

CRIMINALIZING DELINQUENCY: THE DETERRENT EFFECTS OF THE NEW YORK JUVENILE OFFENDER LAW

SIMON I. SINGER
DAVID McDOWALL

New York's Juvenile Offender (JO) Law of 1978 is a significant step away from separate systems of justice for adults and juveniles. The law requires that juveniles accused of violent offenses be tried in criminal court, and it provides penalties comparable to those for adults. This paper evaluates the impact of the JO Law on violent juvenile crime rates in New York City and in upstate New York. Analyzing arrest data through the use of an interrupted time series model, we conclude that the JO Law has not been effective in reducing juvenile crime.

I. INTRODUCTION

During the past decade, growing concern about crime has resulted in a shift away from separate systems of justice for adults and juveniles. The treatment-oriented philosophy of the juvenile court has come under attack, and there has been a new emphasis on formal and punitive policies toward young people accused of serious crimes. This paper evaluates the effects of one such reform, New York State's Juvenile Offender (JO) Law of 1978 (1978 N.Y. Laws § 481).

The provisions of the JO Law are a significant step away from a separate system of juvenile justice. Under the law, violent offenders as young as thirteen are considered to be legally adults and can be tried directly in criminal court. Although virtually all states and the District of Columbia allow some juveniles to be tried as adults (Feld, 1987), New York's law is unique in the wide range of offenses it excludes from juvenile court jurisdiction. The JO Law also provides penalties comparable to those for adults, and

Authorship of this paper was equally shared. We thank Mohsen Bazargan for his research assistance and Robert Kidder, Frank Munger, and anonymous reviewers for helpful comments. This research was supported by Grant 85-IJ-CX-0026 from the National Institute of Justice, U.S. Department of Justice. Points of view or opinions stated are those of the authors and do not necessarily represent the official position or policies of the U.S. Department of Justice.

LAW & SOCIETY REVIEW, Volume 22, Number 3 (1988)

it specifies that all sentences be served in secure facilities. Overall, the JO Law has been characterized as the most punitive delinquency law in the nation (Smith *et al.*, 1980; also see Ross, 1980; Sobie, 1981; Woods, 1980).

New York's move away from separate legal systems for adults and juveniles represents an attempt to deter¹ crime by increasing the risks of criminal punishment. This paper examines whether the JO Law has in fact resulted in lower juvenile crime rates. The first two sections of the paper discuss the provisions of the JO Law and consider the degree to which it has been implemented in practice. The research design of the study is then described, and results are presented concerning the law's effect on crime patterns. We conclude that the JO Law has not reduced levels of crime. The final section of the paper considers some implications of this finding for juvenile justice policy.

II. BACKGROUND

Throughout the twentieth century, juveniles have generally been subject to a system of justice separate from that provided for adults. The procedures of the juvenile court have involved an informal process in which offenders are considered "delinquents" rather than criminals and are subject to a "hearing" rather than to a formal trial. In essence, the guiding philosophy of the juvenile court has been to create a system in which juveniles are *treated* rather than *punished*.

In recent years the philosophy of the juvenile court has come under attack. Although sources of dissatisfaction with the court are very diverse (Empey, 1982), a persistent criticism has been that the court's treatment-oriented dispositions are too lenient to deter crime. A number of studies have found that rehabilitative approaches to delinquency are ineffective (e.g., Lipton *et al.*, 1975), and that the bulk of serious delinquency is committed by repeat offenders (e.g., Wolfgang *et al.*, 1972). There is also some evidence that young people themselves perceive the juvenile court's sanctions to be relatively light (Glassner *et al.*, 1983; Ruhland *et al.*, 1982). Based on such results, critics of the juvenile court argue that crime rates could be decreased if the distinction between the adult and juvenile justice systems were reduced.

Feld (1981; 1987) distinguishes two approaches to narrowing the legal separation of juveniles and adults. The first approach is judicial waiver, which allows juvenile court judges to waive accused offenders to criminal court provided they meet statutory requirements. The second approach is legislative exclusion, which

¹ In principle, the JO Law could reduce crime by either deterring potential offenders or incapacitating repeat offenders. In fact, transcripts of the legislative debate on the law indicate that its supporters justified it primarily on grounds of deterrence. We thus use the word "deter" throughout the paper, but recognize that the law may also have incapacitative effects.

Table 1. Provisions of New York's Juvenile Offender Law

Acts Covered	Ages Affected	Terms of Confinement
Murder 2, excluding felony murder	13-15	Minimum: 5-9 years; maximum: life
Murder 2	14, 15	Minimum: 5-9 years; maximum: life
Kidnapping 1, Arson 1	14, 15	Minimum: 4-6 years; maximum: 12-15 years
Manslaughter 1, Rape 1, Sodomy 1, Burglary 1, Robbery 1, Arson 2, Attempted murder 2, Attempted kidnapping 1	14, 15	Minimum: 1/3 of maximum; maximum: 3-10 years
Assault 1, Robbery 2, Burglary 2	14, 15	Minimum: 1/3 of maximum; maximum: 3-7 years

entirely abolishes juvenile court jurisdiction over some offenses. This approach confers "automatic adulthood" on juveniles charged with these offenses, thus requiring them to be prosecuted in adult court.

New York's JO Law uses legislative exclusion to substantially eliminate juvenile court jurisdiction over a wide range of violent offenses. Before the law was enacted, the age of criminal responsibility in New York was sixteen for all crimes, and New York was alone among large states in lacking any sort of waiver mechanism. All persons younger than sixteen were tried in the state's Family Court, where they were subject only to the "rehabilitative" programs characteristic of juvenile justice systems. The JO Law lowered the age of criminal responsibility to thirteen for murder and to fourteen for assault, arson, burglary, kidnapping, and rape (see Table 1). The age reduction precludes Family Court jurisdiction, and thus automatically subjects accused juvenile offenders to adult prosecution.

Placing juveniles within the criminal court organization may itself act as a deterrent to crime, regardless of the court's dispositions. Lempert (1982: 535), for example, suggests that "it is possible for organization differences to be more important than sanction rate differences in determining how legal threats are perceived." That is, the threat of punishment may be effectively communicated through a criminal court trial, whether or not the criminal courts are more punitive in their sentences.

If convicted in criminal court, a juvenile offender is subject to penalties similar to those for adults and much more severe than those available in the Family Court. For example a juvenile con-

victed of Murder 2 under the JO Law faces a minimum sentence of five to nine years and a maximum of life. Prior to the law the same youth could have been committed to state custody for a maximum of five years. A complete list of the penalties provided by the JO Law is included in Table 1.

In addition to longer periods of incarceration, the JO Law also requires that all sentences be served in secure facilities. Although initial placements are in juvenile institutions, once a juvenile offender reaches age twenty-one he or she must be transferred to an adult prison to serve the remainder of the sentence. The provision requiring secure confinement is more stringent than that for adults where correctional personnel have discretion in assigning an appropriate level of security.

The JO Law provides that accused juvenile offenders may be waived to the Family Court, but the circumstances in which this is permitted are very narrow. The waiver process involves a complicated set of criteria that emphasize public protection, and it leaves the waiver decision with the criminal court judge.² Most other states allow serious offenders to be waived "upward" from the juvenile to criminal courts. In contrast, the JO Law requires a "downward" waiver, creating a presumption that accused juvenile offenders should be tried as adults.

Proponents of the JO Law argued that it would help deter violent juvenile crime (McGarrell, 1985; New York State Legislative Assembly, 1978).³ By removing serious offenders from the protective philosophy of the Family Court, the authors of the law sought to reduce crime rates. In the next sections of the paper we consider the extent to which the law was in fact successful in decreasing crime.

III. IMPLEMENTATION OF THE JO LAW

In evaluating the effects of the JO Law on crime, an important issue that must first be addressed is the extent to which the law has been implemented. A law that is neither widely used by law enforcement authorities nor known to potential offenders could not be expected to have an effect on crime patterns. A reasonable level of implementation must therefore be demonstrated before an impact assessment can be undertaken.

Substantial efforts have been made in the news media to alert juveniles to the existence of the JO Law and its provisions. Limited survey evidence indicates that juveniles in New York are in

² The circumstances in which waivers are permitted are quite complicated and will not be considered here. Singer and Ewing (1986) provide a detailed discussion.

³ McGarrell (1985) also attributes the law partly to a desire by incumbent legislators to appear to be tough on crime during an election year. Although part of the motivation for the law may thus have been symbolic, the symbolism involved was clearly that of deterrence.

fact generally aware of the criminal laws that apply to them and of the JO Law in particular (Bucci, 1985; Glasner *et al.*, 1983).

More direct evidence on implementation concerns the extent to which the JO Law has been applied to juvenile arrests. The available data indicate that the law has been widely used in New York City but relatively less applied in the rest of New York State.

In New York City, a conservative estimate⁴ is that about 31 percent of juveniles arrested for homicide, rape, robbery, assault, or arson⁵ were charged under the JO Law between September 1978 and December 1984. Among these five crimes there was considerable variation in implementation: 85 percent of homicide arrests were charged under the law, as were 56 percent of rapes, 38 percent of assaults, 33 percent of arsons, and 27 percent of robberies. Beyond the arrest stage, about 71 percent of those indicted were convicted, and about 52 percent of those convicted were sentenced to periods of confinement (Division of Criminal Justice Services, 1984). For both arrests and convictions, implementation increased rapidly for the first few months after the law went into effect and then maintained an approximately constant level.

These figures indicate that, although the JO Law has not been universally applied in New York City, the chances of an arrested juvenile being charged, convicted, and confined under the law are relatively great. Overall, about 14 percent of the eligible juveniles arrested in New York City through 1983 were sentenced to periods of confinement by the criminal court. In comparison, Boland and Wilson (1978) cite studies conducted in New York City before the JO Law was enacted that show sentences of confinement were imposed on only 3 to 9 percent of juveniles arrested for serious offenses. Their data also show that only *one-tenth of 1 percent* of New York City juveniles arrested for robbery in 1973 were sentenced to the *secure* confinement required by the JO Law. Given the discretion that attends the application of laws governing delinquency, the level of implementation of the JO Law in New York City thus appears quite high.

In the rest of New York State, the JO Law has not been as

⁴ To assess arrest implementation the number of JO arrests for homicide, rape, robbery, assault, and arson reported by New York State's Division of Criminal Justice Services is compared with the total number of juvenile arrests for these crimes reported by the FBI. Except for homicide, this comparison results in conservative estimates, in part because the FBI data include all rapes, robberies, arsons, and assaults, while only some of the acts in these categories are JO offenses. In addition, the JO Law applies only to 14- and 15-year-olds for these 4 crimes, but the FBI data includes arrests of 13-year-olds as well. All homicides in the FBI data may be charged as JO offenses, so the implementation figures for homicide are exact ones.

⁵ Although burglary and kidnapping are also included in the JO Law, these crimes are omitted from the analysis. Only a small fraction of total burglaries are covered by the law, and few juvenile burglars have in fact been charged. Similarly, there are too few kidnapping arrests to perform a meaningful analysis for this crime.

widely implemented. A conservative estimate is that between January 1979 and December 1983 only about 18 percent of upstate juveniles arrested for homicide, rape, robbery, arson, or assault were charged under the law. Implementation at the arrest stage was higher upstate than in New York City for homicide (90%) and rape (72%), but lower for assault (2%), arson (8%), and robbery (23%). About 59 percent of the juveniles indicted upstate were convicted, and about 71 percent of those convicted were sentenced to confinement (Division of Criminal Justice Services, 1984).

Based on these data, it appears that the JO Law has not been ignored in upstate New York. However, it has been invoked much less frequently there than in New York City, and overall an upstate juvenile has less than half the chance of being charged with the law as does a City juvenile. Our evaluation of the effect of the JO Law on crime therefore stresses its impact in New York City. We also separately analyze crime patterns in the rest of the state, but we place somewhat less weight on this analysis.

IV. RESEARCH DESIGN

To evaluate the impact of the JO Law on crime, we use an interrupted time series analysis. The basic research design compares the levels of a time series before and after an intervention is introduced. In this case the time series consist of monthly juvenile arrests between January 1974 and December 1984. The intervention is the introduction of the JO Law in September 1978.⁶ If the JO Law is effective in reducing crime, arrests should decrease following its introduction.⁷

The series that are analyzed are monthly arrest totals provided by individual police jurisdictions to the Uniform Crime Reporting Division of the FBI. We analyze arrest totals of persons between the ages of thirteen and fifteen for each of five crimes: homicide, rape, robbery, assault, and arson. For each crime, separate analyses are performed for New York City and for upstate New York. The upstate series are aggregated from jurisdictions that reported monthly arrests to the FBI continuously over the study period.⁸

⁶ Zimring (1984) also studied the impact of the JO Law on homicide arrests in New York City. The monthly series used here are much longer than the annual series available to him, and so permit more accurate estimates. However, Zimring's findings are in accord with ours.

⁷ The analysis reported here does not allow for any effects of the Juvenile Justice Reform Act (JJRA) of 1976 (1976 N.Y. Laws § 878). The JJRA was a relatively minor reform of New York delinquency laws, providing Family Court judges with the option of somewhat more punitive dispositions (see McGarrell, 1985). In preliminary analyses, models were estimated that included effects both for the JJRA and the JO Law and for the JJRA alone. None of these models suggested conclusions different from those presented here.

⁸ We defined continuously reporting jurisdictions as those that reported at least 11 months of arrest data each year. The upstate aggregate includes all

Each arrest series is analyzed using the methods of Box and Jenkins (1976; Box and Tiao, 1975; see also McDowall *et al.*, 1980). In general terms, the procedure starts with the development of a "noise model" to account for seasonality, nonstationarity, and autocorrelation in a time series. These characteristics are predictable sources of within-series variation and must be controlled prior to the impact analysis. After a noise model has been developed, an "intervention model" is added to represent the effects of the law. If there is a change in a series following the introduction of the law, this is reflected in the estimates of the intervention model.

For each series, three types of intervention models were considered: an abrupt and permanent change model, a gradual and permanent change model, and an abrupt and temporary change model. Other models are possible, but these three are reasonable and do not require elaborate assumptions about impact patterns (McCleary *et al.*, 1980: 168–171). Based on statistical analysis, we were led to the abrupt permanent change model as the most appropriate for each series.

A. Possible Threats to Validity

Cook and Campbell (1979) have pointed out that the interrupted time series design is among the strongest quasi-experiments, controlling for most threats to the validity of inferences drawn from it. The major threat to validity *not* controlled by the basic design is *history*—the possibility that other events occurring at the same time as the intervention were responsible for an effect. To reduce the possibility of historical threats, control series are analyzed for each crime.

For New York City juvenile arrests two types of control series are analyzed: arrests of sixteen- to nineteen-year-olds in New York City and arrests of thirteen- to fifteen-year-olds in Philadelphia. Neither control series was subject to a change in legal policy during the period of the analysis. Otherwise, both series should have been exposed to influences similar to those affecting thirteen- to fifteen-year-olds in New York City.

If New York City juvenile arrests decrease following the intervention but do not decrease for the control series, we conclude that the law affected crime rates. If arrests decrease both for New York City juveniles and for at least one of the control series, we conclude that historical events were responsible for the effect. For juveniles in upstate New York we use a similar strategy based on a single control series: arrests of sixteen- to nineteen-year-olds. The control series cannot totally eliminate historical explanations.

large upstate cities except Buffalo, which was dropped from the analysis because of apparent discrepancies in the reports for 1975.

However, they do imply that such explanations would have to be quite complicated and thus are relatively implausible.⁹

Besides history, a second possible problem for the design is the use of arrests as a measure of crime. The advantage of the arrest data is that they provide age-specific information on crime patterns. The difficulty is that arrests are only imperfectly related to crimes and are influenced by the behavior of police officers as well as juveniles. Time series designs can tolerate some measurement bias, as long as this bias does not itself change at the intervention point. The most notable threat posed by the imperfect relationship between arrests and crimes is thus that the relationship changed when the law was introduced—a threat Cook and Campbell (1979) call *instrumentation*.

Instrumentation threats could operate in several ways, but one possibility is especially worthy of consideration: It is conceivable that the police changed their arrest practices as a result of the law. In particular, the police may have started to charge relatively minor offenders with less serious crimes to avoid subjecting them to the rigors of criminal court. As with history, this threat cannot be completely dismissed, but it appears to be quite unlikely. To affect the analysis, changes in arrest practices would have to be widespread, involving a formal policy shift or the individual activities of many police officers. We have searched for such changes, using official accounts and interviews with law enforcement administrators, and have found no evidence they exist.¹⁰

It is not possible to rule out completely all threats to validity. The interrupted time series design is a strong one, however, and the analysis can reasonably be expected to detect any nontrivial effect of the JO Law on crime.¹¹

⁹ The control series also help allow for demographic changes. Although monthly population data are unavailable, annual estimates indicate that the number of 13- to 15-year-olds decreased slightly in the postintervention period. If arrest rates remained constant, the number of arrests would thus decrease after the beginning of the law. The numbers of 13- to 15-year-olds and of 16- to 19-year-olds are highly correlated however ($r > .97$ both in the City and upstate), so any decrease in arrests due to declining population will be reflected in both the experimental and control series.

¹⁰ Interviews were conducted in the summer of 1986 with officials of the following agencies: Crime Analysis Division, New York City Police Department; Erie County Central Police Services; Bronx County District Attorney's Office; and King's County District Attorney's Office. The implementation data also show that the law was not applied in all cases, further weakening the argument that the police would feel compelled to change arrest practices to avoid subjecting minor offenders to adult prosecution.

¹¹ A final possible problem is that the arrest series contain some cases not covered by the JO Law. The JO Law applies only to 14- and 15-year-olds for assault, robbery, rape, and arson, but the arrest series also include 13-year-olds. Further, not all assaults, robberies, rapes, and arsons are eligible to be charged under the law, but all arrests for these crimes are included in the series. Except for homicide, the series thus include some "irrelevant" cases. These cases will not bias the results as long as their proportion is approximately constant throughout the study period. The "irrelevant" cases will decrease the statistical power of the analysis, however, reducing the chances of

Table 2. Summary of Intervention Analysis for New York City

Series	ω	t
Homicides		
New York City—13 to 15	-.9633	-1.62
New York City—16 to 19	2.0370	1.55
Philadelphia—13 to 15	-.6586	-2.71**
Assaults		
New York City—13 to 15	.0230	.81
New York City—16 to 19	-21.3500	-1.49
Philadelphia—13 to 15	-4.7540	-3.32**
Robberies		
New York City—13 to 15	16.0100	.63
New York City—16 to 19	17.3400	.35
Philadelphia—13 to 15	7.4100	1.95*
Rapes		
New York City—13 to 15	-4.1570	-3.12**
New York City—16 to 19	-6.4120	-3.14**
Philadelphia—13 to 15	-.5748	-.92
Arsons		
New York City—13 to 15	-5.5150	-6.27**
New York City—16 to 19	-2.9070	-3.85**
Philadelphia—13 to 15	-1.0260	-1.76*

* $p < .10$ ** $p < .05$

V. FINDINGS

A summary of the results from the time series analysis is presented in Table 2 for New York City and its control series and in Table 3 for upstate New York and its control series. Since the best intervention model was the same for all series, we simply list the estimate of the parameter (ω) that represents the shift in the level of the series following the introduction of the law.¹² The results in the tables indicate that most of the experimental series were unaffected in the postintervention period. Further, where the effects are in the expected direction, similar patterns are apparent for one or more controls.

Given the observed pattern of results, is there any theory that could be used to conclude that the JO Law reduced crime? Clearly the findings for homicide and assault provide no support upon which to build such a theory. Neither homicide nor assault

finding an effect for the law. Because the series are relatively long, it is not likely an effect of reasonable size could be missed. Nevertheless, the analysis will be somewhat conservative, and extremely small effects could be overlooked.

¹² Plots of each series and complete details of the time series models are available from the authors on request.

Table 3. Summary of Intervention Analysis for Upstate New York

Series	ω	t
Homicides		
upstate New York—13 to 15	-.0104	-.37
upstate New York—16 to 19	.0012	.00
Assaults		
upstate New York—13 to 15	4.4230	4.42**
upstate New York—16 to 19	2.2520	1.48
Robberies		
upstate New York—13 to 15	2.6180	1.38
upstate New York—16 to 19	9.9870	3.08**
Rapes		
upstate New York—13 to 15	.4211	1.34
upstate New York—16 to 19	.8510	1.39
Arsons		
upstate New York—13 to 15	-.4510	-.55
upstate New York—16 to 19	.4653	.71

* $p < .10$ ** $p < .05$

arrests changed in the postintervention period in New York City, while arrests for both crimes decreased in Philadelphia. Similarly, homicide arrests remained stable in upstate New York, and assault arrests actually increased. The evidence thus strongly indicates that there was no effect of the law on either homicides or assaults.¹³

Rape and arson arrests for thirteen- to fifteen-year-olds in New York City did decrease following the introduction of the law. However, there was a similar decline in arrests for both crimes among New York City sixteen- to nineteen-year-olds and a decrease in Philadelphia arson arrests. The decreases in rape and arson are thus not specific to New York City juveniles and appear to be part of a general trend that affected other groups as well. In upstate New York, rape and arson arrests did not change following the introduction of the law. It thus seems reasonable to conclude that the JO Law had no effect on rape and arson, either in New York City or upstate.

The conclusion that New York City rapes and arsons were unaffected by the law cannot be as firm as that for homicides and assaults, because rape and arson arrests did decrease. However, the analysis indicates that the decreases were more general than would be expected if the JO Law were responsible, and the evi-

¹³ Although the increase in upstate assault arrests may be an effect of the JO Law, it is in the opposite direction from that expected if the law reduced crime. Given the low level of implementation for upstate assault cases, it seems most reasonable to attribute the increase to chance.

dence against an effect is clearly much stronger than that in support of one.

Perhaps the best case for an effect of the law can be made for robberies. Robbery arrests of thirteen- to fifteen-year-olds increased insignificantly in New York City and upstate following the intervention. However, there were statistically significant increases in two of the *control* series, arrests of Philadelphia thirteen- to fifteen-year-olds and of upstate sixteen- to nineteen-year-olds. It is thus possible to argue that the JO Law stopped an upward shift in robberies that would otherwise have occurred. That is, although the law did not reduce robberies, it may have prevented them from rising.

Although this argument cannot be totally rejected, it is relatively implausible. Arrests of thirteen- to fifteen-year-olds did increase in New York City and upstate, although the size of these effects is statistically insignificant. One of the control series, arrests of New York City sixteen- to nineteen-year-olds also increased insignificantly. The general tendency of all the series is therefore upward, significantly for two of the series and insignificantly for the others. In substantive terms the increases in the experimental and control series are quite similar, and it seems unwise to place much stress on the small differences.

An influence of the JO Law on robberies cannot be completely dismissed. However, the evidence is weak, requiring that a lack of increase in arrests, rather than a decrease, be interpreted as an effect. The most prudent conclusion seems to be that the data are not inconsistent with the idea that the JO Law prevented robberies from rising. This possibility should be treated as a reasonable hypothesis, with limited support based on two of the three control series.

Overall, the analysis most strongly supports the conclusion that the JO Law did not affect juvenile crime. The results of the analysis are complex, but they are clearly inconsistent with a model in which juvenile arrests uniformly declined following the introduction of the law. Indeed, in only two of the ten experimental series (five each in New York City and upstate) was there a statistically significant decrease in arrests, while there were significant decreases in five of the fifteen control series.

Although it is possible to construct a theory that would account for these results and still credit some effect to the JO Law, such a theory would have to be very complex. The theory would require that the decreases in arson and rape arrests for New York City juveniles be attributed to the law. However, at the same time, the decreases in arson arrests in Philadelphia and in arson and rape arrests for New York City sixteen- to nineteen-year-olds would have to be discounted. Finally, the theory would require stressing the increases in robbery arrests in Philadelphia and for

sixteen- to nineteen-year-olds upstate as evidence of the law's preventive effect.

The analysis cannot reject such possibilities, and they should be regarded as interesting speculations, subject to further examination and testing. However, by far the simplest interpretation of the results is that the JO Law was ineffective in reducing crime.

VI. DISCUSSION AND CONCLUSION

There are at least two general explanations for the apparent failure of the JO Law to reduce juvenile crime in New York. The first explanation is that the law was too weak an intervention to produce a measurable effect on crime patterns. The implementation data show that the law has been widely used in practice. Notwithstanding this fact, it is also true that in many cases the full force of the law has not been applied. For example, we earlier estimated that about 14 percent of the JO arrests made in New York City through 1983 resulted in sentences of confinement imposed by the criminal court; this of course implies that 86 percent of the arrested juveniles managed to avoid such sentences. In addition, only about 4 percent of the New York City arrests resulted in sentences longer than the maximum permitted before the JO Law went into effect (Division of Criminal Justice Services, 1984). It is thus possible that the law failed to reduce crime because it did not sufficiently increase the risks of punishment.

If this explanation is accepted, however, it leaves open the question of how high the risks of punishment must be to affect juvenile crime appreciably. Earlier we cited pre-JO Law data from Boland and Wilson (1978) that indicated confinement rates much lower than those that have prevailed since the law was introduced. In fact, since 1977 the number of juveniles confined in New York has increased by 54 percent, and the number of juveniles in secure (prison) confinement has more than tripled. The bulk of this increase is due to the JO Law (McGarrell, 1985: 38).

These figures suggest that the JO Law did substantially increase the risks of punishment.¹⁴ Although even higher levels of punishment may successfully reduce juvenile crime, these levels may have to be greater than any currently envisioned. In theory, enforcement could be expanded, but in fact considerations of justice and the costs of imprisonment may limit the application of statutes like the JO Law. If the law failed for lack of implementation, a level of implementation sufficient to influence crime may simply be unattainable in practice.

A second explanation, and perhaps a more likely one, is that

¹⁴ It should be emphasized that juveniles arrested but not indicted for JO offenses may be waived to the Family Court. In fact, 39% of juveniles arrested but not indicted under the law in New York City were so waived during the study period. Presumably the Family Court imposed its own punitive sentence in many of these cases.

the JO Law failed to influence crime rates because juveniles were not responsive to its provisions. That is, serious juvenile offenders may not have been deterred by the increased certainty and severity of punishment promised by the law. This explanation is compatible with the possibility that the JO Law was insufficiently implemented, but it emphasizes the response of juveniles to the law rather than the organizational response.

Despite its alleged lenience, the Family Court is not without power to sanction serious offenders. Although most juveniles appearing before the Family Court have received relatively little punishment, the probability of a punitive disposition is certainly not zero. The JO Law thus represents an increase in punitiveness over an already existing base, and the increase may not have daunted potential offenders. This possibility is consistent with research on adult populations, which has produced mixed and generally weak findings concerning the deterrent effects of increases in the level of criminal sanctions (see, e.g., Nagin, 1978). The message the New York State legislature intended to send to violent juvenile offenders may thus have gone unheeded.

A final possibility is that not enough time has passed for the JO Law to produce a measurable effect on crime. If the effect were quite small or if it increased very slowly over a long period, more time would be necessary for a statistical analysis to show an impact. Although the length of the postintervention series is sufficient to detect relatively small changes, it is still possible that extremely small effects may have been missed. It is also possible that some subgroups in the juvenile population were influenced by the JO Law while most juveniles were not. It is difficult to imagine how such effects would operate, but again a longer period of time would be necessary to detect these changes using aggregate data.

New York's Juvenile Offender Law is among the first products of a shift away from the separate treatment of juveniles and adults in the legal system. More punitive policies toward serious juvenile offenders have been widely advocated, and some observers have claimed that the traditional juvenile justice system is in jeopardy (e.g., Krisberg *et al.*, 1986). While the results in this paper are inadequate to assess the elimination of juvenile court in general, they do suggest that proponents of punishment may be as overly optimistic as have been advocates of treatment. Shifting the organization of juvenile justice to the adult court does not appear to have influenced crime rates in New York. Although other schemes for reducing the legal separation of juveniles and adults are possible, clearly they will have to be much different than New York's if they hope to be successful in reducing juvenile crime.

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