times as likely to be victims of assault (Snyder and Sickmund 1997). Economists estimate that expenditures on criminal justice and private protection draw $175 billion away from other productive uses each year (Anderson 1999). Similarly, the cost to society of allowing one youth to leave high school for a life of crime and drug abuse is estimated to range from $1.7 to $2.3 million (Snyder and Sickmund 1999, 82). From the perspective of the juvenile, incarceration is associated with a 10-30 percent decrease in earnings (Grogger 1992, 1995; Waldfogel 1994; Freeman 1995; Kling 1997; Western 2001).

For these reasons, researchers have long sought to better understand the determinants of juvenile crime. Studies have suggested a variety of factors related to juvenile crime, including age (Blumstein et. al. 1986), gender (Wilson and Hernstein 1985), family background (Mocan and Rees 1999, Levitt and Lochner 2001), parenting quality (Daag 1991, Sampson and Laub 1993), economic opportunities (Grogger 1998, Lochner 1999) and the severity of punishment (Levitt 1998). Yet the factors underlying juvenile crime patterns and trends remain largely unexplained (Levitt and Lochner 2001).

It is often suggested that one way to reduce juvenile crime is to lengthen the school day or school year and/or to provide activities for young people when school is not in session. The implicit notion behind such program-oriented solutions to juvenile crime is a belief in the importance of incapacitation—i.e., that "idle hands are the devil's workshop" and that keeping

kids busy will keep them out of trouble. Advocates of after-school and other youth programs frequently claim that juvenile violence peaks in the after-school hours on school days and in the evenings on non-school days. Indeed, as we can see in Figure 1, violent crime does in fact follow this pattern.[1]

While the intuition behind such policy prescriptions is sensible, the actual short-term effect of school or youth activities on juvenile crime is far from clear. First, there is no definitive evidence on the causal impact of youth programs on crime (Sherman 1997). Second, the studies of the timing of juvenile crime tend to look *within* the day, but do not address the level of crime *across* days. Implicitly, proponents of after-school program believe that lengthening the school day would lower crime during the afternoon without increasing violence during other periods. However, the fact that 57 percent of violent juvenile crime during the entire year occurs on the 180 days school is in session (Snyder and Sickmund 1999, 57) suggests that there is an association between school and *increased* juvenile violence. Finally, these studies do not consider property crimes or other non-violent crimes.

In this paper, we carefully examine the relationship between school and juvenile crime. This effort will not only help us to better understand the determinants of juvenile delinquency, but also provide some insight regarding the potential impacts of policy initiatives such as lengthening the school day/year or introducing new youth activities. Note that when we refer to the effect of school, we mean a short-run effect that is likely driven by day-to-day changes in the desire and opportunity to commit crime. We do *not* examine the longer-term impact of educational attainment on criminal activity that may operate through changes in the returns to legitimate work, the financial or psychic rewards of crime, or preferences. There is a relatively

[1] Snyder and Sickmund (1999) first documented this pattern using 1991-1994 NIBRS data.

well-developed literature on this latter topic. See, for example, Witte (1997), Lochner (1999) and Lochner and Moretti (2001).

To begin, we develop a simple model of the relationship between school and juvenile crime that emphasizes both incapacitation effects (i.e., keeping kids busy and "out" of trouble) as well as concentration effects (i.e., bringing together a large group of youth together in one location). The model suggests that school will unambiguously decrease the prevalence of juvenile property crime, but will have an ambiguous effect on the prevalence of juvenile violent crime.

To test this model, we bring together data on schooling and criminal activity from a number of cities and towns across the United States. To measure criminal activity, we use daily level reports of criminal incidents, victimizations and arrests for 27 jurisdictions from 1995 to 1999 collected by the Bureau of Justice Statistics and compiled in the National Incident-Based Reporting System (NIBRS). To this crime data, we merge school calendar data for each year collected from the school districts within each reporting jurisdiction. The school calendar data provide precise information on the days that students have off and the reason (e.g., summer break, national holiday, teacher in-service training, etc.), allowing us to exploit the considerable variation in school terms across cities and over time. We attempt to account for unobservable characteristics that may be correlated with the school session and criminal activity by including a series of fixed effects for city, month, year and day of the week. We also control for the level of adult crime, which will pick up unobserved factors such as police presence, city-wide festivals, etc. Finally, we focus on the variation generated by teacher in-service training days, which we argue is extremely unlikely to be correlated with any factors influencing the level of criminal activity.

As predicted by the model, we show that the level of property crime committed by juveniles decreases on days when school is in session. In contrast, we find that the level of violent criminal offenses and victimizations among juveniles *increases* on school days. Several facts suggest that this is not simply due to heightened reporting by school personnel. The estimates for aggravated assault, a serious violent crime that is likely to be reported regardless of the time or location of occurrence, are close to those for simple assault, a relatively minor offense that may well not be reported in many instances.[2] Also, the pattern of results is similar if one excludes all crimes that take place in school and/or during school hours. Moreover, we find no evidence that crime changes on the days immediately before or after school closings, suggesting that the changes in crime are not simply a result of substitution across days.

This analysis provides some valuable insight into the nature and motivation of juvenile crime. In particular, it confirms that when juveniles are not provided with a supervised environment, they are likely to engage in anti-social behavior that manifests itself in increased property crime. However, it also suggests that the degree of interaction among youth plays a significant role in the level of juvenile violence, highlighting the potentially volatile nature of juvenile interactions and the social nature of juvenile violent crime.

This analysis also has several implications for policy. First, it seems clear that policies that introduce after-school programs, lengthen the school year or provide other programs for young people will help mitigate the number of property and other opportunistic crimes committed by juveniles. However, our results suggest that such policies have important tradeoffs in relation to personal or violent crime. Because they increase the concentration of

[2] Ideally, we would like to look at *very* serious crimes such as murder and rape. These crimes are extremely rare, however, making it impossible to estimate the effect of school on these incidents with precision.

young people in certain locations, they run the risk of raising the number of altercations that turn violent.

The remainder of the paper is structured as follows. Section 2 presents a simple model of the relationship between juvenile crime and school. Section 3 discusses the empirical strategy and Section 4 describes the data used in this analysis. Section 5 presents the results and Section 6 concludes.

2. A Simple Model of the Relationship between School and Crime

In order to motivate and interpret our empirical work, in this section we develop a simple model of the effect of school on crime. The implicit assumption behind most of the arguments that propose after-school and other youth programs as a solution to the juvenile crime problem is that school has an incapacitation effect. That is, keeping kids busy and "off" the streets will reduce juvenile crime. The framework we present here not only formalizes this incapacitation effect, but also emphasizes the role of school and other youth programs in bringing adolescents together. On the one hand, school provides activities, structure, and monitoring that may well deter adolescents from committing crime while in school. On the other hand, school increases the geographic concentration of juveniles during the school day, increasing the number of potentially volatile interactions and thereby raising the level of juvenile violent crime.[3]

[3] School may also facilitate the coordination of crime among juveniles. That is, the increased concentration of juveniles may decrease the cost of planning crimes, which may in turn increase the observed level of juvenile crime. This effect is likely to apply to both property and violent crime. Coordination necessarily implies that two or more offenders are involved in the commission of a crime. If school were important for the coordination of crimes, we would expect for school to have a more positive (less negative) effect on the incidence of crimes with multiple offenders. In section five, we see that this is not the case. Therefore, because incorporating coordination effects into our current framework would substantially increase the complexity of the model, and does not appear to offer any additional insight, we have decided not to include it in the formal model.

On school days, we assume that youth spend some fraction of their time in school, *S*, and the rest of their time unsupervised in their own neighborhood, $R = 1 - S$.[4] Since we assume that it is more costly for juveniles to commit property crime at school,[5] the amount of property crime committed by each juvenile, *C*, is an increasing function of the amount of non-school time.[6] In particular, we assume that $C_R(R) > 0$. This implies that there will be less property crime on school days than non-school days, and less property crime as the length of the school year or school day increases.

We assume that juvenile violent crime depends not only on the amount of unsupervised time, but also on the number of interactions among youth, which we denote as *I*.[7] We assume that there is a probability *P* with which any particular interaction results in a conflict. However, we also know that every conflict does not result in violence, so we let *G* represent the fraction of conflicts that lead to violence. Because fighting is more difficult (but not impossible) in school, the ability of juveniles to carry out their violent intentions depends on the amount of free time available,[8] $G = G(R)$ and $G_R(R) > 0$. We can then express the total number of violent incidents in which a student is involved as $V = I \times P \times G(R)$.

While it was straightforward to examine the effect of school on property crime, in order to find the effect of school on violent crime, we need to address how school affects the total number of interactions. In order to address this issue, we will assume that there exists a

[4] We could expand the model to consider time at the end of the day with parents, but this would add little additional insight at the cost of added complexity.
[5] Empirically, very few reported property crimes are committed in school, perhaps because of greater supervision and perhaps because the targets of property crime in school are less attractive or lucrative.
[6] One can obtain this reduced form crime equation by assuming students derive utility from committing property crime and other activities. The juveniles then choose their activities subject to a free time constraint.
[7] This is motivated by the large percentage of juvenile violent crime that involves altercations between two or more juveniles.
[8] A significant of fraction of juvenile violent crime does occur in schools. Even on school days, however, the vast majority occurs in other locations.

continuum of juveniles (the population is normalized to one) and $N > 1$ residential locations. During non-school hours, juveniles are evenly dispersed among these areas (the fraction of juveniles in each location will be $1/N$). When school is in session, for simplicity we assume that all juveniles are concentrated in a single location. We assume that at any given point in time each juvenile interacts with every other juvenile in the location. Thus, the total number of interactions a student faces during non-school hours is

$$(1) \qquad I^R = (1-S)\frac{1}{N}$$

The total number of interactions during school is simply

$$(2) \qquad I^S = S$$

Thus the total number of interactions experienced by students is

$$(3) \qquad I = S + (1-S)\frac{1}{N}$$

Having seen how school affects interactions, it now becomes straightforward to examine the effect of school on violent crime.

$$(4) \qquad V = P \times \left[S + (1-S)\frac{1}{N} \right] \times G(R)$$

Differentiating with respect to the amount of school yields the following (recall that $R = 1 - S$):

$$(5) \qquad \frac{dV}{dS} = \left(1 - \frac{1}{N}\right) \times P \times G(R) - P \times \left[S + (1-S)\frac{1}{N} \right] \times G_R(R)$$

The first term is positive and takes into account that extending the school day increases the time youth spend in a high concentration area, leading to more negative interactions. The second term is negative and shows that as children have less free time, it is more difficult to carry out violent intentions.

This model suggests that one must (a) consider violent and property crime separately and (b) clearly distinguish between the timing and the level of crime. The fact that violent crime rates spike in the late afternoon on school days does not imply that the absence of school increases crime—it may simply reflect the effect of school on the timing of violence. Our model implies that school will indeed decrease property crimes but it may actually *increase* violent crime. Because we are examining the daily incidence of crime as opposed to the timing of crime we are able to test these implications.

This model can be applied to schooling changes on the extensive margin (e.g., increasing the number of school days during the year by, for example, shortening the summer break) as well as the intensive margin (e.g., increasing the length of the school day by, for example, instituting an after-school program). It is worth noting that school days could be more violent than non-school days even if increasing the duration of the school day reduces crime on the margin. The intuition in this model can also be used to better understand the potential effects of other youth programs, activities or neighborhood amenities (e.g., parks, recreation centers, midnight basketball games). To the extent that these opportunities attract a number of juveniles to the same location, they will increase the number of interactions between youth, thereby raising the potential number of conflicts and violent incidents.

3. Empirical Methods

3.1 Identification Strategy

We are interested in examining the impact of school on juvenile crime, a relationship that can be captured by the following simple equation:

(6) $$JuvenileCrime_{dmyc} = \alpha + \delta(NoSchool_{dmyc}) + \varepsilon_{dmyc}$$

where *JuvenileCrime* is a measure of criminal activity on day *d*, in month *m* and year *y*, in city *c*, and *NoSchool* is a binary variable that takes on a value of one when school is not in session and zero otherwise.

If the variable *NoSchool* is uncorrelated with the error term in equation (6), then δ provides an unbiased estimate. However, it is likely that school days share certain unobservable characteristics that are correlated with the level of criminal activity. Saturday, for example, is typically not a school day. Insofar as more people are out shopping and at social events on weekends, the benefits of crime might be greater, which would tend to bias our estimates downward. This is true for the summer days as well since crime rates typically rise with the temperature (Jacob and Lefgren 2002). Conversely, juvenile crime rates are generally lower on holidays, presumably because benefit of criminal activity decreases as people are at home with family and the opportunity cost for potential criminals increases insofar as they are attending holiday celebrations of their own. This would tend to bias our estimates upward.

We address these endogeneity concerns in two ways. First, using detailed school calendar data for each city*year, we are able to precisely identify the days that school is not in session, some of which are unlikely to be correlated with criminal activity for other reasons. The school calendars indicate five broad reasons why school may not be in session. The most obvious reason is summer break. While this is problematic for the reasons mentioned above, the exact timing of summer break can provide us with useful variation. The exact timing of summer break differs across districts and within districts over time. By including district*year*month fixed effects, we can estimate the impact of summer break using variation that will likely be free of many of the worst problems described above.

Students generally do not have school on major state or federal holidays such as Thanksgiving, Christmas and Easter. While these days are likely correlated with other things that influence crime rates, school breaks often extend well before or after these holidays. In most school districts, for example, Spring break lasts at least a week. Moreover, many school districts have breaks that are not directly associated with national holidays, such as week-long mid-winter breaks in February or a multi-day Fall breaks in November.[9] Insofar as they are not holidays for adults, these breaks may be uncorrelated with other factors influencing crime. Fixed effects will again help us to isolate the most useful variation.

Perhaps the best source of variation in school days, however, comes from teacher in-service days. These are days that teachers use for professional development or planning purposes, and are either half- or full-days off of school for students. These days are scattered throughout the academic year, vary considerably across districts and over years within district, and are explicitly *not* scheduled around national holidays, other school breaks or important school events such as standardized testing. For these reasons, it is highly unlikely that these teacher in-service days are different than other days during the year.

In addition to the detail provided by school calendars, we use the level of *adult* crime to account for any unobserved characteristics associated with the aggregate level of crime on a particular day in a particular location, such as the weather, street fairs or the level of police enforcement. In practice, we simply control for the level of adult crime in the model (thereby not restricting the coefficient on adult crime to be one), estimating the following equation:

$$(7) \quad \begin{aligned} JuvenileCrime_{dmyc} = {} & \alpha + \delta_1(Summer_{dmyc}) + \delta_2(Holiday_{dmyc}) + \delta_3(Break_{dmyc}) + \\ & \delta_4(Inservice_{dmyc}) + \gamma(AdultCrime_{dmyc}) + X\beta + \varepsilon_{dmyc} \end{aligned}$$

[9] These breaks are often associated with the change in school quarters or semesters.

where *Summer*, *Holiday*, *Break* and *Inservice* are binary variables that take on the value of one if school is not in session for a particular reason, *AdultCrime* is an analogous measure of criminal activity for adults, and X is a vector that includes fixed effects for the day of the week as well as fixed effects for city*year*month.[10]

3.2 Estimation Issues

From an econometric standpoint, it is important to use an estimation strategy that takes into account the nature of the data. While the simplest strategy is to estimate OLS models using the number of criminal incidents/offenders, this strategy has several problems. Because criminal incidents are positively skewed, it is common to transform the data using logs or log rates. However, because we are using daily data for individual cities and we are differentiating between adult and juvenile crime, there are a non-trivial number of zeros in the data, particularly when we focus on individual crime categories—complicating the use of log crime rates.

In order to address these concerns, we estimate a negative binomial regression model (Greene 2000). The negative binomial model is a generalization of the Poisson regression model and allows for the variance of the outcome measure to differ from the mean. This technique is ideal for dealing with count data with overdispersion. Using this model, we assume that the mean number of occurrences, μ_{dmyc} in a given jurisdiction is given by:

$$\ln(\mu_{dmyc}) = \alpha + \delta_1(Summer_{dmyc}) + \delta_2(Holiday_{dmyc}) + \delta_3(Break_{dmyc}) + \delta_4(Inservice_{dmyc}) + \gamma(AdultCrime_{dmyc}) + X\beta + \varepsilon_{dmyc} \quad (8)$$

We can define u_{dmyc} and λ_{dmyc} in the following way:

$$\ln(u_{dmyc}) = \varepsilon_{dmyc} \quad (9)$$

[10] This subsumes all factors that do not vary within a given month and district. Note that in our sample there are not multiple school districts within a NIBRS jurisdiction.

$$\ln(\lambda_{dmyc}) = \alpha + \delta_1(Summer_{dmyc}) + \delta_2(Holiday_{dmyc}) + \delta_3(Break_{dmyc}) + \delta_4(Inservice_{dmyc}) + \gamma(AdultCrime_{dmyc}) + X\beta \quad (10)$$

Note that λ_{dmyc} is the standard mean/variance parameter from the Poisson distribution. We can now rewrite equation (8) in the following way:

$$\ln(\mu_{dmyc}) = \ln(\lambda_{dmyc}) + \ln(u_{dmyc}), \quad (11)$$

We assume that the unconditional probability distribution of number of crimes is given by:

$$f(y_{dmyc} \mid X_{dmyc}) = \int_0^{\infty} \frac{e^{-\lambda_{dmyc} u_{dmyc}} (\lambda_{dmyc} u_{dmyc})^{y_{dmyc}}}{y_{dmyc}!} g(u_{dmyc}) du_{dmyc} . \quad (12)$$

Assuming a gamma distribution for the error term, it is straightforward to estimate the parameters of the model using maximum likelihood.

4. Data

In order to address our research objectives, it is necessary to combine detailed information on schooling and criminal activity. For information on criminal activity, we rely on the data provided in the *National Incident-Based Reporting Systems* (NIBRS). NIBRS data is crucial for our analysis since it is the only (to our knowledge) large-scale dataset that provides information on reported crimes and arrests on a *daily* basis. The NIBRS data is attractive for a number of other reasons as well. First, the age of the victim and offender are reported. This allows us to focus in on crimes committed by and against juveniles. Second, the dataset reports the nature of the crime. Thus we can examine the effect of school attendance on different types of crime. Third, the data set reports the time and location of crimes. With this information we are able to investigate the effect of schooling on the time and venue of criminal activity in

addition to its level. Finally, NIBRS contains information on enough jurisdictions to lend statistical power to the analysis.

The primary disadvantage of NIBRS is that it only includes information on jurisdictions that have agreed to participate in the system, unlike the Uniform Crime Reports, a monthly compilation of criminal activity collected by the FBI to which all law enforcement agencies throughout the country are required to report. In the first year of our sample, 1995, only 1,255 jurisdictions participated in NIBRS compared with 18,643 that reported data for the UCR. By 1999, 2,852 out of 19,659 jurisdictions were participating in NIBRS. For the purpose of our study, we chose jurisdictions with large populations and/or a high incidence of reported juvenile crime. We ultimately ended up with 27 jurisdictions because the vast majority of participating NIBRS jurisdictions are extremely small towns or rural/suburban jurisdictions.[11] In general we tried to use all days for which the reporting agency participated in NIBRS. For a small number of periods, it appeared that reporting had not been systematic (e.g., crimes drop by 70 percent for a three month period and then return to the previous level). We dropped the time periods for which this was the case.

While our sample is by no means representative of the nation, Table 1 shows that our analysis does include a reasonably diverse collection of jurisdictions that appears to reflect the nation along a number of dimensions. Indeed, the percent white, black, Hispanic and Asian in our sample cities is virtually identical to the nation as a whole. The same is true in terms of the fraction of single-parent households and the fraction of the population aged 10-19. The crime rate in our sample jurisdictions is considerably higher than the national average, although this is

[11] Of the 2,852 jurisdictions that participated in NIBRS in 1999, 34 percent represented county and state police agencies. Of the remaining jurisdictions (city and town police departments), only 1.5 percent (28 jurisdictions) had over 100,000 inhabitants. Only 45 jurisdictions had between 50,000 and 100,000 inhabitants. Seventy-four percent of jurisdictions had fewer than 10,000 inhabitants. The fact that most participating jurisdictions are quite small reflects the fact that there are many more small towns than large cities nationally.

largely due to the fact that we are only looking at cities while the national averages contains suburbs and rural areas with significantly lower crime rates. The average jurisdiction size in our sample is 160,578, ranging from small towns such as Logan, UT and Nampa, ID with populations of 40,849 and 42,737 respectively to relatively large cities such as Austin, TX and Colorado Springs, CO with populations of 560,389 and 357,741 respectively. Moreover, the cities in our sample are spread throughout the country, with one-quarter to one-third of the sample coming from the south, Midwest and west. Relatively few of our jurisdictions are located in the northeast. (A complete list of the jurisdictions and years included in the analysis can be found in Appendix A.)

It is worth noting that the fact that our sample is not nationally representative will not affect the internal validity of the estimates. The use of city*year*month fixed effects implies that all identification is being achieved by high frequency variation within a school district. Unlike estimators that rely on cross-sectional variation, our strategy is unlikely to be affected by the bias associated with a nonrandom selection of school districts. Our estimates, however, may differ from those estimated using all school districts within the nation. As in all such cases, the external validity of the estimates depends on the similarity between the analysis sample and the sample to which one would like to make an inference. Because of the diversity of our sample, we believe that inferences can be made to most cities in the U.S.

To obtain information on schooling, we merge the NIBRS data with detailed school calendar data that we obtained by calling individual school districts. The school calendars provide precise information on the days that students have off from school and the reason (e.g., summer break, national holiday, teacher in-service training, etc.). This not only allows us to minimize measurement error, but also to exploit the considerable variation in school calendars

across cities and over time within a city. Our sample includes calendars from the 27 schools districts in our sample.

Having described our data sources, it is important to describe our measures of criminal activity. As noted above, NIBRS provides information on both incidents and arrests. The primary advantage of arrest data for our purposes is that it allows one to determine the offender age with certainty. However, because not all incidents result in an arrest, this data will tend to understate the true level of criminal activity and provide much less information for our estimation. In order to maximize the power of our analysis, we therefore use incident data in our main specifications, including all incidents for which it is possible to identify the age of the offender. We then test the robustness of our results by using arrest data.

In addition to checking our results using arrest data, we also provide some additional analysis that supports using incident-based data. One concern with focusing on incident data involves the prevalence of missing offender age data. In our sample, only 9 percent of violent crime incidents are missing information on the offender's age whereas roughly 58 percent of property crime incidents are missing this information. If offender age were missing at random, or in a way uncorrelated with the school term, it will not influence the consistency of our estimates. However, one might contend that offender age is *less* likely to be missing on school days perhaps because crimes take place right after school or near school premises and that police and potential victims are more cognizant of juveniles on such days. If this were true, our estimates may be biased upward. In the case of property crime—where missing data is the greatest concern—this suggests that school may decrease property crime even more than we find.

To obtain some sense of how serious an issue this may be in practice, we estimate models similar to (7), but with the outcome variable being the fraction of reported incidents with no

information on offender age. Because the nature of the crimes and degree of missing data varies significantly across crime type, we estimate separate models for violent and property crime. For both types of crime, we find neither statistically significant nor substantively important relationships between teacher in-service day, school breaks or summer vacation and the fraction of data that is missing offender age.[12] This suggests that the missing offender information in incident reports is unlikely to seriously bias our findings.

Another concern involves the *accuracy* of the offender age information. Fortunately, the existence of arrest information linked to the incident reports provides us with a way to examine this issue. If the incident results in an arrest, the age of the arrestee is always collected. In our sample, roughly half of reported incidents include information on the offender. Of these incidents, roughly 48 percent result in arrests, which means that in about one-quarter of all reported incidents we have information on the offender's age as reported by witnesses or victims as well as the exact age of the suspect arrested for the offense, allowing us to check the accuracy of reported offender age. The correlation between reported offender age and actual arrestee age is roughly 0.96.[13] Moreover, of all of the offenders who were reported as juveniles (ages 5 to 17) by victims or witnesses, less than 3 percent were in fact 18 years or older (at the time of the incident) based on the arrest records, and nearly all of these 3 percent were 18 to 20 years old according to arrest records. Virtually none of the offenders who were reported as 18 or older by victims or witnesses were actually younger than this based on arrest records.

[12] On national holidays, reported property crimes are *more* likely to be missing offender age information, but reported violent crimes are *less* likely to be missing offender age. This may be because a greater proportion of violent crime on holidays involves family members, or that property crime during the holidays includes a disproportionate number of home burglaries where the offender is rarely known. Regardless, this correlation is not problematic since we rely on teacher in-service days in our analysis. All of these results are available from the authors upon request.

[13] Because there may be more than one reported offender and/or arrestee per incident, we look at both the maximum and minimum ages. The correlation between minimum reported and actual age is 0.95 and the between maximum reported and actual ages is 0.98.

A final concern—reporting bias—applies to both incident and arrest data. Both measures miss crime that goes unreported to the police and/or do not result in an arrest. As long as unreported crime is distributed randomly, this should not influence our estimates. However, if the likelihood a crime is reported is correlated with the school term, our results may be biased. We discuss this issue in greater length in the following section, providing evidence that reporting bias is not in fact driving our results.[14]

Table 2 provides descriptive statistics on the primary variables used in the analysis. Note that the unit of observation is a city*day. Our primary outcome measure is the number of reported juvenile offenders in each city*day. We see that there are an average of 5.6 juvenile offenders for all serious offenses with a standard deviation of 5.8. The 25th percentile is 2 and the 75th percentile is 8. For 7.6 percent of our sample there are no juvenile offenses. Looking at data on the key independent variables—school session—we see that nearly 23 percent of the days in our sample consist of summer vacation, 2.2 percent consist of national holidays, 3.8 percent are school breaks and 1.3 percent are teacher in-service days.

5. Results

5.1 The Four "W's" of Juvenile Crime

In order to provide some background on juvenile crime, this subsection will describe *who* commits juvenile crime, *what* crimes juveniles most often commit, and *where* and *when* juvenile crimes occur. Table 3 presents some basic descriptive statistics regarding juvenile offenders and their victims. We see that offenders are most likely to be white, male and are 15-17. The

[14] Others have used survey data to better measure the true level of criminal victimizations and to assess the degree of unreported crime. Unfortunately, victimization surveys do not provide sufficient coverage to test our hypothesis. They also suffer from the measurement error and biases inherent in self-report data.

average age of juvenile offenders is 14.4 to 14.7 years depending on the nature of the crime. A quarter to a third of juvenile offenses in our sample is committed by youth age 12-14 while the majority is committed by youth age 15-17. A very small fraction of juvenile crime is committed by youth under 12 years old. Blacks commit over 40 percent of reported juvenile violent crimes in our sample, but less than 30 percent of other types of crime. Whites commit the majority of crimes in our sample and relatively few crimes are committed by individuals of other races. It is important to mention that the offender data does not contain information on ethnicity so we are unable to identify individuals of Hispanic descent. Males commit over 70 percent of all reported crimes.

Table 4 shows what types of crimes juveniles are most likely to commit. Among NIBRS Type A offenses (i.e., serious), the most common crimes committed by juveniles are shoplifting, simple assault and vandalism, which account for nearly half of all crimes committed by juveniles. Larcency, drug violations, burglary, aggravated assault and motor vehicle theft account for an additional 30 percent of juvenile crime. Very serious violent crimes such as rape and murder account for a tiny proportion of juvenile crime (less than 1 percent of serious offenses). The bottom panel includes NIBRS Type B (i.e., minor) as well as serious offenses. We see that runaway, curfew/loitering/vagrancy, disorderly conduct and liquor law violations are the most common minor offenses committed by juveniles, accounting for roughly 18 percent of all juvenile offenses.

Table 5 provides a snapshot of where juvenile crimes occur. The majority of thefts occur at stores or restaurants, reflecting the prevalence of shoplifting among juveniles. On school days, however, roughly 7 percent of thefts occur within school buildings and, in these cases, presumably involve one youth stealing something from another. While over three-quarters of

vandalism incidents take place in public places or residences, nearly 11 percent of all reported vandalism incidents on school days take place in school. Similarly, a large proportion of assaults and drug violations (nearly 25 percent) take place in school. On days when school is not in session, assaults and drug violations nearly always occur in public locations or individual residences.

Figure 1 shows the timing of juvenile crime on school and non-school days. We see that on school days, violent crime peaks at around 3 p.m.—immediately after school gets out. The rate then declines steadily through the evening until midnight.[15] On non-school days, crime rises gradually until the afternoon and then plateaus until after midnight. The timing of property crime appears similar with the incidence of crime rising gradually until early evening and then declining.

5.2 The Impact of School on Juvenile Crime

Having described the basics of juvenile crime, we now examine whether there is evidence of a relationship between school and juvenile crime. Table 6 shows the mean number of juvenile offenders per 100,000 residents on school and non-school weekdays in our sample. There seems to be little differences between aggregate property crime rates on school versus non-school days. However, focusing on the cleanest comparison—between teacher in-service days and school-days—we see that the juvenile property crime rate tends to be substantially higher on in-service days. The incidence of these crimes also appears to be slightly higher during the summer and breaks. In contrast, the juvenile violent crime rate appears to be substantially lower on all non-school days, regardless of the reason. Indeed, juvenile violent

[15] The level of crime around midnight is somewhat inflated because of a tendency for law enforcement agencies to list midnight as the time of occurrence for a disproportionate number of crimes occurring from 11 pm to 1 am. We thank Howard Snyder for bringing this point to our attention.

crime rates are roughly 20 percent lower on teacher in-service days compared with school days. The pattern is less striking for other serious offenses and minor offenses.

It is not clear, however, that the difference in means represents a causal effect of school on crime. We know that the summer vacations and national holidays are different than school days along a number of dimensions. Similarly, it is conceivable that breaks and in-service days may be correlated with unobservable factors related to crime. For example, school districts that schedule many teacher in-service days may differ systematically from other school districts, or the scheduling of breaks or in-service days may take place during times of the year that tend to have low (or high) crime.

Table 7 shows the exponentiated coefficients from negative binomial regressions, which can be interpreted as incidence rate ratios. The dependent variable is the number of reported juvenile offenders. The key independent variables are binary variables that indicator whether the day is a summer vacation, national holiday, school break or teacher in-service day, with the omitted category being a typical school day. All regressions also include controls for the day of the week, the number of adult offenders in the particular offense category and a set of fixed effects for city*year*month. This specification should eliminate bias associated with static (or even slowly changing) differences between districts. The fixed effects also subsume changes in criminal activity due to the time of year.

If we consider all crimes together (in column 1), there does *not* appear to be a strong or convincing relationship between school and crime. We see that there are 25 percent fewer reported juvenile offenders on national holidays, and 4 to 5 percent fewer reported offenders during summer vacation and school breaks. Unfortunately, as we discussed earlier, there are good reasons to believe that unobserved factors on these days may be influencing juvenile crime

rates. Turning to our most convincing source of variation for school—teacher in-service days—we see that there is no statistically significant effect on reported juvenile crime.

When we consider violent and property crimes separately, however, a quite different picture emerges. As we saw in a simple comparison of means, the level of juvenile property crimes is 15 percent higher on teacher in-service days and 10 percent higher during school breaks.[16] In contrast, the level of juvenile violence is 20 and 34 percent lower on teacher in-service and school break days respectively. There is little apparent effect for other crimes, although there is a statistically significant negative effect associated with school breaks. This makes sense insofar as drug violations comprise the majority of other crimes, and one might think that kids have fewer friends to whom to sell drugs on these days.

Table 8 presents similar estimates for individual offenses separately. Among violent crimes, we see that school appears to have a similar effect on all assaults and only aggravated assaults—though the aggravated assault point estimates are less precisely estimated. The various property crimes are all consistent with the general finding. On teacher in-service days, the percent of reported juvenile offenses increases 24 percent for burglary, 20 percent for vehicle theft, 27 percent for shoplifting, 17 percent for vandalism and 27 percent for robbery. The effects are generally similar for school breaks, with the exception of robbery where it appears that robberies decrease during school breaks. Other crimes are interesting and consistent with the model. Drug violations appear to decrease when school is out, likely because it is harder for the potential buyers and sellers to connect with each other. Curfew and loitering violations increase by 21 percent during teacher in-service days, although this effect is not statistically

[16] It is worth noting that summer vacation appears to have only a modest and insignificant effect on property crime. This might be because students are more likely to vacation with family or engage in other forms of summer recreation as opposed to committing crime. Table 8 shows that the small effect is largely driven by the fact that summer vacation appears to have a minimal effect on shop-lifting. The incidence of other property crime, such as burglary, robbery, vandalism, and vehicle theft does increase during summer vacation.

significant. On the other hand, school seems to increase the prevalence of disorderly conduct among juveniles. On school breaks and teacher in-service days, disorderly conduct offenses decrease by 48 and 37 percent respectively.

5.3 Robustness Checks: Testing for Temporal Displacement, Reporting Bias, and Specification Concerns

While the results above provide strong evidence that school increases the level of violent crime and decreases the level of property crime among juveniles, it is important to examine whether days off from school are correlated with other factors that affect the incidence of crime. In our main specification, we attempt to address this concern by relying on a plausibly exogenous source of non-school time (teacher in-service days), including city*year*month fixed effects and controlling for adult crime.

Another way to address this concern is to examine the degree to which *adult* crime is affected by days off from school. Since school should not influence adult crime, we would not expect to find any effects. Table 9 presents the results of this falsification exercise. Here we estimate the effect of school on the number of *adult* offenders, controlling for the number of juvenile offenders and the same fixed effects and controls found in the previous specifications. We find that in-service days and summer vacation do not predict the incidence of any type of adult criminal behavior. We find, however, that national holidays and school breaks are associated with adult crime.[17] These findings reassure us that variation in teacher in-service days and possibly summer vacation is likely to affect juvenile crime only through its effect on school

[17] Interestingly, the holidays and breaks appear to have a very different effect on adult criminal behavior as on juvenile behavior. The increase in violent adult crime associated with these days may be driven by the increased use of alcohol.

attendance. Furthermore, one should be cautious in interpreting the findings using variation in breaks and holidays.

Even if our results are driven by exogenous variation in school attendance, two factors—temporal displacement and reporting bias—may confound our estimates. In particular, it is possible that school is not changing the *level* of criminal activity in aggregate, but merely shifting the time of occurrence. In this case, our estimates would not truly be capturing a change in the level of crime. For example, a juvenile who is planning to steal something from the neighborhood store may find it more convenient to do so on a day that he has off of school. Similarly, the youth planning to take revenge on a rival may choose to do so on a school day because he knows the other youth will be nearby or because he wants others to witness the attack. In both cases, we would observe changes in the level of juvenile crime associated with schooling that would not represent aggregate changes in the levels of crime.

To examine whether school is merely displacing juvenile crime, one can examine the level of juvenile crime on the days just before and just after school breaks, holidays, and teacher in-service days. If juveniles simply saved all of their grudges, vendettas and other violent acts for days when school was in session, one might expect there to be unusually high levels of crime on the days just before or after school holidays or other non-school days to compensate for the unusually low levels of violent crime on non-school days. Similarly, if juveniles shifted property crimes to non-school days, one would imagine the level of property crimes to be somewhat lower than usual on days surrounding days when school was not in session.[18]

[18] An alternate way to address the temporal displacement issue is to examine the effect of the total *number* of school days in a year on the crime during that year. However, it is quite difficult to find sufficient exogenous variation in the length of the school year to provide precise estimation with this strategy. Almost all school districts have school in session for roughly 180 days per year. Moreover, the factors that influence the length of the school year in practice such as adverse weather conditions (e.g., snow days) may have an independent effect on the incidence of crime. Teacher strikes provide another potential source of useful variation, but are so rare (and limited to a select subset of districts) that they cannot be used to generate precise estimates.

Table 10 provides some evidence on such temporal displacement. For this and the remaining tables we focus on our preferred source of variation, teacher in-service days. In addition to the variables included in earlier models, the specifications here include binary indicator variables for the three days immediately preceding and the three days immediately following all non-school days. In column 1, we see that while juvenile violent crime is 30 percent lower on teacher in-service days, there is no significant difference in the level of juvenile violence on the three days immediately before or after in-service days, as compared with the rest of the school term. This suggests that school does indeed increase the level of violent crime. In column 2, we see that there is no statistically significant difference between the level of property crime on the days following teacher in-service days, but there is roughly 9 percent more reported juvenile property crime on the day before teacher in-service days, just the opposite of what one would expect in the presence of temporal displacement. This appears to be because juveniles are more likely to be out late at night the day before a teacher in-service day, knowing that they will not have to get up for school the next day. In fact, this effect is driven entirely by crimes occurring in late night (after 9 p.m.) of the previous day.

Even if temporal displacement is not a concern, our estimates may still suffer from reporting bias. In particular, juvenile crime may be more likely to be reported on school days because of greater supervision on the part of school personnel or greater vigilance by law enforcement. This problem is likely to be most severe with regard to less serious violent crimes such as simple assault. For example, one could imagine that a relatively minor fight between adolescents would not come to the attention of authorities on non-school days, but on school days, a teacher may see the juveniles fighting and intervene. Alternatively, petty property crimes

committed at school may never be reported to police but rather handled by school personnel. This would be consistent with more property crime being reported on non-school days.

One way to examine the possibility of reporting bias is to focus on serious crimes that one believes are likely to be reported to authorities regardless of the time or location of occurrence. In Table 8, we saw that the effect of school on the prevalence of simple and aggravated assault was similar. Aggravated assault involves an attack wherein the offender uses or displays a weapon, or the victim suffers obvious, severe or aggravated bodily injury involving broken bones, loss of teeth, possible internal injuries, etc. While it is conceivable that the reporting of simple assaults is influenced by the presence of school personnel, it seems likely that aggravated assaults would be reported regardless of the scenario. This suggests that reporting bias is not driving the violent crime results.[19] Similarly, the results for shoplifting, burglary, and motor vehicle theft are all consistent with a negative effect of school on property crime. This suggests that reporting bias is also not driving the property crime results.

Another way to address the issue of reporting bias is to examine the timing of juvenile crime *within* the school day. Insofar as the majority of juvenile violent crime on school days takes place after school hours (e.g., from 3 to 6 pm), the estimates above are less likely to be driven by reporting bias. Table 11 shows the relationship between school and juvenile crime separately by the time of day. The outcome variable in the second row is the number of reported juvenile offenders committing crimes from 7 am to 3 pm – i.e., during school hours. In the third row, the outcome is the number of offenders committing crimes during non-school hours. We see that the results are consistent across rows. For example, teacher in-service days are associated with 13 percent more property crime during school hours and 16 percent more

[19] As mentioned earlier, it would be even more convincing to examine extremely serious crimes such as rape and murder, but these crimes are too rare to provide useful estimates.

property crime during non-school hours. Looking at violent crime, we see that teacher in-service days are associated with substantially lower violent crime levels, both during school and non-school hours. The fact that the pattern remains significant and strong in magnitude when one considers non-school hours suggests that reporting bias is not playing a large role in the effect.

Table 11 also presents results from several alternative specifications. To the extent that victims or others who report an offense cannot accurately tell the age of the offender, the number of juvenile offenders may be measured with some error. If this measurement error is uncorrelated with the school calendar, our estimates will not be biased. However, one might believe that this measurement error is correlated with school. For example, on days that it is generally known that school is off, such as Spring break, someone who witnesses an act of vandalism from the distance may be more likely to report that the offender was a "kid" because he or she knows that children are not in school. For this reason, the specification in row 4 uses the number of juvenile arrests as the outcome variable. In these cases, we can be certain that the age of the individual offender is correct. The estimates from this model are virtual identical to the baseline model, suggesting that this type of measurement error in reporting is not biasing our results.

Another potential concern is that school may increase the number of juveniles involved in any given offense without actually increasing the number of offenses. This might be true if would be offenders recruit accomplices in school. In row 5, we show results in which we focus on the number of offenses in which at least one juvenile is involved. These results are virtually identical to the baseline estimates suggesting this concern is unwarranted. On the same note, if we find that school has a different effect on the incidence of crimes involving single and multiple juvenile offenders, we may be concerned that our theory is incomplete. In particular, school may

facilitate the coordination of crime among juveniles. In this case, school should have a different effect on crimes committed by single and multiple offenders. In rows 6 and 7, we see that this is not the case.

In our baseline specification, days are measured in a typical fashion—from midnight to midnight. However, in considering the timing of daily activity, it may make more sense to think of crimes committed in the early morning hours (say 3 am) of Day 2 as having occurred during Day 1. Consider, for example, a group of adolescents committing a series of vandalism over an evening. Any acts they commit at 10 pm would count toward the total level of crime in Day 1, but acts they commit three hours later at 1 am, during the same crime "spree," would count toward the level of crime in Day 2. The specification in row 8 shows that reclassifying early morning crimes to the previous day does not change our results.

Finally, in row 9, we show results that use the number of crimes occurring outside of school on a particular day as the outcome variable (as opposed to all crimes occurring on that day). This is yet another way of examining the possibility of reporting bias (i.e., the notion that crimes are more likely to be reported on school days because they will be seen by school officials). The point estimates for property crime are nearly identical to the baseline results and those for violent crime are similar in direction, though somewhat smaller in magnitude and insignificant. Overall, the general story seems to remain the same.

5.4 Who Wins and Loses as a Result of the Schooling Effects on Juvenile Crime

Having shown that school increases violent crime among juveniles but decreases property crime among juveniles, it is interesting to ask who are the particular winners and losers as a result of these effects. To do so, Table 12 shows estimates of the relationship between the school

term and the number of reported victims of juvenile crime, distinguishing between victims who are juveniles, adults and institutions. First consider property crime. In column 1, we see that juveniles are no more or less likely to be victimized by other juveniles on teacher in-service days. Not surprisingly, the true beneficiaries of schooling in terms of juvenile property crime are adults and institutions (e.g., shops, stores, restaurants, etc.). The number of adults who report being victimized by a juvenile for a property offense increases 13 percent on teacher in-service days; the comparable figure for institutions is 8 percent.

Next consider violent crimes committed by juveniles. Juveniles are the overwhelming beneficiaries from non-school days, which is not surprising since the victims of juvenile crime are most often juveniles themselves. The number of juveniles who report violent victimization by other juveniles decreases by over 20 percent on teacher in-service days. However, adults also benefit from lower violent victimization levels on in-service days. The number of adults reporting violent victimizations by juveniles declines by roughly 12 percent on teacher in-service days. The effect of school on the incidence of juvenile violence toward adults is somewhat puzzling in light of our theory. Previous evidence in the paper suggests that this not because schools facilitate the coordination of violent acts. It may be that school forces the interaction between students and adults they would prefer to avoid (e.g. teacher and administrators). Given that the coefficient is only marginally significant, however, it may not be wise to read too much into the finding.

5.5 The Heterogeneity of Schooling Effects Across Juvenile Offenders

Table 13 examines whether the effects of school vary across juvenile offenders. Overall, the sign of the coefficients is invariant to race, age, gender, and residence. Additionally, there

appear to be only minor differences in the magnitude of effects across subgroups. At the risk of over-interpreting the data, the results suggest that the behavior of older offenders is less sensitive to school being in session. This could be the case if high-risk juveniles are more likely to drop out of school—limiting the impact of school schedules. On the same note, if high-risk juveniles are more likely to dropout in larger cities, we might also expect juvenile crime in these cities to be less sensitive to school schedules than in other cities.

6. Conclusions

In summary, we find that school appears to reduce the incidence of juvenile property crime by about 15 percent, but increases the level of juvenile violent crime by nearly 20 percent. Our evidence suggests that these effects are not driven by reporting bias and do not simply reflect temporal displacement. These findings are consistent with a theoretical framework in which school provides monitoring, structure, and activities that reduce property crime while at the same time increasing the level interaction among adolescents, thereby raising the likelihood of violent conflicts.

One important limitation of our study is that it only examines the effect of a full day of school in comparison to no school whereas after-school programs will generate smaller changes in the duration of the school day. Our theoretical model suggests that the impact of marginal changes may be significantly different from the effects that we estimate. Unfortunately, in practice it is quite difficult to estimate the effect of school duration on juvenile crime.[20]

[20] This is difficult for two reasons. First, daily school schedules often vary within a city while school calendars are generally consistent across all schools in the same city. Existing crime data do not allow us to match juvenile offenders to the school they attend. Second, there is substantial day-to-day variation in whether school is in session but little in school ending times. This means that one would probably have to rely on cross-sectional or low frequency variation to identify the effect of school timing on crime.

Our estimates suggest that lengthening the school year by one day will lead to a decrease of 0.42 property crimes and an increase of 0.26 violent crimes in a city with a population of 130,000.[21] Given the average reported value of stolen or damaged property in our sample of $1,211, the reduction of property crime would result in a savings of approximately $509. It is more difficult to denominate the cost of violent crime in monetary terms. Miller, Fisher, and Cohen (2001) present evidence suggesting that the total cost of an assault committed by a juvenile is $8,515. This figure includes the direct costs of the offense (e.g. medical care and foregone wages) as well intangible costs such as pain, suffering, and fear. If we use this measure as the cost of each violent offense, the violence associated with having school in session another day costs about $2,214. Additionally, there are costs associated with processing these incidents through the criminal justice system.[22]

While this analysis is likely to be of minimal importance for determining the length of the school year or the provision of summer school,[23] we believe that this analysis provides some valuable insight into the nature and motivation of juvenile crime. Both incapacitation and social interaction models appear to be important determinants of juvenile crime. In particular, it confirms that when juveniles are not provided with constructive activities, they are likely to engage in anti-social behavior that manifests itself in increased property crime. In addition, it suggests that the degree of interaction among youth plays a significant role in the level of

[21] This is calculated by the authors using estimates of the percent effect of school on property and violent crimes that are committed along with the average daily number of such offenses in our sample.
[22] It is unclear how one would measure the negative effect of crime on the offender's taste for future criminal activity and other intangible effects on the perpetrators of crime.
[23] Crime considerations are likely to be second order relative to the cost and benefits of providing schooling. In particular, according to the 2000 Census, about 20 percent of the U.S. population is between the ages of 5 and 19. This means that reducing school by one day in the average city in our sample would reduce human capital acquisition by 26,000 days or 144 school years. At any reasonable rate of return, the effects of school on human capital acquisition are orders of magnitude more important than the effects of school on crime. Though smaller than the human capital effect, the marginal cost of employing teachers another day is also substantially more important than the crime effects of school.

juvenile violence. The increase in violent crime induced by school attendance reflects the potentially volatile nature of juvenile interactions.

Furthermore, our findings may have significant ramifications for other youth activities that do not have an explicit educational component, such as midnight basketball or other programs designed primarily to keep youth busy and "off the streets." The increased violence generated by bringing together youth may more than offset any societal gains associated with reduced property crime.[24] In general, our findings suggest that summer youth employment programs or smaller, neighborhood based after-school programs, that provide structured activities for adolescents but do not substantially increase their concentration, may be the best way to reduce juvenile crime.

To our knowledge, this is the first study that looks at the effect of school on the level as well as the timing of juvenile crime. By highlighting the role of social interactions as well as incapacitation, we believe that the findings presented above shed light on the nature of juvenile crime and provide some guidance to those developing prevention programs aimed at youth. Finally, the evidence provided in this paper underscores the importance of social interactions in analyzing individual outcomes.

[24] To the extent that these programs involve a select group of youth or include components that serve to mitigate negative interactions or foster interpersonal relationship among youth, it is possible that they may not have the same negative effects on violent crime as school.

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Table 1: Summary Statistics for Jurisdictions in the Sample

Variable	Analysis Sample	The Nation
Racial, Age and SES Composition		
Fraction White	0.724	0.751
Fraction Back	0.154	0.123
Fraction Hispanic	0.119	0.125
Fraction Asian	0.028	0.036
Fraction single-parent households	0.119	0.127
Fraction of the population 10-19	0.146	0.145
Crime Rate		
Reported incidents per 1,000 people	80.2	46.7
Region		
Northeast	0.078	0.190
South	0.242	0.356
Midwest	0.303	0.229
West	0.377	0.225
City Population Size		
Average Population Size	160,578	--
Population is < 50,000	0.037	--
Population is 50,000 – 150,000	0.519	--
Population is 150,000 – 300,000	0.333	--
Population is 300,000 – 500,000	0.074	--
Population is > 500,000	0.037	--
Number of jurisdictions	27	--

Notes: The analysis sample contains summary statistics for all of the cities included in our sample. Population figures weight each city equally while all other statistics are population weighted. All information comes from the 2000 Census with the exception of crime rates, which are calculated using data from the 1999 Uniform Crime Reports, which is compiled by the FBI.

Table 2: Summary Statistics on Crime and School Sessions in the Analysis Sample

Variables	Mean (sd)	25th Percentile	75th Percentile
Number of Juvenile Offenders			
All Serious Offenses	5.593 (5.79)	2	8
Violent Offenses	1.372 (1.97)	0	2
Property Offenses	2.745 (3.31)	0	4
Other Serious Offenses	1.476 (2.65)	0	2
Minor Offenses	2.319 (3.92)	0	3
Number of Adult Offenders			
All Serious Offenses	17.950 (19.13)	6	25
Violent Offenses	6.148 (7.17)	1	9
Property Offenses	7.055 (7.45)	2	10
Other Serious Offenses	4.747 (7.18)	1	6
Minor Offenses	9.254 (12.80)	2	11
School Session			
Summer Vacation	0.229 (0.420)	--	--
National Holidays	0.022 (0.146)	--	--
School Breaks	0.038 (0.192)	--	--
Teacher in-service Days	0.013 (0.111)	--	--
Observations	33,560		

Notes: This table contains summary statistics for all of the days in our analysis sample. The unit of observation is city*day.

Table 3: Who are the offenders and victims in juvenile crime?

	Violent Crime		Property Crime	Other major crimes
Variable	Offenders	Victims	Offenders	Offenders
Age	14.424 (2.31)	19.792 (12.85)	14.712 (2.03)	14.751 (2.34)
Under age 12	0.109 (0.31)	0.199 (0.40)	0.071 (0.26)	0.096 (0.29)
Age 12-14	0.330 (0.47)	0.213 (0.41)	0.323 (0.47)	0.256 (0.44)
Age 15-17	0.561 (0.50)	0.212 (0.41)	0.606 (0.49)	0.648 (0.48)
Age 18-24	--	0.123 (0.33)	--	--
Over age 24	--	0.253 (0.43)	--	--
White	0.577 (0.49)	0.651 (0.48)	0.697 (0.46)	0.752 (0.43)
Black	0.408 (0.49)	0.323 (0.47)	0.280 (0.45)	0.235 (0.42)
Other	0.014 (0.12)	0.026 (0.16)	0.024 (0.15)	0.015 (0.12)
Male	0.720 (0.45)	0.405 (0.49)	0.729 (0.44)	0.840 (0.37)
Female	0.280 (0.45)	0.595 (0.49)	0.271 (0.44)	0.160 (0.37)
Observations	46,046	43,585	92,112	49,536

Notes: This table shows the average characteristics of juvenile and adult offenders in our sample. The unit of observation is the juvenile offender. Standard deviations are in parentheses.

Table 4: What types of crime do juveniles commit most often?

Serious Offenses	**Frequency**
Shoplifting	0.198
Simple Assault	0.152
Vandalism	0.134
All other larceny	0.072
Drug Violations	0.070
Burglary	0.062
Aggravated assault	0.049
Motor vehicle theft	0.037
Theft from motor vehicle	0.035
Drug equipment violations	0.032
Theft from building	0.025
Intimidation	0.023
Weapons law violation	0.022
Robbery	0.022
Stolen property offense	0.013
Other offenses	0.054
Total	1.000
All Offenses	
Shoplifting	0.140
Simple assault	0.107
Vandalism	0.095
All other type B offenses	0.090
Runaway	0.070
All other larceny	0.051
Drug violation	0.049
Curfew/loitering/vagrancy violation	0.047
Burglary	0.044
Aggravated assault	0.035
Disorderly conduct	0.032
Liquor law violations	0.027
Motor vehicle theft	0.026
Theft from a motor vehicle	0.024
Drug equipment violations	0.022
Trespass of real property	0.021
Other offenses	0.120
Total	1.000

Notes: This table shows the relative frequency of each type of offense for juvenile offenders in our analysis sample. These results are computed for crimes for which the age of the offender is known. Serious offenses are defined as NIBRS category A offenses.

Table 5: Where Does Juvenile Crime Occur?

	Theft		Vandalism		Assault		Drug Use	
Venue	School Days	Other Days	School Days	Other Days	School Days	Other Days	School Days	Other Days
School	0.071	0.010	0.111	0.036	0.248	0.029	0.240	0.021
Store/Restaurant	0.546	0.566	0.046	0.047	0.031	0.049	0.036	0.057
Other Public Location	0.161	0.181	0.352	0.364	0.333	0.372	0.456	0.621
Residence/Home	0.172	0.191	0.407	0.487	0.264	0.451	0.189	0.236
Other/Unknown	0.050	0.051	0.083	0.066	0.124	0.100	0.078	0.065

Notes: This table shows the fraction of crimes committed in each type of venue.

Table 6: The Mean Number of Juvenile Offenders per 100,000 Residents on School and Non-School Weekdays

	Juvenile Offenders per 100,000 Residents				
	Serious Offenses				Minor Offenses
Independent Variables	All crimes	Property crimes	Violent crimes	Other crimes	All crimes
School Days	5.599 (0.641)	2.672 (0.326)	1.468 (0.184)	1.459 (0.199)	2.196 (0.400)
Non-School Days	5.229 (0.614)	2.760 (0.340)	1.028 (0.120)	1.441 (0.193)	1.756 (0.305)
Summer Vacation	5.400 (0.661)	2.836 (0.361)	1.066 (0.131)	1.498 (0.208)	1.891 (0.328)
National Holidays	4.000 (0.480)	2.063 (0.236)	0.824 (0.099)	1.113 (0.190)	1.178 (0.199)
School Breaks	5.027 (0.623)	2.777 (0.363)	0.866 (0.111)	1.384 (0.227)	1.498 (0.247)
Teacher in-service days	6.050 (0.561)	3.335 (0.362)	1.154 (0.128)	1.560 (0.192)	2.138 (0.431)

Notes: This table shows the mean number of juvenile offenders per 100,000 residents within a city. The means are computed using the daily incidence of crime divided by the population for all of the city-days in our sample, excluding Saturdays and Sundays. The standard error of the mean is in parentheses. The standard errors are cluster-corrected to account for the fact that observations within a city are not independent.

Table 7: The Relationship between Reported Juvenile Offenders and the School Term

	Dependent Variable = Number of reported juvenile offenders for the following offenses				
	Serious Offenses				Minor Offenses
Independent Variables	All crimes	Property crimes	Violent crimes	Other crimes	All crimes
Summer Vacation	0.962 (1.86)	1.035 (1.30)	0.797 (6.70)	1.025 (0.78)	0.864 (4.95)
National Holidays	0.745 (10.10)	0.851 (4.25)	0.547 (11.75)	0.849 (3.19)	0.638 (9.85)
School Breaks	0.949 (2.44)	1.104 (3.64)	0.663 (10.64)	0.849 (3.19)	0.786 (7.27)
Teacher in-service days	1.032 (1.01)	1.152 (3.50)	0.811 (3.89)	1.043 (0.74)	0.924 (1.69)
City*month*year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	32,191	32,099	32,192	32,130	32,975

Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics are included in parenthesis. The regressions also include controls for day of the week and the number of adult offenders for the particular offense on that day. Serious offenses are defined as NIBRS type A offenses and include primarily property, violent, and drug crimes. Minor offenses are defined as NIBRS type B offenses and include offenses such as trespassing, loitering, disorderly conduct, and liquor law violations. National Holidays include days such as Thanksgiving, Christmas, New Year's Day, Easter, President's Day and Memorial Day. School Breaks are defined as the days off of school surrounding national holidays (e.g., the days during Spring Break *other than* Easter). Teacher in-service days are days during the regular school year that students do not attend school and districts reserve for teachers to attend professional development workshops or school-based planning activities.

Table 8: The Relationship between Reported Juvenile Offenders and the School Term

	Dependent Variable = Number of reported juveniles offenders for the following offenses									
	Violent Crimes		**Property Crimes**					**Other Serious**	**Other Minor**	
Independent Variables	Simple Assault	Aggravated Assault	Burglary	Vehicle Theft	Shop-lifting	Vandalism	Robbery	Drug Violations	Disorderly conduct	Curfew or loitering violation
Summer Vacation	0.726 (7.73)	.964 (0.59)	1.232 (4.03)	1.113 (1.59)	1.026 (0.66)	1.140 (3.37)	1.169 (1.81)	0.869 (3.08)	0.718 (4.69)	1.097 (1.53)
National Holidays	0.435 (12.53)	0.729 (3.05)	0.888 (1.23)	1.024 (0.21)	0.770 (4.60)	0.955 (0.69)	0.860 (0.99)	0.758 (3.44)	0.520 (5.58)	1.068 (0.51)
School Breaks	0.596 (10.67)	0.869 (1.84)	0.975 (0.33)	1.170 (1.83)	1.274 (6.82)	1.081 (1.52)	0.800 (1.83)	0.761 (4.37)	0.518 (7.10)	1.129 (1.23)
Teacher in-service days	0.775 (3.96)	0.851 (1.33)	1.241 (2.12)	1.195 (1.45)	1.269 (4.40)	1.174 (2.17)	1.270 (1.55)	0.935 (0.79)	0.633 (3.22)	1.212 (1.33)
City*month*year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32,037	28,731	30,855	28,342	31,701	30,790	20,459	30,551	27,300	16,199

Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics are included in parenthesis. The regressions also include controls for day of the week and the number of adults arrested for the particular offense on that day. National Holidays include days such as Thanksgiving, Christmas, New Year's Day, Easter, President's Day and Memorial Day. School Breaks are defined as the days off of school surrounding national holidays (e.g., the days during Spring Break *other than* Easter). Teacher in-service days are days during the regular school year that students do not attend school and districts reserve for teachers to attend professional development workshops or school-based planning activities.

Table 9: The Relationship between Reported Adult Offenders and the School Term

	Dependent Variable = Number of reported adult offenders for the following offenses				
	Serious Offenses				Minor Offenses
Independent Variables	All crimes	Property crimes	Violent crimes	Other crimes	All crimes
Summer Vacation	1.000 (0.00)	0.993 (0.39)	0.999 (0.04)	1.019 (0.80)	1.022 (1.24)
National Holidays	1.032 (2.05)	0.865 (6.48)	1.322 (14.65)	0.977 (0.77)	0.931 (3.21)
School Breaks	0.999 (0.07)	0.939 (3.70)	1.066 (3.83)	1.015 (0.64)	0.984 (0.98)
Teacher in-service days	1.001 (0.06)	0.980 (0.76)	1.028 (1.05)	1.017 (0.46)	0.987 (0.50)
City*month*year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	32,372	32,219	32,282	32,161	33,286

Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics are included in parenthesis. The regressions also include controls for day of the week and the number of juvenile offenders for the particular offense on that day. Serious offenses are defined as NIBRS type A offenses and include primarily property, violent, and drug crimes. Minor offenses are defined as NIBRS type B offenses and include offenses such as trespassing, loitering, disorderly conduct, and liquor law violations. National Holidays include days such as Thanksgiving, Christmas, New Year's Day, Easter, President's Day and Memorial Day. School Breaks are defined as the days off of school surrounding national holidays (e.g., the days during Spring Break *other than* Easter). Teacher in-service days are days during the regular school year that students do not attend school and districts reserve for teachers to attend professional development workshops or school-based planning activities.

Table 10: The Temporal Displacement of Juvenile Crime Across the School Term

	Dependent Variable = Number of Reported Juvenile Offenders	
Independent Variables	Violent crime	Property crime
Teacher In-Service Days	0.799**	1.148**
1 day preceding in-service	1.018	1.085*
2 days preceding in-service	1.009	1.040
3 days preceding in-service	0.955	0.994
1 day following in-service	0.916	0.958
2 days following in-service	0.930	0.960
3 days following in-service	0.929	0.943
City*month*year fixed effects	Yes	Yes
Observations	32,099	32,129

Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. The regressions also include controls for day of the week and the number of adults arrested for the particular offense on that day, as well as indicator variables for national holidays, summer vacation days, school breaks, and the three days preceding and following school breaks. * indicates significance at the 10 percent level. ** indicates significance at the 5 percent level.

Table 11: Other Robustness Checks

	Dependent Variable	
Specification (Cells contain coefficient estimates of the in-service variable)	**Juvenile Property Crimes**	**Juvenile Violent Crimes**
Baseline	1.152 (3.50)	0.811 (3.89)
Considering only crimes committed during school hours (7 a.m. to 3 p.m.)	1.130 (1.85)	0.760 (3.32)
Considering only crimes committed during non-school hours.	1.163 (3.19)	0.853 (2.38)
Using the number of juvenile arrests as the dependent variable	1.161 (2.98)	0.804 (2.86)
Using the number of incidents involving at least one juvenile as the dependent variable	1.147 (3.99)	0.822 (4.08)
Using the number of incidents involving *only one* juvenile as the dependent variable	1.118 (2.65)	0.829 (3.45)
Using the number of incidents involving *two or more* juveniles as the dependent variable.	1.200 (3.28)	0.803 (2.26)
Assigning early morning crimes to the previous day	1.147 (3.39)	0.805 (4.07)
Only considering crimes that do NOT take place inside school	1.186 (4.17)	.938 (1.12)

Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics are included in parenthesis. The regressions also include number of adult offenders (or arrestees), controls for day of the week, and fixed effects for city*year*month.

Table 12: The Relationship between Reported Juvenile, and Adult Victims of Juvenile Crime and the School Term by Crime Type.

	Dependent Variable = Number of Reported Victims of Juvenile Crime				
	Juvenile Victims		Adult Victims		Institutional Victims
Independent Variable	Property	Violent	Property	Violent	
Teacher in-service days	0.966 (0.28)	0.779 (3.77)	1.132 (1.99)	0.876 (1.67)	1.082 (1.99)
City*month*year fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	29,353	31,979	31,947	31,702	32,099

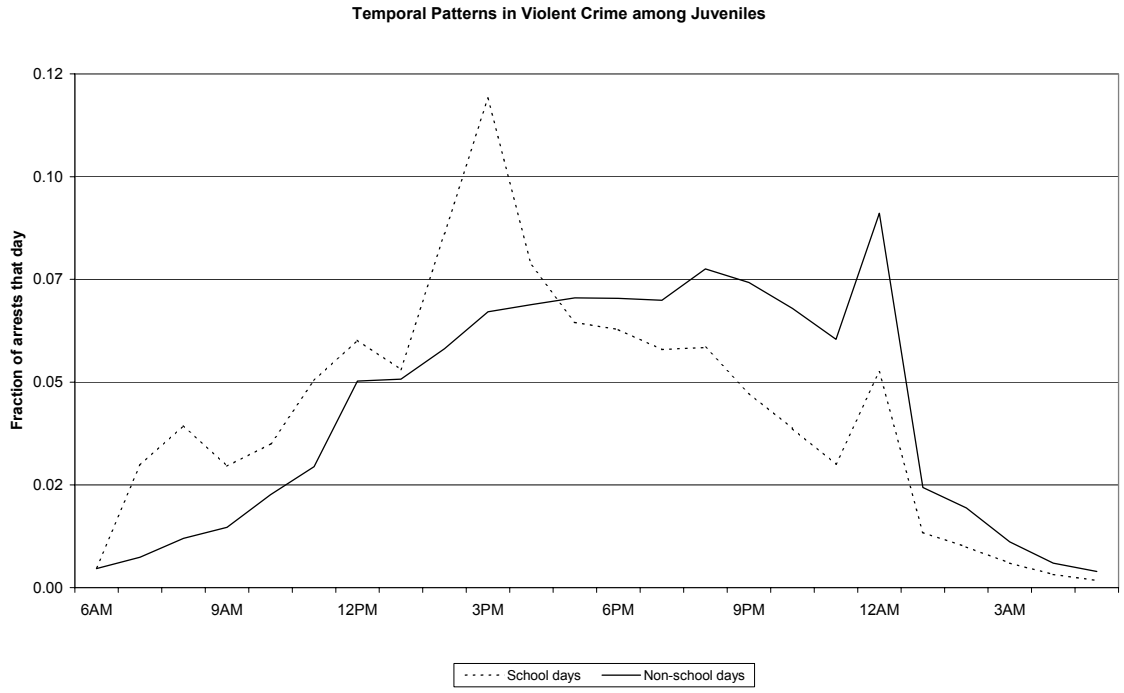
Notes: The estimates come from negative binomial regressions. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics included in parenthesis. The regressions also include controls for day of the week, summer vacation, national holidays, and school breaks. Institutional victims include businesses, government entities, etc.

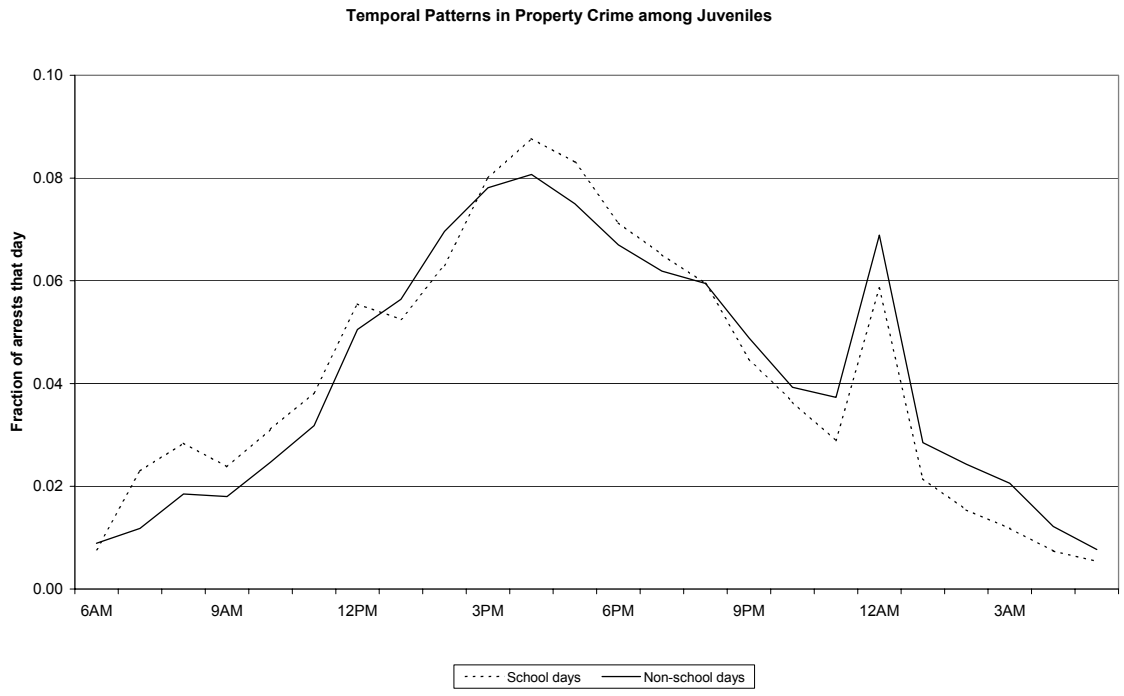
Table 13: The relationship between Juvenile Crime and In-Service Days by Offender Characteristic

	Dependent Variable	
Sample	Juvenile Property Crimes	Juvenile Violent Crimes
Baseline – All Offenders	1.152 (3.50)	0.811 (3.89)
Race of the Offender		
White offenders	1.174 (3.44)	0.818 (3.03)
Black offenders	1.102 (1.40)	0.823 (2.39)
Age of the Offender		
Offenders age 15 - 17	1.097 (1.94)	0.876 (1.97)
Offenders age 12 - 14	1.243 (3.78)	0.766 (3.52)
Gender of the Offender		
Male offenders	1.120 (2.46)	0.781 (4.04)
Female offenders	1.203 (2.81)	0.864 (1.61)
Residence of the Offender		
Population < 120,000	1.189 (2.89)	0.697 (4.03)
Population > 120,000	1.107 (1.88)	0.905 (1.53)

Notes: The estimates come from negative binomial regressions. The coefficients shown reflect the effect of in-service days on violent and property crime. The cells contain exponentiated coefficients, which can be interpreted as incidence rate ratios. Z-statistics are shown in parenthesis. The regressions also include summer vacation, holidays, breaks, number of adult offenders (or arrestees), controls for day of the week, and fixed effects for city*year*month.

Figure 1: The Timing of Juvenile Crime on School versus Non-School Days





Appendix A: Cities included in the analysis

City	Population	Time in Sample
Akron, OH	216,620	Jan 1999-Dec 1999
Aurora, CO	222,460	Jan 1997-Dec 1999
Austin, TX	560,389	Jan 1998-Dec 1999
Boise, ID	160,702	Jan 1999-Dec 1999
Cedar Rapids, IA	114842	Jan 1999-Dec 1999
Cincinnati, OH	337,815	Jan 1998-Dec 1999
Colorado Springs, CO	352,386	Jan 1997-Dec 1999
Columbia, SC	112,539	Jan 1995-Dec 1999
Davenport, IA	97,078	Jan 1995-Dec 1999
Dayton, OH	168,180	Jan 1998-Dec 1999
Des Moines, IA	191,345	Jan 1995-Dec 1999
Greenville, SC	57,168	Jan 1995-Dec 1999
Idaho Falls, ID	49,023	Jan 1995-Dec 1999
Lakewood, CO	139,819	Jan 1997-Dec 1999
Layton, UT	55,901	Sep 1995-Dec 1999
Logan, UT	40,849	Jan 1995-Nov 1999
Nampa, ID	42,737	Jan 1995-Dec 1999
Newport News, VA	180,760	Jul 1998-Dec 1999
Provo, UT	112,001	Jan 1995-Dec 1999
Richland, SC	190,585	Aug 1998-Dec 1999
Sandy, UT	100,607	Jan 1995-Dec 1999
Springfield, MA	148,820	Jun 1996-Dec 1999
Taylor, MI	72,895	Jan 1996-Dec 1999
Waterloo, IA	63,858	Aug 1996-Dec 1999
West Valley, UT	100,795	Aug 1998-Dec 1999
Westminster, CO	55,617	Jan 1997-Dec 1999
Worcester, MA	167,295	Jan 1995-Dec 1999

Note: Inclusion in the sample was based on the availability of crime and school calendar data. Among the cities listed, we excluded a small number of months in which it appeared that crimes were not reported systematically. A list of these months is available from the authors.