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The Impact of Juvenile Justice Involvement on Educational Outcomes

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Abstract

Despite its increased emphasis on punishment and accountability, juvenile justice policy still coheres around rehabilitative aims such as the placement and reentry of adjudicated youth into appropriate educational settings. However, theories of the impact of sanctions such as labeling and defiance—as well as exclusionary trends in public schools suggest that juvenile sanctions may actually hinder the performance and enrollment for inner-city youth. This prediction was largely confirmed in empirical research involving a sample of 778 urban youth of color who were arrested at least once. Separate subsamples of youth arrested while enrolled in seventh, eighth, or ninth grade, respectively, are compared, with respect to subsequent school performance in that grade, to youth from the same grade who are arrested in the subsequent grade.

The results of these quasi-experimental, multivariate comparisons suggest that first arrest has no main effect on math and reading achievement test scores during seventh and eighth grade, but it does increase the odds of repeating eighth grade. During ninth grade, being arrested dramatically increases the risk of dropping out, and substantially lowers attendance and grades, net of a comprehensive set of selection factors. The adverse consequences of arrest are magnified for youth who spend time in detention or who are arrested multiple times, while measures of available social support following arrest inconsistently blunt the effects of arrest.

In order to further explore omitted and moderating variables, as well as the processes mediating sanction effects, supplementary interviews focused on the psychological, social, and institutional dynamics that twenty youth aged 18-20 encountered following sanctioning. Eleven youth showed declines in educational performance following arrest while nine youth evidenced at least one improvement in school performance following arrest. The interviews suggest that youth whose performance declines following arrest are less likely to receive and capitalize on support from family, teachers, and probation officers following their arrests and are more likely to allege serious mistreatment by school and justice system authorities. Taken together, the statistical and anecdotal evidence suggest that, in the most disadvantaged inner-city communities, juvenile justice involvement is so pervasive and so closely associated with school termination, that it helps mark the transition to adult status, thereby usurping some of the traditional functions of scholastic achievement.

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Acknowledgements

This dissertation is an unusual fusion of perspectives and methods. While I can accept most of the credit (or take most of the blame depending on how effective this integration is) for its execution, much of the credit for its conceptualization and development belongs to the scholars and researchers who have exerted a direct, diverse, and supportive influence over my work.

First and foremost, I am grateful to Professor Tom Cook. Dr. Cook nurtured my initial interest in youth in relation to social institutions, provided me full access to the Comer School Development Program Evaluation data, integrated my project into the Spencer Foundation grant proposal, and—perhaps most importantly—suggested and guided the quasi-experimental approach that gives this research its unique flavor and force.

Aside from being a quasi-experimental examination of the impact of legal sanctions on school performance, my work also integrates and attempts to extend insights from criminology. For this reason I must acknowledge the contribution of two prominent criminologists. John Hagan and John Laub not only share a first name and seemingly boundless knowledge but also an interest in my intellectual development and this project in particular. Professors Hagan and Laub helped keep the project focused and relevant to criminology, referring me to relevant literature and suggesting solutions to various theoretical and methodological issues that arose over the years.

In addition to bringing quasi-experimental method to bear on a novel criminological question, the research also seeks a deeper, interpretive understanding of the statistical findings.

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In this connection, my first strategy was to conduct supplementary interviews with youth about their experiences in school and in the justice system. I am indebted to Kathryn Edin for inspiring and emboldening me to pursue such research and to Tim Nelson for his invaluable tips on the mechanics of interviewing young men of disadvantaged backgrounds. The second strategy for deepening my understanding was to situate all my findings within the macro-sociology of punishment. Professor Bernard Beck was perhaps the most helpful in this endeavor simply through his bold and expansive intellect, unafraid to search for connections where none have thought to look.

Thanks are also due to those who provided data for this project. Words cannot express the depth of my gratitude to Dr. David Reed, who generously shared with me the juvenile justice data that he worked so hard to obtain and prepare and without which my project would not have been possible. In addition, Dr. Wesley Skogan generously provided data on neighborhood crime rates. Next, Shazia Miller, Elaine Allensworth, and Todd Rosenkrantz of the Chicago Consortium on School Research were all very helpful in providing and explaining official school records data.

Various institutions provided invaluable support for this research. The National Institute of Justice and National Science Foundation provided financial support and generous extensions on the deadlines for expenditure of those funds. The Institute for Policy Research provided invaluable assistance in grant application and administration and, through its Joint Center for Poverty Research, provided one full year of funding. In addition, the Illinois Department of Corrections, the Cook County Department of Corrections, and the Cook County Probation

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Department deserve thanks for helping me locate interview participants and/or facilitating interviews with them.

Thanks to Thomas Cook and my institutional sponsors, I was also fortunate to enlist the help of several undergraduate research assistants who lent their talents in various ways including merging data, finding and recruiting interview participants, statistical programming, and transcribing and coding interviews. These dedicated and motivated young people are Monica Holliday, Chris Sherman, Christina Saenz, Jennifer Chambers, Shannon Mok, and Jinna Yun.

I have also not forgotten those who provided helpful materials, technical help, and advice at various points in this research including Carol Heimer, Jens Ludwig, Spencer Di Li, Michael Alan Sacks, Stephanie De Luca, and Robert Murphy. Finally, I was the beneficiary of an extraordinary team effort in the final stretch to proofread my entire dissertation. The members of my editorial team were Joseph Hirschfield (who provided the discerning eye and devotion to task only a parent could provide), Yarimar Bonilla, Kashka Celinska, Steve Chung, Brian Bigelow, Jamshid Mousavinezhad and Monique Payne. One of my editorial draftees, Kashka Celinska, also deserves special mention for her unending love and support and for collaborating with me on another recent project, our daughter Julia, who gave me the strength and drive to finally see this seemingly endless endeavor through to completion.

Despite all the extra eyes and ears attuned to this project, any and all errors and misconceptions are my own and those of my informants. While my informants shall forever remain anonymous, I hope their days of collective obscurity are numbered.

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INTRODUCTION

The incarcerated and the overall correctional population (which includes jail inmates and those under probation and parole supervision) have increased steadily since 1972. Largely owing to the War on Drugs and various sentencing reforms at the federal and state level (e.g. determinate sentencing schemes, mandatory sentencing, sentencing enhancements) correctional populations skyrocketed during the 1980's and most of the 1990's (Mauer, 2001). From 1990 to 2002 the number of incarcerated in America's jails and prisons nearly doubled from about 1.2 million to over two million (Harrison and Karberg, 2003). During this period, the incarceration rate in America rose by a factor of 1.6. These increases are even more dramatic among African-Americans and women. The idea that this phenomena was not merely a criminological and criminal justice trend but also an important sociological and demographic one was introduced to the public the Sentencing Project, who revealed in 1990 that almost one in four African-American males in the age group 20-29 was under some form of criminal justice supervision (Mauer, 1990). Just five years later, as the prison boom continued, the organization updated this figure to nearly one in three (Mauer and Huling, 1995).

Given the important role afforded the State in sociological analyses of poor communities—whether in discussions of welfare, child welfare, schools, the labor market, or youth programs—one would expect that many scholars devoted to these subjects would find these trends deeply troubling, provocative, and worthy of further exploration. Why did so few sociologists ask, "what does expanded criminal justice involvement mean for all the other means

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of social regulation in poor communities and for the collective well-being of the poor themselves?"

Apparently, the gentle exhortations from scholars and the alarms sounded by advocates and their supporters in politics and the media eventually grew too loud to ignore. Alternatively, the objective prevalence of justice system involvement exceeded the threshold of sociological awareness. That is, it became visible on many sociological radar screens tuned to the ghetto, even those tuned squarely onto other social objects. In any case, recent years have witnessed a burgeoning of work on the broader social implications of expanded criminal justice and mass incarceration. Even some non-criminologists have entered the fray. For instance, Kathryn Edin and Thomas Nelson led a multi-year study of marriage and child-rearing in poor black communities. They were struck by the large portion of marriages that were delayed indefinitely or interrupted by the male partner's incarceration as well as the number of men whose commitment to fatherhood was threatened or, in some cases, strengthened by their incarceration (Edin, Nelson, & Paranal, 2001).

Low rates of marriage and high rates of absent fatherhood are just two among a growing list of the social costs now commonly attributed to mass incarceration. Researchers have adopted the collateral consequences framework for understanding the effects of criminal justice on the labor market, voting, rural economic life, urban social organization, and children's well-being. This line of inquiry and advocacy recently culminated in a book, *Invisible Punishment: The Collateral Consequences of Mass Imprisonment* (Mauer and Lind, 2002).

While the collateral consequences of mass incarceration have proved a fertile area of inquiry, vast stretches of this field remain largely unexplored. In contemporary America, owing

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to severe segregation of communities by race, income, and crime (Clear, 2002), most large cities contain communities where correctional supervision is an inescapable aspect of daily life and looms over the future of most male youth. In the predominantly black District of Columbia, for instance, over half of black males between eighteen and thirty-five are under some form of correctional supervision (Braman, 2002). Moreover, over 75% of the District's black men face incarceration at some time in their lives (Braman, 2002). Todd Clear (2002), who argues that incarceration can reach levels high enough to undermine a community's level of social stability and organization, states that various cities have neighborhoods which, on any given day, have lost to incarceration up to twenty-five percent of their adult male residents. Thus, virtually all members of these communities, who are not themselves under correctional supervision, have a close friend or relative who is.

If no person in those communities has been left untouched by criminal justice involvement, it follows that no aspect of social interaction or institutional life has been spared its influence. The number of sociological questions that could germinate from the reality of normative incarceration is as boundless as sociology itself. For example, what are the implications of normative incarceration for adolescent identity development, for status transitions and rites of passage, for social cohesion within low-income communities, for family socialization practices, for workplace interactions, and for the content of a community's shared values and norms?

This work explores the reverberations of mass incarceration and expanded criminal and juvenile justice involvement for the domain of social life whose neglect by sociology is most conspicuous and unfortunate—public education. Education is a critical topic to explore in this

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connection largely because of the vital societal functions it performs, both latent and manifest. First, schools are a primary means through which society imparts shared norms and values and soft and hard skills, all of which are preparation for work and civic participation. Second, schools are a mechanism of social mobility and social reproduction, providing the credentials one needs to attend college or to maximize one's job prospects. Given that individuals and social groups have differential success in obtaining from diverse schools the cultural capital, connections, and credentials that determine occupational success, schools also play a key role in social stratification. Third, schools provide social control over potentially unruly youth; they "keep kids off the street" (as well as away from exploiters of child labor). Fourth, and this is a function under which many critical theorists would subsume the first three functions, the school system is one possible means through which privileged groups maintain the perpetual subjugation of disadvantaged ethnic minorities and the poor. Do schools perform these functions less or differently in communities where criminal justice involvement is normative? What impact does pervasive juvenile or criminal justice involvement have on how and how well schools fulfill their various functions?

This work does not attempt to address the impact of all aspects of expanded criminal justice on all aspects of education. Rather, I focus specifically on the impact juvenile justice involvement, because juvenile justice is the institution that should most directly impinge on schooling, given that many or most of its "clients" are at least nominally enrolled in school. Second, I focus on the impact of juvenile justice involvement on tangible indicators of school performance like test scores and dropping out because such outcomes are easily measured, nearly

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universally recognized as important for long-term life chances, and a frequent focus of policy concern and intervention.

Mission Declaration, Defense, and Downscaling

Because this field of inquiry is underdeveloped both theoretically and empirically, my mission is two-fold. First, I describe several overlapping yet competing theoretical frameworks which lead to the prediction that juvenile justice involvement adversely affects school engagement and performance. I also consider conditions in which juvenile justice involvement may actually "deter" school failure or even improve school performance.

Second, I hope to bring empirical evidence to bear on these theoretical frameworks. Researchers often have myriad empirical tests to choose from, with the choice of questions and methods governed by the interests and skills of the researcher, as well as the nature of available data. Empirical "tests" of the impact of sanctions may assume two general forms. Quantitative researchers begin with a hypothesis concerning the main effect of sanctions on behavior. Next, they attempt to compare the behavioral outcomes of two groups that are alike except for the type or level of sanctioning that the youth experience. Quantitative studies on sanction effects have been criticized for failing to ensure that treatment and control groups truly are comparable and for offering an overly deterministic model of the impact of sanctions. Sanctions effects researchers typically treat offenders as passive recipients of institutional inputs that serve either to reduce or amplify deviant behavior and circumstances. The linear and mechanistic models of human behavior embedded in quantitative research on sanction effects ignores the capacity of diverse offender population and their significant others to subjectively interpret, to resist, and even to alter the constraints and pressures that shape their responses to sanctions.

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For these reasons, rather than simply asking *what* are the effect of sanction we should be asking *when* are sanctions most likely to have particular effects, *for whom* and *why*. The research strategies that are most attuned to the subjective interpretations and idiosyncratic circumstances mediating and moderating the impact of sanctions are qualitative. With respect to research on the impact of legal sanctions on school performance, participant observation and interviews provides a window into how sanctioning effect may reverberate through school, family, peer group or workplace contexts, as well individual consciousness and behavior and how the interplay of these forces may impinge on school behavior.

Both methodological approaches to sanction effects research have serious strengths and weaknesses. The advantages and disadvantages of both hard and soft scientific approaches are debated extensively in nearly every substantial methods treatise and need not be elaborated here. One consensus that has begun to emerge out of this factious debate is that no one has a monopoly on truth. No single study or even single methodology can provide a panoramic view of a social phenomenon. A greater depth and breadth of understanding can be achieved when an issue is examined from a variety of angles using a plurality of methodologies. It is in this spirit of relativism and pluralism, that I employ both quantitative and qualitative methods in this research.

The multi-methodological approach does have at least one significant downside, however. At the root of both quantitative and qualitative approach are certain basic epistemological assumptions. These respective sets of assumptions are, in some respects, clashing. Even among only those operating in a highly positivistic tradition, sizable rifts are apparent. Quantitative purists tend to believe that causal inferences require a representative sample, precise measurement (preferably at multiple time points) of outcomes and their potential

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causes, and a research design and statistical tools that effectively rule out alternative causes—also known as threats to internal validity (Shadish, Cook, and Campbell, 2001).

Qualitative researchers, on the other hand, adopt a different standard of "knowing" with respect to causality. At the risk of over-simplifying and homogenizing a diverse set of scholars, most qualitative researchers would contend that causal reality is too complex to be neatly distilled into scientific models. Many would contend that the causal narratives that emerge from interviews, archival methods, and other "soft science" methods in combination with inductive and deductive logic (and related qualitative research tools) are just as "valid" as the causal inferences emerging from statistical analyses.

The danger in incorporating both methods into one study is that overall work may appear disjointed or self-contradictory at times. In this work, I make only feeble attempts to reconcile the conflicting assumptions of the two approaches. I merely provide the standard caveats that the qualitative results should be viewed as exploratory as well as interpretive. Those who are skeptical of the generalizability of my anecdotal findings and the validity of my subjective interpretations, should at least see the merits of interviews with respect to generating interpretive theories, which can, at some later date, be subject to more "rigorous" testing. Those who find the sudden shifts in theoretical and epistemological assumptions, scientific lingo, and other professional conventions associated with the faithful use of multiple methods to be overly disjunctive should view this work as a sampling of angles and methodologies from which researchers wishing to tackle similar questions can choose.

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A Tale of Two Studies

Now that I have discussed the larger objective and the broad strategy of my mission, I turn next to its substance. The first component of this research is a quantitative study on the impact of juvenile justice involvement on engagement in the educational institution. Based on research and theory on the processes of labeling, defiance, criminal embeddedness, and institutional exclusion weighed against support for deterrence as well as prior evidence on the impact of legal sanctions on educational attainment I hypothesize that, in the aggregate, the juvenile justice system has adverse effects on school performance among those sanctioned. I test and largely confirm this prediction using data on juvenile justice involvement and school performance collected from a large sample of contemporary inner-city youth. I do not explore in this work the next logical step—the educational failure or exclusion following juvenile justice involvement in facilitating further involvement in the criminal justice system—although other work has investigated these causal linkages.

The mechanisms through which juvenile justice involvement affects school performance receive considerable attention in this research. My qualitative empirical exercise, the second act in this two-part play, seeks to illuminate these mechanisms through interviews with former inner-city students, drawn from the same sample as the quantitative research, about their school experiences following their juvenile justice involvement. The interviews, though limited to one meeting each with a small sample of youth and separate token meetings with some carefully and some accidentally chosen adults, shed interpretive light on the statistical relationships I observed.

In examining the mechanisms through which arrest and confinement affect educational experiences—mechanisms which include shifts in self-image, attitudes toward authority, family

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and peer relationships, and school interactions—in the context of impoverished communities the interviews give a human face and context to the quantitative findings. They also serve a much broader purpose. They are a brief but enriching excursion into how the institutions of juvenile and criminal justice—deeply embedded as they are in ghetto communities—structure social interactions, systems of meaning, values, and expectations in the educational realm and in other domains. This study, like other qualitative research on social reproduction extends far beyond the parameters of sanction effects theory in highlighting the role of the criminal justice system in shaping the institution of schooling and behavior within it.

This work makes a significant contribution to at least three established lines of research. First, this research offers the first systematic inquiry into the impact of juvenile justice involvement on several indicators of educational attainment, including dropping out of high school. When viewed on a larger scale, my findings suggest that juvenile justice involvement—which is allocated, in part, along race, class, and gender lines—may even play a significant role in explaining educational and, by extension, occupational stratification. Second, my work encourages researchers who are interested in why or under what circumstance legal sanctions impact criminal behavior, to consider school-related consequences of sanctions and the role of the structural context (e.g. normative incarceration) in conditioning the impact of sanctions. Third, to practitioners and researchers concerned about the practical aspects of educating delinquents, my work highlights educational and juvenile justice inputs that appear help offenders obtain a meaningful education, as well as restraints on offenders' educational success that, with the proper amount of will and coordination, could be greatly loosened.

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Overview

This work can be divided into four sections. The first section, which is comprised of the first chapter, lays the theoretical groundwork for the empirical work to follow. The chapter outlines several pathways though which juvenile justice involvement may impede academic progress. I consider the following theoretical perspectives: (1) labeling, defiance, and criminal embeddedness theories which suggest that juvenile justice involvement predicts declines in school performance owing mainly to social-psychological processes and social network dynamics (2) institutional exclusion theory which suggests that institutional practices of schools and their links with criminal justice institutions are the primary cause of a relationships between juvenile justice involvement and school failure, and (3) various theories that suggest particular categories of the youth population for whom or particular circumstances under which, juvenile justice involvement should not lower school performance.

The next section provides the quantitative empirical analyses. Chapter 2 describes the sampling and data sources that I employ in my research. Chapter 3 describes the quasi-experimental strategy I employ, known as the staggered replication design. It also contains related sample selection procedures to test the hypothesis of a negative impact of juvenile justice involvement on school performance. Chapter 4 describes the selection, independent, and outcomes variables employed in the analyses. Chapters 5 and 6 present the results of models of the impact of arrest during elementary school and high school, respectively. My central findings are that being arrested for the first time in eighth grade increases one's risk of eighth grade retention relative to eighth graders who are not first arrested until ninth grade, and that those who are not first arrested until ninth grade show elevated rates of dropping out and absenteeism

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relative to later arrestees. Finally, in Chapter 7 I address possible limitations of the analysis and alternative interpretations of the results. I attempt in that chapter to establish that the existence of a harmful impact of arrest, at least on absences and dropping out, is the only logical conclusion one can draw from my results.

The third section presents the qualitative analysis. In chapter 8, I describe a specialized interview sampling strategy known as case-specific sampling that permits the systematic examination of mediating mechanisms. I then describe my tracking and recruitment methods. I also describe two other analytic strategies—deviant cases analysis and analytic induction— that I employ in my attempts to infer causal (in a less scientific sense of the term) mechanisms from a small number of interviews. I then devote three chapters to a description of my qualitative results. Chapter 9 attempts to explain the observed focal school changes following arrest, arguing that, while confounding explanations are disarmingly prevalent, juvenile justice involvement clearly played a modest or major role in a substantial share of the focal school changes. The next two chapters draw on the interview data to explain the impact of juvenile justice involvement on school outcomes in light of the four theories described in chapter two. Chapter 10 focuses on mediating processes, labeling and defiance, that operate primarily outside of the school or through student's subjective perceptions and interpretations. Chapter 11 describes processes that are directed more by the actions of social networks and institutions.

Lastly, in the concluding chapter I summarize my qualitative results and situate them alongside the quantitative findings. In addition, I share insights from the interviews that bear on the relationship between juvenile justice involvement and school performance but do not fall neatly within any of the four theoretical frameworks that guided this research. My interviews do

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not uncover cogent evidence of a smooth transition from juvenile justice involvement to school failure. Rather, as expected, I find a variety of complex and contradictory processes at work. While school and juvenile justice personnel sometimes conspire to push students in legal trouble out of school, teachers and probation officers also sometimes work jointly and concertedly to re-engage some of these youth in school.

At the same time, my interviews highlight some of the ways in which the embeddedness of criminal justice in the social structure and in the social and cultural milieu of low-income urban minority communities, including schools, can redefine the importance of and otherwise conflict with the provision of education. Incarceration is so common among adults in the ghetto, and it so commonly blocks passage through the schoolhouse doors, that it has become a normative social transition, helping to signal the end of childhood and associated tasks such as schooling and the beginning of adulthood. Like other responsibilities of adulthood, including work and marriage (Sampson & Laub, 1993a), the responsibilities associated with managing criminal justice supervision, including reporting to one's judge and probation officer, evading the police, earning money to survive prison and its aftermath, and maintaining one's "tough" reputation established by serving time, all divert one's attention and energies away from school. Thus, it is my original, though largely speculative, contention that criminal justice embeddedness in low-income communities, not only has affected the rituals, disciplinary procedures, and treatment from authorities that youth encounter in school, but also conditions the meaning and importance that inner-city youth attach to schooling.

Sociologists have thus far been slow in giving the phenomena of widespread criminal justice involvement the full sociological examination that it deserves. I hope to encourage other

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sociologists to look beyond the end products—the collateral damages that incarceration exacts on individuals, their families, their communities, and gang and youth culture. The sociological lens, while perhaps slower to focus than the advocacy, demographic, criminological or journalistic lens, can provide greater depth, texture, color, and dimensionality to the issue. Sociologists and ethnographers are poised and trained to explore social processes associated with normative criminal justice and to understand how normative criminal justice can influence how people see themselves and their social world, and how they respond to these perceptions. My work hopefully patches a few holes in our understanding of the influence of criminal justice on the broader society and on inner-city schooling in particular.

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CHAPTER 1:

JUVENILE JUSTICE AND SCHOOLING: FOUR THEORIES

Scholars in various disciplines have increasingly addressed how the expansion of the penal system has impinged on other social institutions. This work has assessed the impact of criminal justice intervention on labor markets and occupational attainment (Western and Beckett, 1999; Grogger, 1995; Kling, 2002; Bushway, 1998), families and children (Braman, 2002; Edin, Nelson, & Paranal, 2001), neighborhood cohesion (Rose & Clear, 1999), welfare and housing benefits (Beckett & Western, 2001; Rubinstein & Mukamal, 2002), and voting participation (Uggen & Manza, 2002). Collectively, these works suggest that "mass incarceration" undermines the redistributive as well as social integration and social control functions of various institutions.

While work on the "collateral damages" of incarceration has blossomed one major domain of social life has been ignored in the academic literature—public education. This lack of attention likely reflects the difficulty of obtaining data on both justice involvement on school performance as well as the challenge of linking theories of legal sanctions and their effects to theories of school behavior. The most appropriate focus for work on the impact of justice involvement are juvenile justice involvement and inner-city students, given that students in trouble are most likely to be processed in the juvenile justice system and inner-city students are the most likely to get in trouble.

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Trends in Schools and Juvenile Justice

The effects of juvenile justice involvement on school performance are likely dependent on the nature of both juvenile justice and educational institutions. Thus, discussion of some relevant trends in both institutions is in order. Scholars of juvenile justice have highlighted the gradual erosion of the distinction between juvenile justice and adult criminal justice. At the end of the 19th Century, The Juvenile Court was consciously devised as a sharply contrasting alternative to the adult criminal court and its penal system. Rather than focusing on punishing offenders the juvenile court was envisioned as a "substitute parent" providing the counseling and supervision that delinquent youth are often unable to receive from their families. Given that the juvenile justice system was founded upon different principles, it still attracts many with a social service orientation. Juvenile justice personnel still overwhelmingly endorse the principle of rehabilitation and many define their role in those terms (Leiber, Schwarze, Mack, & Farnworth, 2002).

The criminal justice system has always placed a premium on justice and its precepts of individual moral culpability and punishment/penance as well as due process rights of the accused. The juvenile justice system, by contrast, espoused the mutability of the child given proper guidance and supervision. Accordingly, educational and family-based assessments and services have always been at the center of juvenile justice efforts to return wayward youth to the mainstream. And consistent with the notion of rehabilitation, juvenile court records were expunged when youth reached eighteen and youth were handed a clean slate.

Although the juvenile justice system, on the whole, never actually operated in a non-punitive and rehabilitative manner (Feld, 1993; 1999), it at least maintained the pretense of being

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a separate system. Over time, however, as procedures in the juvenile court became more adverserial and penalties more actuarial, automatic, and punitive, the similarities between the two systems began to overshadow the differences (Feld, 1999; Singer, 1996). Many states have eased long established confidentiality restrictions. In 1994, the Illinois legislature gave the general public access to the names of youth convicted of offenses involving firearms and felony drug offenses (O'Reilly, 1995). Governor Edgar, who signed the new law, remarked that the old law allowed "these thugs…(to)…hide behind the fact that if they are convicted, it cannot be released to the public" (cited in O'Reilly, 1995).

As the juvenile court has been refashioned in conformance to the adult criminal court model, its social welfare orientation has given way to an emphasis on accountability and control (Feld, 1998; Kurlychek, Torbet, & Bozynski, 1999). Assuming that changes that have occurred in juvenile justice represent transformations of practice and not simply policy reforms, they may also be said to have altered the function of the juvenile justice system with respect to schools and the criminal justice system. When the juvenile justice system was more service-oriented, its mission was more congruent and overlapping with schools and family/child welfare services. Accordingly, its secondary societal function—after social control of the poor and minorities (Feld, 1999)—could be described as rehabilitation or social integration with respect to involvement in conventional institutions. The modern juvenile justice system, functions essentially like the adult criminal court (including its "jail"), frequently remands youth to adult court, can affect outcomes in adult proceedings (Sanborn, 1998), and often closely precedes adult incarceration (Sampson & Laub, 1993a; Brown, Miller, Jenkins, and Rhodes, 1991). The

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juvenile justice system, thus, no longer possesses a broad cultural and policy mandate to facilitate the successful reentry of delinquents into schools.

Paralleling trends in juvenile justice, public schools and inner-city schools in particular have also adopted a less forgiving and rehabilitative attitude toward trouble-making youth. Like juvenile justice systems, public schools have also been the target of state and national policy initiatives to promote "accountability." These efforts target the performance and behaviors of school administrators, teachers, and students. Some fear that the accountability movement may inadvertently promote the neglect or exclusion of certain students (Parsons, 1999). For instance, as part of the movement for "accountability" and the related push for "school choice", state policy agencies publicize and penalize schools whose students fail to achieve threshold levels on standardized tests. Statewide school accountability reforms in Illinois (e.g. "The School Report Card") also promoted high stakes testing which encourages teachers to devote their attention to students who stand a decent chance of boosting schools' test scores and to neglect and exclude students who are likely to depress school means (Bryk et al., 1998; Parsons, 1999). The diffusive warping of educational priorities by high stakes testing is facilitated in Chicago's policy of placing schools with failing test scores or poor attendance (but not those with high dropout rates) on 'probation,' which resulted in some schools' dropping 'truants' to boost scores and attendance rates (Kelleher, 1999)[1].

Accountability is also the buzzword with respect to school disciplinary policy. Under the Federal Gun-Free Schools Act of 1994—passed in the wake of several well-publicized school

[1] School chief Paul Vallas changed this policy in 1999 under pressure from critics concerned about rising drop out rates and declining high school enrollments (Kelleher, 1999). Fewer than half of Chicago public high school students graduate (Miller, Allensworth, and Kochanek, 2002).

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shootings—public schools must expel for at least a year students who bring “firearms” to school. Politically mobilized by this law, a large majority of schools also adopted “zero tolerance” policies for other weapons, alcohol, tobacco, drugs and violence (National Center for Education Statistics, 1999). The Department of Education's Office of Civil Rights estimates that from 1974 to 1997 the rate of suspensions increased steadily from 3.7% of all students to 6.84% (cited in Brooks, Shiralidi & Zeidenberg, 1999).

Concerns over school safety prompted schools to adopt a number of other measures which may reduce crime at the expense of an atmosphere of tolerance, individualism, personal growth, and inclusiveness (Brotherton, 1996; Devine, 1996). These measures include school uniforms, video cameras, high perimeter fences, computerized ID cards, metal detectors, random locker and bag searches, and the installation of full-time security staff and police officers into schools. By the 1996-97 school year, 54% of all public high schools had “police or other law enforcement representatives” available to the school, and 18% of each had police present on a regular basis (National Center for Education Statistics, 1999). In some jurisdictions like New York City and Chicago, school police officers often treat the school just like any other police beat. (Hagan, Hirschfield, and Shedd, 2002; Devine, 1996; Ince, 2001). Accordingly, arrests in schools are quite common. In 1999 the full-time armed school police officers stationed in every Chicago public high school and most of its elementary schools reported 10,631 ‘serious Uniform Disciplinary Code violations’ occurred (23 per 1000 students)—78% of which were referred to the Chicago police (Chicago Public Schools, 2000). Collectively, the three charges of battery, criminal trespassing, and disorderly conduct comprised 53 percent of all arrests in the 1992-93 school years. Almost no evidence demonstrates that expanded use of physical (metal detectors,

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locked doors, etc.) and personnel-based security strategies improves school safety (Mayer and Leone, 1999).

Statement of Research Hypotheses

The preceding discussion suggests that contemporary inner-city students who become involved in the juvenile justice system may find little sympathy among either among agents of the juvenile justice system or the schools, between whom it has become increasingly difficult to distinguish. Juvenile justice involvement and the inter-agency exchange of information about it, may also help schools identify certain students as troublemakers, thereby facilitating their failure in and exclusion from school. From the foregoing set of trends and assertions, the general form of a simple hypothesis may take shape. The operational form of this hypothesis will be presented later. Getting involved with the juvenile justice system should increase school exclusion and failure among inner-city student.

One may question my choice of research questions, which clearly lends itself to statistical modeling, on the grounds that it embodies a simplistic and mechanistic image of reality. One way to minimize over-determinism in the context of causal modeling is to strive to "model" the contradictions and complexities that are present in the real world. Juvenile justice involvement may have different effects on different people and in different contexts. Some students may reinterpret and resist the messages and directives of school and of juvenile justice authorities causing these communications to have a different effect than what was intended. Likewise, some schools or personnel within schools may resist the "criminalization" of their students, perhaps by resisting or co-opting the policies implemented in or the juvenile justice agents stationed in their schools. The review of the literature on the impact of juvenile justice sanctions which follows,

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suggests a number of testable sources of variation in the outcomes of juvenile justice involvement.

Another way in which empirical research can be sensitive to the complexity that inheres in all social reality is to explore the mechanisms through which hypothesized effects and opposing effects occur. This chapter outlines four overlapping yet mutually distinguishable theoretical frameworks, which explain why juvenile justice involvement may diminish educational prospects and also suggest conditions in which juvenile arrests may have the opposite effect. This discussion sets the stage for a second empirical "test"—this time consisting of observations gleaned from retrospective interviews of the various social processes that students experience following their involvement in the juvenile justice system.

Theories of the Impact of Sanctions on Schooling

How does getting in trouble with the law affect subsequent educational attainment? The literature on the impact of sanctions only superficially addresses how legal sanctions impinge on the behavior of school authorities. It does, however, suggest several other means through which legal sanctions may influence school performance. These means, labeling, defiance, and criminal embeddedness assign primary agency to the juvenile justice system and to individual offenders. School authorities and school practices play a very limited and passive role within each framework, which led to the development of a fourth theoretical framework, "institutional exclusion theory," in which these practices take center stage. Next, I describe, in turn, each of the three theories that are derived from criminological theory.

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Labeling Theory

Labeling theory (Lemert, 1951; Becker, 1963) emphasizes that the process of sanctioning deviant behavior often involves the explicit or implicit attachment of negative or stigmatizing (Braithwaite, 1989) labels to individuals. When a student is arrested, handcuffed, detained, censured by a judge etc.--in short, treated like a criminal--he may be perceived as a criminal. If teachers, students, and social service personnel learn of the student's arrest or detention, these negative perceptions may extend into the school. When applied to minority students, these labels may confirm negative stereotypes on the part of teachers (Valdivieso and Nicolau, 1992).

This labeling process may contribute to poor school performance through affecting student behavior and teacher behavior. First, students, according to the symbolic interactionist perspective, often behave according to how others define the social order and their role in it. Through this process of role-taking, a student may actually come to believe or "reflect" the appraisals of others (Matsueda, 1992). Since delinquency and school failure tend to go hand in hand, students may internalize a bad student label as well, and lose the motivation to work hard. While the effect of formal sanctions on peer and teacher perceptions of the labeled appears to be unexamined, research does demonstrate an effect of parental appraisals on self-appraisals (Matsueda, 1992), and of teacher's expectations (Winfield, 1986; Roscigno, 1999) and student motivation (Kaplan & Liu, 1994) on student performance.

Teacher perceptions of students as potential trouble-makers and bad students may lead them, possibly inadvertently, to grade them and punish them more harshly. These experiences may lower a student's enjoyment of and engagement in school (Natriello, 1984). Poor relations with teachers and frequent school discipline are frequent predictors of and frequently cited

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reasons for dropping out of school and school failure (Jordan, Lara, and McPartland, 1996; Myers et al, 1987). Thus, the strong assumption of the labeling perspective I have described is that either being processed as a "criminal" affects youth's self-concept and self-efficacy or that teachers or students will learn of a youth's apprehension and, as a response, alter their treatment of the student. Consistent with the institutional exclusion theory framework I describe later, labeling theorists emphasize the reactions of informal agents of socialization and social control to a student behavior or labels signaling such behavior. However, institutional exclusion theory, unlike labeling theory, describes the formalization or routinization of the labeling process in the context of schools.

Defiance Theory

The second theory predicting a negative effect of formal sanctions on educational attainment, defiance theory, also emphasizes hostile relationships with teachers but traces their roots back to formal sanctioning irrespective of any reflected negative appraisals. And similar to labeling theory but in contrast to institutional exclusion theory, it depicts the reactions of teachers and other school personnel as individualized rather than as institutionalized.

Hagan and McCarthy (1997), in their discussion of deviance amplification effects, summarize this theory, which combines elements of Braithwaite's (1989) reintegrative shaming theory with Sherman's (1993) original statement of defiance theory, and ideas from the sociology of emotions (cited in Hagan and McCarthy, 1997). According to this perspective, formal sanctions of deviant acts often invoke feelings of shame. When the person being sanctioned feels no bonding to the sanctioner(s), feels mistreated by the sanctioner(s) (Tyler, 1993), and/or has no opportunity to rebuild his or her relationship to the community

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(Braithwaite, 1989), these feelings of shame can turn to anger. Sherman (1993) describes how offenders, who do not respond violently to the sanctioner, may displace their anger onto a symbolic victim. Likewise, McCarthy and Hagan (1997) allege that youth with past stigmatizing punishment experiences are often poised to violently respond to even minor social rebukes. These ideas find support in the work of Paternoster et al (1997) and Sherman et al. (1992) which, taken together, suggest that arrestees who are either unemployed (a proxy for low sense of bonding to the sanctioning agent) or feel they are treated unfairly by the police are more likely to re-victimize their victims (as compared those who are warned instead of arrested). Those who felt they were treated fairly by the police had lower rates of recidivism than those who were not arrested.

Tanner, Davies, and O'Grady (1999) suggest that the apparent "spirit of defiance" that may result from sanctions may lead offenders to carry a disrespect of authority into the school and the workplace. Thus, disapproving teachers may become the punished student's "symbolic victim". This mutual disrespect may hinder the ability of school personnel to induce youth to internalize and comply with the teacher's and school's values and norms, and thereby contribute to disciplinary problems and/or disengagement and dropout from school.

One should bear in mind that the defiance should lead us to predict a negative relationship between sanctions and school performance only if we first assume that offenders typically feel a sense of hostility and/or inequity in response to being sanctioned. Farrington's (1977) finding that being ruled delinquent is associated with increasingly hostile attitudes toward the police provides some support to this idea. Possible alternative sanctioning experiences and student reactions will be discussed later.

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Criminal Embeddedness Theory

The third theory, which I derive from Hagan's theory of social or criminal embeddedness, like labeling and defiance theories, emphasizes the role of social relationships in shaping academic outcomes but posits that sanctions act on social relationships directly—rather than through intervening social-psychological mechanisms. Hagan (1993) argues that involvement in delinquent activities can precipitate, solidify, or expand one's participation in criminal networks. At the same time, embeddedness in criminal networks may weaken one's participation in pro-social networks. Not surprisingly, greater participation in criminal networks can increase deviant behavior while pro-social networks foster conformity to conventional behavior.

This shifting balance of social network participation resulting from criminal behavior can have implications that extend beyond criminal behavior. Social network ties can be instrumental in obtaining employment (Hagan & McCarthy, 1997). Conversely, "the criminal involvement of street peers are more likely to integrate young people into the criminal underworld than into referral networks of legal employment" (Hagan & McCarthy, 1997, p. 233).

Network embeddedness can influence one's performance in school. Youth who have more connection to pro-social peers and adults are more likely to learn about special programs and services related to school. And, because they have access to a network that could be mobilized in response to mistreatment or denial of services, they are less likely to be mistreated and more likely to be admitted into programs with good standing, net of any of the psychological or interpersonal dynamics at the core of labeling and defiance theories. Thus, pro-socially embedded youth are more likely to receive academic support from friends, families, and teachers

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in the form of tutoring or studying tips. Not only are social networks a potential source of academic and social support, but they can also, through forging and sustaining social bonds, be a source of social and academic control. According to Hirschi (1969), social bonds prevent people from following their natural desires, which for most students does not typically include working hard in school. Defenders of both social control and social support perspectives would adduce studies which show that bonding to the school (Berends, 1995) and positive family relationships (Janosz, LeBlanc, Boulerice, & Tremblay, 1997; Richman & Bowen, 1998)—alleged indicators of both social control and social support— affect a youth's scholastic achievement and chances of dropping out. In short, social networks broker an individual's access to a community's social capital, which allocates both social control and opportunity (Hagan & McCarthy, 1997).

Thus, to the extent the juvenile sanctions impinge on a youth's social network participation, they can also affect a youth's school performance. But how do juvenile sanctions influence a youth's social network participation and a youth's access to social capital? Surprisingly little research has addressed this question with juvenile samples.

Most studies involving adults have concluded that arrest and/or incarceration can hurt both an individuals standing in his job network (Kling, 1997) and family (Weishat & Klofas, 1990; Nightingale & Watts, 1996, Moore, 1997; Lynch & Sabol, 1998; Edin, Nelson, & Paranal, 2001), while undermining, largely through destabilizing the population, the informal social control capacities of neighborhoods (Rose and Clear, 1998).

There is much cause to suspect that juvenile sanctions levied in today's harsh political context exert similar effects. Arrests and court involvement can be a major source of concern and disappointment for family members and is likely to strain family relationships. Likewise,

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juvenile detention and incarceration involves the forcible removal of youth from their home environment and clearly has the capacity to disrupt friendships and romantic relationships (especially tenuous ones), while possibly depriving youth of the academic support they received at home. Ostracism from pro-social peers resulting from negative labeling of course could exacerbate this process, but juvenile detention disrupts social network participation irrespective of negative labeling.

At the same time, many have maintained that jails and detention facilities are "schools of crime" providing opportunities for delinquent youth to network with other delinquents, thereby providing access to what Hagan (1994) calls alternative forms of social capital. Likewise, while having been arrested or having served time may mark one as a pariah in the eyes of some, they may enhance one's value in the eyes of others, such as a neighborhood youth gang, who may seek out those who exhibit a commitment to an unconventional lifestyle (reinforced by blocked access to conventional opportunities in school or the labor market) and who have acquired or sharpened criminal skills while incarcerated. Needless to say, embeddedness within these networks and the application of such forms human capital acquired while in confinement directly conflicts with the effective performance of the student role.

On the other hand, juvenile justice sanctions also represent an opportunity to intervene in the lives of youth in such a matter as to disrupt criminal networks, provide supportive services such as mentoring and tutoring, and increase the level of social control youth experience with respect to their academic behaviors. How the juvenile justice system operates in practice is a, therefore, a question of paramount importance to social embeddedness theory.

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Institutional Exclusion Theory

The fourth mediating theory of the relationships between juvenile sanctions and school performance, far more than the previous three, privileges the role of institutional responses to official sanctioning. This theory, which I term *institutional exclusion theory*, treats the responses of school authorities to officially sanctioned youth as a feature of the socio-legal context. It therefore differs from the theories of labeling, defiance, and social embeddedness, which treat institutional responses as wholly a function of individual student behavior, reputations, or accessibility to social support. The patterned or institutionalized responses described by institutional exclusion theory, while conditioned in part by the patterned behavior of students and other individual school characteristics, are described mostly as a product of policy, law, and other constraints imposed by the structural dependence of schools on other social institutions.

While the academic literature on the institutional practices of schools is extensive, most of what is known about their institutional practices with respect to those involved with the juvenile justice system comes from the practitioner literature. The exclusionary practices identified in this literature and in limited academic literature can be classified as "push," "pull", or "de facto." Push factors refer to school policies and practices that aim to purge or screen out adjudicated delinquents from their student bodies. Those subject to these types of direct exclusion are referred to in the literature as "push outs." Exclusion via pull process occurs when participation in the stages of juvenile justice processing (e.g. court, detention) precludes one's attendance in school. De facto exclusion is exclusion by default. This occurs when juvenile justice and/or school agencies have no effective policies in place to allow students to move between the two educational domains without seriously disrupting their educational progress and

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schools have no programs in place to address the special challenges and needs of the delinquent population. These three types of exclusion are not mutually exclusive; a single institutional practice may manifest all three types.

As mentioned, the majority of schools have adopted a “zero tolerance” approach to serious school crime. Under such laws, schools in many jurisdictions were granted the statutory authority to exclude students whom they deem a threat to safety--however defined--in the school. Under this broad exclusionary criterion, some school districts like Chicago’s, define the commission of any ”serious” crime, whether occurring on or off campus, as grounds for removal and/or banishment from school. Similarly, Texas in 1995 passed a law mandating that students who have engaged in conduct punishable as a felony (even if it is not prosecuted as such or if prosecution is "deferred") be removed from school and referred to an "Alternative Education Program" (Bickerstaff, Leon, and Hudson, 1997). It would be instructive to learn how often schools transfer delinquents to alternative school or to other regular schools even in the absence of such policies.

Chicago Public Schools exemplify another form of direct exclusion. Since 2001, Chicago Public schools are no longer required to re-admit students who are returning from juvenile detention or juvenile corrections facilities. Rather, the policy stipulates that such students sent to one of the District’s “Regional Safe Schools” to undergo careful assessment and “reintegration training.” Under the policy, students who are deemed not ready to return to school presumably will be banished from public school despite legal entitlements to a public education until age 21.

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While such extreme forms of exclusion provide strong confirmation of my research hypotheses, they may be among the more rare forms. Researchers have noted a number of tools that educators and school officials have at their disposal that achieve the same outcome—exclusion—in a less direct and dramatic way. In the context of an "organizational commitment to ridding the school of its presumably 'difficult students,'" Fine (1986) describes how schools use subtle techniques like misinformation to encourage certain students to dropout. Other not-so-subtle techniques to coerce dropping out such as harassment and threats of transfer or expulsion have also been reported (Skiba & Peterson, 1999). Thus, the line between dropping out and exclusion is a fuzzy one. Both dropping out of school and being excluded from school may at times, result from institutional responses to justice system involvement.

The concept of exclusion is a fuzzy one in other respects. Youth who are expelled or otherwise pushed out of school, especially those young enough to live under compulsory education requirements, do not always stay home. The degree to which such institutionalized responses to criminal sanctions on the part of schools actually lower educational attainment depends in part on the frequency of transfer to alternative schooling arrangements and on whether being transferred to an alternative school actually lowers educational attainment. With respect to school achievement, the results on the effectiveness of alternative schools are mixed (Mathematica, 2000). It is clearly the case that some students enjoy the less structured and more flexible environment of alternative high schools (Macleod, 1995) and improve their effort and achievement (relative to similar students who stay in pubic schools) as a result (Gold and Mann, 1982). However, many alternative school environments appear less likely to increase a student's attachment to school. Labeling and defiance theories suggest that the most "popular" alternative

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school approach, which includes "the punitive procedures found in correctional institutions" (Portner, 1995) is unlikely to appeal to most students. The high dropout rates present in alternative schools has led some observers to describe the modal experience of alternative or transitional schooling as a stage in the transition out of school rather than as step toward a return to mainstream schooling (Kelly, 1993).

Outcomes of alternative schooling are likely differentiated by individuals as well as by schools. A reasonable supposition is that a student who is enrolled in school and doing acceptably well is more likely to drop out after being involuntarily sent to an alternative school than he would have had he been allowed to stay in a regular school. Struggling or rebellious students, by contrast, may stand a greater chance of improvement following transfer.

The next type of exclusion, pull processes is, to a large extent, true by definition. Each stage of the juvenile justice process--apprehension, pre-trial detention, assessments, court processing, incarceration, and visits with one's probation officer--automatically or potentially involves missing regular school. If attendance is one’s measure of exclusion or school performance then the effect of these type of juvenile justice involvement on these measures borders on tautological. What is not quite as obvious, however, is the extent to which repeated court visits, short detention stays etc. leads one to fall behind or lose interest in school once a student returns. We know that being absent or suspended from school is associated with lower school performance (Bonikowske, 1987; Jordan, Lara, and McPartland, 1996). However, in the juvenile justice context, forced absence from school often entails forced attendance in a detention center or prison school and/or forced attendance in school as part of court supervision or probation.

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It seems likely that the academic toll exacted by forced absence from school depends on the school's response to the forced absence, as well as the responses of students and juveniles justice professionals. This brings me to the final type of exclusionary practices, those that are de facto in nature. Such practices were the focus of one component of a joint Initiative by Office of Juvenile Justice and Delinquency Prevention and the Department of Education known as the Youth out of the Mainstream Initiative. The initiative mobilized policy makers and practitioners across various states to exchange information and "best practices" concerning the problem of reintegrating youthful offenders from correctional facilities into mainstream educational environments. Among the findings included in the report is that in some jurisdictions over 90 percent of adjudicated delinquents are unable to make a successful transition into any school environment following their release from a youth correctional facility (Stephen & Arnette, 2000).

The authors of the report suggest that reintegration difficulties typically do not result from direct exclusion but rather from a failure to accommodate the needs of delinquents. The authors describe the respective failure of juvenile justice and school facilities to tailor services and curricula to the needs of individual students. Efforts to do so are often hampered by a lack of information sharing between the two institutional domains. Furthermore, schools often have unclear or inconsistent policies with respect to the acceptance of credits earned in correctional facilities, especially when the facility's school lacks official accreditation, or when the admission of students occurs mid-semester. The policy established in Chicago and elsewhere (Jordan, Lara, and McPartland, 1996) of automatically dropping students who have twenty or more consecutive absences may have inadvertently dropped students whose absence were due to their juvenile justice sanctions and who may be reluctant to admit it (Sullivan, 1989). Finally, upon their

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return to school, students are rarely provided the tutoring or other services that they need to make a successful transition. Thus, the frequent failure of schools to accommodate the needs of students under juvenile justice supervision (and other truants) may result in lowered school performance (Wilhite and Cessna, 1996), and amount to their *de facto* exclusion from school.

Theories of Interaction

Embedded in each of the above theories is a set of psychological, social, or institutional process that supposedly occurs in response to a youth's juvenile justice involvement. The theories do not presume that these responses are automatic but rather probabilistic and non-stochastic. The conditional nature of the impact of sanctions has led at least one prominent sanctions theorist (Sherman, 1993) to decry the lack of research attention to the factors that condition the effects of sanctions.

In light of Sherman's charge to criminology, as well my own desire to render a faithful representation of the aftermath of sanctions, I now seek to specify several of the factors that may condition the effects of sanctions on school performance. Put differently, I am asking which conditions blunt the effects of sanctions or even yield a positive effect on school performance? Among the multitudes of possible conditioning factors to consider, I shall primarily focus on those that I will be able to examine empirically.

When reality contradicts core assumptions of the previous theories we can expect a reversal of the negative effect of sanctions. For instance, a central assumption of social embeddedness theory and, to some extent, labeling theories is that legal sanctions negatively influence the relationship between juveniles and their significant others. However, a study of juvenile offenders by Morash (1982) found that perceived negative labeling by police did not

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predict offenders' perceived negative labeling by parents, neighbors, and friends. The absence of ostracism, the literature suggests, is more likely to occur in the context of warm family relationships (McCarthy and Hagan, 1998) and in communities with such high rates of criminal justice involvement that being arrested and officially "labeled" no longer carries a stigma (Nagin, 1998). (The latter condition characterizes the vast majority of the communities providing subjects for my investigation and may explain a failure to reject the null hypothesis).

Under the foregoing conditions, one may even predict an increase in social support from concerned friends, neighbors, and family following a sanction. Acts of love and support in the face of punishment are capable of inducing the type of shame necessary for behavioral change (Braithwaite, 1989; Sherman, 1993). Those who feel the most shame (Grasmick and Bursik, 1990; Williams and Hawkins, 1989) and the most bonded to the sanctioner (Sherman, 1993) are the most likely to respond positively to the sanctioning of a deviant act. While no one appears to have studied these interactions with respect to juveniles, it is reasonable to assume that youth who are socially integrated into the community are the most likely to feel shame and bonding to the sanctioner. Trying hard in school may be a means through which such youth may restore eroded social bonds.

The effectiveness of social support in protecting juvenile offenders against the harsh consequences of sanctions is likely enhanced by several means. Several of these factors are evident in Sullivan's (1989) contrasting description of the effects of criminal justice involvement for youth in white and black Brooklyn neighborhoods. Many of the white youth had relatives in the police department and parents who were able to use their social and financial resources to "manipulate the system" and secure lenient treatment for the delinquent youth. One can

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reasonably surmise that youth possessing such forms of financial and social capital, will also have many effective allies in their attempts to stay in their schools against opposition from schools officials and to keep up with their school work. Moreover such parents may increase their monitoring of youth's social and academic behavior following sanctioning, which may result in an improvement in the youths' school performance (White & Kaufman, 1997; Clark, 1983)

Sullivan's work suggests other ways in which neighborhood social capital and social control may condition the effects of sanctions. In the white neighborhoods, the presence of jobs sustained various networks of reciprocal exchange. Generally speaking, in a context where multiple means of informal, collective community control (jobs, competitive sports etc.) exist, neighborhoods and schools may be less reliant on formal, exclusionary means of control (e.g. the expulsion from school of students who are adjudicated delinquent) (Braithwaite, 1989). However, such contexts may be extremely rare in U.S. cities owing to America's individualistic ethos, (Bellah, et al, 1985; Braithwaite, 1989; Messner & Rosenfeld, 2001).

The characteristics of teacher/student relationship may also condition these relationships. Since teachers interface directly with disciplinary authorities in the school and, at least in Chicago, have the discretionary authority to cause a student to be arrested or suspended, how well a student gets along with his teachers may be a critical conditioning factor. Teachers, who are endeared to a particular offender, may resist the negative label by the criminal justice system and, perhaps, provide extra assistance and consideration to youth hindered academically by their legal entanglements. Several school contextual factors, such as a high teacher to student ratio and a fair, flexible disciplinary climate, may be conducive to supportive teacher/student relationships.

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While my data does not permit me to test these interactions systematically, I am able to compare the impact of sanction in elementary schools and high schools, between which these dimensions likely vary in ways that favor softer impacts of sanctions on elementary school students.[2]

Lastly, one should consider some other factors which are shown to promote deterrence. After all, the decision to desist from crime is associated with a decision to conform to expected social roles such as that of a good father or good student (Sampson & Laub, 1993a). Some evidence suggests that young or novice delinquents, perhaps owing to greater trust in or fear of authorities, are more likely to cease offending once their behavior draws police attention (Smith and Gartin, 1989). Among novice delinquents, deterrence may be further conditioned by gender. Keane, Gillis, and Hagan (1989) found that, among females, police contact negatively predicts subsequent marijuana use (net of prior marijuana use) while, among males, it is associated with increased marijuana use. This difference may reflect the females' greater levels of social integration and social support (Braithwaite, 1989). To the extent that increased recidivism is associated with lowered school performance, girls and novice offenders should be less likely to experience negative sanction effects on school performance.

Explaining Sanctions

Before embarking on a study of the causal impact of juvenile justice involvement one must consider the role of factors that may confound the relationship between juvenile justice involvement and school outcomes. Youth who get involved in the justice system differ in many observable ways from those who do not. Likewise, various known factors explain why youth

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experience sanctions of varying type or severity. If these "selection" factors also influence subsequent school outcomes they may actually account for the relationship between juvenile justice involvement and school outcomes.

The concern that the apparent effects of juvenile justice involvement are an artifact of the selection process has received some empirical support. For instance, Smith and Paternoster (1990) raised the specter that the widely demonstrated deviance amplification effects of juvenile court sanctions on youth (Shannon, 1980; Gold & Williams, 1970), were actually due to the selection for harsher sanctioning of those juveniles who posed a greater risk of recidivism. Using three instrumental variable methods, a bivariate probit model, and the model of Barnow, Cain, and Goldberger to model selection bias and purge "the referral [to court] variable of its assocation with unmeasured correlates to recidivism" (p. 1124) they found that referral to court actually has a negative relationship to recidivism.

A positive association between sanctions and school failure may deservedly draw the same skepticism as the apparent delinquency amplification effects of sanctions. Thus, in order to accurately measure the causal effects of sanctioning on school outcomes, one must first ask which factors predict both sanctions and educational attainment and then rule them out as explanations of observed temporal associations.

First and foremost, punished juveniles may perform worse in school following their arrest than their peers, not because of the sanctions they endure but because those selected for sanctioning were already on a path of academic decline. Such youth may be more vulnerable to arrest if their pattern of school failure is linked to truancy. Truants are more likely to draw the attention of the police than non-truants because truancy is a status offense to which police can

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legally respond. In the 1992-93 school year, the Chicago school patrol unit rounded up 71,094 truants (Chicago Police Department, 1993). Truancy "arrests" acquaint police with particular youth and may increase a youths' likelihood of arrest in future encounters.

In addition to truancy, sanctioned juvenile offenders disproportionately experience other school problems. Finn, Stott , and Zarichny (1988) found that 70% of adolescents in juvenile court were "barely passing or worse" in their course work. While, this fact may largely reflect the relationship between school failure and delinquency (Gottfredson, 2001), an offender's problems in school may actually have a strong influence over the decision to detain youth and refer them for further processing (Leiber and Jamison, 1995). It follows that sanctioned youths' initially high levels of schools problems like absenteeism, poor grades or test scores, and retention may explain a greater subsequent likelihood of school failure rather than anything having to do with sanctioning itself.

In addition to prior school performance, several other variables that predict both sanctions and educational attainment are suggested by the literature. Again, emphasis is placed on variables which will be measured in the quantitative analysis.

1) Delinquency and Drug Use. For obvious reasons, various measures of the extent and severity of a youth's involvement in delinquency have a strong relationship to his or her being arrested and to the severity of his or her processing by the court (Sampson, 1986; Bishop and Frazier, 1996; Smith and Paternoster, 1990). Some types of delinquency also appear to be associated with negative school outcomes (Tanner, Davies, and O'Grady, 1999; Kaplan and Liu, 1994), particularly among lower income populations (Hagan, 1991; Jessor, Donovan, and Costa, 1993).

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2) Peer Delinquency. Youth who spend time with delinquent peers may draw the attention and punitive treatment of the police irrespective of their own involvement in delinquency. Youth are particularly likely to get painted with the same dark brush as their peers when they are members of a gang. Brownfield et al (2000) and Sampson (1986) were unable to find evidence from the late 1970's Seattle Youth Study that gang members face an elevated arrest risk. However, as Brownfield et al (2000) point out, owing to anti-gang hysteria beginning in the early 1990's, differential surveillance and arrest of gang members have actually been consecrated in policy and practice as exemplified by gang sweeps, gang injunctions, and gang loitering laws.

Delinquent peers and gangs may negatively affect academic performance through a couple of mechanisms. First, they may encourage participation in non-academic activities and devalue commitment to and participation in education. Second, association with deviant and gang-involved peers can provoke negative appraisals and treatment from teachers and other school personnel. Accordingly, a negative association has been detected between deviant peers and school achievement and drop out (Grenier and Roundtree, 1987; Kasen, Cohen & Brook, 1998)

3) Time Allocations. As the literature comparing middle class and lower class delinquents attests (Stinchcombe, 1963; Chambliss, 1999), not all delinquents and delinquent peer groups stand an equal chance of being arrested. Though it may seem obvious, delinquents are more likely to be arrested if they spend time with their peers (as distinct from simply having delinquent peers). The "group hazard" hypothesis (Erickson, 1973) holds that groups are more likely to draw police attention than individuals because of their higher visibility. This hypothesis

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receives some support in Brownfield et al (2000) who find that time spent "hanging around" with one's best friends outside of school increases one's risk of arrest. Also increasing one's visibility to the police are certain "routine activities" like spending time outside, particularly in public places. Especially, in jurisdictions like Chicago and New York that have clamped down on loitering and "quality of life" or "public order" crimes (e.g. drinking in public) spending time outside can increase one's chances of arrest.

Time allocations, of course, also influence school outcomes. Youth who spend most of their free time "hanging out" with one's friends outside tend to devote less time to their home work. Of course, youth who spend most of their free time indoors, while evading police scrutiny, are not necessarily doing their homework either. Research suggests that youth who engage in more passive activities (e.g. music and movies) are more likely to drop out while those who participate in more constructive activities (e.g. sports and hobbies) are less likely to drop out (Janosz et al, 1997).

4) Demeanor. The amount of deference demonstrated by offenders toward authority is another consistent predictor of their probability of arrest (Sherman, 1980; Worden, Shepard, & Mastrofski 1996) and the outcomes of court processing (Fenwick, 1982). As implied by the earlier description of the defiance process, one's attitudes and behaviors toward authority may predict one's demeanor with respect to teachers as well criminal justice officials. Negative teacher relationships, in turn, are associated with poor school performance and dropout (Meier, 1999).

5) Gender. Irrespective of their apparent involvement in delinquency, males are more likely than females to be arrested (Sherman, 1980) and to be treated harshly at various stages of

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juvenile justice processing (Bishop and Frazier, 1996). By contrast, gender often appears to lack independent effects in various models of school dropout (Kaplan & Liu, 1994; Meier, 1999). Yet, being male may lower grades, net of other factors (Meier, 1999; Quane & Rankin, 1998). However, these models often include variables, such as school discipline, that may mediate and, therefore, attenuate the effects of gender on educational attainment.

6) Race. As the "driving while black" controversy has highlighted, blacks are more likely than whites and Hispanics to be stopped and searched by the police (Bureau of Justice Statistics, 1999), which, in turn, helps account for higher arrest odds among blacks (net of criminality) (Brownfield, Sorenson, & Thompson, 2000; Sampson, 1986; Mauer, 1999). Limited evidence also suggests that these discrepancies hold true for pedestrian stops and among juveniles (Werthman & Piliavin, 1967; Anderson, 2000). Blacks and Hispanics are equally likely to be searched following a traffic stop and are each more than twice as likely to be searched than whites (Bureau of Justice Statistics, 1999). In potential arrest encounters, blacks are typically shown to be modestly more likely than whites to be arrested once the effects of offense severity and demeanor are controlled (Smith, 1986). No studies were located that compare the arrest risk of blacks with that of Latinos.

While blacks are more likely than whites to be arrested independent of controls for delinquency, they are also more likely than whites to perform poorly academically net of many background factors (Meier, 1999; Fordham & Ogbu, 1986; Roscigno, 1999) . This relationship is bound up with various family, community, structural, peer, and school factors (Roscigno, 1999) that also account for racial disparities in arrests—not all of which are likely to be measured in any one study. Latinos, particularly females, are more likely than whites to drop out

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of school (Valdivieso and Nicolau, 1992) a finding also bound up with cultural differences and ethnic discrimination. Consequently, race is an important control variable to include in a study of the impact of arrest on school outcomes even if race is only a proxy for other factors that influence school outcomes directly.

7) Socioeconomic Disadvantage. The impact of race on criminal justice decision-making and school outcomes is closely intertwined with the impact of social class or economic disadvantage. Some researchers (Chambliss, 1973; Brownfield, et al 2000; Wolfgang, Thornberry, & Figlio, 1987) have speculated and found evidence that police arrest decisions are biased against the poor or the working class. The first source of this discrimination may be the associations that police draw between poverty and criminality. The second source may be the greater vulnerability of lower income individuals to aggressive arrest tactics. Analyses of arrests in Seattle (Sampson, 1986) and contemporary New York City (Fessenden & Rohde, 1999) suggest that lower income groups are disproportionately subject to false or flawed arrests. Perhaps, higher income and better educated individuals are less susceptible to such arrests (and legitimate arrests for that matter) because they are considered more credible in court (Fessenden & Rohde, 1999) and in possession of the resources necessary to amass a good defense and/or pursue legal action against the police.

Socio-economic disadvantage also represents a handicap in the educational context. The poorest students often lack access to basic studying essentials such as school supplies, lighting, and study space. Further, the educational assistance offered by better educated parents tends to be of greater quantity and quality than that offered by less educated parents. Accordingly, researchers consistently find a positive association between measures of social class (e.g. family

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income, parents' education, occupational prestige) and indicators of school performance (Meier, 1999; Tanner, Davies, O'Grady, 1999).

8) Family Structure, Family Support and Control. In the same way that race and poverty may affect the judgments and decision-making of law enforcement, so may the related factor of family structure and relational dynamics. Probation officers and judges may view youth from *single parent families* as in greater need of intervention or supervision and therefore sanction them more harshly (Smith and Paternoster, 1990). Family structure does not appear to weigh independently into police arrest decisions, however (Sampson, 1986). Especially in anonymous urban settings, I suspect, police typically are not aware of a youths' family structure at the time of an arrest decision.

Even more important than family structure may be the level of support and control that families provide. In ways described earlier, strong relationships with family may, when used in combination with other sources of *social capital*, help sway juvenile justice authorities toward lenient decision-making (Hagan, 1994; Fenwick. 1982). Families which fail to provide effective monitoring of youths, on the other hand, may prompt the criminal justice system to take more coercive and restrictive actions with troubled youth (Fenwick, 1982). Furthermore, youth who spend more of their time under parental supervision are likely to spend less time with their peers, which, as discussed earlier, may draw police attention. No work was found assessing the impact of family support or control on the arrest decision.

Family structure (Meier, 1999; White and Kaufman, 1997; Myer et al, 1987), *family support* (Meier, 1999), and *parental control* (White & Kaufman, 1997) are all predictive of

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various indicators of scholastic achievement. Hence, these factors, if they influence arrest risk, could partially confound the impact of sanctions.

9) Extreme Stressor Events and *Mental Health.* As should be quite clear by now, not all delinquents stand an equal chance of being arrested. One of the factors that determine one's chances of being caught is the care one takes to avoid detection by the police. According to the deterrence and rational choice model efforts to avoid detection are a function of subjective perceptions of the certainty of being caught and the value one places on the perceived costs of apprehension which may include punishment by the police and courts and the disapproval of loved ones (Grasmick & Bursik, 1990). It follows that factors that can influence ones ability to make rational assessments of the certainty and severity of punishment and the concern one feels about social disapproval and other costs should affect the efforts that delinquents exert to avoid detection. A major stressor event like a death in the family or parents' separation, I surmise, can cloud judgment and induce a sense of apathy or ambivalence regarding the costs of offending. A more stable mental health measure, like depression, which is associated with feelings of worthlessness and apathy, may also be associated with a lack of concern about (or even a masochistic desire for) the negative consequences of arrest.

10) Neighborhood Factors. Neighborhood characteristics may influence both youths' chances of surveillance and detection by the police and their chances of being arrested following detection. Sampson (1986) found that interviewer ratings of the socio-economic status of youthful respondents' neighborhoods were strongly negatively associated with a the odds of arrest net of demographic factors, individual delinquency and other controls. The impact of neighborhood SES was so strong that it eclipsed all other factors save black racial identification

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and almost completely negated the impact of individual SES. Smith (1986) uncovered a similar finding in the context of a 1977 observational study on police arrest decisions in 60 neighborhoods across three metropolitan areas. He found that neighborhood socioeconomic status[3] was a robust negative predictor of the odds of arrest in police encounters, even after controlling for the characteristics of the offense and the offender. Underlying this finding may be a process of "ecological contamination" by which "all persons encountered in bad neighborhoods are viewed as possessing the moral liability of the areas itself" (1986, p. 316; see also Werthman and Piliavin, 1967). Interestingly, other possible sources of ecological contamination such as the crime rate in the neighborhood and percentage non-white in the neighborhood did not independently influence the probability of arrest. However, various elements of today's policing context—the racialized war on drugs (Chambliss, 1999), the use of reported crime data to allocate police patrols (e.g. "hotspots" policing and Compstat) (Parenti, 2000) and the emphasis on policing public disorder (Wilson and Kelling, 1982)--suggest that factors such as neighborhood racial composition, crime, disorder and other sources of ecological contamination may have gained salience as determinants of individual arrest risk.

Smith's (1986) work highlights other neighborhood factors that may play a limited role in the arrest decision. First, he found that *income heterogeneity* has a positive influence on the decision to arrest. This finding fits neatly within the social conflict or social threat framework (Liska, 1992; Sampson & Laub, 1993ab). Police may perceive a greater need to exercise coercive control in neighborhoods where relatively wealthy and influential individuals—who

[1] In contrast Sampson (1986), Smith employs aggregated citizen survey measures of median family income, owner-occupied housing, and percentage of family incomes above $5000 to measure neighborhood SES.

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view the poor and blacks as a threat and have greater political leverage—live in close proximity to the poor.

Income heterogeneity, like *ethnic heterogeneity, residential mobility, family disruption* and *neighborhood poverty* may also serve to undermine community cohesion and the ability of residents to enforce shared community norms (Shaw and McKay, 1942; Sampson and Groves, 1989). However, the direction of the impact of these determinants of neighborhood social organization and informal social control on individual arrest risk is unclear. Residents of neighborhoods with high levels of social cohesion, owing to greater personal ties with their neighbors, may be more inclined to call a misbehaving juvenile's parents than the police. At the same time, neighborhoods characterized by high levels of distrust and anonymity, may be more reliant on the police to exercise control and, therefore, have higher rates of arrest contingent on crime. Furthermore, residents of poor neighborhoods are less able to mobilize against the excessive police scrutiny and predatory law enforcement that often characterize the war on drugs (Tonry, 1995; Mauer, 1999) which may place the most powerless elements of society under an elevated risk of arrest (Lundman, 1975).

A lack of community cohesion and collective efficacy can work in the opposite direction, however. While some have bemoaned the over-policing of vulnerable communities (Parenti, 1993; Chambliss, 1995), an equally cogent group have shown that a lax police response to crime frequently characterize some of the most disorganized, crime-ridden and vulnerable African-American communities (Illinois Advisory Committee to the United States Commission on Civil Rights, 1993; Klinger, 1997). Likewise, some commentators (Bursik & Grasmick, 1993) suggest that neighborhoods containing strong local networks (e.g. block associations) and interlocking

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local institutions—which are constitutive of *social capital*—are better able to mobilize external resources like the police to respond to their complaints. Perhaps social capital—though the processes described above—simultaneously exerts upward and downward pressures on arrest. This could help explain why Smith found a negative impact of neighborly interaction on arrest risk but failed to find a greater likelihood of arrests in neighborhoods characterized by high residential mobility, high racial heterogeneity, high family disruption, and high levels of poverty *and* instability.

While the impact of indicators of neighborhood social capital on arrest risk is ambiguous, the predicted impact of neighborhood poverty, crime, and social capital on school outcomes is more consistent. A high level of neighborhood poverty and crime can lower the quality of students, teachers, and school activities in the schools students attend, and increase the fear of victimization and helplessness (Schmitz, 1992) as well as isolation from conventional norms (Wilson, 1987) that students experience. Schmitz (1992) even suggests that living in certain communities within neighborhoods such as public housing projects can function as a label which contributes to placement in less challenging classrooms environments and hinders their forging of social bonds to teachers and peers. While empirical evidence for the impact of social capital on school achievement is somewhat limited, Meier (1999) finds that proxies for individual access to social capital such as intergenerational closure, parental involvement in school, and the frequency of residential moves are associated with positive outcomes with respect to grades or dropping out.

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Working Hypotheses

The foregoing summary of the literature can be distilled into several research hypotheses. Theoretical and practical considerations guide my choice of hypotheses to test.

1) Being arrested has a positive causal impact on absenteeism, retention, and the chances of dropping out of school, while it negatively predicts achievement test scores and school grades, net of various selection and control factors delineated in hypothesis 3.
2) A negative impact of arrests on school performance will be less likely for female, novice and higher SES offenders; they will also be less likely when the student experiences juvenile detention, higher levels of parental support and control, warm and respectful relationships with teachers, higher neighborhood income, organization, and social capital.
3) Individual and peer delinquency, a juvenile's hostility towards authority, maleness, membership in a single parent family, negative life events, and neighborhoods levels of crime and disorder, are among the factors positively predicting the probability of arrest. Neighborhood social capital and family support negatively predict the probability of juvenile arrest.

Note that though much of the literature I reviewed concerns the effects of legal sanctions on behavior, my hypotheses concern the effects of arrest and ensuing juvenile justice involvement. This distinction should not undermine that relevance of this research to the criminological theory of the impact of legal sanctions as well as to juvenile justice policy. In the context of much of America's criminal justice system most relevant social processes and legal processing likely occur prior to any formal punishment. Nationally, in 1996 only an estimated 20% of arrests of juveniles resulted in formal adjudication of delinquency (Office of Juvenile

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Justice and Delinquency Prevention, 1999). To appropriate Malcolm Feeley (1979), in the context of American juvenile justice, "the process is the punishment"

Prior Evidence

Very few published studies have tested the first hypothesis. I reviewed evidence in support of some components of the third hypothesis earlier. Only an ethnographic study speaks directly to the second hypotheses—as well as the first--and thus merits first mention. Sullivan's interview data with young adults in Brooklyn, New York, revealed that the black and Latino youth who were under the supervision of the juvenile justice system had a difficult time attending classes and were sometimes transferred to other, less challenging schools. On the other hand, some students found that the teachers at the juvenile detention center actually renewed their interest in schooling. The students from the white neighborhoods, for reasons described earlier, did not appear to suffer because of their justice system entanglements.

Unfortunately, adequate statistical confirmation of these anecdotal findings apparently has not been published. First, three of the five published studies I found fail to clearly establish the temporal order between criminal justice sanctioning and "prior" educational goals and "prior" educational achievement (Monk-Turner, 1989; Janosz, LeBlanc, Boulerice, & Tremblay, 1997; Tanner, Davies, and O' Grady, 1999). If prior educational goals or performance are actually negatively affected by sanctions, this would result in an underestimation of the negative effect of sanctions on school performance.

This problem may help explain why Tanner, Davies, and O' Grady (1999) failed to find an effect of being "stopped by police, or booked, or charged, or convicted" on the highest grade completed and the probability of obtaining a high school diploma among a representative sample

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of adolescents. In addition, this study's inclusion of four separate control measures of delinquency (skipping school, drug use, property and violent crime)—each highly correlated with and potentially reciprocally related to criminal justice contact--may also explain the absence of an effect. Finally, their criminal justice contact involvement, which includes the common and relatively trivial "stopped by police," can, at best, provide a highly diluted (mean for males=.30) measure of involvement that one may not expect it to independently affect educational attainment.

Monk-Turner (1989), by contrast, employed a much more serious measure of involvement in the juvenile justice system—the status of being on probation from a correctional institution. However, this factor is also subsumed under an overlapping dummy variable representing whether a person was suspended or expelled in school or on probation from a correctional institution. The inclusion of this variable prevented accurate assessment of the effects of being on probation alone. The sizable unique effect of the study's joint measure of sanctions does not offer convincing support to a sanction effect either, because the model does not control for delinquent behavior.

Kaplan and Liu's (1994) examination of the mediating role of "negative social sanctions" in the effects of drug use on dropping out also employs measures with limited relevance to my hypotheses. To illustrate, their measure of negative social sanctions—like Monk-Turner's key sanction measure—combines both school discipline and police contact. However, the strength of their study is that they were able to demonstrate that sanctions strongly and positively influenced the probability of dropping out net of prior sanctions, prior delinquency, prior grades, and other important controls.

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Janosz, LeBlanc, Boulerice, & Tremblay (1997) provide the best recent evidence in support of Hypothesis 1. Utilizing 1985 cross-sectional survey data on a sample of 791 white French-Canadians, aged 12-16, this study's only two major weaknesses appear to be its lack of temporal ordering between sanctions and some of sanctions' hypothesized antecedents and its uncertain applicability to a U.S. sample. Nonetheless, the authors found a significant effect of the number of arrests on the probability of dropping out even after controlling for a multitude of variables I hypothesized to be predictive of both sanctions and school performance including school performance and commitment, family disruption, parental supervision, parental rules, level of identification with parents, delinquent behavior and norms, and respect of authorities. This finding is particularly impressive in light of the fact that some of these indicators are possible mediators, and therefore, attenuators of the effects of arrests.

Li (1999) examined the impact of a more serious level of juvenile justice involvement—delinquency convictions. Utilizing multiple waves of panel data collected from working class London boys beginning in about 1965, Li examined the impact of having a conviction between ages 14 and 16 on a construct of achievement at age 18-19, which included secondary school completion,[4] employment status, and job stability. Li found that, in structural equation models, convictions negatively predicted achievement net of social bonds, self-reported delinquency at age 14-15, convictions at age 10-13, and a set of psychological and social background variables.

The preceding review of prior evidence reveals several glaring gaps in the literature, which my study will begin to fill:

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[2] The construct included secondary school completion, employment status, and job stability. Secondary school completion was indicated by a subjects' taking of any General Certification of Education examination.

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1) No study in this field has included controls for contextual variables (e.g. neighborhood characteristics).
2) No study has reported factors that interact with sanctions in affecting school performance.
3) No study has measured the effects of sanctions on retention, absenteeism, achievement test scores, and grades.
4) No single study has systematically attempted to distinguish the separate effects of different levels of juvenile justice contact (e.g. arrests with and without juvenile detention, multiple arrests).
5) No American study has been conducted involving sanctioning events occurring after 1980 (and after 1985 for Canadian studies). The need for contemporary studies stems from changes in juvenile justice and educational policy and practice as well possible changes in neighborhood and juvenile arrestee demographics since 1980. For instance, perhaps, increasing intervention by the criminal justice system and school police decreased stigma overall while also resulting in the arrests of more "decent" students whose school performance has further to drop (Anderson, 1987; Lynch & Sabol, 1994, cited in Mauer, 1999).
6) No study has been comprised largely of young people in impoverished neighborhoods—where nearly every young male faces a sizable risk of arrest and, many youth, a risk of becoming seriously enmeshed in the juvenile justice system. Consequently, it is doubtful that most studies contain a sample of youth representing the wide range juvenile offenses and of sanctioning options available to the juvenile justice system (e.g. community service, long-term incarceration, transfer to adult court etc). Furthermore, surveys on more

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demographically diverse samples, while offering some advantages, may suffer from construct validity and reliability problems when varying cultural interpretations of survey items yield different responses even when the actual answers are identical (and vice versa).

7) No study has incorporated both quantitative and qualitative data.

Thus, the current literature on this topic, even if it were more methodologically rigorous, would still provide a very limited window into the world of juvenile offenders and their educational experiences following sanctioning. My empirical investigation is designed to assess, for a contemporary sample of inner-city students, the impact of juvenile justice on school performance and also to examine the mechanisms through which and the conditions under which juvenile justice involvement exerts it effects.

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CHAPTER 2: THE SAMPLING POOL

The Setting

The sample is drawn from some of the poorest minority neighborhoods in Chicago. A full understanding of the nature of my sample begins with an examination of the wider context of Chicago in relation to its poor. The first chapter described various features of the structural context in Chicago that likely have implications for the nature of the impact of juvenile justice involvement on school performance. Most notable are the installation of full-time police officers in most Chicago public schools, the "school accountability" movement that penalizes scores with low test schools and attendance rates, and exclusionary policies that mandate that schools be informed of a youth's arrest and that allow for exclusion of youth who are arrested or released from detention or correctional facilities.

Several other features of the wider context are relevant to the impact of juvenile justice involvement. First, court dockets are so crowded in Cook County that a single case can involve dozens of court visits. The number of continuances in Cook County court increased from about 100,000 in 1985 to 300,000 in 1994 (Reed, Perlmutter, Gill, 1997). 80% of Cook County cases originated in Chicago. The experience of going to court multiple times on a single case may not only be disruptive to one's studies but it may also spark feelings of defiance. On the other hand, perhaps crowded dockets in Chicago mean that cases are more likely to be dismissed earlier in Chicago than in other jurisdictions.

Another reason that arrests in Chicago may be unusually likely to spark a defiant reaction is that relationships between youth and the police in Chicago are quite volatile. 84% of 968

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Chicago high school students surveyed in 1993 reported either being treated disrespectfully or seeing others treated disrespectfully (Wisby, 1995).[5] Additionally, about half of the survey respondents reported having been stopped by the police unjustly.[6] Given that blacks are more likely to report distrust of and negative experiences with the police (Flanagan & Longmire, 1996; Wordes & Bynum, 1995), the relations between black and Hispanic Chicago delinquents and the Chicago police are, without a doubt, even more antagonistic than is indicated by surveys of general high school student populations.

Further evidence that low-income minority Chicagoans may not feel "bonded" to the police and other justice institutions comes from a large-scale survey of residents of most Chicago neighborhoods. Sampson and Bartusch (1998) report high levels of "legal cynicism" and "police dissatisfaction" in low-income black and Latino neighborhoods.

Some features of Chicago may have implications for the labeling process as well. Arrests of minority youth in some neighborhoods of Chicago may be so common that arrest no longer carries a stigma in such contexts. Not only do acts of school misconduct routinely land Chicago students in the custody of the police stationed in nearly every public school, but ordinary adolescent behavior on the street can also result in arrest.

In 1992, the city council passed a "gang loitering" law which permitted police to arrest anyone they suspect of being a gang member for congregating with no apparent legal purpose.

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[5] Disrespectful treatment included use of racial slurs, name calling, or swearing, or telling young people to "shut up."

[6] Wisby (1995) reports that 71% had been stopped by the police and more than 2/3 of these felt the police lacked probable cause to stop them.

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This crackdown resulted in more than 42,000 arrests in three years before a string of court decisions ruled the law unconstitutional (Spielman, 2000).[7]

State law[8] and Chicago's local ordinance described earlier may also facilitate the labeling process by mandating that police inform a school whenever one of its students is arrested. It is unclear, however, whether police actually provide this information to, whether it is disseminated in, and how it is utilized by schools. What is clear, however, is that schools have a right to use that information to exclude students. While Chicago public schools have long had statutory access to information on the off-site crimes of their students, they were permitted to use this information not to penalize students but, rather, to help them. In 1996, the Illinois State Legislature amended Article 22 of the Common Schools School Code to indicate that the purpose of the knowledge of students' crimes was no longer merely to help schools "rehabilitate" delinquents but also to "protect the safety of students and employees in the school."

Chicago during the 1990's appears to be a dynamic and politically charged setting with respect to the treatment of accused juvenile offenders by the police, juvenile justice agents, and schools. One cannot expect to gain a full understanding of these dynamics in one study, especially since they are steadily changing. However, one can be assured that many of the patterns I have uncovered in Chicago extend beyond its temporal and spatial borders.

The Comer School Development Program Evaluation

[7] Thanks to recommendations in a ruling by the U.S. Supreme Court, this law was recently revived and revised, but the new law took effect after the final year in my study range.

[8] Law enforcement and juvenile court personnel in Illinois are required by Article 22 of the Common Schools School Code to "…report to the principal of any public school in this State whenever a child enrolled therein is detained for proceedings under the Juvenile Court Act of 1987…or for any criminal offense…. The report shall include the basis for detaining the child, circumstances surrounding the events which led to the child's detention, and status of proceedings…" (Chapter 105, Sec. 22-20 Common Schools School Code, 1993).

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The samples for this research, including the qualitative component, are drawn from The Comer School Development Program Evaluation. This Evaluation involved twenty-two elementary schools located mainly in Chicago's South- and West- sides beginning in the early 1990's. The Comer model of school reform (Comer, 1997) calls for greater participation in school activities and policy on the part of parents and a greater understanding of human development on the part of teachers. To implement and evaluate Comer's vision in an experimental fashion, interested schools were randomly assigned to either the treatment or the control group. Treatment group schools' implementation of Comer-based reforms, while varying from school to school, consisted of the integration of mental health services into the schools along with efforts to democratize school management and to build cohesion among students, parents, and school personnel around shared school reform goals. Implementation was carried out by Youth Guidance, a Chicago youth services organization.

Schools selected for inclusion in the study were each led by principals who asked to be in the study. Ten of these schools, Phase I schools, began their participation in the fall of 1991 or 1992 and are located in neighborhoods on Chicago's south and west sides, which are among the city's poorest. The other twelve schools which began implementation in 1993-94 school year were termed Phase II schools (Cook, Hunt & Murphy, 1999). These schools were deliberately chosen from slightly less impoverished parts of the city (88% of their students came from homes eligible for free lunches versus 97% of the students in Phase I schools). The ethnic composition of the schools reflected the ethnic make-up of the surrounding neighborhoods. 74% of the sample were African-American; 22% were Latinos--nearly all of Mexican heritage and first or

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second generation; while 4% were "Asian-Americans of Chinese extraction and predominantly first generation." (Cook, Murphy, and Hunt, 2000).[9]

Schools were randomly assigned to the treatment and control groups at both Phase 1 and Phase 2 of the treatment assignment process. Overall, thirteen schools partially or fully implemented the Comer model between 1992 and 1997,[10] while nine schools served as controls.

Sample Representativeness

Drawing a sample from a pool of students whose schools participated in a school reform initiative may raise concerns that study's findings cannot be generalized to poor minority students in Chicago who did not participate in this intervention. The overall generalizability of my sample is already somewhat limited by the choice of one city and the fact that the Comer program was limited to twenty-two elementary schools. However, the sample is also ideally suited to test the impact of juvenile justice involvement on school performance. This is because people who live in the poorest minority neighborhoods tend to face the highest risk of juvenile and criminal justice involvement, as well as school failure.

That said, the generalizability of the sample outside of the schools and neighborhoods involved in the Comer program is much greater than one may expect, owing to the geographic dispersion of the sample over time. In the spring of 1995, the mid-point of the Comer evaluation, half of the 7,217[11] black students in the sampling pool still enrolled in a Chicago

[9] In-school testing was done in English, Spanish or Chinese.
[10] Three of these schools dropped out of the study before the conclusion of the evaluation period.
[11] 560 students enrolled that semester had an unknown ethnicity and hence are excluded for that figure.

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public school were residentially concentrated in only seventeen census tracts.[12] By contrast, by spring of 1999, when most of the sampling pool is still in high school, only 36% of the sample were concentrated in the most common twenty census tracts. This finding reflects the high rate of residential mobility present in the sample. 27% of the sample reported on one of the annual student surveys of 5th to 8th graders that their family had moved to a new home in the past year, while 16 percent reported changing schools.

Another way to assess geographic diversity is to examine how many of Chicago's school and census tracts are represented in the sample. Even in spring 1995 when 66% of the sampling pool were enrolled in school and were attending elementary schools participating in the Comer evaluation, the sampling pool was still dispersed across 454 schools in 576 census tracts.

Most students in the sample reside in and move to other poor minority neighborhoods. However, a substantial share reside in lower poverty neighborhoods. To illustrate, 63% of black students in the sampling pool enrolled in spring '95 resided at that time in census tracts with poverty rates of thirty percent or less. By comparison, only 48% of blacks in Chicago, according to the U.S. census, reside in such census tracts.

Due to the fact that the sample was deliberately concentrated in some of the poorest neighborhoods in Chicago, residents of public housing are over-represented in the sample. The experience of public housing residents may be unique in several ways that are relevant to this research. First, public housing developments exhibit the trappings of prison life including tight security, high fences, and random police searches (Wacquant, 2001). Juvenile justice

[12] This figure understates the residential dispersion because students who move often lie about their address in order to remain in their school.

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involvement may have less of a psychological impact for students who are accustomed to a carceral environment. Second, many crimes committed near housing projects carry greater penalties and may therefore be especially disruptive to education. Third, housing projects in Chicago face an elevated risk of erupting in sudden violence, which may lead to sudden changes in delinquency and school performance, as well as arrests. Such sudden, yet endogenous shocks pose a problem for statistical researchers who typically predict behavior in a static system or who model changes in behavior across a couple of distal time points. As many observers of high-rise public housing projects or other severely disadvantaged neighborhoods will attest, these places contain very few "static systems."

While the characteristics of the sample are likely generalizable to a much larger population of Chicago youth, the generalizability of any effects observed during the elementary school years are less clear. Specifically, roughly half the samples attended at some point a "Comer school," which may reduce delinquency (Cook, Hunt, & Murphy, 2000). Research in progress on a limited sample of evaluation participants (Vandna, Hirschfield, and Cook, in progress) suggests that the students and alumni of Comer and control schools exhibit comparable rates of arrest, but Comer schools appear to be less likely to graduate students who are detained or incarcerated during the elementary and high school years.

Comer schools may also respond differently to delinquents than typical Chicago public elementary schools. For instance, perhaps their integration of parents and mental health professionals into the school environment helps instill in school staff an understanding of child development—which can reframe delinquent behavior as a challenge of child development challenge rather than as an intractable pathology.

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However, as will become apparent later, a substantial share of the students in my sample were not in study schools at the time of their focal arrests. Dispersion throughout elementary schools not involved in the Comer evaluation as well as the city's predominantly minority high schools can only enhance the applicability of the findings to elementary schools and high schools attended by minority students.

Data Sources

While 11,255 students were surveyed at least once as part of the Comer study, only a small portion are eligible for inclusion in my quasi-experimental assessment of the impact of arrest on school outcomes. The most important and decisive selection criterion is that youth are arrested after completing a self-report survey. Because sample selection is dependent on the availability of data on school and juvenile justice experiences, a discussion of the data sources available to this project must precede discussion of the specific and complex selection rules employed.

The Student Survey

As part of the evaluation of the Comer School Development Program, survey data was collected on an annual basis from multiple panels of 5th through 8th grade students in the twenty-two public elementary schools in Chicago that participated in the Comer evaluation. Students from certain schools differ markedly in their probability of being surveyed on multiple occasions. In two of the schools, which were dropped from the study after the first year, students were surveyed only in 1992. In the other schools, surveys were conducted in either 1992 or 1993 and then were repeated annually through the Spring of 1997 (except for one which was surveyed only through Spring of 1996).

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Two separate student surveys were conducted annually in each school. The surveys were typically administered in the Spring, except for the 1992-93 when a base line survey was conducted with Phase 1 in the Fall. The first survey, an adolescent attitudes survey, measures a wide range of academic, behavioral, and attitudinal variables. The second, a school climate survey, measures student perceptions and attitudes toward school norms, instructional practices, teacher behavior, and the regulatory and disciplinary climate of the school. Specific indicators obtained from the surveys will be discussed later.

Official Student Records

The Chicago Consortium on School Research furnished individual students' records from The Chicago Public School System (CPS) from the 1991-1992 school year through Fall of the 2002-2003 school year.[13] Thus, for most students, educational records date back to at least 5th grade and extend through their high school years. For a sizable share of students, records date back as far as first or second grade. The only dependent variables of interest included in our records of the elementary school years, are grade retention, school exits, and achievement test scores. However, for the high school years, these records detail accurate information on these outcomes of interest as well as course credits, grades, and absences.

The school records also contain information which directly or indirectly indicates whether a youth was expelled, transferred to an alternative school, removed from school to enter a correctional institution, or attended school in a correctional institution. Other reasons for leaving school are also noted and help in examining attrition biases. Finally, school records contain the census tracts of residence during each semester in which a youth is enrolled in public

[13] The staggered replication analysis utililzes data primarily through the 1999-2000 school year.

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school. This information permits the merging of individual student data with neighborhood social climate data (see below).

Neighborhood Characteristics

Survey data on the level of disorder and social processes of 343 clusters of "socially similar and contiguous census tracts" (865 census tracts in total) (Sampson, Raudenbush & Earls, 1997) in Chicago was collected by the Project on Human Development in Chicago Neighorhoods (PHCDN). The survey, which ultimately involved 8,782 residents, was initiated in late 1994 and continued in 1995. Residents were asked a battery of questions regarding neighborhood social organization, social control, social life and their individual participation in them all.

Youth Arrest and Juvenile Justice Data

The following justice system data are employed in this project.

1) Youth Community Adjustment Records from the Chicago Police Department. These records contain information on youth who were arrested by the Chicago Police Department and then "station-adjusted." In such circumstances, apprehended youth are taken to the police station and then released to a guardian without charges. These records should contain all juveniles who are community adjusted from approximately 1983 through February of 1997, and provide indicators of prior delinquency and police contacts. These records provide a gauge of the initial parity between treatment and control groups in the staggered replication analyses.
2) Police Arrest Records. These records contain identifying information and information on the offenses for which youth have been charged. The majority of those arrested are also the

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subjects of a juvenile delinquency petition from the States Attorneys Office. These records contain all juveniles arrested in Chicago from 1983 until February of 1997.

3) Detention screening data. This data set contains records on all individuals detained for up to twenty-four hours (seventy-two hours if on the weekend) by the police and then considered for placement in pretrial detention. The data set includes information on offenses committed and detention placement received (either secure detention, referral to alternatives to detention programs, or release) as well as some recent prior history information. Given that the vast majority of arrests result in a detention screening hearing, this data set provides the most reliable source of information on first events after February, 1997 when the arrest data set no longer provides data. The data set extends from February 1995 through July 1999.
4) Cook County Clerk Records. Through spring 2001, I also have obtained records on juveniles petitioned to court and the dispositions of their cases. While a potentially rich data source in its own right, in this study it is used primarily to cross-validate the selection of first arrest events from the arrest and screening data and, occasionally, to provide new first events for analysis.

Merging Files

A prior step to running any analyses was to match the records from the disparate data sources. First, CPS and student survey (both attitude and school climate) data were matched by student ID number. Later, the neighborhood climate data from the Project on Human Development were linked to this data via the census tract in which a youth was living at the time of his arrest.

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The process of merging records from the Comer study with the hundreds of thousands of cases in the various juvenile justice data sets was arduous and complex. The presence of so many cases with identical identifying variables raises the possibility of false positive matches. At the same time, the presence of so many misspellings and other sources of mismatch can result in false negatives—youth being falsely labeled as non-arrested. Detailed information on strategies employed to minimize the number of false positives and false negatives are available by request from the author.

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CHAPTER 3: THE IMPACT OF FIRST ARREST ON SCHOOL PERFORMANCE: AN INNOVATIVE APPROACH

The main challenge of measuring the impact of arrests on school outcomes is that, unless arrests are assigned at random, confounding selection factors differentiate individuals who are arrested and those who are not arrested. Confounding selection factors are characteristics that are associated with both the independent variable(s) of interest (in this case, arrest) and measures of the outcome (subsequent school performance). It is virtually impossible to reliably control for all possible confounds of arrest and achievement because not all covariates of both arrest and school outcomes are known and measurable, and because a lag invariably occurs between measurement of the selection variables and measurement of arrests. This lag inevitably introduces unmeasured *historical selection factors* (Shadish, Cook & Cambell, 2000) (e.g. a gang fight at school, a mother's abusive boyfriend moving in) that contribute to arrests and school problems but occur after the measurement of the independent variables. For these reasons, omitted variable bias is the most intractable problems in the sanction effects literature (Smith & Paternoster, 1984), generating many false attributions causality to arrests and other forms of justice system involvement.

Researchers have developed several strategies to minimize selection bias in the assessment of sanctions' effects when, as is typically the case, random assignment is not an option. One approach to addressing the endogeneity of sanctions is the instrumental variable method (Smith & Paternoster, 1984). This method substitutes the sanction indicator for a variable indicating the probability of being sanctioned. This predicted probability must be derived from models of sanctions that incorporate strong exogenous predictors of sanctions that

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bear no obvious relationship to the outcomes. In this way, the independent variables are collectively purged of their relationship to the disturbance term in the models of the outcomes, and the effect of the probability of sanctions is an estimate of the effects of sanctions with minimal selection bias.

While instrumental variable methods appear successful in a study of the impact of juvenile court referrals on recidivism (Smith and Paternoster, 1984), applying these methods to the impact of arrests may be problematic. Arrests often have a strong component of luck (good luck from the perspective of the police officer and bad luck from the perspective of the apprehended). Therefore, arrests (especially arrests for victimless crimes) lack the strong exogenous predictors that do not also predict educational outcomes which are necessary to develop a reliable instrument. While one's chances of being arrested are certainly linked to such seemingly exogenous factors as district-level police organizational practices (e.g. arrest quotas, police management style) (Smith, 1984), such practices are difficult to measure across several police agencies and are extremely unlikely to bear sufficiently strong relationships to actual sanction risk to yield a robust model of arrest risk.

Like the instrumental variables approach, quasi-experimental methods also address the problems of endogenous predictors and omitted variable bias. This chapter describes a quasi-experimental method—the staggered replication design—that I will employ to examine the causal impact of first arrests on dropping out, retention, test scores and other school outcomes.

Research Design

The staggered replications design is a variation on the "switching replications" quasi-experimental design (Shadish, Cook & Cambell, 2000). "Switching replications" denotes a

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controlled design in which investigators administer an intervention to the treatment group and compare subsequent outcomes (time 2) to a non-equivalent control group, generally net of baseline (time 1) measures. Then, treatment is removed from the treatment groups and *switched* to the control group. The original treatment group then serves as the control group for a second set of comparisons involving time 3 outcomes (net of time 2 outcomes). If the effects of the intervention are the same regardless of which group serves as the treatment and which serves as the control, the validity of causal inferences with respect to the treatment is strengthened. In such a scenario, fixed differences (observed or unobserved) between the non-equivalent groups cannot account for the observed effects because stable differences cannot logically produce opposite outcomes at two observation points.

A genuine switching replications design cannot be reliably employed with respect to measuring the impact of arrests because the ensuing juvenile justice services and obligations which likely mediate the impact of arrests are "removed" at inconsistent intervals (and, for very serious charges, not removed for years). The data available to this project does not consistently and clearly mark the cessation of juvenile justice involvement.[14] Since most of the treatment group cannot be switched to the control group, I employ a *staggered* replications design, which is depicted in Figure 1. In this design, only the original control group switches to treatment group status for the time 3 comparisons. The control group for the time 3 treatment group consists not of youth who comprised the time 2 treatment group—as the switching replications design would entail—but rather of youth who will be arrested a year later.

[14] However, an imperfect switching replications design could still be carried out through such means as switching only those in the treatment group who were arrested in the first but not the second period.

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Hence the design as employed here better approximates a natural experiment than it does a controlled design. In contrast to the switching replications technique, which effectively reduces the correlation between the treatment and unmeasured selection factors by effacing the distinction between treatment and control, the staggered replications technique, as employed in this study, reduces omitted variable bias by matching those who are arrested during time period T_2-T_1 with those who will be arrested during time period T_3-T_2. These two groups are likely to share particular dispositions, propensities, and situational circumstances associated with being arrested that are not proximally measured.

		T1		T2		T3		T4		T5
	6^{th}		7^{th}		8^{th}		9^{th}		10^{th}	
N=97	NR	O_1	X	O_2		O_3		O_4		O_5
N=197	NR	O_1		O_2	X	O_3		O_4		O_5
N=243	NR	O_1		O_2		O_3	X	O_4		O_5
N=161	NR	O_1		O_2		O_3		O_4	X	O_5

Figure 1. The Staggered Replications Model. (NR= Non-random assignment; O= Outcome Measurement Point; X=First Arrest)

Rather than situating the outcomes along a fixed time interval, outcomes are arrayed on the school calendar, which varies slightly from year to year. Furthermore, unlike many school interventions, the "intervention" whose effects I am measuring can occur during any school year from 1992-93 to 1998-1999 (plus the summer before the 1999-2000 school year). Given that too few arrests occur during each of these school years to make separate analyses for each school

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year possible, I instead combine all school years into three separate sets of comparisons--one set for each grade, seven through nine. That is, all youth arrested in a selected grade (say, seventh) are treated as a single "treatment group" regardless of the school year in which the arrest took place.

The first treatment group consists of youth who are first arrested in the seventh grade.[15] The corresponding comparison group consists of youth also in the seventh grade whose first arrest does not occur until eighth grade. In the second replication, the control group from the seventh grade replication becomes the treatment group in the eighth-grade comparisons. In the third and final replication, the control group from eighth grade replication becomes the treatment group in the ninth grade comparisons. The control group for the final replication consists of youth who were first arrested in tenth grade. The replications are staggered across three grades, which earns the design the name "staggered replication."

Some may question my choice to treat juvenile justice involvement as an event rather than as a series of events or stages. The work of Sampson and Laub (1997) suggests that legal sanctions exert their effects on an individual's life course not through single events but as part of an interactive process of deviance and institutional reactions that leads to cumulative disadvantage over time. According to this perspective, the effect of a single arrest, especially the first arrest, may be negligible.

Despite the greater construct validity offered by modeling sanctions as a cumulative, iterative process rather than as an event, I choose an opposing approach, mostly for reasons of

[15] Models of achievement test scores include in each treatment group youth from the previous grade who were sanctioned after that year's achievement test was administered. Likewise, the treatment groups from a particular grade exclude those sanctioned after that grade's tests were administered.

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internal validity maximization. I model the impact of first arrests, and whatever court processing and further arrests occur in the interim, on school outcomes that semester or school year. I measure the impact of an event rather than a cumulative measure of sanction experiences in order to minimize the respective time lags between measurement of selection factors, arrests, and outcomes, thereby reducing the threats to internal validity arising from selection history and selection maturation.

I choose to focus on first arrests for three reasons. First and foremost, measuring the impact of first arrests ensures that the effects of juvenile justice involvement are not tightly bound with the effects of prior justice system involvement and that the control group is a truly "untreated" comparison group.[16] Second, had I chosen to focus on only second arrests my sample would be much smaller, given that many youth are arrested only once. Third, the choice can be justified on theoretical grounds. Arrests are accorded special significance in labeling theory; the initial imposition of the official label of "delinquent" is theorized to be highly consequential with respect to subsequent delinquency and school performance. Subsequent arrests occur after the label has been imposed and may therefore be less significant. The fourth reason applies only if the opposite is true. That is, first arrests are handled more leniently than later arrests as a matter of policy and practice. Under such conditions, the choice of first arrest permits the most conservative test of the theory.

[16] Both the treatment and control groups may exhibit prior justice system involvement in the form of arrests resulting in a "station adjustment" or "community adjustment." These experiences typically entail a short ride to the police station, a stern warning, and two hours in a holding cell or room—give or take an hour depending on how long it takes for a guardian to arrive at the police station to pick up the youth. These experiences, though prompting minimal legal consequences, can still reflect and exacerbate negative behaviors and interpersonal conflicts in a youth's life. Therefore, the number of prior station adjustments is treated as a potentially confounding variable in some analyses.

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Sample Selection Criteria

As explained above, three separate sub-samples from the Comer School Development Program evaluation are required for this research. To be eligible for the "treatment group" in each sub-sample, a youth must have experienced his or her first arrest while enrolled in the focal grade for that sub-sample (seventh, eighth, or ninth). To qualify for each sub-sample's non-arrested control group, a youth must have been enrolled in the focal grade exactly a year prior to his or her first arrest.[17] Furthermore, each treatment youth must provide indicators of various selection factors (most of which were measured on the student attitude and behavior survey) antecedent to the first arrest. For the control group, the selection factors must be measured prior to the date exactly one year before the first arrest, heretofore referred to as the comparison date.[18] I turn now to the mechanics involved in rigidly applying these selection criteria.

First Arrest Requirements

Students were eligible for the analysis only if their first arrest could be identified. Owing to data limitations, this determination could not always be made with absolute certainty. As

[17] The choice of an arrest date for the control group is somewhat arbitrary. This is due to the fact that the timing of arrests is more ambiguous for the comparison groups than it is for the treatment group. By definition, the control group is *not arrested* during their comparison year. While selection of a "non-arrest" or comparison date is arbitrary, some means to assign this date are better than other means. In order to achieve parity between the treatment and comparison groups with respect to measurement reliability, the ideal comparison dates would exhibit an equal temporal distance from measurement of the selection variables on the preceding survey and the outcome variables (generally measured at the end of the school year). The comparison date that best achieves this purpose (while also ensuring an equal distribution between treatment and controls with respect to seasonal variation in sanctioning) is exactly one year prior to the actual arrest date. These comparison dates minimize total variation between treatment and control groups in the lags between measurement of the antecedent variables, the events, and the outcome.

[18] This criterion is relaxed slightly for the control group under conditions specified later in the paper.

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noted in Chapter 2, arrest data is available only for Chicago and only through January 1997. Given that each arrestee is assigned a unique ID in the arrest file and the Comer and arrest data sets were matched on multiple sets of individual identifiers, fairly reliable information on first arrests were obtained from the Chicago police department arrest file. First arrests subsequent to January 1997 were generally identified from the detention screening file and occasionally from the juvenile court petition file. Missing from the detention and court files, obviously, are arrests that did not result in a screening or a court referral. It is crucial that youth who serve as controls for the youth first arrested in seventh grade are not first arrested until eighth grade, and so forth. Undetected prior arrests can violate the integrity of the research design, as the timing of one's first arrest is pivotal to treatment group classification.

Whenever possible, youth who were arrested prior to their first arrest recorded in the detention screening or court petition files were identified and omitted from the sample. Two sources provide limited information on the occurrence of such arrests. First, the official school data reports whether a youth's most recent school at the time of the fall or spring "snapshots"—which occur at the beginning of October and the beginning of May, respectively, of each school year—is the detention center or jail school. Second, the screening instrument reports whether a youth has experienced any arrests or court referrals in the past twelve months.[19]

Among the remaining arrests identified as debut events after January, 1997, an unknown share might have prior arrests that are not recorded in either the school or detention screening instrument. What follows is a meticulously detailed description of an analysis I conducted to

[19] Arrests within the past twelve months are reported in the first screening instrument through the summer of 1995, while prior court referrals are recorded in succeeding years.

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show that the share of cases whose false identification as first arrests could undermine the integrity of the design is quite insignificant. Readers who are inclined to accept such assurances at face value and wish to keep their minds uncluttered by trivial details are advised to skip to the next section.

To estimate the extent of first arrest misidentifications, I measured, during the years when all data sets are available, the share of first recorded screening or court events that were preceded by earlier arrests that are not detectable through means other than the arrest file. 556 first known screening or court events occur from February 1995 through January 1997 when all three data sets provide overlapping information. 307 of these first recorded events are court petitions; 249 were detention screenings. Among the 307 court events, 239 have first arrests within 45 days prior, 95% of which appear to be the arrest that resulted in the first recorded court cases.[20] Thus, petitions whose precipitating arrests were more than 45 days prior are more likely to draw false first arrest attributions. Still, even in the worst case (and highly improbable) scenario that all of the petitions coming 45 days or more after the first arrests were precipitated by a different offense than that which generated the first arrest, we estimate that twenty-six percent of the court petitions are vulnerable to a false label of first arrest.

Turning next to detention screening events, it was determined that 204 of the 249 first detention screening events come within 48 hours after the first arrest. Thus, a maximum of 18% of the screening events could have been mislabeled as a first arrest if no arrest data were available. Combining these upper-bounded estimates and taking into account that the detention

[20] A sub-analysis comparing specific charging and police district information for the arrests and petitions suggests that at least 95% of these first 239 arrests match with the first petitions.

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screening file was the source of the vast majority (all but 36) of post January 1997 events[21], first occurring events after the period of overlapping data sets fail to correspond to the first arrest a maximum of less than twenty percent of the time.

While a maximum false identification rate of twenty percent may seem problematic, only a minority of the misidentified cases pose a threat to internal validity. A falsely labeled first event is most problematic when the actual first event occurs during or prior to the school year in which an individual provides a comparison group observation. This condition results in measurement error on the most crucial independent variable—"treatment" status (arrest status during that grade). By contrast, if the real first event occurs during the same school year as the observed first event and after measurement of the antecedent selection variables, no measurement error on the key independent variables will occur. Small to modest levels of measurement error would occur only for variables that indicate the time lapse between the event and other dates as well as the characteristics of offense. The time lapse variables include time between survey participation and the event and age at event.

The closer an observed first event is to the actual first event the less overall measurement error will exist. Nearly half (thirty) of the 63 first court events whose arrest events are more than 45 days prior had actual first arrest events that occurred during the same school year as the petition. This means that the share of petitions falsely labeled as first arrests *and* actually posing a serious threat to internal validity as a result is less than thirteen percent.

[21] This reflects the fact that the initial samples for this research were drawn prior to the receipt of the juvenile petition file. After merging in the petition file, I omitted screening events whose prior petitions occurred during a previous school year. Thus, several cases whose first recorded event was a court petition were omitted from the analysis for practical reasons, resulting in a disproportionate share of cases retained for the samples whose first recorded events were detention screenings.

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Among the forty cases of detention screening events more than two days after an arrest, fifteen exhibited a first arrest and screening event during a different school year. This brings the estimate of similarly problematic screening cases down to only ten percent. While thirteen and ten percent maximum error rates are not trivial, they are small enough that inclusion of problematic cases would only modestly bias the results (and would work against a treatment effect in any case), and their exclusion would not greatly diminish the external validity of the results.

However, the threat to internal validity due to falsely labeled first arrest events increases when outcomes are semester-to-semester changes as opposed to year-to-year changes. In such situations, when the actual first event comes during the previous semester, it precedes the selection variables that are measured at the end of the previous semester. To the extent that these measures are influenced by arrest events, they may mediate and therefore attenuate the measured impact of sanctions on school performance that semester. I must stress, however, that these errors will operate in a conservative direction, and that the results of such analyses should be viewed accordingly.

Aside from the roughly ten percent of cases that may pose a serious threat to internal validity owing to the occurrence of a first arrest in a previous school year (or semester), one should also consider the very small share of cases whose first arrest is correctly identified with the first screening or court event, but which is assigned to the wrong school year (and school grade). To explain, first screenings or petitions sometimes occur during a different school year (or semester) than the arrest that eventually generated the screening hearing or court petition. Since juveniles must be screened for detention within 72 hours but may be petitioned to court

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anytime after an arrest, this uncertainty is of potential significance only for events generated by the petition file. Furthermore, given that petitions generally follow detention screening hearings, only a small percentage of cases are generated solely by the court petition file. Among events that will be selected for one or more staggered replications according to the criteria described in this chapter, only thirty-six were generated exclusively by the court petition file. Since the average lag between arrest and matching court petitions is only three weeks, court petitions occurring in the summer before the second week of August (five to six weeks into the summer) are the only court petitions whose corresponding arrest stands a reasonable chance of occurring during the previous school year. Of the 36 court petitions, only five (14%) occurred before August 7th and only one within three weeks of the start of the summer vacation.

Grade requirements

Samples were selected for three separate staggered replications, depicted in Figure 1. As is evident from Figure 1, the youth in each sample were first arrested in seventh, eighth, ninth or tenth grades. Each control group is arrested in the grade subsequent to the grade of the treatment group. The nature of the design and the data necessitated two exceptions to these selection criteria, however. The first exceptions are analyses involving achievement test scores. Achievement test scores are measured in March or April each school year.[22] A child who is sanctioned after test administration (say, in late May of eighth grade) cannot be included in the treatment group for the eighth grade replication because the outcome would precede the treatment or the first arrest. A wiser alternative to excluding such cases from the test score

[22] During high school years, not all schools administer the achievement tests that generate the test score measures employed in this study in every grade, which is one of the reasons I do not include an analysis of high school achievement test scores.

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analysis is to treat their arrests as having occurred in ninth grade, although they actually occurred in eighth grade (and controlling for grade of arrest if there is reason to suspect that the rate of grade mismatch differs by treatment and control or that one's grade at the time of sanction conditions the impact of the sanction). Correspondingly, such youth would be included in the eighth grade control group rather than the seventh grade control group.

This strategy should not result in younger treatment groups than control groups. The eighth grade treatment group includes youth who were actually sanctioned at the end of seventh grade, while the eighth grade control group also includes seventh graders, who were not sanctioned until the end of eighth grade. Note that this decision rule calls for the exclusion of some youth arrested in tenth grade and for the inclusion of some youth arrested in sixth grade from the study.

The second source of treatment group and grade mismatch with implications for sample selection is grade retention. Grade repetition is particularly problematic if it is concurrent with the sanction or comparison school year. The issue that arises in that case is whether to treat youth who were sanctioned during their second attempt at, say, eighth grade, as eighth graders. Should they be compared to youth who are enrolled in eighth grade for the first time?

The answer to this question depends on our assumptions and the particular dependent variable of interest. I will describe the grade requirements for the dropout analysis first. If first time and second time eighth graders faced similar odds of dropping out, then treatment group assignment based on grade alone would be permissible. However, we know that dropping out is conditioned by age as well as by grade in school—a distinction that becomes particularly evident

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in high school, when youth drop out in large numbers and schooling attendance becomes voluntary after a certain age irrespective of grade.

A resolution of this matter was only necessary with respect to the ninth grade comparison, because, as we shall see, dropping out is too rare in seventh and eighth grade to merit analysis. While dropping out during or just after the repetition of eighth grade is quite common in my sampling pool (more common, in fact, than dropping out after ninth grade)[23], such behavior occurs under very special circumstances. Since 1997, eighth grade retention in Chicago has generally resulted from poor performance on a set of standardized tests, preparation for which is the focus of considerable effort on the part of teachers and students. Furthermore, youth who are deficient in a particular skill are sometimes mandated to attend one of the city's regional "transition schools" which may be inconveniently located and lack any of the attractions of regular high school (e.g. sports, curricular choice, cheerleaders, etc). For this reason, it does not make sense to treat this group of youth either as eighth graders or as ninth graders; they are in limbo.

The control equivalents of such youth (who for the sake of parity need to be excluded as well) are youth who are sanctioned in ninth grade but whose comparison year occurred during a repeat of eighth grade. While such youth are indeed excluded from the control group, they are allowed in the treatment group and treated as ninth graders. Their inclusion in the treatment group is permissible as long as their control equivalents are also included. Their control

[23] To illustrate, 18.3% of youth who repeated eighth grade in the 1997-1998 school year dropped out by the fall of ninth grade, compared to 8.6% of ninth graders who were in eighth grade during the previous school year.

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equivalents are youth who are arrested in 10th grade, whose comparison year is ninth grade, and who were retained in eighth grade.

While special circumstances surrounding retention in eighth grade make it difficult to treat the eighth grader retainees as ninth graders in the drop out analysis, ninth grade retention is a different story. In contrast to eighth grade retention, retention in ninth grade does not result from test scores; it simply represents the failure to earn enough credits to be considered a sophomore. Ninth grade retention mandates repetition of only the required courses that one has failed and does not entail the transfer to a transition school. Thus, ninth grade repetition is simply an indicator of school failure. Given that dropping out is an increasingly age-based phenomena as one proceeds through high school, I see no reason not to treat those who repeat ninth grade as tenth graders in the dropout analysis.

This decision rule results in the inclusion of youth sanctioned during their second year of ninth grade not in the ninth grade treatment group but in the ninth grade control group. Correspondingly, youth whose comparison year falls during the repetition of ninth grade are excluded from the control group. Interestingly, including such youth in the control group would actually result in greater age parity between treatment and control, owing to high rates of retention in earlier grades among the treatment group. However, greater age and past grade retention parity is not worth the loss of balance in dropout propensity that would result. Repetition of ninth grade, which occurs among about forty-six percent of youth first arrested in ninth grade or tenth grade, typically results from the failure to pass most of one's courses. Most of these youth are disengaged from school and frequently drop out within a year or two.

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Including youth who have just repeated ninth grade in the control group but not in the treatment group unduly biases the results in the dropout analysis against a treatment effect.

While assumptions about the relative importance of age and grade with respect to dropping out are open to interpretation, these assumptions are unambiguous with respect to test scores. Youth repeating a grade also repeat the achievement test given to students in that grade. Their test scores for that test are not directly comparable to the other students also sanctioned in that grade because the latter group of students tend to be younger, with only one year to learn the material covered on the test. While repeat students can be manually promoted in the analysis, and variables measuring age and the actual grade in which the test was taken can be statistically controlled, a simpler, preferred solution is to exclude any student who had already taken the grade-specific test in question from the test score analyses.

All students retained during or just before the grade of arrest (or the comparison grade for controls) must also be excluded from the analysis of retention because different standards and norms presumably apply to the retention of students who have already been retained once for that grade. While social promotion no longer gives students a pass the first time they fail a standardized test, it does seem to be at play in the promotion of students who have already been retained once.

Survey Requirements

To be included in a staggered replication as a treatment or a control case, one's treatment year or comparison year (with a few exceptions for the latter described below), respectively, must be preceded by an attitude and behavior survey. This criterion is important because the attitude survey measures key antecedent selection variables like delinquent behavior. Thus,

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inclusion based on this criterion is contingent upon the timing of arrest and the timing of the attitude surveys.

For the treatment group especially, it is imperative that the attitude survey precedes the arrest date. Otherwise the effects of arrests may be mistaken for causes (selection factors)—despite the fact that many survey measures, like prior delinquency, reference a period six months or more prior to the survey date. Unfortunately, only the month and year of survey participation are typically known. Thus, each survey date was computed as the 15th day of the known month of survey administration.

The precise temporal ordering of the survey dates and the comparison date is less important for the control group. For the control group, including a "non-arrest" date shortly before that month's estimated survey date (say, on the 11th of that month) is not problematic because, even if the actual survey was not administered until the 22nd of that month, one can reasonably expect that constructs measured on the 22nd are excellent proxies for what such constructs would have been on the 11th.

One should not take the proxy notion too far lest one introduce unwanted variation between treatment and control groups in sample attrition and in the reliability of variable measurement. However, compelling practical reasons justify including a case in the control group even if the antecedent variables are not measured until after the comparison date. I permitted cases in the control group to violate the sequential ordering selection factor measurement and comparison date only if the nearest survey was within two months after the comparison date *and* no other prior attitude survey measure existed. These strict criteria for the

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relaxation of temporality requirements resulted in the eligibility of only five additional cases as seventh grade controls and six cases as eighth grade controls.[24]

Enrollment Requirements

The next important sample selection criterion is that students must be enrolled at the time of arrest for treatment cases or one year prior to arrest for control cases. This requirement is not absolutely necessary, given that those who are not enrolled at the time of arrest may re-enroll and still possess measures of both the control and outcome variables. I exclude non-enrolled cases to increase the level of initial similarity between treatment and control cases.

The decision rules for determining enrollment status are complex and need not be fully elaborated here. Primary indicators of enrollment at various times of the year included separate indicators of the last date enrolled as of a fall "snapshot" in early October and a spring "snapshot" in early May, last date left school as of the October and May snapshots, and a binary indicator of enrollment status measured in early October and early May. The determination of enrollment status required consideration of other factors when the arrest or comparison date occurred from December (two months or more before the fall snapshot) through April (prior to the Spring snapshot) and the spring enrollment date was either blank or subsequent to the arrest. A subsequent spring enrollment date occurred if a student either transferred to another school or entered the detention center school following the event date. For ninth grade students in these circumstances, determination of enrollment was based on additional criteria such as whether a student earned course grades or credits during the semester in which enrollment was in question.

[24] No ninth grade controls cases are added because elementary school surveys are always measured before ninth grade.

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An infinite set of contingencies necessitated making many determinations on a case by case basis. For instance, ninth grade students arrested early in the spring semester with excessive absences in the fall semester and detention center enrollment at the time of the spring snapshot were generally not considered enrolled. However, if the student also had an achievement test score from an April test prior to detention center admission or had occupation listed in the arrest file at the time of first arrest as "student," this determination might change.

Students prior to ninth grade were generally more difficult to classify because data on course absences and credits are not available from the elementary school years. On the other hand, the date of a subject's completion of an attitude and/or climate survey provided additional indicators of enrollment during the elementary years.

When no other determination could be made, instances of possible or probable enrollment were marked as such so such cases could be omitted from some analyses. Among students with dubious enrollment status, students who are 14 and under are more likely than older students to be recorded as probably enrolled simply because younger students are less likely to have dropped out of school.

Initial Sample Selection

Using the criteria described above, both treatment groups and comparison groups were selected for three sets of staggered replications. To recap, the treatment groups consist of youth whose first recorded sanction event (arrest, screening, or court petition) occurred while the student was enrolled in seventh, eighth, or ninth grade and after the youth completed a survey on attitudes and behavior. Inclusion in the comparison groups is based on the same criteria, except

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that a comparison date—one year prior to one's first recorded sanction event—is used in place of first recorded event.

Specific refinements to these sample selection criteria such as grade level requirements are described in the analyses involving each outcome variable. Chapters six and seven are devoted to the measurement of the impact of first arrest on elementary and high school outcomes respectively. The outcome that deserves first consideration in both elementary and high school is dropping out, because dropping out and other forms of attrition from the school records condition the interpretation of the remaining analyses. With respect to the analysis of grade retention, high rates of attrition may impose downward bias on retention rates since those who drop out are more likely to have been retained had they remained in school. In an analogous fashion, the results of both the dropout analysis and the retention analyses, in turn, condition interpretation of the test score analysis.

Thus, the first staggered comparisons should be of the risk of dropping out. Table 1 (columns b and c) present dropout base samples which, in accordance with the sample selection criteria, include youth who were both enrolled in school and surveyed prior to the arrest or comparison date.[25]

[25] The seventh and eighth grade treatment group includes youth who were sanctioned during the repetition of the previous grade and therefore were one grade behind most the rest of the treatment group. The control group, by contrast, counts youth who are retained during the comparison year as being in their actual grade during the comparison year. Thus, the eighth grade treatment group in the dropout base sample includes youth who are in seventh grade for the second time. Youth whose comparison date falls during the second seventh grade are treated as seventh graders and are therefore included in the seventh grade comparison group (and the eighth grade treatment group). Likewise, the eighth grade control group includes youth who are in eighth grade for the second time. The ninth grade drop out sample, by contrast, is limited to youth whose arrests or comparison dates occur during one's first year in high school. The retention samples, presented in columns d and e, like the ninth grade drop out sample, do not include any youth whose arrest or comparison year is a repeat of the previous one.

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As is apparent from Table 1, not all cases who are in a treatment group are also included in the control group for the previous staggered replication. Most glaringly, the control group for the seventh grade staggered comparison is 28 percent smaller than the treatment group for the eighth grade staggered comparison. This disparity reflects the fact that a substantial share of youth whose first recorded events occurred in eighth grade participated in a survey prior to this event but did not participate in a survey before or within two months after their comparison dates.

Table 1
Sample Sizes for Various Dropout and Retention Analysis

	Base dropout sample		Base retention sample	
Staggered comparison	Treatment	Control	Treatment	Control
7th grade	97	142	93	135
8th grade	197	254	188	244
9th grade	243	158	243	158
(a)	(b)	(c)	(d)	(e)

While the base samples appear large enough to provide sufficient statistical power in analyses of most outcomes, whether this is true for binary outcomes depends in part on the distribution of the dependent variable. The sample size may prove inadequate if the dependent variable of interest is a rarely occurring event. Thus, the next preliminary analytical step is observing the distribution of possible dependent variables.

The first dependent variable considered is dropout status by the end of the school year. Table 2 presents the share of each treatment group across each staggered comparison who dropped out by the end of the school year along with a separate percentage of youth in the sample who left by the end of the school year for reasons other than dropping out (e.g.

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transferred to another school or attended school in juvenile detention or other correctional setting). Youth who left the school for reasons other than dropping out are excluded from the denominator in the dropout rate calculation.

The rate of dropping out (number dropped out/valid N) is so low among seventh and eighth graders that the base sample would be too small to detect any statistical effects. The low drop out rate even for ninth graders likely reflects the reality that dropping out represents a cyclical process of failure (or deviance) and negative reaction from others or self, followed by more failure and disengagement from schooling rather than as a distinct event occurring at a precise moment in time (Finn, 1989). Given the lag between the onset of serious school failure and one's actual departure from school, it makes sense to measure the lagged effect of sanctions on dropping out. Extending the lag too far would be problematic, because, as the lag between measurement of the treatment and outcome increases, threats to internal validity can mount. With the goal of raising the prevalence of dropping out in the sample without unduly elevating the threat to internal validity, I decided to extend the range of the dropping out measure to any time before the fall snapshot after the event.[26]

The overall incidence of next fall dropping out among constituents of the seventh grade and eighth grade staggered comparisons is still too low—2% and 2.6% respectively—to permit fruitful multivariate analyses of these outcomes with my modest sample. Among constituents of the ninth grade staggered replication, on the other hand, the overall incidence of dropping out doubles from 7% by the end of the school year to 14% by the fall snapshot. This suggests that

[26] Students who drop out by the end of the school year but re-enroll in time for the fall snapshot are still recorded as dropouts.

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Table 2

Enrollment Status by the End of the School Year among Base Sample by Treatment Group and Staggered Comparison

	Treatment				Control			
Staggered comparison	Valid N (total N)	Enrolled (%)	Dropped out (%)	Other school exits (%)	Valid N (total N)	Enrolled (%)	Dropped out (%)	Other school exits (%)
7th grade	95	92	3	2	142	142	0	0
	(97)	(97)	(3)	(2)	(142)	(100)	(0)	(0)
8th grade	184	181	3	13	254	251	3	0
	(197)	(98)	(2)	(7)	(254)	(99)	(1)	(0)
9th grade	238	218	20	5	155	145	10	3
	(243)	(92)	(8)	(2)	(158)	(92)	(6)	(2)

Table 3

Enrollment Status by the Next Fall Snapshot among "Pure" Drop out Sample by Treatment Group and Staggered Comparison

	Treatment				Control			
Staggered comparison	Valid N (total N)	Enrolled (%)	Dropped out (%)	Other school exits (%)	Valid N (total N)	Enrolled (%)	Dropped out (%)	Other school exits (%)
7th grade	97	93	4	0	103	99	0	0
	(97)	(96)	(4)	(0)	(103)	(100)	(0)	(0)
8th grade	191	183	8	6	182	180	2	0
	(197)	(96)	(4)	(3)	(182)	(99)	(1)	(0)

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fall dropout is a more appropriate and useful outcome variable than dropping out by the end of the school year.

Although this measure of the outcome variable carries the benefit of a less skewed distribution, it also has a drawback. When all cases in the base sample are included, the comparison group includes youth whose comparisons dates occur in the summer or early fall and whose arrest dates, therefore, occur prior to next October's measurement of dropping out. This means that the outcomes for some control group cases could actually be affected by subsequent treatment. To preclude this possibility, I restrict the comparison group in some analyses to only cases whose sanction occurs after the fall snapshot. I refer to this sample as the *pure* dropout sample, because it retains the integrity of the pre/post design. As Table 3 illustrates, the use of the pure drop out sample, elevates the drop out rate of the ninth grade arrested student to twenty-one percent and reduces the drop out rate of the control group to only three percent—a striking difference.

This remedy, however, introduces unwanted variation between the treatment and control groups with respect to variables conditioned by the timing of sanction—including the lag between sanction dates and survey participation as well as age at the time of event—both which appear to influence dropout risk. One response to this problem is to use the base sample instead, despite the fact that some members of the control group drop out after their sanction and are therefore not really "untreated" controls. The inclusion of these design integrity violators increases the dropout rate of the control group by the next fall to 9.7% (18/186)— still much lower than the rate derived when these youth are not included. For the sake of a larger sample size and greater similarity on selection variables, it makes sense to begin with the base dropout

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sample, despite the conservative bias this exerts. Chapter seven will revisit the issue of differences between the base and pure dropout samples.

To recap, the staggered replication, like its parent design (the "switching replication") capitalizes on the exogenous component of the variation that occurs in the onset of an intervention (juvenile justice involvement, in this case), to improve the external and internal validity to levels above those exhibited in standard longitudinal regression designs. It improves external validity because similar comparisons are conducted across multiple samples. And although, unlike the switching replication, the staggered replication fails to control for all unmeasured fixed effects by swapping treatment and control groups, it does, in the interest of internal validity maximization, control for much omitted variable bias by comparing arrested youth with "soon-to-be-arrested" youth.

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CHAPTER 4: VARIABLE MEASUREMENT

Measures

A broad range of control variables were considered for inclusion in models of the impact of first arrest on various school outcomes for three sub-samples (divided by school grade) of disadvantaged urban minorities (mostly black males). The diverse set of potential controls reflects both the extensive number of variables available for this research and the fact that few studies on the impact of arrests and the determinants of school outcomes focus specifically on this population. Thus, the empirical literature is not an exhaustive guide. In order to minimize my chances of leaving out a possible confounding selection factor that past researchers and theorists may have overlooked, I consider as a potential selection factor any variable that has withstood empirical or theoretical scrutiny from past theorists and researchers or meets my own standards or reason and common sense.

Chapter two describes a number of variables that may impact youths' risk of arrest while also affecting school outcomes. These factors include individual delinquency and drug use, family socio-economic status, peer delinquency, family structure, family support, ethnicity, gender, neighborhood poverty, neighborhood crime rate, demeanor toward authority, depression, engagement in school, neighborhood social capital, and time spent hanging out with friends. Because many of these variables have not been thoroughly examined in relation to both arrest and school outcomes, multiple indicators of each variable are employed when available. Table 1 provides a summary description of each individual level variable combined with its

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Table 1
Means and Reliabilities of Individual Level Variables

Construct	# of items	Average reliability (α)	Source	Sample survey item(s)
Positive parent-child relationship	6	.81	Climate	My parents…respect my ability to make decisions.
Acting out	11	.87	Attitude	In the past year, how often have you…done some pretty dangerous things just for fun?
Delinquency	4	.80	Attitude	In the past year, how often have you…?
School delinquency	3	.63	Attitude	Been sent to the principal's office after doing "something wrong?"
Soft drug use	2	.65	Attitude	During the past month or 30 days how many…times have you used marijuana?
Hard drug use	4	.87	Attitude	How many times have you used each of these drugs in the past six months…pills (uppers, downers)?
Delinquent peers	5	.85	Attitude	Think of the friends you spend most of your time with. During the last year how many of them….stolen anything?
Lack of anger	4	.81	Attitude	In the last year, how often have you felt…really mad at other people?
Personal valuing of education	3	.69	Climate	School is boring (Reverse-coded)
Structured activities	6	.859	Attitude	On a school day, how many hours do you spend… in school clubs or groups?
Indoor activities	10	N/A	Attitude	On a school day, how many hours do you spend watching television or videos?

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Construct	# of items	Average reliability (α)	Source	Sample survey item(s)
Unstructured activities	10	.87	Attitude	On a weekend, how many hours do you spend talking on the telephone?
Parental control	6	.66	Attitude	In your home, how often do you have to follow rules about…what time you go to bed at night?
Parental academic monitoring	5	.72	Attitude	How often does someone at home…tell you to read?
Negative coping strategies	5	.72	Attitude	When you don't do well on school work, how often do you…think it's not important?
Perceived caring from school staff	11	.87	Climate	How much do the following people in your school care about you as a person…Teachers? The Principal?
Perceived respect from school staff	6	.74	Climate	How many of the teachers really listen to suggestions made by students?
Lack of depression	5	.79	Attitude	In the last year, how often have you felt…hopeless about the future?
Life satisfaction	11	.80	Attitude	How happy are you with…how popular you are?
Positive coping strategies	3	.68	Attitude	When you don't do well on schoolwork, how often do you…tell yourself you will do better next time?
Attachment to the school	6	.87	Climate	If you had to stop going to this school, how much would you miss…your fellow students?

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measurement reliability[27].

Table 2 provides a description of each neighborhood variable from the Project on Human Development in Chicago. Neighborhood-level means and reliabilities for some of these indicators are reported elsewhere; the reliabilities are uniformly high (Raudenbush and Sampson, 1999). For instance, the alpha value for social disorder is .89 while this value for social cohesion is .80. (See further discussion of reliability below).

Table 2
Neighborhood Level Variables

Construct	# of items	Source	Survey items (or sample survey items)
Social disorder	6	PHDCN	How much are the following a problem in your n'hood: drinking in public, drug use or selling, teenagers hanging out?
Physical disorder	3	PHDCN	How much are the following a problem in your n'hood: litter, graffiti, vacant or deserted buildings?
Social cohesion	5	PHDCN	This is a close-knit neighborhood, people are willing to help neighbors, people don't get along with each other, peoples share same values, people can be trusted.
Social capital	5	PHDCN	Parents in neighborhood generally know each other.
Neighborhood anonymity	3	PHDCN	How many (adults, children) do you recognize in this neighborhood?
Neighborhood attachment	2	PHDCN	Do you like or dislike this neighborhood as a place to live? If you HAD to move from this neighborhood would you miss the neighborhood?
Child centered social control	3	PHDCN	Neighbors are willing to do something about: children skipping school, children painting graffiti etc.

[27] Reliabilities are computed utilizing the entire sampling pool and therefore do not draw any distinctions across the staggered samples. See section in this chapter on measurement reliability for a description of the computation of reliability statistics

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Construct	# of items	Source	Survey items (or sample survey items)
Criminal victimization	3	PHDCN	While you've lived in this neighborhood in the past six months: has anyone used violence against you or your household, has your home ever been broken into, has anyone stolen anything from outside your home?
Perceived violence	5	PHDCN	During past 6 months, how often: a fight with weapon, violent argument between neighbors, gang fights, sexual assaults/rape, robbery/mugging?
Neighborhood decline	4	PHDCN	Personal safety worse, neighborhood looks worse, people in neighborhood less helpful, level of police protection worse?

Tables 3, 4 and 5 provide proportions, reliability-adjusted means and standard deviations for each of the three staggered comparison sub-samples. Table 3 and 4 presents the individual

Table 3
Individual Level Characteristics of the Base Staggered Samples by Percentage (Binary Variables Only)

Variable	7th grade	8th grade	9th grade
Treatment group (arrested=1)	41.2	42.4	57.7
Black	87.0	87.3	85.8
Latino	13.1	12.0	12.7
Comer status (Comer=1)	61.6	61.4	61.8
Drug offense	24.9	37.3	39.6
Property offense	31.4	23.4	22.2
Violent or weapon offense	42.0	37.1	35.5
Emotional or behavior disorder	2.8	2.3	1.5
Learning disorder	12.4	12.7	11.2
Male	80.8	79.7	78.1
No biological parent	36.2	35.2	27.8
Single parent	44.1	46.2	53.3
Fall first arrest	43.5	37.3	44.7
Spring first arrest	35.6	43.1	32.5
Summer first arrest	20.9	19.5	22.8
Parent employed part-time	11.0	19.4	16.3
Unemployed parents	15.6	16.2	11.8
N (for all variables)	163.79	381.89	334.45

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Table 4
Individual Level Characteristics of the Base Staggered Samples (Ordinal and Continuous Variables Only)

Variable	7th grade		8th grade		9th grade	
	Mean	SD	Mean	SD	Mean	SD
Acting out	2.01	.58	1.98	.63	1.93	.59
Age at attitude survey	12.27	.78	13.19	.94	13.68	.89
Age at climate survey	12.36	.76	13.17	.89	13.79	.91
Age at event	13.03	.59	14.04	.57	14.88	.54
Average math and reading	26.39	15.14	28.54	13.78	30.73	12.71
Math achievement	25.78	17.43	26.45	15.79	29.55	13.36
Reading achievement	27.68	15.91	30.24	14.22	31.25	15.15
Blames teacher	1.96	1.34	2.04	1.40	1.97	1.34
Delinquency	1.70	.70	1.66	.73	1.57	.65
Educational expectations	3.10	.97	3.11	.97	3.05	.91
Gang affiliated	2.07	1.41	1.94	1.31	1.91	1.27
Hanging out with friends	3.48	1.65	3.54	1.63	3.61	1.58
Hard drug use	4.03	.01	4.06	.23	4.05	.16
Home academic support	3.02	.55	2.99	.61	2.93	.64
In-school delinquency	1.89	.55	1.92	.62	1.88	.59
Indoor activities	25.55	15.65	25.31	16.67	24.47	15.89
Lack of anger	2.95	.96	2.93	.98	3.00	.98
Lack of depression	3.63	.88	3.70	.87	3.81	.88
Lag between event and attitude survey (months)	9.22	7.90	10.27	9.45	14.73	10.28
Life satisfaction	4.00	.58	4.02	.55	4.03	.56
Negative coping strategies	3.85	.76	3.96	.72	4.01	.68
Parental control	3.80	.55	3.69	.54	3.63	.58
Parental education	2.69	1.21	2.39	1.22	2.59	1.16
Parent-child relations	3.96	.67	3.98	.74	3.88	.80
Peer delinquency	1.96	.93	1.91	.91	1.81	.87
Peer drug use	1.54	1.15	1.76	1.29	1.85	1.35
Positive coping strategies	4.05	.65	4.12	.62	4.05	.67

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Variable	*7th grade*		*8th grade*		*9th grade*	
	Mean	SD	Mean	SD	Mean	SD
Prior station adjustments	.83	1.19	.90	1.28	1.00	1.17
School attachment	3.37	.96	3.20	1.13	3.22	.99
Soft drug use	1.69	1.24	1.86	1.36	1.92	1.41
Stressor events	.76	.79	.78	.76	.85	.76
Structured activities	2.38	1.03	2.33	1.00	2.22	.98
Teacher caring	3.64	.70	3.57	.76	3.54	.74
Teacher respect	3.55	.81	3.52	.89	3.43	.62
Teacher caring and respect (mega construct)	3.64	1.05	3.57	1.05	3.47	.75
Total grade retentions (back to 5th grade).	.23	.42	.11	.32	.07	.27
Unstructured activities	3.23	.94	3.21	.96	3.27	.96
Valuing school	3.11	.63	3.07	.67	3.04	.64
Year of event	1995.0	1.57	1995.1	1.58	1995.5	1.51
N (for all variables)	163.79	13.72	381.89	20.94	334.45	6.11

level binary variables. Table 4 presents the individual level ordinal and continuous variables.

Table 5 displays the neighborhood level variables. For the sake of simplicity, variety, and relevance, I choose for each of the sub-samples the cases that will be utilized in the most important analysis involving each of the sub-samples (with the exception of those deleted for missing values and other technical reasons). The reason why each of these analyses is the most important will be evident later. The second and third columns present the means and standard deviations for the sample utilized in the analysis of seventh grade test scores. The fourth and fifth columns present a description of the sample utilized in the eighth grade retention analysis. Finally, the last two columns depict the ninth grade base dropout sample. Recall that measures are typically obtained from the most recent attitude or climate survey prior to one's

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Table 5
Neighborhood Level Characteristics of the Base Staggered Samples

	7th grade		8th grade		9th grade	
Variable	Mean	SD	Mean	SD	Mean	SD
Adult labor force unemployment rate	28.94	14.29	28.06	14.38	28.11	14.40
Anomie	2.65	.20	2.64	.21	2.64	.20
Child social control	3.02	.35	3.05	.37	3.06	.35
Concentrated disadvantage	6.86	5.32	6.26	5.39	6.21	5.30
Concentrated poverty	1.51	1.19	1.41	1.19	1.32	1.17
Crime rate (/1000 people)	202.14	142.91	189.83	123.70	184.94	111.52
Criminal victimization (last 6 months)	-2.40	.52	-2.35	.57	-2.40	.53
Declining police protection	2.10	.21	2.11	.22	2.10	.22
Ethnic heterogeneity	.17	.23	.20	.26	.20	.26
Homicide rate 1995	.00	.00	.00	.00	.00	.00
Immigrant concentration	-.42	.72	-.34	.80	-.36	.78
Income heterogeneity	.54	.22	.56	.21	.56	.22
Neighborhood disorder	3.44	.31	3.45	.30	3.46	.30
Neighborhood anonymity	2.10	.26	2.08	.29	2.09	.26
Neighborhood attachment	2.74	.25	2.74	.26	2.75	.24
Neighborhood decline	2.04	.23	2.06	.24	2.04	.22
Neighborhood organizations	-.14	.55	-.15	.60	-.21	.56
Neighborhood organizational involvement	-3.78	.43	-3.71	.45	-3.75	.47
Neighborhood ties	2.48	.20	2.49	.20	2.49	.20
No excessive force problem	2.34	.35	2.34	.35	2.36	.33
Number of murders in 1995	5.08	3.25	5.06	3.37	5.24	3.66

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	7th grade		8th grade		9th grade	
Variable	Mean	SD	Mean	SD	Mean	SD
Perceived delinquency problem	-1.33	.86	-1.30	.94	-1.37	.91
Perceived violence	2.41	.37	2.42	.38	2.39	.38
Percent adults (25+) less than high school	5.15	11.88	49.13	12.11	49.20	11.62
Percent female headed households	66.61	14.65	66.98	14.40	66.24	15.53
Percent households receiving public assistance	41.26	18.78	39.15	18.46	39.46	18.43
Percent Latino	14.01	25.34	15.89	25.84	15.20	25.16
Percent white	.10	.17	.13	.19	.12	.19
Percent young adults who are dropouts	22.18	13.85	22.05	13.27	21.01	13.43
Physical disorder	2.02	.33	2.02	.35	1.99	.34
Police response to crime victims	2.78	.47	2.78	.48	2.76	.46
Police responsiveness	2.13	.29	2.13	.30	2.15	.30
Poverty rate	49.60	22.74	47.16	22.53	47.56	22.65
Residential stability	-.24	.59	-.24	.63	-.21	.64
Social capital	3.51	.19	3.51	.20	3.51	.20
Social cohesion	3.17	.21	3.17	.20	3.17	.21
Social control	3.65	.29	3.67	.31	3.68	.29
Social disorder	2.42	.31	2.42	.31	2.40	.31
Social exchange on child issues	2.36	.23	2.37	.23	2.37	.23
Social group tensions	2.29	.33	2.29	.31	2.30	.33
Tolerance of deviance for 13 year olds	4.29	.27	4.28	.25	4.28	.25
Tolerance of deviance for 19 year olds	3.91	.49	3.92	.47	3.91	.47
Total population[b]	8690	2204	8502	2405	8621	2428
Wrongfulness of fighting	4.12	.39	4.12	.38	4.11	.39

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	7th grade		8th grade		9th grade	
Variable	Mean	SD	Mean	SD	Mean	SD
Youth service organizations	-1.47	.83	-1.50	.86	-1.56	.79
N[c]	168		386		334	

[a] This table provides the average of neighborhood characteristics across individuals.
[b] Numbers are rounded for ease of table formatting.
[c] For the census variables, these sample sizes are 173, 392, and 336 respectively.

sanction or comparison date.

Demographic Factors

Dichotomous dummy variables indicate whether a youth is *black*, *Latino*, or *male*. (Only token numbers of whites and Asians are in the samples). Two indicators of socio-economic status are level of parental education and parent's work status. *Parent's education* is measured as the highest level of education attained by either one's mother or one's father,[28] with responses ranging from 1 (did not graduate from high school) to 4 (graduated from college). Similarly, two parental work status dichotomous variables indicate the highest work status of either parent. *Part-time* and *unemployed* are the two dummy variables, with full-time serving as the reference category. Finally, *age* at the time of first arrest or comparison date is precisely and continuously measured using exact dates of events and the most reliable birth date available.

Prior School Performance and Engagement

[28] Mother and father are defined as whomever the respondent associates with those roles

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Pre-sanction school performance is measured in eight ways. First, *math achievement* is measured as the normalized curve equivalent score (NCE)[29] on the standardized math achievement examination from the previous grade (or from the same grade if arrested in the spring semester after test administration).[30] Normalized scores are preferable to raw scores because normalization adjusts for year to year, grade to grade[31], and student to student variation in the version of the test administered. Normalized math achievement during grades sixth through eighth is assessed on the Illinois Test of Basic Skills (ITBS), while math achievement in ninth grade is measured on the Test of Achievement and Proficiency (TAP). The procedure for computing *reading achievement* is identical to that of math achievement. In addition, to examining separate math and reading achievement, an *average achievement* score is computed as the mean of these measures.[32] Another indicator of past disengagement or truancy from school is *grade_retention*, which is measured by the number of times a youth was retained from 5th to 8th grade.

Next, another official mechanism through which schools mark students as deficient or difficult is through facilitating the diagnosis of students as having "special needs" or disabilities. Only labels that are subjectively or pejoratively applied, and are associated with negative school

[29] The normalized curve equivalent (NCE) score measures how well a youth performed relative to the national population of test-takers in the same grade. Thus, a math NCE score of 25 indicates that a youth scored better than twenty-five percent of nationwide test-takers in his or her grade.
[30] The date of test administration is estimated as the mid-point of the test administration dates (which vary by test form and school year). For this purpose, all tested youth are assumed to have taken this test on the regular dates of administration rather than the test make-up date which generally begin about a week after the regular test administration period ends.
[31] Normalization does not adjust for differences between scores on the elementary school tests (ITBS) and the high school tests (TAP).
[32] The mean is comprised of only one score when the other score is missing.

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outcomes are included. These various diagnostic labels were distilled into two categories.[33] *Learning difficulties* is overwhelmingly comprised of youth classified as learning disabled, but also includes youth identified as educable mentally handicapped, moderate learning disability, and speech/language impairment. The second dummy variable, *emotional-behavioral difficulties*, consists only of youth labeled as emotionally or behaviorally disabled. Excluded from both of these categories are the few students classified with physical or health impairments. The reference category thus consists of youth with a different diagnosis or no diagnosis at all.

Given that subsequent achievement is a product of effort and motivation, as well as aptitude and performance, attitudinal indicators of school commitment and engagement were derived from students' most recent school climate survey (relative to date of sanction or non-sanction). *Educational expectations* indicates the response to a single Likert scale asking students how far they expect to go in school. The response set ranged from 1 ("go to high school and not graduate") to 4 ("finish college.") *School attachment* is measured by a scale employed in the evaluation of the Comer School Development Program (CSDP) (Cook, Murphy & Hunt, 2000). It is the mean of six Likert response scales in which students are asked to rate, in the event that they had to leave their school, how much they would "miss" various aspects of the school experience, including the principal, teachers, students, and the treatment of students. Possible responses range from 1 "not at all" to 5 "a lot". Also developed for the CSDP, student's *personal valuing of education* is the mean of three Likert scales that gauge students motivation to attend school and general satisfaction with the school experience ("school is

[33] The label must be applied to youth during the nearest snapshot (fall or spring) preceding the sanction or comparison date in order to receive a score of one on these dummy variables.

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boring", "when I wake up in the morning, I often don't feel like going to school"). Responses range from 1 ("strongly agree") to 5 ("strongly disagree").

Delinquency and Drug Use

Because delinquency is arguably the most important control variable, it is especially important to rule out the possibility of lurking variation between each grade's arrestees and non-arrestees that is either missing from a parsimonious delinquency index or diluted by the other items in an overly exhaustive index. For this reason, three measures of self-reported delinquency are derived from the adolescent attitude questionnaire while self-reported drug use and prior police record measures are also measured. *Delinquency* indicates the mean frequency of involvement during the past year in four behaviors that, if detected by authorities, are typically grounds for an arrest. These consist of stealing something from a store, stealing a car, getting into a gang fight, and bringing drugs to school. Responses range from 1 (never) to 5 (ten or more times). *School misconduct* analogously measures frequency of cheating on tests, skipping school, and being sent to the principal's office after doing "something wrong." *Acting out*, which was employed in the CDSP (Cook, Murphy & Hunt, 2000), comprises all of the items measuring delinquency and school misconduct, and adds doing "something dangerous just for fun," hitting someone, damaging someone's else's property, and lying to one's parents "about where you have been or who you were with."

Two indices of drug use were created for this research. *Soft drug use* is the mean frequency (in the past 30 days) of drinks of alcohol and episodes of marijuana use. Responses range from none to "more than 20." *Hard drug use* in the past six months was measured as the

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mean of four Likert-response scales ranging from 1 (none) to 6 (more than twenty times) indicating the frequency of use of pills, crack, powdered cocaine, and heroin.

In addition to these measures of delinquency and drug use, dummy variables were also included which indicate the general category of one's first offense. Drug offenses, Violent/Weapons offenses (includes sex crimes), property offenses, and public order offenses were all coded as binary categories. The purpose of this control variable is that youth arrested in different grades differ in the nature of offense committed. For instance, younger youth are more likely to be arrested for fighting while older youth are more likely to be arrested for drug crimes. Likewise, the nature of the offense may be related to school outcomes. Controlling for this variable precludes the possibility that it is the nature of one's offense preferences rather than the experience of sanctions that accounts for observed outcome differences.

The final measure of prior delinquency indicates the number of *prior station adjustments* (arrests that did not result in formal charges prior to the sanction or comparison date). This variable, which is reliably measured through February 1997, is a more proximate, and, consequently, often more reliable, indicator of delinquent behavior than the self-report measure. However, among a delinquent sample, the variation in number of station adjustments may be a reflection of variation in police practices even more than it is a reflection of variation in individual behavior. Given that variation in police behavior likely underlies much of the differential probability of arrest net of delinquency and bears no obvious relationship to school outcomes, controlling for this variable threatens to attenuate the estimated causal impact of being arrested. I thus include it in models sparingly and cautiously.

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Peer Delinquency and Gang membership

The variable *peer delinquency* was computed as the mean of a series of items on the attitude questionnaire, which asked youth the portion of their friends who committed various delinquent acts in the last year. These acts included damaging property, selling drugs, and taking drugs. Responses ranged from 1 (none) to 5 (all). More recent and serious *peer drug use* was measured by the following single item, "In the past month how often were you with people about your own age when they were using drugs other than marijuana." The ordinal response set included never (1), 2 or 3 times (3), and 4 to 6 times (5).

Unfortunately, the only available indicator of *gang involvement* is likely to misclassify an unknown portion of gang members as non-gang members. The degree to which youth are involved in gangs is measured by the question—which is a component of the delinquency and acting out indices described above—of how often youth took part in gang fights during the past year.

Time Allocations

Several control variables tap into how youth allocate their time. Participation in *structured activities* was computed as the sum of six items on the attitude questionnaire in which youth indicate the amount of time they spend on a typical weekend or weekday participating in particular organized activities including school clubs, church activities, or lessons. Responses to the individual items were converted from an ordinal scale with values 1 through six to estimates of the actual number of hours. For instance, the value of three which indicates 2-3 hours was recoded as 2.5 while the value of five which represents 6-7 hours was recoded as 6.5 hours.

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Another variable analogously measured participation in ten *unstructured activities* that include watching television, playing video games, and talking on the phone with friends. Among the items comprising the unstructured activities index, one of them, *hanging out* with a "bunch of friends", was measured as a separate variable because it is an activity that may draw police attention. Only the item gauging the amount of time youths to devote to this activity on a school night was included because such behavior is probably less likely to draw police attention on weekends. Similar reasoning also called for the inclusion of a variable indicating how much time youth spent outside of police scrutiny. *Indoor activities* is measured as the sum of items indicating school day time allocations to activities (both structured and unstructured) that take place indoors. These activities include doing homework, watching television, and listening to music.

Demeanor

Several items are included in order to measure the emotional dispositions and attitudes that may influence a child's demeanor during an encounter with the police—which could in turn, play a pivotal role in the police officer's arrest decision. *Lack of anger* assesses a youth's general propensity for feeling anger and his capacity to control it. It is measured as the mean of four Likert scales indicating, for instance, how often a youth felt s/he "couldn't control your temper" and "so upset you wanted to hit or hurt something."

Three variables measure attitudes toward one's teachers. The selection of these variables is predicated on the high degree of concordance evident in prior research in measures of attitudes toward the police and teachers (Nevitte & Kanji, 1999; Krause, 1975). The first variable, *teacher respect*, was measured on the school climate survey and assesses the perceived respect

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that students receive from school staff. The scale is a mean aggregate of five-point Likert scales items in which students indicate the share of teachers who, for example, "really listen to what you have to say" and "treat students the same no matter which sex they are" (responses range from none to all) and one five-point Likert scale item that indicates agreement with, "My teachers don't seem to accept me as I am" (responses range from strongly agree to strongly disagree). The second teacher attitude variable, also gleaned from the school climate survey, measures the students' perceptions of how much their *teachers care* about them. It includes eleven 5-point Likert scales regarding the willingness of teachers, the principal and other staff to help students and the level of compassion and caring they display toward students. I also employ a mega-construct combining perceived teachers' caring and respect that was developed as part of the Comer School Development Program Evaluation. The final teacher attitude variable, *teacher's fault*, is a single item that also helps make up the negative coping scale. This item asks students how often they blame the teacher when they fail to do well in their school work. Reponses range from 1 (never) to 5 (all the time).

Mental Health

Several mental health indicators from the attitude and behavior survey are included because they, in addition to influencing the sustainability of delinquent patterns of behavior, may affect a delinquent's rational estimation and valuation of the risks and costs of apprehension and the amount of care that they take to ensure that their delinquency is not detected by the police. *Lack of depression* is measured by the mean of five Likert scales indicating often a youth felt, for example, "really down about life" or "you were not worth anything". Responses range from everyday (1) to never (5). *Life satisfaction* is a similar

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measure that is comprised of nine Likert sub-scales which indicate youths' degree of happiness with various life domains including relationships with parents, teachers, and friends as well as "the kind of person you are." Responses to each item range from "not at all happy" (1) "to very happy" (5).

Negative stressor events was measured as the unweighted sum of items on the attitude survey indicating whether or not a disruptive event (also called a stressor event) occurred in the last year that, I suspect, could lead to a sudden change in behavior and an increase or decrease in arrests. The events included in this event count measure are moving to a new home, a parent's loss of a job, the death of someone close to the respondent, the exit of a parent or step parent from the home, and a mother's new boyfriend.

The next two items, *positive and negative coping strategies* were measured as a possible gauge of a student's ability to cope with an adverse situation, like a police encounter, in a positive way or in a self-destructive way. Although the questions refer specifically to responses to poor school performance ("When you don't do well on schoolwork, how often do you:….") they may signal coping styles more generally. One of the three items exemplifying positive coping strategies is "try to think of what you did wrong so that it won't happen again." Two of the five items exemplifying negative coping strategies are "tell yourself it didn't matter" and "say other students were to blame."

Household/Family Structure and Relationships

The central source of information on family structure is the school climate survey, which includes items in which youth categorize their relationships to those with whom they live. The

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status of living with a *single parent or blended family* is coded as a binary category. A second categorical variable indicates whether or not one is living with *neither biological parent*. Hence, living with both biological parents is the reference category.

Given that a substantial share of sampled youth did not complete a school climate survey prior to the event, and that others completed one long before the event, the school climate survey is an insufficient source of information on family structure at the time of the event or comparison date. For this reason, variables from other data sources figure into the determination of family structure. The school record also contains a variable for each fall and spring semester indicating the relationship of each youth to his or her guardian. This variable appears to be biased in favor of a youth's mother as the mother is often listed as the guardian even when the survey or the arrest data indicates that a youth lives with his/her father as well, or lives with neither biological parent. For this reason, I relied on the school record for guardianship information only under two circumstances. The first is when no self-report survey data is available on family structure prior to the event date. The second circumstance is when the school record consistently lists a guardian other than one's mother or father. Youth under the second circumstance were recorded as living with neither biological parent.

The second indicator of the strength of one's family network derived from the school climate survey is more direct. The construct *positive parent-child relations*, developed for the Comer evaluation, is the mean of responses on six items indicating parents' modal behaviors. For instance, youth are asked how much they agree with statements that their parents, "respect my ability to make decisions," "discuss important family matters with me," and "trust me" and other indicators of relationship quality.

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The next family relations variable, *parental control*, indicates the extent of parents' rule-making and the efficacy of these rules in influencing their children's behavior. The six items comprising the scale include four that ask students how often they have to follow rules about curfews, bed times, choice of companions, and informing parents of destinations when leaving. Responses range from 1 (never) to 5 (all the time). The scale also includes two items indicating who usually decides curfews and "where and how you spend your time after school." Responses range from 1 (I decide by myself), 3 (we decide together after talking about it), and 5 (a parent decides). Responses range from 1 (never) to 5 (very often).

A final indicator of family social support, *home academic support*, does not bear an obvious relationship to arrest risk over and above parental control, but is predicted to be a stronger predictor of school performance, as well as a moderator of the impact of arrest on school performance. Home academic support is measured by five scales asking, "How often does someone at home…" for example, "take you to the library" or "look to make sure you've done your homework."

Neighborhood Factors

Measures of various dimensions of neighborhood social climate were obtained from the first wave (1994-5) of the Chicago neighborhood survey of the Project on Human Development in Chicago Neighborhoods (PHDCN). Further details of many of these measures are available from the Project's codebook (Earls, 1999) as well as various articles describing research utilizing this data set (Earls and Visher, 1997; Sampson, Raudenbush, and Earls, 1997; Sampson, Morenoff, and Earls, 1999). Raudenbush and Sampson (1999) have demonstrated that many of the neighborhood scales that they have constructed exhibit a high inter-rater reliability within

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neighborhoods while the neighborhood indices tend to exhibit high reliability (the degree to which items that comprise a neighborhood index hang together) at the neighborhood level as well. They also found reasonably high levels (according to the standards set by past neighborhood surveys) of variation between neighborhoods relative to variation with neighborhoods.

Neighborhood data from the PHCDN's survey and the census bureau were matched to each youth on the basis of the census tract entered in the school record for the most recent snapshot prior to a youth's arrest or comparison date. When no census tract was available at the previous snapshot in the school record, it was generally derived from the subsequent snapshot or—via the Census web-site (www.americanfactfinder.gov)—from addresses provided in the juvenile justice records, or in the Comer survey data (see section below, "Missing or Questionable Data" for more information on procedures used to handle missing data for these and other variables).

Several scales constructed by PHDCN were assessed as potential covariates of arrest because they describe neighborhood conditions that could attract a larger, more aggressive police presence (or if theory or research suggests that they would lead to more lenient arrest decisions). The PHCDN includes a few relevant crime incidence measures. *Murder rate* is computed as the log of the homicide rate for 1995. The *number of murders* in each neighborhood cluster in 1995 is included, because police departments are more likely to be responsive to individual homicides occurring in a community rather than the rate of homicide. *Criminal victimization* was measured by whether anyone in the respondent's household was the victim of various crimes in

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their neighborhood during the last six months. These crimes include burglary, violence, theft, and property damage.

Four other measures of neighborhood conditions that could be a source of "ecological contamination" of potential arrestees were derived from the PHCDN survey. *Social disorder* measures residents' perceived severity of the problems of three disorderly behaviors in their neighborhoods (drinking in public, drug sales or use in public, and groups of people "hanging out" and causing trouble).

Analogously, *physical disorder* measures the perceived severity of problems of debris on the sidewalk, graffiti, and vacant buildings. Responses to the items comprising these two scales range from 1 (not a problem) to 3 (a big problem). *Delinquency problem* incorporates elements of both of the above scales. It includes perceptions of "hanging out" and graffiti problems and converts them to dummy variables (problem or not). It also includes a variable about the frequency of gang fights during the last six months that was also recoded as a binary measure indicating the presence or absence of gang fights in one's neighborhood. Next, *perceived violence* is measured by a series of five items in which respondents assess the frequency of violent behaviors (e.g. gang fights, robbery/mugging) in the neighborhood in the past six months. Responses range from 1 (never) to 4 (often). Finally, *neighborhood decline* is comprised of five items measuring residents' perceptions of changes in personal safety, the appearance of the neighborhood in the neighborhood, the quality of people living in the neighborhood, and the level of police protection in the neighborhood generally during the past five years. Response range from 1 (better) to 3 (worse).

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Affecting police behavior perhaps even more than individual perceptions and self-reported victimization is the actual *reported crime rate* in a community. Other advantages of using crime rate rather than the survey data are that it is available each year and by census tract, a unit more representative of a youth's "neighborhood" than the census tract clusters constructed by PHCDN. Crime rate was measured in each year in the study from 1992 through 1999 and for each census tract as the total number of index crimes (felony assault, auto theft, burglary, murder, rape, robbery, and felony theft) per 1000 people living in that census tract.[34]

Other items were included because prior research suggests they may condition arrest practices and decisions. *Percentage black* and *percentage Latino* is measured as the percentage of residents of a youth's neighborhood cluster that are of these respective racial/ethnic groups. Following the example of Smith (1986), *racial/ethnic heterogeneity* was computed for each census tract according to the formula, $1-[(\%\ \text{black})^2-(\%\ \text{Latino white})^2-(\%\ \text{non-Latino white})^2-(\%\ \text{other})^2]$. The first step in computing *income heterogeneity* was to collapse the twenty income categories on the U.S. census into five. Next, the variable was computed using a formula similar to the above formula for racial heterogeneity, except it uses percentages in each of the five income categories as opposed to ethnic group category. *Percentage poor* and *labor force participation rate* were obtained for each census tract from the 1990 census.

[34] This data was furnished by Wesley Skogan of the Institute for Policy Research, who obtained this data from the Chicago Police Department. The population for each year was derived as follows: first, 1995 census tract populations were estimated as the average of 1990 and 2000 populations. Second, the 1992 and 1993 crime rates by census tract were computed using 1990 population as the base. Next, 1994 through 1996 crime rates were computed using estimated 1995 population as the base. Finally, the remaining years' crime rates were computed with year 2000 population as the base.

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The PHCDN neighborhood survey provides a plethora of other variables that tap into the social and normative climate of the neighborhoods. As explained in chapter two, these items are important to this study because they may affect a neighborhood's propensity and capacity to resolve disputes informally without the help of the police. They may also influence residents' trust in and leverage over police—both which likely affect the likelihood of police being summoned and the likelihood of a police officer to make an arrest. All of these factors in turn influence a youth's chances of arrest conditional upon his commission of or implication in a crime. At the same time, these neighborhood social climate measures likely affect the amount of normative and social support available to youth who wish to stay engaged in school.

Recall that the purpose of this study is not to develop a comprehensive model of the mutual determinants of both arrest and school performance. Rather, the aim is simply to rule out any other explanation, aside from the arrest itself for the hypothesized relationships between arrest and school performance. For this reason, I cast a very wide net in selecting variables from the PHCDN survey and the census. Here I will describe a few of these variables that were theoretically linked to arrests in chapter 2 or turned out to be empirically linked to both arrest and school performance in some exploratory analyses. *Social capital* is measured as the mean of five scales that indicate the degree to which social networks extend across households and across generations. For instance, on one item respondents indicate their agreement with the statement, "adults in this neighborhood know who the local children are." *Neighborhood anonymity* also assesses the knowledge of one's neighbors but its three items speak directly to the prevalence of strangers in the neighborhoods. One item asks, "how easy is it for you to pick out outsiders or people who don't live in the area."

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Social cohesion is measured by five Likert scales that tap the strength of the social ties and sense of unity among residents. Finally, *child-centered social control* measures directly an implication of high levels of social capital and social cohesion. Residents of such neighborhoods are more inclined and empowered to "do something" in response to youthful deviance. At least one of the items has direct implications for school performance. This item reads, "neighbors would do something if a group of neighborhood children skip school and hang out on the street corner."

Note that in addition to the variables mentioned below, I also controlled for anomie, tolerance of deviance among thirteen year olds, number of friends and relatives in the neighborhoods, neighborhood ties, social exchange pertaining to children, perceived neighborhood danger, prevalence of neighborhood organizations, involvement in neighborhood organizations, availability of services for youth, percent female-headed households (census), the percent of young adults without a high school diploma (census), and several other variables that overlap greatly with other neighborhood variables. I will furnish information on each of these variables upon request.

Social capital may exert downward pressure on arrest through lowering the crime rate and reliance upon formal controls, but upward pressure on arrests through increasing the likelihood of residents to call the police and the responsiveness of police to those residents. It, therefore, makes sense to control for crime rate or police responsiveness in models in order to partially disentangle the allegedly contradictory effects of social capital. PHCDN's neighborhood survey provides two measures of police responsiveness. The first indicates the degree to which residents believe that "police do a good job in responding to people in the

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neighborhood after being victims of crime." Responses range from 1 (strongly disagree) to 5 (strongly agree). The second survey question, which seems to bear more directly on a youth's chances of being arrested, asks, "how much of a problem is police not patrolling the area or responding to calls from area?" A response of 1 signifies "a big problem", 2 means "somewhat of a problem," and 3 means "a big problem." Another police response which is reflective of low levels of social cohesion and social capital, and may decrease a youth's risk of arrest is excessive force. Perhaps, some police substitute immediate physical sanctions for time-consuming arrests in communities where an overabundance of reported crimes seriously drains police manpower and where communities lack the interlocking social networks necessary to mobilize serious opposition to excessive force. It is only with serious hesitation that I employ these police variables in models of school outcomes, since I see no theoretical reason to suspect that police responsiveness and excessive force influence school performance. If these items do not influence school performance but are strongly related to arrest they may unnecessarily attenuate the impact of the arrest status variables.

Reliability of Measurement

Three issues arise with respect to measurement reliability. The first concern is the variable lag between measurement of the antecedent control variables—particularly those that display considerable instability over time—and the timing of arrests (treatment group) and comparison dates (comparison group). This lag is a source of excess measurement error and possibly a source of differential measurement error by treatment status. The second concern pertains to the reliability of multi-item indices which is assessed by the degree to which the items comprising an index "hang together." Measuring the intercorrelations between indices and their

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known covariates provides an additional check on reliability while also speaking to a third issue—construct validity. With respect to variable measurement, construct validity refers to the degree to which a variable measures the construct (e.g. family support, neighborhood cohesion) it was intended to measure (Shadish, Cook, and Campbell, 2002). I attempted to establish the construct validity of select indicators by computing the correlations between indicators and their known covariates and, whenever possible, choosing multiple items or variables that represent the same construct.

To address the first issue—a differential lag between measurement of the antecedent control variables and the primary independent variable of interest—this lag was computed for each case. Separate variables indicate the time (in months) between the event or comparison date and the most recent attitude survey and the most recent school climate survey. These lags, the *attitude survey lag* and the *climate survey lag*, are reported in the descriptive tables that precede the analyses of most outcome variable.[35]

As shown in Table 2, the average estimated lag between the nearest attitude and school climate survey is below a year for the seventh grade samples (except for the attitude lag of the 7th grade treatment group) and is about a year for the eighth grade samples. These lags for the 9th grade samples are all well above a year, reaching nearly seventeen months for the 9th grade treatment group. In other words, on average, members of the 9th grade treatment group were last surveyed in the spring of 7th grade (given that most of the attitude surveys were administered in the spring).

[35] Recall that these lags are not precise estimates because survey date is always recorded as the 15th of the month in which the survey was administered and because, for some youth, only the date of court petition is available which occurs, on average, three weeks after the arrest.

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These temporal delays are far from ideal, as optimal designs exhibit minimal delay between measurement of pre-treatment factors and the onset of treatment. Including cases with extreme lags—for example, those arrested in 9th grade whose self-reported delinquency was last measured in 5th grade—seems highly problematic. The degree to which unusually long temporal delays detract from the overall reliability of measurement depends on how well earlier levels of the various factors predict later levels. Fortunately for this research, prior evidence suggests a high degree of stability in some of the key control variables. For instance, measures of family structure and parent-child relational dynamics as well as deviant propensities are highly stable throughout childhood and early adolescence (Farrington, 1995; Walker, Stieber, and Bullis, 1997, Simons, Wu, Conger, & Lorenz, 1994).

To show that this stability characterizes my sample as well, I examined a sample of youth (n=948) who have been arrested at least once. I measured the intercorrelations between the nearest survey measure prior to arrest date of three important control variables—acting out, anger, and school attachment—and the survey immediately prior to that. These inter-correlations (rho) are .304 (n=401), .308 (n=409), and .248 (n=304) respectively. While these correlations are reasonably large ($p<.00001$), they provide little reassurance that sixth grade levels of delinquency is an adequate proxy for eighth grade levels. After all, many factors could precipitate an undetected surge in delinquency in the eighth grade treatment group. On the other hand, by controlling for a large-set of sanction antecedents that are correlates of delinquency (including some like prior station adjustments, retention, and achievement that are measured proximally to the sanction) we minimize the probability that an unmeasured difference between treatment and control students could precipitate the arrests of the treatment group.

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The undetected differences between treatment control groups that may arise from an excessive delay between control measurement and treatment are compounded if the size of these lags varies by treatment status. As presented in the upper portion of Table 6, the attitude and school climate survey/event lags are consistently longer for each treatment group than they are for each comparison group (using the base samples depicted in Table 2). The treatment/control differences range from 1 month to 2.7 months. This reflects a pattern by which treatment group youth—those arrested a year earlier than their counterparts and possibly more truant and transient—completed fewer prior surveys than the control group. However, these differences are statistically significant ($p<.05$) only with respect to the attitude survey lags in the 7th grade sample.

What are the implications of differential lags for the measurement of the control variables? A longer lag should correspond to a younger age at the time of the most recent

Table 6
The lag between survey and event and survey ages by survey type and staggered comparison (Base Samples) [1]

Staggered comparison	Treatment	Control	p[2]	Treatment	Control	p[2]
	Attitude lags (months)			Climate lags (months)		
7th Grade	11.9	9.2	<.01	10.4	9.0	.16
8th Grade	12.2	11.2	.26	12.3	11.3	.36
9th Grade	16.7	15.8	.38	15.4	13.9	.18
	Age at attitude survey			Age at climate survey		
7th Grade	12.28	12.36	>.5	12.38	12.34	>.5
8th Grade	13.09	13.14	>.5	13.05	13.09	>.5
9th Grade	13.57	13.62	>.5	13.63	13.77	.121

[1] Control groups (7th and 8th only) exclude cases in which the comparison date precedes the survey.
[2] Equal variances between treatment groups are not assumed in these or any other t-test comparisons

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prior survey. Since most of the antecedent control variables presumably change for the worse as youth progress toward their first arrest, a reasonable supposition is that pre-sanction delinquency and other problems are, as a consequence, underestimated for the treatment group. These underestimates would give the false impression that treatment groups exhibit the same or even lower levels of delinquency than the control group, which would yield over-estimations of the negative effects of arrest on school performance. However, as the lower portion of Table 5 shows, the longer survey lags do not correspond to significantly younger ages at the time of survey for the treatment group. Despite the fact that all of the survey/event lag comparisons are greater than a month, only one age at the time of survey difference is greater than one month. Thus, while the differential survey lags still pose problems in terms of differential reliability of measurement they also carry the benefit of adjusting survey measures for the differences in age that would have occurred in the presence of equal lags. The discordance between treatment control differences in the event/survey lags and in the ages at the time of the most recent survey is attributable to the fact that the treatment groups are slightly older at the time of their event dates.

Thus, most of the differences in the timing of the sanctions and in the timing of prior surveys in relation to each other and to the life course are, fortunately, too small to mask significant differences in the relatively unstable control variables between treatment and control. Still, the analyses in the next two chapters will take steps to safeguard against possible confounding effects of differential measurement reliability.

The second type of reliability relates not to the temporality of measurement but to the indices themselves, i.e. whether the items comprising an index are internally consistent. The

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degree to which the items in each multi-item hangs together was assessed by computing Cronbach's alpha, which is based on the average inter-item correlation among the items comprising a scale. Given that each of the survey items are measured during multiple school years and that data from multiple school years were ultimately pooled in the creation of control measures, the best indicator of reliability is the average reliability across multiple waves of the surveys. These reliability averages for each of the Comer survey items are presented in Table 1.[36]

Reliabilities are computed from the overall sampling pool rather than the staggered sub-samples. Thus, all youth who completed a survey in a particular school year, irrespective of grade attended or delinquency/juvenile justice involvement, provide information that is used to compute survey variable reliabilities. The assumption that present or future delinquents complete surveys with the same degree of diligence and sincerity as non-delinquents should not be taken for granted, however. To test this assumption, I assessed the reliability of a diverse set of variables (also differentiated by survey year) using only the arrested sample. These tests demonstrated a remarkably similar degree of (and sometimes a stronger) measurement reliability on each selected indicator, despite the substantially reduced sample sizes. For instance, the 421 youth in the juvenile justice sample that completed all items comprising the peer delinquency scale on the second wave of the survey exhibited a reliability of .86 on that measure, while the

[36] Reliabilities tend to be very stable across survey years, wavering by only a few hundredths of a percentage point at the most. The two exceptions to the norm of high and stable reliabilities, in school delinquency and soft drug use, owe this distinction partially to the small number of items comprising these scales.

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overall sample of 3,895 youth who completed these same measures yielded a smaller alpha value of .84.

No reliability is presented for the negative life events index because it is measured as a count of disruptive life events. That index is reliable to the extent that each of the events is disruptive and the addition of each event to one's life adds to the degree of disruption—both highly defensible assumptions. By the same token, *indoor activities* is measured as the total amount of time spent in various indoor activities. The degree to which these time allocations hang together is not important—only the amount of time spent indoors is important—so the reliability of this measure is not computed either.

As Table 1 reveals, virtually all multi-item indicators selected for further analysis are highly reliable. Nearly all of them have a Cronbach's alpha of close to or above .70 which is considered highly reliable. The majority of scales have an alpha value above .80! Nevertheless, variation in reliability across variables and across surveys within a single variable calls for some statistical adjustment. Specifically, less reliable variables, owing simply in part to noise, produce values that deviate more from the mean. One must somehow restrain this variation, lest measurement error will be mistaken for variation in the actual construct. To adjust for this differential reliability he deviation of an observation from the mean was reduced in inverse proportion to the reliability of the variable.[37]

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[37] The following formula was utilized to perform such reliability adjustments: $X_{adj=} X_{mean} + \alpha(X_{i} - X_{mean)}$, where X_I is the value on the variable X for individual I, α is reliability of a variable X on survey j, X_{mean} is the mean of variable X on survey j, and X_{adj} is the reliability-adjusted value for individual i on survey j. The values for each of the elements of the formula varied depending on which of the five surveys collected the observation. Thus, observation of variables that have a reliability of .5 will have their observed deviations from the mean cut in half while the observed

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The individual level and neighborhood level reliabilities for some of the scales developed from the PHDCN survey are provided in prior published research (Raudenbush and Sampson, 1999). While these reliabilities suggest that overall the constructs hang together at the neighborhood level, it is possible that measurement error may be more pronounced in some neighborhoods than others owing to differential inter-rater agreement. Low inter-rater agreement within a neighborhood could reflect high rates of perceptual dissension among raters or high levels of internal diversity with respect to the conditions present within a neighborhood cluster. While inter-rater agreement levels are not readily available specifically for the neighborhood clusters providing most of the cases for this study, inter-rater agreement within a cluster varies in direct proportion to the resident survey sample size for that cluster. Raudenbush and Sampson (1999) report that intra-neighborhood reliabilities range from .70 to .90 when 20 neighborhood residents were surveyed. Reliabilities increase rapidly with increases in the number of raters; the reliability range increases to .83 to .95 for neighborhoods rated by 40 residents. Over 60 percent of the 8th grade base sub-sample, the largest sub-sample in this research, resided in neighborhood clusters from which PHDCN drew at least twenty raters.

Missing or Questionable Data

Missing data are present for most of the variables employed in this research. Appropriate strategies to address missing data varied by variable type (demographic, school records, attitude/behavior, and neighborhood variables, etc.). The strategies were implemented separately for each of the three staggered comparison sub-samples.

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deviations from the mean of variables with extremely high reliabilities will remain basically unchanged.

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Missing data were quite uncommon with respect to the demographic variables such as race, gender, family structure, and parent's education and work status. In the rare cases when race and gender were missing or inconsistent across data sources, the correct designation was verified through cross-validation across multiple sources (school record, surveys, and juvenile justice sources). Earlier I described circumstances in which other data sources would replace the school climate survey as the source of family structure information. Similar procedures were employed if information on family structure was missing from the most recent school climate survey. If family structure information was absent from the most recent survey, the values from the following sources were substituted in the following order of preference—earlier versions of the survey beginning with the most recent, later versions of the survey, guardianship information in the station adjustment if the station adjustment date was within a few months of the sanction date or comparison date (or if this record revealed the relationship of the guardian listed in the school record or arrest record), and the school record. Multiple sources of data on family structure were also cross-checked across sources to ensure the validity of the chosen designation. This process omitted virtually all missing data from the family structure variable.

Similar procedures partially addressed missing values on the parental education and parent work status variables. Specifically, prior and later survey data (in order of preference) were imputed when data on this variable was missing from the most recent prior survey.

Prior survey information, however, was not always available for these and other variables. Furthermore, while the imputation of distal or future survey data seems acceptable for the relatively stable characteristics above, one should minimize the use of distal substitutes for proximal data on relatively unstable measures of pre- and early- adolescent attitudes. The

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average lags between the sanction and comparison dates and the most recent survey date—which are high according to the standards set by prior research on the impact of events—already raises serious concerns about measurement reliability; simply replacing missing values on the most recent survey with the values on a previous survey compounds those concerns.

A sounder method for retaining cases with missing data on unstable characteristics is to estimate missing data using multiple imputation methods. Given that the sample for this research includes only youth who have at least one prior or proximal attitude and behavior survey, and 90% of these youth also took a school climate survey, nearly every youth provides information on multiple survey-based constructs. Multiple imputation methods involve estimating models of each variable of interest, $Z_{I,}$ based on the cases that contain data on Z_i and the independent variables X_i. Then, the model is used to generate predicted values, ž, for cases with missing values on $Z_{i.}$ Finally, imputation of missing data is performed through multiple iterations that allow parameters to vary (usually along a normal distribution) to account for error in the estimation of the predicted values.

Multiple imputation methods provided substitute values for a large share of the variables employed in the staggered replications. Imputations were carried out using the software program NORM. The program generates imputation models by modeling each variable with missing values as a function of all the other variables in the data set. It estimates initial parameter values using an EM algorithm, which uses the observed data to estimates relevant regression statistics (e.g. means, standard deviation) and then derives the parameters that have the maximum likelihood of yielding the observed data (maximum likelihood estimation) (See Dempster, Laird, and Rubin, 1977). Then, beginning with the starting parameter estimates generated by the EM

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algorithm, the program uses a Bayesian estimation procedure known as data augmentation to generate successive imputations drawing from the posterior distribution of the parameters. The program continues iterating between random imputations until the estimated parameters become independent of the initial values (convergence). Schafer (1997) provides more explanation of these imputation procedures.

Allison (2000) found that NORM generates estimates of values that were remarkably close to actual values under the assumption that the probability that a case is missing particular data depends only on data are observed and included in the imputation models (missing at random assumption).

The first step in data imputation, prior to initiating imputation procedures, was to determine, separately for each staggered sample, which variables to include in the data sets that generate the imputation models of each variable. The ideal data set for this purpose includes every variable that may be of importance in the analyses as well as strong covariates of those variables. I was not able to utilize all the variables; however, some of the variables are highly inter-correlated and multicollinearity can bias the imputed estimates. When two or more variables were excessively inter-correlated, only one of these variables was included in the imputation data set. The variable that was chosen for inclusion in the imputation model was the one that, overall, was the strongest potential covariate of both treatment group and the various outcomes (based on the sum of p-values of independent sample t-test comparisons of treatment groups and various outcomes). Based on this criterion, no multiple imputation was performed for delinquency, gang affiliation, and structured and unstructured activities for at least one of the staggered replications.

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All dichotomous variables from Tables 3 and 4 (absent the reference category of course) like race, family structure variables, and offense type were included in the imputation data sets. However, the imputed values of binary demographic characteristics generated by NORM were not utilized; cases with missing data on these variables are excluded from analyses involving these variables by listwise deletion. Imputed values were utilized largely for ordinal or continuous individual level variables that change over time in a non-fixed pattern.

For station adjustments the imputed values generated by NORM were only partially utilized. Imputed values for missing prior station adjustments were utilized only for cases whose arrests or comparisons dates occurred beyond the range of the station adjustment file *and* which were not found in the arrest or the station adjustment file. If a case was not found in the station adjustment file even though the first arrest or comparison occurred within the time range of the station adjustment file, or the case had an arrest recorded in the arrest file, the number of prior adjustments was recorded as zero.[38]

Missing data was also evident on some of the neighborhood variables. One source of missing data was easily remedied while the other was nearly impossible to reliably remedy. The first source of missing data was the school record that sometimes was inexplicably missing

[38] Given that youth can be first arrested up to 2.5 years after the expiration of the station adjustment data, cases vary in the extent to which their prior station adjustment data is right-censored. Net of age, a later arrest exerts downward bias on the number of prior station adjustments. Given that treatment groups' arrest dates are a bit later on average, this downward bias may be more evident among the treatment group. On the other hand, a greater share of prior station adjustments among treatment cases will go completely unmeasured. If imputation methods are reliable, these cases should have more prior station adjustments on average than slightly earlier cases with detectable station adjustments. Thus multiple imputation methods exert differential upward bias on prior station adjustments for the treatment group (the effect of which should be to diminish the impact of sanctions on dropping out). The inclusion of a control for year of event will help adjust for bias in the number of station adjustments owing to the differential timing of treatment and comparison dates.

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census tract data. This problem was generally easily fixed by locating the address in which a youth lived at the time of their sanction or comparison date--which was available from juvenile justice or school records—and deriving the corresponding census tract from the Census web-site. These census tracts were then linked to census data and to the neighborhood cluster data.

The second cause of missing neighborhood data was the fact that PHCDN did not aggregate data on all Chicago neighborhoods. About three percent of cases across my research sample lived in neighborhoods that were not subject to PHCDN's survey. The available remedies--attempting to impute neighborhood cluster by multiple imputation based on the relationship between census tract variables and neighborhood cluster variables or substituting the characteristics of a nearby neighborhood cluster that appears similar on a range of attributes—seem quite precarious in comparison to the scale of the problem. Hence, cases missing neighborhood cluster variables are simply excluded from models including these variables.

Multiple imputation methods, by definition, involve multiple data sets with different sets of imputed values. In the next two chapters, I describe analyses I conducted initially using multiple data sets and averaging the parameter and standard error estimates. Fortunately, the results proved sufficiently consistent across imputed data sets that the models I tested—and there are many—were generally able to rely on only one imputed data set.

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CHAPTER 5: THE IMPACT OF ARREST ON ELEMENTARY SCHOOL PERFORMANCE

As explained earlier, dropping out during or shortly after 7th or 8th grade is too rare even in the delinquent sub-samples studied here to warrant multivariate analysis. Also, unfortunately for my purposes, the elementary school years provide few indicators of school performance. Data on grades and truancy, for example, are not available. Standardized achievement test scores in math and reading and grade retention are available, however, and they are the focus of the analysis of the impact of arrest on elementary school performance.

Test scores and retention during elementary school are important to examine for several reasons. First, since test scores and grade retention are meaningful and commensurable outcomes only during the elementary school years, such analyses will provide a sense of whether the impact of arrest is conditional upon age and school grade. Second, an examination of the impact of arrest on test scores, retention and high school outcomes like grades will potentially highlight some of the mechanisms that may underlay the impact of arrests on the most pivotal outcome of all, dropping out. If the effects of sanctions on test scores and grades are as strong as their effects on dropping out, this suggests that arrest has an effect on academic motivation and engagement. On the other hand, if the effects of arrest on tests scores and grades are substantially lower than the effects on dropping out, this provides support to the notion that exclusionary institutional responses to arrests push some students toward dropping out irrespective of their academic motivation and performance.

The Impact of Arrest on Test Scores

Although I do not subject dropping out to systematic analysis among the seventh and eighth grade sub-samples, the rates of dropping out retain a modicum of importance as a minor

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source of attrition. Recall from chapter four that three and two percent of the seventh and eighth grade treatment groups drop out by the end of the school year of their first arrests in comparison to a virtual absence of drop outs in their respective control groups. Such youth typically do not provide achievement test scores in the spring after their arrests. Likewise, the treatment groups contain a greater portion of youth who are enrolled in school yet fail to take the achievement tests, presumably due in large part to truancy.

Greater rates of dropping out and truancy in the treatment groups yield a highly conservative test of the impact of arrest on academic skills. Had drop outs and truants actually taken the tests, they would likely lower the mean test scores of their respective treatment groups. Given that arrested youth are far more likely than their respective comparison groups to miss the next achievement test following their arrests, treatment group scores are biased upward. This bias is known as attrition bias. To illustrate the extent of attrition bias, arrested youth in the seventh grade-staggered sample are missing a post-arrest reading score 21.5 percent of the time. By contrast, only ten percent of their non-arrested counterparts are missing a reading score following their comparison date. Likewise, 28% of the treatment group in the eighth grade-staggered sample are missing eighth grade math scores. This attrition rate towers over that of the eighth grade comparison group, which stands at only eleven percent. Unfortunately, since dropping out or transferring to other schools explains less than 1/3 of the attrition, the reasons for this attrition cannot be thoroughly explored. It seems reasonable to suspect, however, that elevated absences and/or disengagement from school among the arrested youth is responsible for the differences in missed achievement tests.

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Table 1
Mean Pre- and Post- Math and Reading Achievement Test Scores

Sample and Treatment Group	Reading scores Pre	Post	Change	Math scores Pre	Post	Change
			7th grade sample			
Arrested	26.01	26.31	+.30	24.03	21.99	-2.04
Not-arrested	27.96	29.12	+1.15	26.13	23.95	-2.18
Total	27.14	27.96	+.82	25.21	23.18	-2.03
Sample size (arrested:control)	80:109	73:104		83:107	67:103	
			8th grade sample			
Arrested	29.27	29.03	-.23	23.73	29.04	+5.32
Not-arrested	30.47	30.77	+.30	27.12	29.60	+2.48
Total	29.96	30.10	+.14	25.69	29.39	+3.69
Sample size (arrested:control)	147:202	127:203		147:203	121:194	

As Table 1 shows, differences between arrest and treatment groups in the average pre-post change in both seventh and eighth grade test scores are small and unlikely to render large statistical effects. While two achievement test scores were available for each sample and both were analyzed, only the results of the test that was most likely to yield an effect is presented.

With respect to the seventh grade staggered sample, differences between treatment and comparison youth in changes in reading scores following arrests, while small, are greater than for math, which calls for the selection of this outcome. Among youth in the eighth grade-staggered sample, by contrast, markedly greater treatment/comparison differences occurred for changes in

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math scores. Unexpectedly, arrested youth saw their eighth grade math scores improve more than non-arrested youth.

Thus, for the eighth grade staggered sample only the math scores are presented here. Not surprisingly, given the results in Table 1, analyses uncovered no effect of arrest on seventh grade math scores or on eight grade reading scores or on the average of math and reading scores in either grade.

Thus, the first outcome we consider is seventh grade reading achievement tests scores. These scores are available for the vast majority of youth arrested in seventh grade and for the seventh grade comparison group. Analyses minimize omitted variable bias as an explanation of treatment and comparison differences by controlling for prior reading scores. That is, many of the unmeasured antecedent differences between arrested and non-arrested youth that could explain differences in seventh grade tests scores are captured by the differences in 6^{th} grade test scores.

The sample includes youth who were enrolled in seventh grade at the time of the arrest or comparison dates and had completed at least one attitude survey prior to this date. Recall that a minor modification to the base sampling frame is required for the test score analyses. Students whose sanction or comparison dates occur shortly after administration of the test are viewed as being in the subsequent grade. Thus, the seventh grade treatment group includes 12 sixth graders who were arrested after sixth grade tests and excludes 20 seventh graders who were arrested after the measurement of the seventh grade test.

The first step in the analysis is to select a set of control variables based on their theoretical and observed association with being arrested and with achievement test scores. For a

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variable to qualify as a control variable, it must be related to the focal independent variable, arrest, as well as the outcome variable (Allison, 1999).

Independent sample t-tests and chi-square tests in the case of dichotomous variables compared treatment and comparison youth. These t-test and chi-square tests yielded a consistent result: very few factors distinguished the treatment and control groups according to the standard threshold of statistical significance ($p<.05$). Among the well over one hundred variables that were tested as covariates of being arrested, only seven were statistically significant in each set of the t-tests.[39] In other words, they differ on about the same number of variables as one would expect the two groups to differ due to chance sampling variability alone.

Convergence on such a large number of measured covariates suggests a high degree of convergence on unmeasured covariates as well (the few that remain). While low statistical power may help explain the scarcity of covariates, this pattern of minimal selection differences, as we shall see, was repeated from the larger eighth grade and, to a lesser extent, the ninth grade staggered replications as well. Thus, it appears that the staggered matching approach has fulfilled its stated aim of producing highly similar treatment and comparison groups.

Only one of the predictors of arrest is also significantly associated with seventh grade reading scores. Rather than assuming that almost no confounding selection factors exist, standard covariate selection criteria are relaxed and a broader number of potential covariates are considered. Variables are included for consideration in initial models if they discriminate between treatment and comparison youth—in t-test or chi-square tests as appropriate—*and* are

[39] Variables are excluded from this calculation if they are derivative of another variable that is already counted.

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correlated with seventh grade reading scores at the p<.25 level. Through this relaxed filter, fourteen potential covariates were selected for initial models. Table 2 characterizes the arrest and non-arrested youth in the seventh grade staggered sample and indicates the statistical significance of the treatment/control differences and, the correlation of the variable with 7th grade reading scores (among those that are significantly correlated with test scores only).

Table 2
Characteristics of the 7th Grade Treatment and Control Groups[a]

Variable by type	Control mean	Treatment mean	P	Correlation with 7th grade reading (p<.10)
		Reliability controls		
Lag between event and survey (months)	8.14	12.06	.00	--
Number of missing variables	1.82	2.02	.55	--
Structured activities	2.33	2.47	.39	-.27
		Background measures		
Black (%)	85.3	91.4	.17	N/A
Latino (%)	14.7	8.7	.18	N/A
No biological parent in HH (%)	37.9	41.9	.56	N/A
Single parent (%)	48.3	33.3	.03	N/A
Male (%)	80.2	79.6	.91	N/A
Age at sanction date	12.98	13.24	.01	-.13
Grade	6.98	6.71	.12	--
		Emotional/behavior measures		
Prior police station adjustments	.66	1.19	.01	-.25
Marijuana and alcohol use	1.56	1.91	.07	-.16
School deviance	1.84	1.96	.15	--
Violent or weapons offense (%)	40.5	43.4	.69	N/A
Drug offense (%)	30.2	20.5	.12	N/A
Property offense (%)	26.7	36.1	.16	N/A

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		Neighborhood variables		
Neighborhood attachment	2.75	2.69	.09	.19
Neighborhood anonymity	2.07	2.12	.14	--
Perceived violence	2.39	2.49	.05	--
Participation in organizations	-3.00	-3.13	.07	--
Sample size range	88:116	71:93		

[a] Values re for valid cases only; no statistically imputed values are included.

Next, the family structure variable was imputed manually while the continuous variables were imputed using the multiple imputation procedures described in the previous chapter. Highly collinear variables were omitted from the imputation models on the basis of theoretical and empirical relevance.

Table 3
Linear Regression of 7th Grade Reading Scores on Arrest and Selection Controls

	Model 1			Model 2		
Variable	B	Beta	p	B	Beta	p
Arrested	-1.47	-.05	.45	-.84	-.03	.69
Sixth grade reading	.48	.51	.00	.39	.43	.00
Structured activities	-			-1.94	-.13	.06
Neighborhood attachment	-			5.07	.09	.23
Neighborhood anonymity	-			-6.42	-.11	.12
Age at sanction date	-			-.96	-.04	.57
Prior station adjustments	-			-1.07	-.09	.22
School deviance	-			1.78	.07	.32
(Constant)	15.28		.00	31.52		.24
Valid sample size	176			167		
Durbin-Watson	2.18			2.18		
R^2	.27			.30		

It is important to note that the nearly three point difference in seventh grade reading scores the model will attempt to explain is not even statistically significant. And when seventh grade scores are regressed, using a bare bones ordinary least squares regression model estimated

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in SPSS, on only treatment status and prior reading scores, the negative effect of sanctions on reading scores is cut in half and is statistically insignificant. This result is displayed as Model 1 of Table 3. These results suggest that arrest does not have an impact on seventh grade test scores, except in the unlikely event that the impact is being suppressed by the association of arrest with observed variables that are positively associated with seventh grade test score.

Little evidence of a suppressed negative impact of arrest on test score is evident in the final model of the main effect of arrest on test scores. The inclusion of theoretically and empirically relevant control variables further attenuates the impact of sanctions on test scores. Variables were selected for inclusion in the final model through an iterative process which eliminated variables that made no contribution to the model or to the coefficient for arrest. Furthermore, some variables like in-school delinquency were accorded preferential treatment in the selection decision owing to their theoretical relevance.

Next, model 2 was run in two other data sets with different sets of imputed values. No major differences emerged, although the other two data sets generated a larger negative effect of arrests of –1.24 and –1.27 respectively. Setting upward limits on the lag time between the most recent survey and the arrest or comparison date and on the number of variables missing from the missing values imputation model (not shown) made no appreciable difference either. Tolerance statistics and the Durbin-Watson statistics yield no evidence of multicollinearity and autocorrelation respectively.

Conditional models of seventh grade test scores

The next step was to examine, using variants of model 2, whether the impact of arrest on reading achievement depends on individual's characteristics or circumstances. Given that a high

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degree of statistical power is necessary to detect interactions, and the sample size for the seventh grade test scores analyses is small, the odds are against the discovery of significant statistical interactions.

The first factor that may condition the impact of arrest on seventh grade test scores is the number of arrests during the school year. To test this interaction, two dummy variables were computed—one indicates whether a youth was arrested only once during the school year (1=arrested once, 0= not arrested once). The second measures whether a youth was arrested multiple times during seventh grade (1=arrested multiple times, 0=not arrested multiple times). The reference category for these two nominal categories, of course, consists of the comparison group, who are not arrested at all during seventh grade. Among the arrested treatment group, 55 have a single event during the school year while 34 have multiple events. The first conditional model in Table 4 suggests that those who are sanctioned multiple times may experience more adverse consequences with respect to reading test scores than those arrested only once (both single and multiple arrests are indicated by dummy variables; the comparison group are the reference category). However, the sizable impact of multiple arrests is not statistically significant. The p-values are provided (in parenthesis) for effects that are theoretically important or marginally significant.

Next, a number of other conditional hypotheses were tested. Results (not pictured) suggest that the impact of arrest does not vary by the number of prior station adjustments, gender, and age. Bear in mind that tests of interaction are quite conservative, not only because of sample size, but also because muticollinearity between the main effect and interactions terms

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inflate the standard errors of the effect estimates. Consequently, the apparently modest evidence of an arrest and family structure interaction is quite compelling. As model 2 in Table 11 demonstrates, having no biological parent in the home is predicted to increase the "harmfulness" of arrest with respect to reading scores by 6.2 points.

Table 4
Conditional Linear Regression Models of Seventh Grade Reading Scores

	Model 1		Model 2		Model 3	
Variable	*B*	β	*B*	β	*B*	β
Arrested	N/A	N/A	1.48	.05	-10.88	-.37(.22)
Single arrest	.12	.00	--		--	
Multiple arrests	-2.35	-.05	--		--	
6th grade reading	.39	.42***	.42	.45***	.38	.43***
Structured activities	-2.00	-.14*	-1.81	-.12*	-2.11	-.15**
Neighborhood attachment	5.21	.09	5.26	.09	[a]	
Neighborhood anonymity	-6.34	-.11(.13)	-6.44	-.11(.12)	-6.69	-.12*
Age at sanction date	-1.09	-.04	-.88	-.04	-1.89	-.08
Prior station adjustments	-1.00	-.08	-1.02	-.08	-.78	-.07
School deviance	1.76	.07	1.91	.07	[a]	
No parent	--		4.54	.15*	--	
Arrested * no parent	--		-6.20	-.15(.14)	--	
Parent-child relations	--		--		-4.31	-.20**
Arrested * parent-child relations	--		--		2.42	.33(.27)
Constant	32.65		27.08		79.50	***
R^2	.30		.31		.31	
Valid sample size	167		167		150	

Note. The numbers in parentheses indicate *p* for marginally significant or important variables.
[a] In the interest of parsimony, this variable was dropped from the model because it exerted a negligible independent influence on both the outcome variable and the coefficient for arrest.
p<.10. ** p<.05. *** p<.01.

The statistical interaction exhibited in Model 2 was confirmed under conditions of drastically reduced multicollinearity by examining (models not shown) whether the impact of sanctions varied between a no-parent sub-sample and a sub-sample of children living with one or both parents. Among those with no parents in the home (n=61), arrest was associated with a –8.5

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(beta=-.312) decrease in reading test scores (p=.014).[40] Among this sample, the effect is, remarkably, substantially stronger than the impact of 6th grade reading scores (beta=.210, p=.113). Among the sample living with at least one biological parent, the impact of arrest is actually in a positive direction, although far from statistically significant.

In order to help preclude the possibility that this significant finding is a chance occurrence, other interactions were tested between arrest and theoretically similar variables. Under the assumption that youth who live with neither biological parent are more vulnerable to negative consequences of arrest because they disproportionately lack a social and emotional safety net to help them contend with the disruptions associated with an arrests, the conditional influence of other measures that tap into this safety net were measured. As model three reveals, a similar finding did emerge with respect to the moderating influence of parent-child relations. While the coefficients of the main effect terms should be viewed very cautiously, controlling for the interaction of parent-child relations and arrest yields a suspiciously large negative main effect of both arrest and parent-child relations and an interaction coefficient that suggests that higher levels of parent-child relations are associated with less harmful effects of arrest. On the other hand, I failed to obtain additional support for the "safety net inoculation" theory in tests of statistical interactions between arrest and home academic support, teacher respect, and neighborhood social capital.

[40] Because the sample size is too small, the statistical power of the analysis was increased by omitting two variables, prior station adjustments and structured activities, that contributed almost nothing to the final model.

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Thus, I found only limited evidence that arrests lower academic achievement. Perhaps those whose academic motivation or skills are impaired by an arrest tend to stay home on test day.

The Impact of Arrest on Eighth Grade Test Scores

The analysis of eighth grade tests followed similar procedures as the seventh grade analysis, except that the analysis focuses on math scores rather than reading scores. This choice reflects that the fact that greater disparities between arrested and non-arrested eighth graders exist on the change in math scores than for the change in reading scores. As Table 1 shows, arrested youth experienced double the improvement in math scores compared to non-arrested youth.

Recall, however, that the five point improvement in normalized math scores among arrested youth does not indicate that arrested youth, on average, experience a substantial improvement in math skills relative to national norms and to comparison students. Rather, the finding must be tempered with the fact that 28% of the arrested eighth graders have no eighth grade math score to speak of (24% are missing reading scores).

Once again, the share of factors that statistically distinguished arrestees from non-arrestees was about the same as one would expect if youth were assigned to the arrest and non-arrested groups at random. On the other hand, the treatment and comparison groups, even more so than these groups in the seventh grade staggered sample, differed on several measures that one would expect to be strongly associated with arrest, such as prior police contacts, peer delinquency, and peer drug use. This suggests that non-random confounding differences

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Table 5
Characteristics of the 8th Grade Treatment and Control Groups[a]

Variable By Type	Control	Treatment	P	Correlation with 8th grade math (p<.10)
		Reliability controls		
Survey/event lag (months)	10.01	11.69	.08	-.10
		Background measures		
Black	.88	.87	.81	
Latino	.11	.13	.52	
No biological parent in HH	.37	.39	.63	
Single parent	.47	.43	.45	
Male	.82	.77	.18	
Age at event	13.88	13.97	.13	
Seventh grade math	27.12	23.73	.05	.68
		Emotional/behavior measures		
Delinquent activity	1.61	1.70	.24	-.15
Lack of negative coping strategies	3.99	3.82	.03	.30
Life satisfaction	4.04	3.96	.20	.13
Negative stressor events	.70	.85	.06	
Peer delinquency	1.83	2.03	.04	
		Neighborhood variables		
Neighborhood cohesion	3.16	3.19	.12	.17
Police responsiveness	2.11	2.16	.17	
Unemployment rate	28.43	26.72	.25	-.11
Sample size range	192:218	144:166		

[a] Values are for valid cases only; no statistically imputed values are included.

between the two groups do exist and need to be controlled statistically.

Table 5 displays the characteristics that both distinguish arrested and non-arrested eighth graders and are correlated with the outcomes, as well as some other key descriptive

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characteristics.[41] Variables were selected for inclusion in initial models and missing values were imputed for the ordinal or continuous variables in the model following the procedures described for the seventh grade test score analysis. The imputation model included twenty-nine variables.

Table 6 displays the results of several models of the impact of arrest on math scores in the eighth grade. Consistent with prior analyses, all the sub-samples include some youth (about 7% of the

Table 6
Linear Regression of 8th Grade Math Scores on Arrest Status and Controls

	Model 1		Model 2		Model 3	
Variable	B	Beta	B	Beta	B	Beta
Arrested	.50	.02(.68)	.61	.02(.62)	.47(.72)	.02
Seventh grade math	.55	.65***	.49	.58***	.54***	.66
Lack of negative coping strategies			3.43	.18***	1.37	.07
Delinquent activity			-.74	-.04	-1.22	-.07
Parental control			-2.69	-**	-3.78**	-.14
				.10		
Life satisfaction			1.05	.04	1.52	.07
Survey/event lag (months)			-.08	-.05	--	
Negative life events			1.35	.08*	.92	.05
Peer delinquency			.70	.05	--	
Neighborhood cohesion			5.74	.09**	--	
Non-employment rate					-.06*	-.09
(Constant)	14.99	***	-9.93		24.02***	
Valid sample size	300		297		218	
Durbin-Watson	1.94		1.95		1.69	
R^2	.42		.48		.56	

* p<.10 ** p<.05 *** p<.01

treatment group and 10% of the comparison group) whose arrest or comparison date falls in seventh grade after the seventh grade achievement tests were administered. To enhance

[41] The t-test comparisons and correlation matrices that generated the final list of variables in Table M were conducted for both a base sample and a sample of youth with 12 or fewer months lag between the arrest or comparison date and the most recent attitude and behavior survey.

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measurement reliability, the sample excludes anyone with missing values on more than ten variables in the multiple imputation models. Initial models were generated using several imputation data sets. After establishing that results were similar across multiple imputations, the imputed data set that generated the results that were most typical was used for all remaining analyses of eight grade test scores.

Model 1 is the most basic model that includes only the treatment indicator and prior test scores. It seeks to confirm the unexpected initial impression that arrested youth experience a disproportionate gain in test scores relative to comparison youth. The results show that, while the effect of arrest is in a positive direction, it is far from statistically significant. Model 2 incorporates the control variables that survived excision owing to collinearity, irrelevance, and other factors. The inclusion of these variables in the model increases the estimated effect of arrest very slightly. In addition, the explanatory power of the model, as indicated by the R-square statistic, increases only slightly. This underscores the efficacy of the staggered replication design in limiting selection bias. Few variables both distinguish arrestees and non-arrestees and are strong predictors of test score. A few of the control variables—parental control, peer delinquency, and negative life events—appear to exert effects on test scores in the opposite direction from what would be expected. With respect to peer delinquency and negative stressor events, these effects are not maintained in models with fewer variables. Greater parental control on the other hand is robustly associated with lower test scores. This suggests, perhaps, that at time of survey participation, parents of the arrested group had already responded with increased monitoring to their children's problems at school and with the police.

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To more reliably control for differences between arrested and non-arrested youth the model was estimated for only youth who had a recent attitude survey (12 months or less prior to arrest or comparison date). Three variables, retention, event/survey lag, and peer delinquency were dropped due to irrelevance. In addition, neighborhood non-employment rate replaced neighborhood cohesion, because it was a much stronger covariate of arrest and post-arrest math scores among the restricted lag sample. Once again, the results, shown in Model 3, suggest no effect of arrest on eighth grade math scores. The fact that this mode, with fewer variables, shows a lower impact of arrest and a higher R-square statistic than previous models suggests that this model more effectively controls for differences between arrested and non-arrested youth that are strongly related to test scores.

Conditional Models of Eighth Grade Test Scores

The next set of models, in keeping with prior analyses, tests whether the impact of arrests on eighth grade test scores may depend on individual characteristics or circumstances. The first model of Table 7, model 1, tests whether the 51 youth in the base model who are sanctioned multiple times are more likely to experience harmful effects of arrest than the 107 youth who are arrested only once. Model 1 provides no evidence that youth who are sanctioned multiple times experience more harm. If anything they are more likely to benefit. It must be noted, however, that an analysis of the treatment group alone (not presented) found that the total number of events is negatively associated with eighth grade math scores (p=.269).

Next, I tested other anticipated interactions. The impact of sanctions does not appear to vary by gender. Furthermore, as was the case with effects of seventh grade arrests, the impact of eighth grade arrests does not appear to be conditioned by most of the social support measures.

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These include parent-child relations, home academic support, parental control, and neighborhood cohesion and social capital. And, as was true with the seventh grade results, the impact of

Table 7
Conditional Models of the Impact of Arrest on 8th Grade Math Achievement

Variable	B	Beta	B	Beta	B	Beta
Arrested			-1.10	-.04(.48)	5.42	.20*
Single arrest	.87	.03				
Multiple arrest	1.52	.03				
Seventh grade math	.49	.58***	.47	.56***	.50	.59***
Lack of negative coping strategies	3.49	.18***	3.37	.18***	3.38	.18**
Delinquent activity	-.82	-.04	-1.27	-.07	.56	.03
Prior grade retentions	-2.51	-.03				
Parental control	-2.72	-.11**	-3.08	-.12**	-2.84	-.11**
Life satisfaction	1.05	.04	1.23	.05	1.09	.05
Survey/event lag (months)	-.07	-.05	-.10	-.06	-.09	-.06
Negative life events	1.27	.07(.11)	1.19	.06	1.22	.07(.12)
Peer delinquency	.75	.05	.90	.06	.74	.05
Neighborhood cohesion	5.82	.09*	6.97	.11**	6.14	.09**
No biological parent in HH	-		-3.17	-.11		
Arrested * no parent	-		4.31	.11*		
Single parent HH	-		-1.44	-.05		
Arrested * delinquency	-				-2.92	-.21*
(Constant)	-10.39		-9.76		-12.75	
Valid sample	297		287		297	
R^2	0.479		0.472		0.482	

* p<.10 ** p<.05 *** p<.01.

eighth grade arrests also appears to depend on family structure. However, strangely enough, youth who live apart from their biological parents are more likely to improve in tests scores following an arrest. The results of Model 2 suggest that having no biological parent in the home increases the positive effect of arrest on math score by 4.4. The interaction is so strong that the

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inclusion of the interaction term in the model actually switches the direction of the main effect of arrest to its expected negative one. This interaction was verified in sub-sample analyses (not pictured), which reveal that the effect of sanctions for youth in the no parent group is in a positive direction, while the effect of arrest is in a negative direction for youth in either single parent or two parent households. None of the effects are statistically significant, however.

Other youth that are expected to be especially harmed by sanctions are those with a negative disposition toward authority, who may be more likely to view their sanction experience negatively (Paternoster et al., 1997). While no interactions were evident between arrest and teacher respect or anger control, youth who exhibit higher levels of delinquent behavior are more likely to experience an apparently negative impact of sanctions. As Model 3 demonstrates, each unit increase on the delinquency scale is associated with a three point decrease in the impact of arrest on test scores. Indeed, the coefficient representing the interaction effect is so strong that its inclusion actually renders the main effect of arrest statistically significant. A sub-sample analysis involving youth who scored in the top 33% on the delinquency scale confirmed that youth with high rates of delinquency experience, on average, a three point drop in math test scores, if they are arrested ($p=.11$). Bolstering the reliability of this interaction is the finding of a similar though not as strong interaction between being arrested and delinquent peers (not pictured). On the other, the impact of arrests does not appear to depend on the number of prior station adjustments (also not pictured).

The Impact of Test Scores on Eighth Grade Retention

Aside from test scores, the other available indicator of academic achievement in seventh and eighth grade is grade retention. To a significant extent, grade retention is a counterpart to (or

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an extension of) the test score measure, given that poor academic progress is the most common reason for grade retention. As mentioned earlier, beginning in the spring of 1997, the relationship between eighth grade test scores and grade retention was formalized; graduation to ninth grade was suddenly conditional on eighth grade achievement test scores. Youth who fail to take the achievement tests may also be retained. Low grades, poor attendance, and behavior problems may also be a factor in the retention decision for both grades. Retention in seventh grade was not made systematically dependent on test scores and is consequently less common. Among the seventh grade base staggered sample, 6.6 percent of the arrested group are retained following their arrest compared to 7.4 percent of the comparison group. According to a chi-square test as well as multivariate models (not shown), these proportions are statistically equivalent. This analysis will focus on eighth grade retention since it is both more common and more consequential than seventh grade retentions.

In keeping with previous analyses, the base "retention sample" includes youth who were enrolled in eighth grade at the time of the arrest or comparison dates and had completed at least one attitude survey prior to this date.[42] Those who are missing more than two variables in the model are excluded from the regression models because their imputed data presumably is less reliable.

Table 8 presents the characteristics of the treatment and comparison groups in the base sample. First, note the large disparity between treatment and comparison group in the rate of

[42] Youth who dropped out of school before the start of ninth grade or whose transfer to another school, detention, or incarceration censored their data on retention are included in the variable selection procedures but excluded from statistical models of the odds of grade retention. The sample also excludes youth who are already in the process of repeating eighth grade year.

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eighth grade retention. Arrested youth are nearly four times as likely to be retained after eighth grade. This is true despite the fact that the arrested and comparison groups differ neither in 7th grade nor even eighth grade test scores. As explained earlier, the treatment groups differ markedly in the share who failed to complete an eighth grade test.[43] The two groups also significantly diverge in the number of prior police contacts, and less markedly, in the level of prior delinquency.

The difference in retention rates reflects, in no small part, the timing of sanction dates in relation to the passage of the automatic retention ordinance in Chicago. 39% of the treatment group were arrested during or after the 1996-97 school year, when retention followed automatically from test scores. By contrast, the comparison dates of only 23% of the comparison group fell during this time period. Hence, the arrested group face an elevated probability of retention following arrest owing simply to the timing of arrest.

Table 8 displays other characteristics that distinguish arrest from non-arrested eighth graders that may also be related to retention. The next step was to distill the pool of potential covariates into a much smaller pool using analogous criteria ($p<.25$ for relationship of covariates with arrest and grade retention) which I utilized for test score analyses.

Then, the causal impact of first arrest and ensuing juvenile justice involvement on grade retention was estimated by modeling the odds of eighth grade retention as a logit function of arrest and its covariates. The logistic model can be written as: $y_i^* = x_i\beta + \varepsilon_i$ where y_i^* represents the latent propensity to be retained, x_i is the vector of selected independent variables including

[43] The rates of completion presented here apply only to youth whose sanction or comparison dates occurs during youths' first attempt at eighth grade and prior to the test date. Consequently, the rates of completion differ from those reported earlier from the eighth grade test score sample.

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Table 8
Characteristics of Eighth Grade Retention Sample[a]

	Mean/percent		SD		
Variable	Control	Treatment	Control	Treatment	p
Retained in 8th grade (%)	4.5	16.2			.00
Automatic retention policy in effect (%)	23.0	38.8			.00
Peer drug use	1.59	1.87	1.20	1.42	.05
Teacher caring and respect	3.51	3.61	.93	1.13	.37
Soft drug use	1.86	1.87	1.37	1.34	.93
Average prior achievement	28.64	27.54	14.34	12.74	.42
Delinquency	1.60	1.70	.66	.78	.19
Parental control	3.68	3.73	.53	.55	.33
Previous math	27.30	25.65	16.74	14.82	.31
Previous reading	30.23	29.41	14.64	14.07	.58
Post (8th grade) reading (% completion rate)	30.69 (93.9)	28.55 (76.3)	15.78	13.98	
Male (%)	81.1	78.7			.54
Latino (%)	10.7	13.4			.39
Emotional/behavior disordered (%)	1.6	3.7			.20
Learning disordered (%)	13.9	12.8			.72
Drug offense (%)	41.8	29.3			.01
Violent/weapons offense (%)	32.0	43.1			.02
Property offense (%)	23.4	26.1			.52
No parent in the home (%)	37.0	32.2			.35
One parent in the home (%)	44.0	46.7			.61
Age at event	13.99	14.11	.55	.59	.03
Prior station adjustments	.62	1.10	.91	1.42	.00
Sample size range[b]	190:244	147:188			

[a] Sample includes youth who are disqualified from regression analyses due to missing values.
[b] Sample size range does not encompass the sample who completed an eighth grade reading test, which is an unusually small sample owing to attrition and the exclusion of those sanctioned after the test date.

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arrest, β is the vector of coefficients for those variables, and ε_i represents the error term with respect to x_i. The relationship between the latent variable and x_i is estimated using maximum likelihood estimation procedures. From this equation a multivariate regression model known as the logit is derived. The logit model presents the log odds of the dependent variable as a linear function of the explanatory variables. The coefficients in the logit model, β_i, can be interpreted as the change in the log odds of being retained associated with a unit increase in the of retention associated with a unit increase in the coefficient.

The first model in Table 9[44] indicates that being arrested in eighth grade is associated with more than a three-fold increase in the risk of grade retention, even after controlling for the

Table 9
Logit Models of the Impact of Arrest on 8th Grade Retention

	Model 1		Model 2		Model 3	
Variable	B	Exp(B)	B	Exp(B)	B	Exp(B)
Arrested	1.17	3.21***	.64	1.90(.19)	.53	1.69(.30)
Automatic retention policy	1.15	3.15***	1.24	3.45**	1.35	3.88**
Peer drug use	.38	1.46**	.23	1.26	.26	1.30
Perceived school caring	-.36	.70*	-.45	.64	-.41	.67
Marijuana and alcohol use	-.30	.74*	-.25	.78	-.30	.74
Academic achievement	-.02	.98	-.03	.97(.12)	-.02	.98
Neighborhood unemployment rate	.02	1.02*	--		--	
Prior station adjustments			.14	1.15	.16	1.17
Positive coping strategies			-.32	.72	-.25	.78
Number of missing values			--		.11	1.12**
Constant	-3.30	.04**	.40	1.49	-.80	.45
Valid N	392		247		247	
-2LL	199.25		129.63		124.27	

* p<.10 ** p<.05 *** p<.01.

[44] The final pool of covariates excludes delinquency, parental control, negative coping strategies and neighborhood decline because they were irrelevant to the explanation of eighth grade retention and to the impact of arrest on the odds of grade retention.

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impact of the automatic retention policy and other covariates like individual and peer drug use,[45] neighborhood unemployment, the mega-construct combining teacher caring and respect, and the average of prior math and reading scores. Moreover, according to the Wald statistic, arrest is one of the strongest predictors in the model, second only to the automatic retention policy variable. Durbin-Watson and tolerance statistics suggests little or no bias associated with autocorrelation and multicollinearity, respectively.

Next, I tested the model among youth with more proximally measured selection factors—which allows selection bias to be more reliably measured and controlled. Because the base retention sample is, relatively speaking, so large, restricting the sample to youth with event/attitude survey lags of six months or less still yields a sizable sample of about 150. However, substantially reducing the sample also markedly decreases observed instances of grade retention. Balancing the need to reduce selection bias with the need to maintain the power to statistically detect effects of arrest on retention, I restricted the sample to those with event/survey lags of ten months or less. This sample modification reduces the number of retained cases from 35 to 22, and the incidence of grade retention among this sample declines to 12.9% among arrested youth and 6.2% for controls.

The corresponding logit model of the impact of arrests and proximal covariates on the odds of grade retention is presented under Model 2 in Table 9. Because reducing the lag tends to uncover additional distinguishing characteristics of arrestees and non-arrestees that are also covariates of the outcome, the model includes two additional variables—the use of positive

[45] The surprising results for the negative effect of drug use on grade retention reflects the impact of drug use only after the impact of other variables like the peer drug use are taken into account. In more parsimonious models, drug use has no impact on retention.

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coping strategies and the number of prior police contacts. It also drops the neighborhood unemployment indicator for its irrelevance in final models. Thanks due in unknown portions to better statistical control of proximal selection factor and weaker statistical power, the impact of arrest is smaller—"only" twice the odds of grade retention among arrestees—and is not statistically significant (p=.19). Evidence that statistical power may play a large role in the attenuation of the arrest effect is the fact that among the effects of the other covariates, only the effect of automatic retention policy remains significant. The effects of the covariates in this model are smaller for this sample than they were for the base sample, even when their relationships with arrest and retention are comparable across the two samples.

While the results in the reduced lag model are not incompatible with a causal impact of arrests on grade retention, the inclusion of an additional control variable for differential reliability throws this conclusion into doubt. Specifically, when the variable that indicates the number of values missing among the variables in the multiple imputation model is included (see model 3 in Table 9), the effect of arrests and its statistical significance is substantially reduced. At least two reasonable explanations of this finding can be entertained. First, the number of missing values can be interpreted as an indicator of the reliability of the variables in the model (since the reliability of imputed data is a function of the prevalence and reliability of observed data). This variable's usurpation of the effects of arrest on grade retention is indicative that covariates are more reliably measured for the control cases and that differential measurement reliability has inflated the impact of arrest. A second possible interpretation is that the effect of the number of missing values, rather than representing differential measurement error with respect to the covariates, is itself a meaningful indicator and a confounding explanation of the

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impact of arrest. Perhaps the number of missing items on a survey is indicative of a youth's interest in schooling and other conventional activities like responsible survey participation. This reliably measured behavioral indicator of interest may help predict both arrest and grade retention. Regardless of what this variable measures, be it rates of measurement error or levels of disengagement, excluding youth who exhibit elevated levels yields results that more decisively indicate a negative impact of arrest on grade retention.

Conditional Models of Eighth Grade Retention

The next step in the analysis is, of course, to examine whether particular characteristics or circumstances condition the impact of arrest on grade retention. To increase statistical power, interaction models utilize a sample with a relaxed event/survey lag restriction (less than 24 months instead of 10 months) and set an upper bound on the number of missing values of 14 in order to increase reliability without jeopardizing statistical power.

Interaction models are presented in Table 10. The first model demonstrates that youth who are arrested only once are only slightly more likely than the comparison group to be retained and the effect is not statistically significant. Those arrested multiple times, by contrast, are 240% more likely to be retained. The impact, however, is not quite statistically significant. Note that the effect of multiple sanctions approaches statistical significance (p=.10) in the original base sample but that its effect is diminished when the number of missing variables in controlled. Other lurking sources of variation between multiply arrested and non-arrested youth have not been thoroughly explored. The lack of statistical significance of the effect of multiple events could also reflect the fact that there are only 39 youth arrested multiple times (six of whom are retained). On the hand, it could be the case that elementary schools have little desire to "retain"

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youth who are arrested multiple times and therefore choose to put aside academic considerations and either promote such youth to high school or send them to an alternative school setting. Consistent with this interpretation, a sub-analysis involving only the treatment group youth revealed that the number of arrest events was not at all associated with the odds of retention. However, inconsistent with this interpretation, arrested youth with high levels of in-school delinquency were not less likely to be retained.

Next, I assess whether the impact of arrest depends on the level of various social supports. The results of interaction models suggest that it does not depend on neighborhood

Table 10
Conditional Models of the Effect of Arrest on Eighth Grade Retention

	Model 1		Model 2		Model 3	
Variable	B	Exp(B)	B	Exp(B)	B	Exp(B)
Arrested	--		-4.68	.01**	.43	1.54
Single arrest	.50	1.65(.32)	--		--	
Multiple arrests	.88	2.40(.14)	--		--	
Automatic retention policy	1.38	3.96***	1.29	3.62***	1.36	3.90***
Peer drug use	.41	1.50**	.40	1.49**	.43	1.54**
Perceived school caring	-.33	.72	-.30	.74	-.32	.73
Marijuana and alcohol use	-.37	.69*	-.39	.68*	-.37	.69*
Academic achievement	-.02	.98	-.02	.98	-.02	.98
Neighborhood unemployment rate	.02	1.02	.01	1.01	.02	1.02
Parent-child relations	--		-.91	.40**		
Arrested * parent-child relations			1.48	4.40**		
Prior police station adjustments	--		--		-.43	.65
Arrests * station adjustments					.61	1.84(.14)
Number of missing values	--		--		.08	1.08
Constant	-2.87	.06**	.34	1.41	-3.47	.03**
Sample Size	334		334		334	
-2LL	156.42		146.78		150.27	

* p<.10 ** p<.05 *** p<.01

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social capital, home academic support, or perceived levels of school staff caring and respect. The effect is conditioned by the warmth of parent-child relations, but in an unexpected way. As model 2 shows, each unit increase in the warmth of parent-child relations is associated with a 4.4 odds ratio *increase* in the relationship between arrest and retention. The effect remains strong even after controlling for the number of missing values among variables in the imputation model. This interaction reflects the fact that among youth with high levels of parent child relations (scoring within the top 40% on this measure), none of the 85 non-arrested youth were retained compared to 15% of 68 arrested youth. One way to read this finding is that warm relationships with parents can reduce grade retention, but only among youth who have not been arrested.[46]

In the same fashion as attachment to parents, greater attachment to school is also associated with a stronger impact of arrest. However, this interaction is not statistically significant (p=.19) in part due to the high variance inflation factor of the interaction term.

The only other possible moderating factor uncovered is the number of prior station adjustments. As model 3 demonstrates, arrest may be more harmful with respect to increases in grade retention, among youth with more prior station adjustments. This finding may be a quirk of the random imputation process, however. When only cases with actual (non-imputed) data on prior police contacts are included (which reduces the sample size by nearly 100 cases) the interaction is no longer statistically significant (though the size of the effect does not diminish).

[46] While the negative direction of the coefficient representing the main effect of arrest on retention in Model 2 underscores the strength of the statistical interaction it should not be interpreted as a measure of the true affect of arrest. Sub-sample analyses (not shown) reveal that whether parent child relations are warm or cold, arrest still increases the odds of grade retention.

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Thus far, the evidence that arrest impairs educational performance is highly suggestive but not definitive. The next chapter investigates whether this uncertainty fades as youth make the giant leap into the unfamiliar, often unwelcoming, world known as the inner-city high school.

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CHAPTER 6: THE IMPACT OF ARREST ON HIGH SCHOOL OUTCOMES

The analyses in the last chapter suggest that being arrested does not affect math and reading skills but that it probably increases retention in eighth grade. These results seem somewhat contradictory given that eighth grade retention is contingent on achievement test scores. The key to resolving this discrepancy appears to be the greater tendency of arrested youth to miss the test, which can lead to a higher rate of retention while placing upward (attrition) bias on test scores. While I cannot pinpoint exactly why arrested youth are more commonly "no-show" on test day, the most logical explanation appears to be truancy and disengagement from school. For the ninth grade staggered replication, I am able to subject this notion of an effect of arrest on truancy and disengagement to a direct test as data are available on absenteeism and dropping out.

One may expect that the next logical step within the staggered replication framework, instead, should be to assess the impact of ninth grade arrests on ninth grade retention and test scores. Unfortunately, reliable and comparable assessments of these impacts are not possible with these data. First, as mentioned earlier, retention in high school carries a different meaning than retention in elementary school years. While elementary school retention decision may result from social and behavioral assessment as well as academic progress, retention in high school is based purely on whether a student has earned the grades and credits necessary to advance to the next grade. Retention in high school, thus, is largely an extension of course grades. Since course grades are available each semester and are a continuous rather than a dichotomous measure, they are better suited for testing the impact of arrest on achievement.

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High school test scores are also not examined for the simple reason that too few youth actually take the ninth grade test. Specifically, 53% and 52% of students in the ninth grade staggered sample provide a post math and reading score respectively. Once again, treatment youth were markedly less likely to provide a ninth grade test score than the comparison group. Multivariate analyses suggest no difference in scores net of previous scores among youth who took the test but these results are weakened by low statistical power. Consequently, they were not subject to multiple imputations and the full range of model selection and diagnostic procedures explicated in the previous chapter and applied to all the analyses in this chapter as well (unless otherwise stated).

The Impact of Arrest on Dropping Out

The analysis of dropping out will focus on those who are arrested while enrolled in ninth grade and their comparison group who are not arrested until a year later. Chapter three described the two main ninth grade samples that provide cases from the drop out analyses. To review, the base sample includes all youth whose sanction or comparison date occurs while youth are enrolled in their first year of high school or the summer . The pure drop out sample excludes from the comparison group youth whose actual sanction date occurs prior to the fall snapshot of their 10^{th} grade year because they are no longer an untreated comparison group at the time the outcome is measured.

Control Variable Selection

The next step was to select variables for models of dropping out of school by October of 10^{th} grade. The first step in variable selection was to compare treatment and control groups on all the variables described in Chapter four and a few more that have evaded theoretical or

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empirical scrutiny. Independent samples t-tests and chi-square tests in the case of dichotomous variables compared treatment and comparison youth within both the base sample and the pure drop out sample. In addition, given that differences between and comparison youth may not emerge until the two groups get closer to the arrest of the treatment group youth, another set of t-test comparisons involving both samples included only youth whose most recent attitude survey preceded their sanction or comparison data by no more than eighteen months.

These t-test and chi-square revealed that only a very small number (seven to be precise) out over a hundred variables significantly ($p<.05$) distinguished arrested from non-arrested youth. The number of significant differences that emerged from the four sets of t-tests ranged from five to seven—fewer than what one would expect based on chance alone.

A conclusion of statistical parity for the dropout sample is premature and misguided, however. First, if significant differences on a small number of variables emerged by chance alone one would expect the variables featured prominently in the theoretical literature as potential covariates of arrest and dropping out to stand the same chance of making the list of significant covariates as variables afforded less attention in the literature. This is clearly not the case. The various sub-samples consistently yielded a list of significant variables at the .05 or .10 level that the literature suggests bear a strong and direct relationship to arrest. For instance, the treatment and comparison groups differed significantly in soft drug use ($p<.10$) among the base sample, the pure drop out sample, and the pure drop out sample with a reduced lag ($p<.01$). Likewise, the amount of delinquency and time spent indoors, which clearly affect police scrutiny of behavior, and level of anger and perceived respect from teachers, which may proxy for the demeanor youth present to the police, significantly distinguish arrested and comparison youth in

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at least one sub-sample. The consistency of observed covariates with the literature suggests that spurious correlation is a real concern that must be addressed empirically.

The second reason to doubt a conclusion of statistical parity is that, given the long lag between the "pre-test" and the onset of "treatment" and the high prevalence of delinquent and disengaged youth in the sample, many of the control variables are measured with considerable measurement error. To the extent that this measurement error hides true differences between treatment and control groups, it can lead to inflated estimates of the impact of arrest. One way to counter this bias is to err on the side of caution and include a large number of potential covariates.

Thus, as I did in the analyses of elementary school outcomes, I relaxed the .05 standard for the inclusion of control variables, and included *as* covariates in initial models variables that both distinguished arrested from non-arrested youth *and* dropouts from non-dropouts at the .25 level. These tests yielded the aggregate set of covariates listed in Table 1, which provides their means and standard deviations for both the treatment and comparison groups in the base drop out sample and the pure drop out sample. Variables that meet the selection criteria for one or some sub-samples but not others are included based on their empirical importance and theoretical relevance.

Table 1 also includes several variables that are normally associated with sanctions and dropping out in order to illustrate the impressive degree of convergence achieved. Variables like acting out that fail to distinguish at least one of the comparison groups from the arrested group at even at the .25 level are also noted.

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The similarity between treatment and comparison groups with respect to acting out behavior is indeed the most notable, but it is also misleading. When I restrict the comparisons to those with proximal measures of acting out (within eighteen months prior to the event), the differences in acting out become significant—especially among the pure drop out sample. This pattern is also manifest in higher numbers of prior station adjustments and higher levels of soft drug use among the treatment groups. Differences in the nature of the offense committed are also somewhat troubling as they are suggestive of different patterns of proximal offending behavior, which may in turn exert differential independent effects on dropping out.

On the other hand, with respect to proximately measured indicators of prior achievement--reading scores, math scores (not shown) and 8th grade retention—differences are trivial or non-existent. Discrepancies favoring the non-arrested are slightly more evident on family structure and neighborhood climate (i.e. neighborhood attachment and crime rate) but the overall pattern is one of concordance between treatment and comparison groups on a range of indicators of personal and neighborhood disadvantage (not pictured).

Variables were selected for inclusion in initial models if they covaried with arrest *and* dropout among any of the sub-samples. The final step in variable selection was to excise from the pool of variables considered for inclusion in the model, variables whose inclusion would present problems of multicollinearity and would contribute no useful information to the actual impact of sanctions on school performance.[47]

[47] When highly collinear pairs or clusters variables like delinquency and acting out or police responsiveness and excessive force both meet eligibility standards and cannot logically be combined into a single construct, I chose only one construct for the models. The choice of variable, which occasionally varied depending on the sub-sample of interest, was based on the sum of the p-values for the t-test of

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Table 1
Characteristics of the Treatment and Control Groups

Outcome variable (by type)	Treatment	Control	Pure Control
Dropout by Fall of 10th grade (%)	21.0	6.4***	2.9***
Reliability controls			
Lag between event and survey (months)	16.64	13.69***	14.85
Number of Missing Variables	5.82	4.04***	3.97***
Prior performance and background			
Male (%)	80.4	76.7†	73.1
Age at sanction date	15.0	14.80***	14.9†
8th grade retention (%)	4.9	2.5	1.0*
No biological parent in HH (%)	35.6	26.6	26.3
Emotional/behavior measures			
Violent or weapons offense (%)	31.8	40.9*	38.5
Acting out[a]	2.00	1.93† [a]	1.87 * [a]
Marijuana and alcohol use	2.08	1.80	1.64***
Prior police station adjustments[b]	1.28	.84***	.87**
Respect from school staff	3.49	3.43	3.47†
Neighborhood variables			
Neighborhood attachment	2.76	2.72*	2.73
Census tract crime rate	186.02	184.49†	179.65†
Excessive force problem	2.38	2.36†	2.41†
Police responsiveness	2.16	2.13†	2.17†
Other variables			
Negative life events	.84	.75†	.77†
Black (%)	88.6	85.5†	85.6†
Latino (%)	9.9	13.8	13.6†
Previous reading score	30.07	31.27	30.07
Property offense (%)	24.5	17.0*	14.4**
Drug offense (%)	41.2	40.3	45.2†
Sample size	208-245	157-159	95-104

[a] Delinquency, in school deviance, and gang affiliation were possible covariates, but among these highly collinear variables, acting out had the strongest relationship with arrest and dropout. When the sample is restricted to those with survey lags less than nineteen months acting out distinguishes treatment and control groups at the .10 level.
[b] The sample size for this variable (N=200 and 81) is considerably lower because the number of station adjustments is only measured for youth who were located in the arrest file or who matched with the community adjustment file through another means. * p<.10 ** p<.05 *** p<.01. † p>.25

differences between treatment and comparison groups and the t-test between dropout and non-dropouts with respect to that variable. The variable for whom this sum was the smallest was retained. When this method did not decisively favor one variable, theoretical relevance was also considered.

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Analysis and Results

The causal impact of first arrest and ensuing juvenile justice involvement on dropping out high school was estimated by modeling as a logit function the odds of dropping out by the fall snapshot (about October 1st) immediately following the sanction year. The modeling, estimation, and diagnostic techniques employed are analogous to those described in the retention analysis in the previous chapter.

Table 2 depicts only the final models employed. Some covariates are excluded from the final models because their presence in the model has little or no impact on the size and significance of the impact of arrest and no significant improvement in the model-chi-square. Additional considerations governing the exclusion of certain variables from the final model included its theoretical significance and its statistical significance in the presence of a large number of other covariates. Using the selection process for the first sub-sample as an illustration, variables of strong theoretical relevance like perceived teacher respect were retained in the model despite their low levels of statistical significance. On the other hand, home academic support, because of its relatively strong influence on the coefficient for arrest, remained in the model, despite its weak theoretical and statistical significance.[48] Prior station adjustment, having no parent in home, indoor activities and neighborhood attachment were excluded exclusively on statistical grounds. No other variables, aside from the lack of a neighborhood excessive force problem, whose main effects approached statistical significance

[48] Over four variables, depression, expectations of positive outcomes, the lag between event and the survey, age at the time of survey, were excluded from the first model because they performed poorly on each of the exclusion criteria.

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Table 2
Models of the Impact of Arrest on Dropping out by the Fall of Tenth Grade

	Model 1: Dropout sample		Model 2: Pure sample		Model 3: Dropout sample (lag < 1.5 years)		Model 4: Pure sample (lag <1.5 years)	
Variable	B	Exp(B)	B	Exp(B)	B	Exp(B)	B	Exp(B)
Arrested	1.06	4.27**	2.21	9.07***	1.66	5.27***	1.39	4.03*
Single arrest	N/A	N/A						
Multiple arrest	N/A	N/A						
Acting out	.48	2.04(.11)	.56	1.75(.10)	.17	1.18(.64)	.67	1.95*
Perceived respect	-.43	.65	-.38	. 68	-	-	-	-
Home academic support	-49	.61	-.52	.60(.11)	-.89	.41**	-.71	.49*
Delinquent Peers	.27	1.31*	.39	1.47*	-	-	-	-
8th grade retention	1.17	3.23*	.98	2.67	-	-	2.18	8.81*
Marijuana/alcohol use	[a]							
Age at event	.68	1.972**	.91	2.48**	.86	2.36**		
Number of missing values			.04	1.04			.07	1.08
Event/survey lag (months)	[a]							
Peer drug use	[a]				.35	1.42**		
No Parent at home	[a]						.91	2.48*
Violent/weapons offense	-.48	.62	-.67	.51	-.86	.42(.10)	-.79	.46
Prior police contacts	[a]	[a]	-.24	.79				
Neighborhood variable								
Neighborhood unemployment rate	[a]	[a]			-.03	.97(.11)		
Crime rate	.001	1.001	.002	1.002			.003	1.003*
Excessive force								
Constant	-11.61	.00***			-13.75	.00**	-3.60	.03**
Sample size	339		288		254		215	
-2LL	226.58		173.49		142.04		123.52	

[a] Variable was omitted from the model for relative lack of statistical and/or theoretical importance.
* p<.10 ** p<.05 *** p<.01

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were excluded from the final models.[49] The final variable selection process operated similarly for the other sub-samples, the details of which are available from the author.

The first logit model, Model 1 in Table 2, depicts the impact of arrest on dropping out for the base drop out sample. A key point of interest is that the comparison group of this sample includes a large share (about thirty-four percent) who were actually first arrested in the summer or fall prior to the measurement of dropping out in the fall of tenth grade. As noted, these cases partially erode the treatment/control drop out rate differences but they also help the analysis achieve a more natural balance between treatment and control groups across other potential covariates of dropping out like age at the time of the event, month of sanction, and the lag between survey participation and event. In addition to the above sampling restrictions, the sample was also confined to students who were missing twelve or fewer of the over thirty variables that comprised the imputation model. This restriction increases the reliability of imputed data and helps reduce the statistical noise associated with measurement error.

The results for Model 1 in Table 2 reveals that despite the prevalence of "treated" cases in the comparison group, being arrested during ninth grade is still associated with an over four-fold increment in the odds of dropping out net of covariates. Furthermore, according to the Wald Statistic, which indicates the relative explanatory power of a variable, being arrested is the most influential predictor in the model (Wald=6.51). The effects of each of the selected covariates were in expected directions.

[49] Neighborhood excessive force problem was not included in most final models because its impact on dropping out may be completely bound up with its effect on arrest risk. Including it in the model only serves to usurp from arrest some of its alleged exogenous impact on dropping out.

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Note that this estimate of the impact of arrest on dropping out is actually an underestimate given that youth who are detained or incarcerated at the time of the tenth grade fall snap shot are excluded from the analysis even though many of them stand a good chance of dropping out after being released from the detention center or youth prison. Given that these youth disproportionately comprise the treatment group appropriately re-labeling some of them as drop outs would appreciably inflate the estimated impact of being arrested.

I will now let actual numbers demonstrate the extent of the underestimation of the arrest effect due to detention. Among the base sub-sample (the Model 1 sample), a total of eighteen youth (8.4 percent) were excluded from the treatment group owing to a missing outcome. Eleven of these youth were detained or incarcerated at the fall snapshot when dropping out is measured. Of these eleven cases, three of them dropped out immediately after release, and five returned to detention or prison soon after release (showing no signs of re-enrollment before or after release) and dropped out later (within a year or so).. Two of the cases re-enrolled,one only briefly and the other in an alternative high school. A final case had so many different spells of detention and incarceration over the course of several years that it is unclear whether he ever re-enrolled in school for any length of time. Still, counting nine of the eleven treatment youth with excluded outcome data as drop outs would result in a 25% increase in the number of drop outs in the treatment group!

By contrast, only four (2.7 percent) of the comparison group are missing outcome data—two of them because they were detained at the fall snapshot. Both of these students, in contrast to their treatment group counterparts, returned to school after their release that fall, where they remained for the rest of the school year. This difference among the detained in the two groups

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could be due to chance but more likely reflects the fact that the detention occurs soon after the first arrest among these cases in the comparison group, but well after the first arrest (and often subsequent arrests) in the treatment group.

The next step was to rule out alternative statistical interpretations of this underestimated effect of arrest. In order to rule out multicollinearity and autocorrelation (owing to spatial clustering of observations), a continuous indicator (previous math test scores) was regressed using ordinary least squares regression on the same set of independent variables. Multicollinearity or near multicollinearity is not present among the independent variables given that the highest variance inflation factor is still less than 1.3. Evidence suggests an absence of autocorrelation in spite of limited clustering in the data (Durbin Watson statistic=1.93).

Model 2 of Table 2 depicts the impact of arrest on dropping out when any youth whose first arrest occurs prior to the fall snapshot comparison is purged from the comparison group. This pure drop out sample trims about a third of the comparison group from the regular drop out sample. The same limits on the number of missing values still apply.

As mentioned earlier, spurious correlation is an even greater concern for this model given that the comparison and treatment groups differ by more than a year in the timing of their first arrest and differ systematically in the timing of their first arrests in relation to the school year. Accordingly, a larger number of covariates were spared excision during the final variable selection procedure. The two additions to the model are the number of missing variables in the imputation model and the number of prior station adjustments. The number of missing variables has no impact on dropping out net of the other variables in the model. The number of prior station adjustments, oddly enough, has a negative main effect on the odds of dropping out that

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approaches statistical significance (p=.14). In other words, for two youth in the same treatment group who have the same age, level of delinquency, level of home academic support etc. it is predicted that more (less formal) prior contacts with the police can actually lower one's odds of dropping out. The relationship between the other covariates and dropping out are all in the expected direction. Again, evidence for autocorrelation and multicollinearity is scant.

The impact of arrest in the pure drop out sample is, as expected, quite large. Youth arrested in ninth grade face nine times the odds of dropping out by the fall of tenth grade, relative to similar youth not arrested in ninth grade net of the few covariates that may distinguish them. Note, however, that this sample contains a substantially reduced number of comparison cases—ninety-five—only two of whom are drop outs.

The third model reduces the sample even more drastically and serves mainly to test against an alternative interpretation of the above results—measurement error. Distal survey measures may not capture differences between treatment and comparison youths that emerge as the sanction date approaches. These differences may conceivably account for the large differences in drop out rate between the two groups. To minimize this possibility, I limit the regression sample to those whose attitude and behavior variables are measured within eighteen months of the arrest or comparison date. This step reduced the average event-survey lag in the pure drop out sample by five months (from 15.4 to 10.5 months). While this average lag is arguably still too long, changes in the pattern of results provide an indication of the extent to which measurement error may have influenced the results of earlier models.

Constraining event/survey lags does indeed alter the influence of certain variables. For instance, the positive effect on dropping out of the acting out measure increases and finally

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attains its expected statistical significance. T-tests confirm that acting out differences between treatment and comparison cases were most pronounced in this sample. Thus, in this model more than the previous two, the impact of arrest on dropping out is measured net of observed differences between treatment and comparison groups in acting out. Having no parent in the home also emerged as an influential variable only in this model despite the fact that it is obviously measured with less measurement error than acting out. On the other hand, increasing measurement reliability has decreased the main effect of some variables on dropping out and/or their influence on the arrest effect to such a degree that they were dropped from the model. Dropped variables include perceived respect, and delinquent peers, and prior station adjustments.

Overall the results of Model 3 suggest that arrest still has a strong positive effect on dropping out even when measurement lags are reduced. Arrested youth still have a predicted odds of dropping out that is five times that of the comparison group.

Model 4, however, deviates from the expected pattern. As the differences between Model 1 and Model 2 attest, the omission of arrested youth from the comparison group should elevate the impact of arrest on dropping out. However, Model 4 shows that under conditions of restricted event/survey lags, the effect of arrest on dropping out is actually lower among the pure drop out sample than among the base sample.

Unfortunately, apart from diminished statistical power, I was not able to come up with a compelling explanation for this reduced impact of arrest in the pure drop out sample. One possibility I considered is that being arrested is positively associated with a variable that predicts less dropping out. If this variable is not controlled it can suppress the impact arrest on dropping out.

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Exploring suppressor relationships only among variables that qualified as statistical covariates, I identified a rather unlikely suppressor candidate. In the pure drop out sample, arrested youth live in neighborhoods characterized by greater level of excessive force (as indicated by a lower mean on the "excessive force not a problem" scale). Unexpectedly, in multivariates models involving the pure drop out sample, less excessive force is associated with greater dropping out. Accordingly, when this variable is included (not shown), the arrest regains statistical significance ($p<.05$) and is associated with a 500% increase in dropping out. Presumably, excessive force is associated with other neighborhood characteristics that both increase arrest risk and lower dropping out—which would explain the role of excessive force as a suppressor of the impact of arrest on dropping out in the pure drop out sample. Further inquiry into this mystery is beyond this scope of this text.

Conditional Models of Dropping Out

The next set of analyses assessed whether the apparently strong effect of being arrested on dropping out apply only to particular types of individuals or whether they vary under varying circumstances. Once again, each interaction is considered separately, in order to preserve degrees of freedom and avoid excessive multicollinearity.

The first interaction model inquires as to whether only one arrest during the ninth grade is sufficient to evidence a negative impact of arrest or whether multiple arrests during that school year are necessary. This interaction was tested both for the basic dropout sample with restricted lags (event is within 18 months of the survey) and for the pure dropout sample with the same lag restrictions. These samples and associated drop out model were selected because they minimize measurement error and provide relatively conservative estimates of the impact of arrests on

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dropping out. Among the 154 youth in the treatment group, which is the same treatment group in both sub-samples, 48 or just under a third are sanctioned multiple times. The remainder are sanctioned only once.

Table 3 present the results of two logit models that parcel out the impact of being arrested only once versus being arrested multiple times. The first logit model reveals that youth in the basic drop out sample who are arrested only once still face over 3.5 times the odds of dropping out relative to youth who are not arrested all net of the set of covariates developed previously (p=.05). By contrast, youth who are arrested multiple times, face ten times the odds of dropping out relative to the control group. Model 2 presents the results for the model determined to be appropriate for the pure drop out sample. The differences between the impact of a single arrest versus multiple arrests are less pronounced than they are for the basic drop out sample, but they are in the same direction. The impact of a single arrest is no longer statistically significant, while youth who are arrested multiple times during ninth grade, according to the model, are 7.5 times more likely to drop out relative to the comparison group. Because the small size of the pure dropout sample can lead to unstable estimates, the remainder of interaction models will be tested

Table 3
The Effects of Single and Multiple Arrests on Dropping Out after Ninth Grade

	Model 1			Model 2		
Variable	B	Sig.	Exp(B)	B	Sig	Exp(B)
Single arrest	1.28	.05	3.61	1.10	.18	3.01
Multiple arrest	2.30	.00	10.00	2.01	.02	7.49
Acting out	.23	.53	1.26	.74	.04	2.09
8th grade retention	1.27	.26	3.56	2.05	.14	7.77
Violent/weapons offense	-.80	.14	.45	-.76	.19	.47
Home academic support	-.90	.02	.41	-.58	.12	.56
Age at event	.85	.04	2.33	-	-	-

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Peer drug use	.35	.03	1.41	-	-	-
Number of missing values	-	-	-	.07	.30	1.07
No parent at home	-	-	-	.78	.11	2.18
Neighborhood variables						
Neighborhood unemployment rate	-.03	.10	.97	-	-	-
Crime rate	-	-	-	.00	.04	1.00
(Constant)	-13.68	.03	.00	-4.20	.01	.01
Sample size	254			218		
-2LL	137.39			124.11		

* p<.10 ** p<.05 *** p<.01

only with the basic dropout sample.[50]

Next, I considered other conditional models suggested by the literature. One hypothesis is that novice offenders are more helped or hurt by arrests than more experienced offenders (Shannon, 1980). This hypothesis was tested by incorporating into the basic drop out models the number of prior station adjustments—the most proximal indicator of prior delinquency—and a variable representing the interaction between being arrested and the number of prior station adjustments. In order to minimize measurement error in the prior station adjustment variable Model 1 of Table 4 excludes youth whose station adjustment data was estimated. All models in Table 4 were also run using imputed data and similar patterns of results (not shown) were obtained.

The coefficient for the interaction term in Model 1, -.1.04, suggests that for each additional prior station adjustment the positive effect of arrest on the odds of dropping out is

[50] An important caveat of this result is that the analysis controls only for differences between the arrested and non-arrested groups rather than differences between multiply arrested and non-arrested youth or differences between multiply and singly arrested youth. Preliminary t-test and chi-square analyses comparing multiply arrest and non-arrest youth suggest only a small number of additional differences between them but the statistical power to detect significant differences is quite minimal.

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reduced by 1.04, which corresponds to a sixty-five percent reduction in the odds of dropping out net of covariates.

In more simple terms, this finding suggests that the strongest impact of arrests on dropping out occurs among those who have no prior station adjustments and a progressively shrinking impact occurs as the number of prior station adjustment increases. This finding is consistent with labeling theory.

To verify the impact of arrests does indeed vary across levels of prior station adjustments—and the statistical interaction is not a spurious consequence of multicollinearity

Table 4
The Interaction of Arrest and Prior Station Adjustments

	Model 1: Base Sample		Model 2: Priors		Model 3: No priors	
Variable	B	Exp(B)	B	Exp(B)	B	Exp(B)
Arrested	2***	7.36	.40	1.49	1.99*	7.28
Arrested * station adjustment	-1.04**	.35				
Station adjustments	.89**	2.44				
Perceived respect	-.24	0.79	-.38	.69	-.35	.71
Acting out	.51(.11)	1.67	.47	1.61	.71	2.03
Violent/weapons offense	-.18	.84	.12	1.13	-.70	.50
Age at event	.56	1.76	.27	1.31	1.36*	3.89
Home academic support	-.34	.71	-.13	.88	-1.47*	.23
8th grade retention	1.32(.11)	3.73	1.28	3.58	1.04	2.84
Delinquent peers	.25	1.28	.12	1.12	.41	1.51
Crime rate	.002	1.00	.003*	1.00	-.003	1.00
(Constant)	-12.02**	0.00	-6.31	.00	-20.49*	.00
Sample size	241		149		127	
-2LL	131.68		122		55.11	

* p<.10 ** p<.05 *** p<.01

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between the interaction and main effect terms—I create two sub-samples that vary in the number of prior station adjustment. As a basic test, I examined the impact of being arrested in one sample that consisted of youth who had at least one prior station adjustment and in a second sample comprised of youth with no prior station adjustments. As the results from Model 2 reveal, arrest has no significant impact on dropping out among those who have prior station adjustments. In raw percentage terms, 18 percent of the arrested youth with prior station adjustments dropped out, while 12.5 of comparison with prior station adjustments did so. By contrast, the third model in Table 4 indicates that among youth who had no prior station adjustments, arrested youth face seven times the odds of dropping out net of covariates. The sub-sample analyses confirm that the number of prior station adjustments conditions the impact of arrest on high school dropping out. The size of the coefficient for arrest, along with its statistical significance, increases among both sub-samples when imputed cases are included.

The next hypothesized interaction I considered is that youth who receive more support from families are more likely to be deterred by sanctions and are less likely, owing to greater academic and emotional support, to suffer academically as a result of being arrested. To test this theory, I computed a variable representing the interaction between arrest and home academic support. The results of this model, which again was estimated using the basic dropout sample with no lag restrictions, are depicted as Model 1 in Table 5. Consistent with the hypothesis, the results of model 1 predicts that the positive association between being arrested and dropping out, decreases by –1.18 log odds for each 1-unit increase in home academic support. This

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corresponds to a 70% reduction in the odds of dropping out. The remaining predictors in the model behave in expected ways, although the odds ratio for the main effect is positively askew.[51] In order to bolster the validity of the statistical interaction between treatment status and home academic support, I estimated the moderating influence of other variables that suggest the amount of social support available to youth after arrest. I was not able to detect interactions of arrest with positive parent-child relations or with the perceived caring or respect from elementary school staff (the latter is not very surprising given that the arrests for this sample occur in high school). I also tested the moderating influence of having no parent in the home under the assumption that youth with no parents in the home have a weaker support network to insulate them from the negative impact of sanctions. The harmful impact of arrest should be more pronounced for them. The results of this test, shown in Model 3 of Table 5, suggest that, rather than increase the harmful impact of arrest on dropping out, having no parent in the home may actually lessen it by 80%. While the odds ratio is sizable, the interaction effect is not quite statistically significant (p=.14). The main effects for arrest and having no parent in the

Table 5
Multiplicative Effects of Arrest and Social Support on Dropping Out

	Model 1		Model 2		Model 3	
Variable	B	Exp(B)	B	Exp(B)	B	Exp(B)
Arrested	4.54**	93.31	1.6**	4.95	-6.5	.00
Perceived respect	-.44	.65	-.36	.70	-.41	.67

[51] The odds ratio corresponding to the arrest main effect is ninety-three--a dramatic increase over the odds ratio for the arrest variable when the interaction term is not in the model, which was only three. The notion that youth who are arrested face 93 times the odds of dropping out net of covariates relative to similar youth who are not arrested does not resonate with common sense. Rather, it is likely the statistical consequences of having too few cases to reliably estimate the odds of dropping out at all arrest status and home academic environment combinations.

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Acting out	.42	1.53	.43	1.54	.4	1.49
Violent/weapons offense	-.44	.65	-.41	.67	-.43	.65
Age at event	.62*	1.85	.56*	1.75	.43	1.54
Home academic support	.49	1.63	-.45	.64	-.36	.70
Perceived respect	1.29*	3.63	1.28*	3.58	1.24*	3.47
Delinquent peers	.31	1.36	.33	1.39	.29	1.34
Crime rate	0	1.00	0	1.00	0	1.00
No parent at home			1.34*	3.84		
Social capital					-3.2**	.04
Interaction variables						
Arrested * home academic support	-1.18*	.31				
Arrested * no parent at home			-1.3(.14)	.27		
Arrested * social capital					2.25(.23)	9.50
(Constant)	-13.53**		-10.81**	.00	2.63	13.86
Sample size	336		336		332	
-2LL	218.38		221.4		213.57	

* p<.10 ** p<.05 *** p<.01

home are reasonable and in expected directions, which lends validity to the interaction. On the other hand, evidence for an interaction disappears when the sample if restricted to those with an event/survey lag of 18 months or less and the model includes the variables that were selected for models involving the basic sample with reduced lags.

Given that evidence for the impact of arrest is conditioned by levels of social support is mixed, a third potential social support moderator was considered. Social capital represents access to a network of potential social support at the neighborhood level. The literature suggests that youth who reside in neighborhoods with a high level of social capital are subject less to exclusionary processes and have better access to sources of support. Model 3 of Table 5 presents the results of a model testing the interaction of arrest and neighborhood social capital.

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Consistent with the unexpected findings for the interaction of arrest and no parent, the adverse effect of arrest on dropping out is more pronounced at higher levels of social capital.

An examination of actual dropout rates across levels of social capital provides preliminary support for this interaction. Among the youth in the base dropout sample whose neighborhood social capital scores are among the top 39 percent (N=153, social capital>3.61), the dropout difference between treatment and control was quite stark (13.6% vs. 1.5%). By contrast, among the youth in the base dropout sample whose neighborhood social capital scores are among the bottom 36 percent (N=133, social capital<3.55), the dropout difference between treatment and control is reduced (22.7% vs. 12.1%). In light of these percentages, the finding of a strong negative yet non-significant main effect of arrest (*B*=-6.5) on dropping out net of the interaction term (with and without the other covariates in the model) should not be seen as a reversal of the direction of the main effect. At higher levels of social capital, the direction of the impact of arrest is not reversed.

In addition to considering the moderating influence of the number of arrests during the school year, the number of prior station adjustments, and various indicators of social support, I tested several other interactions suggested in the literature. I was unable to find any evidence that the impact of arrest varies by gender, ethnicity (black vs. Latino), the type of offense (violent or drug offense), age, and acting out.

Juvenile Justice Interactions

The impact of sanctions may also be conditioned by the nature of the sanction itself. Beginning with Enlightenment thinkers like Beccaria, criminologists have long held that the effectiveness of sanctions in curbing criminal behavior is contingent upon their severity, celerity,

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and fairness. Furthermore, as a result of variation along these dimensions and others, particular justice system interventions—court supervision, probation, community service, home confinement etc.—may vary in their impacts on recidivism. Likewise, sanctions may vary in the amount of opportunities, monitoring, and support they provide with respect to education. Pursuant to institutional exclusion theories, the juvenile justice system intervention that should have the most immediate, direct, and severe impact on education is detention or incarceration.[52] Because the analysis of the impact of detention relative to other sanctions has serious policy implications, extra care is required in ruling out spurious sources of any causal effects of juvenile justice interventions on dropping out.

The next set of models examines whether being arrested but not detained and being arrested and detained exert differential impacts on school dropping out. The first step in this analysis was creating variables that indicate whether or not one was detained during the school year. The detention screening file was the primary source of information on who was detained. The arrest, school, and juvenile court data bases were used to cross-validate this information and discrepancies between data sources were reconciled. The number of detention spells listed in the detention screening file was aggregated across the school year in which the treatment group's first arrest occurred.

Unfortunately, the detention screening file does not record the detention of youth who were arrested and detained prior to February, 1995. For youth whose arrest was recorded in the arrest database, the detention variable described above was computed differently and, sometimes,

[52] I confine my attention to detention rather than incarceration, as the merged data regularly provides information only on detention experiences and each staggered sample lacks sufficient numbers of incarcerated youth.

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not at all. Unfortunately, the arrest data set provides reliable information on who was *not* detained but unreliable information on who was detained. Youth for whom the arrest file is the sole indicator that a youth was detained are counted as missing.[53]

A caveat bears repeating. The models below compare detained and non-detained youth without attempting to identify and control for all the differences between detained and non-detained youth. Fortunately, for my purposes, the selection mechanism that determines whether arrested youth in my sample were detained or released is almost completely measured. Assignment into pre-trial detention is rigidly based on numeric classification criteria--a youths "risk score" at the screening hearing. Risk scores are based largely on items related to the current offense and a delinquent's prior record, that aim to proxy for youths' risk of harm to themselves or others. These risk scores automatically determine whether a youth is released from custody or placed in secure or non-secure detention alternatives. Because risk scores are available for only about half of youth in the basic 9th grade dropout sample only a subset of analyses will employ this rigid selection control.

Table 6, which presents descriptive characteristics of youth who are not arrested, arrested but not detained, and detained, provides a sense of the extent of selection bias that would be present in raw outcome comparisons of detained and non-detained youth. Characteristics in the table are mostly those that are related to sanctions and dropping out. The table also indicates

[53] To elaborate, the arrest file contains a dichotomous variable in which one value indicates that a youth was not detained and another indicates that the youth was detained in a secure facility *or* was placed in a non-secure detention alternative (usually home confinement or electronic monitoring). If a treatment youth was "not detained" according to all the arrest and screening events that occurred during the school year (as well as the school record) the youth was recorded as such. Youth were recorded as "detained" only if identified as such in the detention file, or if the school file indicates their enrollment in a detention center school.

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which variables statistically distinguish each of the treatment groups from the control group. Theses tests of statistical significance likely understate the level of divergence between detained youth and control youth as the small number of detained youth, makes it difficult to detect statistically significant differences.

Nevertheless several notable differences do emerge in Table 6. Both detained and arrested but not detained (referred to henceforth as the non-detained group) are slightly older on average than the comparison group. Not surprisingly, both of the treatment groups also exhibit more station adjustments prior to their first arrests than the comparison groups do before their comparison dates. Given that detention placement decisions are made in large part on the basis of prior delinquency history, it is also comes as no surprise that youth who are placed in detention have more arrests during the school year than non-detained youth. Both groups, especially the detained group, also exhibit considerably higher rates of drug use. While many other attitude, behavior, and prior performance differences from the comparison groups such as in 8th grade retention, home academic support, and peer delinquency are too small to be statistically significant, the differences are in expected directions.[54]

While attitudes toward school may or may not differ across arrest and detention status groups, it is pretty clear that, at least in this analysis, the large difference in the dropout rate (37% in the detention group versus six percent in the comparison group) is not due

[54] At the same time, several unexpected differences and similarities emerged. First, the detained group appears to possess a slightly stronger ability to control their anger. This difference accords with the high proportion of the comparison group that are eventually arrested for violent or weapon's crimes. Similarly, the treatment groups are not less attached to school. If anything, they are more attached. Of course one should use caution in interpreting all of these attitudinal and behavior differences both of the long average lags between event dates and the most recent attitude survey and because these lags are substantially greater for the treatments groups. However, the patterns in anger and attachment across the groups are similar even when the event/survey lags are restricted to fewer than nineteen months.

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Table 6
Descriptive Characteristics by Detention Status among Basic Dropout Sample

Variable	Not arrested Mean or %	Arrested, not Detained (p vs. control) Mean or %	p	Detained (p vs. control) Mean or %	p	Total Mean or %
Dropped out by Fall of 10th grade (%)	5.6	13.8	.02	36.7	.00	12.3
Age at event	14.77	14.98	.00	14.94	.08	14.89
Acting out	1.90	1.98		1.94		1.94
Violent/weapons offense (%)	40.1	32.1		34.2		35.7
Drug offense (%)	40.8	38.3		50.0		40.6
Home academic support	2.97	2.89		2.85		2.92
8th grade retention (%)	2.7	4.9		5.3		4.0
Delinquent peers	1.79	1.84		1.84		1.82
Crime rate	183.74	181.27		189.61		183.23
Arrests during school year	0[a]	1.31	.00[b]	2.97	.00[b]	.94
Prior police contacts	.80	1.15	.01	1.34	.02	1.03
No parent at home (%)	23.8	29.6		23.7		26.5
Latino (%)	15.0	9.9		13.2		12.4
Male (%)	77.6	80.2		84.2		79.5
Prior math	29.45	28.47		34.69	.04	29.57
Prior reading	31.51	29.95		36.44	.11	31.32
Attachment to school	3.15	3.25		3.20		3.20
Perceived respect	3.43	3.46		3.31		3.43
Lack of anger	3.02	2.90		3.24	.22	2.99
Marijuana/alcohol use	1.74	1.97	.13	2.37	.05	1.91
Event/survey lag (months)	13.70	16.12	.04	16.63	.10[c]	15.15
Risk scores[d]	10.73	6.63	.00	15.18	.00	
Sample size	147	162		38		347

[a] This number does not include the arrests that a third of the comparison group experience prior to October of 10th grade.
[b] P-values produced by comparisons of the detained youth and the arrested but not detained youth.
[c] P-values for this t-test comparison assumes equal variances. When equal variances are not assumed p-value is .17.
[d] Risk scrores are the highest risk score among all detention screenings during the school year of a youth's first arrest—9th grade for the treatment group and the subsequent year for the comparison group. Sample sizes for this measure are 100, 68, and 33 for the comparison, non-detained, and detained groups respectively.
* p<.10 ** p<.05 *** p<.01

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to differences in academic achievement. Detained student remarkably score about five points higher than comparison students on both eight grade math scores and reading scores. This difference is maintained even after age and eighth grade retention (both which could increase eighth grade test scores) are controlled (analysis not shown). It also obtains when cases with imputed data are excluded. While this difference merits fascination and attention, higher levels of intellectual prowess do not necessarily translate into higher level academic engagement; brighter students may experience more boredom in some schools.

The next step in testing whether detention exerts a separate influence on dropping out was to develop a multivariate model. Rather than simply adopting the basic model utilized in the other tests of interaction above, I modified that model by replacing some variables with others that appear to be more strongly related to being detained and whose inclusion in the model (during step-wise procedures) has a greater impact on the coefficient for being detained. Following these procedures, the variables acting out, peer delinquency, and neighborhood crime rate, were replaced with soft drug use, previous reading scores, lack of anger and neighborhood attachment. The resultant logistic model is displayed as Model 1 in Table 7.

Table 7 demonstrates that, consistent with the results of the descriptive comparisons, youth who are detained face a much higher risk of dropping out of school even after controlling for covariates like age, drug use, and eighth grade retention. Note that controlling for lack of anger and previous reading scores, which favor detained youth and are negatively associated with dropping out serve to increase the coefficient for being detained. Controlling for prior station adjustments, which is modestly negatively related to dropping out, would have also modestly increased the coefficient for being detained.

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Equally importantly, once the impact of being detained and covariates are controlled, being arrested has a greatly reduced and non-significant impact on the odds of dropping out. Indeed, even if I employ the same set of covariates that yielded strong main effects of being arrested when the impact of detention was not considered, the independent impact of arrest is still greatly reduced (though still approaching statistical significance). These results are presented in model two of Table 7. Not surprisingly, the impact of detention is still quite large in this model. According to the Wald statistic, detention is, by far, the most influential predictors in both models.

Table 7
Logit Model of Dropping out as a Function of Arrest and Detention Status

	Model 1		Model 2	
Variable	B	Exp(B)	B	Exp(B)
Arrested, not detained	.55(.23)	1.74	.74(.10)	2.09
Detained	2.19***	8.93	2.08***	7.99
Age at event	.62*	1.86	.67*	1.96
Violent/weapons offense	-.42	.66	-.33	.72
Home academic support	-.45(.12)	.64	-.39	.68
8th grade retention	1.11(.10)	3.04	1.32**	3.73
Soft drug use	.18(.12)	1.20		
Lack of anger	-.34*	.71		
Neighborhood attachment	.71	2.03		
Prior reading	-.01	.99		
Acting out			.46(.14)	1.58
Delinquent peers			.19	1.20
Neighborhood crime rate			0	1.00
(Constant)	-11.5**	.00	-13.13**	.00
-2LogLikelihood	205.95		199.716	
N	323		323	

* p<.10 ** p<.05 *** p<.01

Given that membership in the detained ninth grade treatment group generally requires either several court cases, one or more arrests for a serious crime, or an arrest on a warrant, while

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the comparison group exhibit no arrests at all during the ninth grade, it is tough to make the case that detention, rather than criminal activity and its attendant ills, is the cause of dropping out. It is possible that the type of youth or the circumstance of youth who are detained are different in undetected ways from youth who are not detained and that these unobserved differences account for their differential drop out risk.

I will address the standard omitted variables retraction incrementally. The first solution preserves statistical power but does not go far enough toward improving the internal validity of the analysis. The second solutions brings omitted variable bias down to levels unprecedented in the juvenile detention effects literature but at the expense of statistical power. Jointly, they make a persuasive case.

Both solutions use inferential logic to argue that the effects of detention are non-spurious. The first point in the argument is that the t-tests and chi-square comparisons established a high degree of convergence between the non-arrested group and the arrested group in the ninth grade staggered sample with respect to alleged selection factors. Secondly, a high degree of convergence also appears to exist between detained youth and arrested but not detained youth. Third, if one can establish that arrest causes dropping out net of selection factors (and relative to the non-arrested) and that being detained causes dropping out net of selection factor (and relative to the non-detained), it should follow that detention causes dropping out net of selection factors (and relative to the non-arrested).

Thus, to make the above argument, one needs to demonstrate that youth who are detained are more likely to dropout relative to a statistically equivalent group of youth who are arrested but not detained. The first solution makes this case using basic multivariate methods, while the

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second solution controls also for the risk score that determines whether a youth spends any time in detention.

So, the next analyses, presented in Table 8, are confined to youth who have been arrested—a treatment group sub-sample. The first logit analysis, model 1, compares detained and not-detained youth from the basic dropout sample in the log odds of dropping out net of covariates. Control variable selection began with the variables from the first full sample model in Table 7, but several variables, age at event, 8th grade retention and home academic support were excluded because they did not distinguish detained from non-detained youth. Conversely, several variables were added to the model according to inclusion criteria discussed earlier. These additional variables are perceived teacher and staff respect, neighborhood crime rate, and prior station adjustments.

As the table illustrates, the model predicts that those who are detained at least once during the school year face an odds of dropping out nearly eight times that of youth who are arrested but not detained. The largeness of this effect is partially attributable to the inclusion of suppressor variables like prior station adjustments, lack of anger, and reading scores.

An alternative explanation of the apparent impact of detention that merits consideration is that the difference between detained and non-detained youth in the odds of dropping out rather than reflecting the impact of detention per se, reflects the fact that youth who are detained are arrested more often, and, by implication, are more criminally active. Model 2 tests this alternative explanation by controlling for the number of arrests during the school year.

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Table 8
Logit Model of the Impact of Detention on Dropping Out Among the Arrested

	Model 1		Model 2		Model 3		Model 4	
Variable	B	Exp (B)	B	Exp (B)	B	Exp (B)	B	Exp (B)
Detained	2.04***	7.65	1.44**	4.24	2.19*	8.89	1.75(.18)	5.74
Violent/weapons offense	-.72	.49	-.69	.50	-3.49***	.03	-3.2**	.04
Marijuana/ alcohol use	.19	1.20	.15	1.16				
Lack of anger	-.69***	.50	-.66**	.52	-.95**	.39		
Neighborhood attachment	2.16**	8.70	2.39***	1.94	2.76*	15.72		
Prior reading	-.02	.98	-.02	.98				
Prior police contacts	-.3(.12)	.74	-.39*	.68	-.74**	.48		
Perceived respect	-.35	.71	-.42	.66				
Neighborhood crime rate	0(.13)	1.00	0(.12)	1.00			0	1.00
Arrests during school year			.5**	1.65				
Acting out					-.88	.42	-.97	.38
Home academic support					-.65	.52	-.87	.42
Age at event							1.32(.11)	3.74
8th grade retention							1.8*	6.05
Delinquent peers							.98**	2.67
Risk score					.03	1.03	.07	1.08
(Constant)	-4.71*	.01	-5.7**	.00	-2.37	.09	-20.74*	0
-2LL	133.79		128.87		56.91		56.03	
Sample Size	180		180		87		87	

* p<.10 ** p<.05 *** p<.01

The inclusion of this variable reduces the coefficient of detention by about 25 percent and its odds ratio by about forty-five percent.[55] However, detention still exerts a strong positive influence on dropping out, independent of the effects of the number of events.

[55] These figures are provided only to provide a general sense of the degree to which the impact of detention is bound up with the number of arrests. They should not be interpreted as an actual measure of mediation or shared variation, as the coefficients across logit models are not as comparable as are coefficients across ordinary least squares regression models.

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The next two models implement the second solution, which rigorously controls for selection. As explained earlier in the chapter, assignment to secure detention is rigidly based (about 90% of the time) on one's score on a risk assessment instruments. Aside from the number arrests during the school year, none of the variables in models 1 and 2 are supposed to have any bearing on individual detention decisions. Youth are detained at least once during the school year because they committed a serious offense (e.g. sexual victimization), because they had a number of prior or pending court petitions, or because a warrant was issued for their arrest (usually after they missed a court appearance or violated the terms of probation or home confinement). At each detention screening hearing the accretion of these circumstances is represented in a separate risk score, whose cut-off point serves as the chief selection mechanism for detention.

Assignment to treatment based on a risk score could lend itself to a quasi-experimental design known as regression discontinuity. If detention has a causal impact on dropping out, sudden shifts in the rate of dropping out should be evident at the cut-off points. Statistically speaking, these shifts are discontinuities in the linear or "linearized" relationship between the risk score and the outcome.

Unfortunately, the data in this analysis and the question at hand are not well-suited to the regression discontinuity design. First, no relationship is evident between the risk score and the outcome after one reaches the cut-off score. Second, youth are sometimes detained without a screening hearing at the order of the judge (often for violations of probation). Finally, youth in the sample vary not only in their measured risk during their first screening event but also in the number of screening events and in the aggregate risk assessed across these events. The

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variability in these dimensions across the sample breaks down the quasi-experimental structure of the data. Still, we know that youth are detained because they exceeded the risk threshold on at least one occasion or that they defied a court order, and that the non-detained youth almost never exceeded this threshold. So, it can be said that the two groups vary in their maximum demonstrated "risk" and in their propensity to defy court orders. One can partially control for this cumulative risk by controlling for a youth's highest risk score across events. In addition, controlling for other attitude and behavior variables, such as anger management and delinquency may help control for remaining differences that are reflected in varying rates of compliance with court orders.

Model three in Table 8 presents the results of this rigorously-controlled model of the impact of detention on dropping out. The small sub-sample is comprised of the only 87 youth—25 of whom were detained at least once—for whom at least one risk score was computed at a detention screening hearing. Variable selection followed similar procedures to those described earlier. Variables that made no contribution to the model ($p>.5$) were excluded. The results for model 3 demonstrate that, even after controlling for the primary and, often, automatic selection mechanism for selection—highest risk score—detention increases still greatly increases one's odds of dropping out. It bears mention, however, when risk score is the only control variable included in the model, the impact of detention, while exhibiting a high odds ratio of 2.35, is not significant. This finding reflects the high degree of collinearity between detention status and the risk score, which inflates the variance of the estimate of the impact of detention by a factor of 2.2. In addition, it suggests that the positive impact of detention on dropping out may be

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suppressed by factors that are positively related to detention but negatively related to dropping out. Only when some of these factor are controlled, does the impact of detention emerge.

The final model in Table 8 tests the impact of detention utilizing the set of control variables that comprised the original base model. In this model, the impact of detention yields an odds ratio of 5.74 but it is no longer statistically significant (p=.18). Again, the non-significant p-value is likely due to the inflation of its variance by multicollinearity.

The Impact of Arrest on Absences

Several empirical questions flow from the finding that arrest increases the odds of dropping out and grade retention. What are the mechanisms by which arrests asserts this effect? Are the effects immediate or do they incubate overtime, perhaps as a youth proceeds through successive stages of juvenile justice involvement. One way to address these questions is through examining measures at the semester level. Perhaps, youth are more likely to drop out because of the forced and voluntary absence from school associated with juvenile justice intervention. Or perhaps, even when students maintain good school attendance (a frequent condition of juvenile probation and parole) they are still "absent" in the sense of disengagement. That is, they are focused on something other than their school work, which may be manifest in declining course grades.

Unfortunately since semester level indicators of school performance are available only for the high school years, prior semester measures of absences and grades are available only for youth whose arrests occur in the spring semester of the ninth grade. Full ninth grade staggered

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sample analyses, consequently, must incorporate other, less ideal, measures of prior school performance.

The first test then is a relatively rigorous one and examines whether being arrested increases the number of average absences from school (averaged across all one's classes) during the semester in which an arrest occurs even after controlling for the number of identically measured prior absences. Youth who drop out, transfer, or are detained/incarcerated for an extended period that semester are generally excluded because they lack measures of subsequent outcomes. Had I estimated absenteeism data for drop outs based on date and reason for dropping out (which often is listed as "excessive absences"), I certainly would have found even more pronounced truancy differences between arrested and non-arrested youth.

Figure 1 in Appendix A visually depicts the rate of absences among four groups of youth. The first group consists of youth who are arrested in the spring and have information on both their fall semester absences and their spring semester absences. The second group is their comparison counterparts.[56] The third group is comprised of youth arrested during the first semester of their first ninth grade and who have post-absences recorded, but who obviously lack a prior ninth grade absence measure. The fourth group is their comparison counterparts. Added to the fall arrest group, are a very small number spring arrestees who lack a pre-measure because of non-enrollment or enrollment in eighth grade during the previous semester (some students are switched from an eighth transition to school to ninth grade in the middle of the school year after scoring high enough on the achievement test to be promoted).

[56] A small portion of their comparison dates actually occur in the fall because youth repeating ninth grade are included in the comparison group and have absences in the spring.

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As Figure 1 illustrates, student who arrested in the spring have on average almost exactly the same average number of absences for each class in the fall semester as the control group—about 22. However, in the spring semester, absences among the arrested group surge to an average of nearly 39 while average absences in the control group increase to only 29--a differential gain of ten absences (p=.002). In a similar fashion, fall arrestees have significantly more absences at the end of that semester (p=.002) than their comparison group.

Notably, youth who are arrested in their first semester in high school also have more absences that fall than youth are will be arrested during the very next semester. In particular, youth are arrested during the fall semester (as opposed to summer) have 29 absences on average in the fall semester compared to youth who are arrested in the spring semester who have 22 absences. Given that these two sets of youth are arrested only one semester apart, it is difficult to reasonably argue that this difference in fall absences is due to anything other than the presence or absence of a fall arrest(s). These differences within the treatment group—between summer, fall, and spring arrestees will be explored systematically later.

On a technical note, the treatment and comparisons differences are actually slightly understated because greater number of arrested students enroll later in the semester and have less time to accrue absences. For instance, 6.5 percent of fall arrestees enroll on October 1st or later—one month into the semester or more—versus three percent of the fall comparison group.

While these findings are quite compelling on their own terms, multivariate analyses may still be necessary to satisfy proponents of interpretations of declines in absences following arrests other than the arrests themselves. Perhaps, for instance, youth who are arrested in ninth grade are more ill-prepared for high school or burn out quicker on the experience. The activities that

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they choose instead of schooling may also draw the attention of police. While available data does not permit a thorough examination of attitudes and circumstances during ninth grade, comprehensive set of selection control are available for earlier years. Convergence between compared groups on a large set of antecedent variables and statistically controlling for any disparities helps rule out a selection bias interpretation of observed effects.

The first analysis of the rigorous sample earns its name, because it examines the impact of arrest on change in absences. A set of possible confounding selection factors for this analysis was generated using methods similar to those used in previous analyses described in this chapter. First, using a data set with imputed values in place of missing data (to increase statistical power) and samples consisting of youth with data on both prior and post absences (the rigorous sample) and those with post absences data (the base sample), t-test comparisons were generated on over 100 potential covariates. Once again, most sets of t-test comparisons yielded fewer characteristics to distinguish arrested and non-arrested youth than one would expect by chance. For instance, among the sample with post-absences data, only five non-redundant variables distinguished arrested and non-arrested youth at the .05 level of below. The two groups did not differ at all on some of the expected covariates of arrest including acting out, delinquent peers, and neighborhood crime rate. Fourteen variables significantly distinguished arrest and comparison groups and were also correlated with post arrest absences ($p<.25$). Table 9 displays the convergence and divergence of arrested and non-arrested groups with respect to several characteristics. The variables displayed in the table are those that qualify as a covariate in at least one model.

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Next, after omitting several highly collinear variables, various OLS linear regression models of absenteeism were tested. Several variables such as the number of prior station adjustments, the number of murders in one's area, and the number of missing values for the imputation model were not at all important in some multivariate models and were therefore excluded from those models. The final set of variables for the initial model involving the rigorous sample is shown in Model 1 of Table 10. Collectively, the control variables do little to erode the expected impact of arrest on absence in the spring semester. Being arrested still predicts an increase in 9.6 absences even net of the control variables. An R-square analysis reveals that being arrested uniquely explains about five to six percent of the variation in that semester's absences; about half of the variation is explain by prior absences.

Table 9
Characteristics of Sample in Absenteeism Analysis

	Rigorous sample			Base sample		
Variables	Control	Arrested	P	Control	Arrested	P
	Individual variables					
Prior absences	16.85	22.86	.02	16.85	22.86	.02
Average prior achievement	31.19	28.39	.28	30.19	30.27	.95
Spring arrest (%)	91	1	.04	27	38	.03
Acting out	1.87	2.02	.2	1.9	2.02	.05
Age at event	15.09	15.25	.05	14.81	14.99	0
School attachment	3.17	3.38	.27	3.18	3.27	.42
Lack of depression	3.87	3.6	.12	3.81	3.81	.97
Peer drug use	1.67	1.85	.43	1.9	1.81	.51
Number of missing variables	4.98	6.13	.3	4.37	6.52	0
Property offense	0.17	.26	.27	.17	0.28	.01
Life satisfaction	4.07	3.98	.44	3.97	4.05	.18
Educational expectations	3.22	2.98	.14	3.11	2.98	.2
Marijuana and alcohol use	1.6	1.9	.15	1.78	2.02	.09

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Police station adjustments	1.02	1.51	.03	.79	1.29	0
Violent or weapons offense	.37	.28	.31	.41	.3	.02
Year of event[a]	1995.89	1996.1	.43	1995.29	1995.61	.04
			Neighborhood variables			
Child control	3	3.06	.32	3.03	3.07	.3
Neighborhood attachment	2.74	2.75	.8	2.72	2.77	.06
Neighborhood population	8900.11	8616.95	.51	8701.02	8600	.7
Sample size range	42:46	81:82		147:155	214:221	

[a] Year of event is measured as the year in which the ninth grade sanction or comparison occurred.

As noted, the control group in this base sample includes some youth who had prior absences, but who were sanctioned in the fall. Since, as revealed in figure 1, absences tend to increase in the spring semester even without an arrest, the presence of fall arrestees in the comparison group may exaggerate the impact of arrest on absence. Model 2 displays the results of the same model when the fall arrestees are removed. The removal of these cases reduced the predicted impact of arrest on absences to 7.1.

Table 10
Regression Models of the Impact of Arrest on Absenteeism

	Model 1		Model 2		Model 3	
Variable	B	Beta	B	Beta	B	Beta
Arrested	9.55	.23***	7.08	.16***	7.78	.18***
Prior absences	.85	.69***	.93	.73***	.88	.71***
Neighborhood population	.00	.08	.00	.09	.00	.09
Child control	-3.47	-.05	-3.59	-.05	-5.32	-.08
Marijuana and alcohol use	-.68	-.04	-1.20	-.07		
Life satisfaction	-2.24	-.06	-1.57	-.04	-3.11	-.08
Lack of depression	-1.60	-.07	-1.91	-.09		
School attachment	-	-	-	-	2.41	.12*
Police station adjustments	-	-	-	-	1.00	.07
Constant	30.18	*	30.06	*	21.71	
Sample size	149		129		102	
R^2	.59		.64		.64	

* p<.10 ** p<.05 *** p<.01

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The next source of potential bias that must be addressed is measurement error. The number of missing values and the survey/event lags failed to exert a significant impact on either absences or the coefficient for arrest. Still, the reliance upon distally measured selection factors threatens to exaggerate the degree of convergence present at the time of ninth grade sanction. Model 3 attempts to minimize this bias by setting a event/survey lag limit of twenty months. Twenty months was the lowest possible lag limit that could still yield a final sample of over 100. About 68% of the base sample have an attitude survey within twenty months prior to their event or comparison date.

Because differences between ninth grade arrestees and non-arrestees are more likely manifest in 7^{th} and 8^{th} grade than they are in 5^{th} and 6^{th} grade, a larger pool of potential covariates must be considered for inclusion. The final lag restricted model includes two additional predictors of both arrest and absences, attachment to school and the number of prior police contacts. At the same time, two others variables from the first two models—soft drug use and lack of depression—are no longer relevant and were dropped. Even with more reliable control measures, arrests are still associated with eight additional absences on average in each class. However, when fall youth are omitted and the total sample shrinks to only 91 youth the impact of arrest shrinks to a gain of five absences ($p=.072$). The effect in turn increase to 5.55 ($p=.03$) when the upward lag limit is increased to 24 months. Declining statistical power may account for some of the large drop in the impact of arrest when fall arrestees are omitted.

Now that I have established that youth who are arrested in the spring of ninth grade have the same number of absences in the fall as youth who are not arrested until the next spring, it seems reasonable to expect that this antecedent similarity in truancy extends to the entire ninth

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grade sample. Broadening the sample to all ninth graders substantially augments statistical power and permits the test of statistical interactions.

While I cannot be certain that youth arrested in the fall also bear a strong resemblance to their comparison group with respect to prior absences some reassuring evidence suggests comparability on a proxy for prior absences. A strong correlate of post sanction absences is pre-sanction test scores.[57] For instance, the correlation between prior math scores and post-sanction absences is a strong -.237 ($p<.00001$).[58] Thus, lower math scores among youth arrested in ninth grade relative to youth arrested a year later, could give cause for concern that differences in post event absenteeism reflect prior differences. In fact arrested youth score on average 1.6 points higher on the math "pre-test" than controls (29.9 versus 28.3).[59]

To create a uniformly meaningful proximal measure of prior achievement, I created a prior measure that consists of only of the most recent eighth grade test score. The measure with the strongest correlation with post sanction absences is the average of math and reading test scores. The measure shares a very strong correlation with post sanction absences of -.219 and will serve as a control for prior achievement analyses of the broader sample. Consistent with the prior absences measure, this prior achievement measure fails to distinguish arrested and non-arrested youth. The two groups have virtually identical scores of 30.2 and 30 respectively.

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[57] These scores come from eighth grade except when the ninth grade sanction or comparison date occurs after a youth takes his ninth grade achievement tests..

[58] Correlation is computed with data that include imputed values. The rest of the comparisons that follow refer to samples that omit missing values.

[59] This difference is likely to be underestimated given that arrested youths' math scores are depressed by a greater share of students whose prior math score are measured in the spring of ninth grade. High school tests are functionally and structurally different than elementary school tests and yield lower scores among my sample and the overall Comer sample.

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Table 11 depicts the linear regression of average absences on arrest, prior achievement, and other controls among this expanded sample. This model includes more variables than the previous models because more statistically significant predictors of arrest and absences naturally emerge in larger samples. The results for Model 1 demonstrate that arrest still has a large robust effect on absences net of a wide array of covariates including acting out, prior station adjustments, and first offense type (violent and property offenses; drug offenders comprise the bulk of the reference category). A spring semester dummy variable was included because arrested youth are more likely to have spring events and absences, as mentioned earlier, are more abundant in the spring semester.

Table 11
The Regression of Absenteeism on Arrest Using the Base Sample

	Model 1		Model 2		Model 3		Model 4	
Variables	B	Beta	B	Beta	B	Beta	B	Beta
Average prior achievement	-.24	-.17***	-.22	-.14**	-.34	-.22***	-.27	-.17***
Arrested	8.54	.22***	7.18	.18***	9.91	.25***	6.83	.18***
Prior police station adjustments	2.74	.19***	3.65	.24***			4.37	.29***
Acting out	3.49	.12**	3.88	.13*			3.26	.11*
Spring arrest	9.61	.24***	9.47	.23***				
Number of missing values	.20	.07	.35	.09				
Property offense	-7.56	-.17***	-7.70	-.17**				
Violent or weapons offense	-4.26	-.11*	-4.00	-.10				
Age at event	-1.89	-.05	-3.53	-.09				
Life satisfaction	-2.46	-.07						
Year of event	-2.62	-.20***	-2.15	-.15**				
Educational expectations	-1.16	-.06						
Neighborhood attachment	-4.57	-.06	-5.28	-.06				

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Peer drug use			.84	.06				
Constant	5301.99	***	4365.46	**	31.48	***	20.50	***
Sample size	358		238		243		243	
R^2	0.29		.30		.12		.21	

* p<.10 ** p<.05 *** p<.01

The second model in Table 11 utilizes a sample that was surveyed within fifteen months of their event—a restriction that still yielded a respectable sample of 238 youth. A search for new covariates in the lag restricted sample resulted in only one additional covariate for the model, peer drug use and in the omission of two covariates, life satisfaction and educational expectations. The more reliable statistical control of selection factors reduces the predicted effect of arrest by nearly 1.5 to seven absences. Still, arrest is second only to prior station adjustments and spring semester arrest in its influence on absences.

Thus, once again the results demonstrate that the inclusion of many attitudinal and behavior covariates of arrest and truancy, fails to serious erode the impact sanctions. Nevertheless, none of the analyses in this chapter are completely invulnerable to the criticism that proximal factors that contribute to both arrest and school absences are not adequately measured and included.

While better survey measurements cannot be obtained, one can employ other methods to reduce the selection differences between arrested and non-arrested youth. The demarcation of high school years by semester allows comparisons on a semester's performance between youth who are arrested in that semester and youth who are not arrested until the next semester. In effect, one can conduct a mini-staggered replication *among* those who are arrested in ninth grade. If we accept the premise that youth are arrested in the fall have similar levels of school performance and delinquency as youth who are arrested in the summer prior or in the following

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spring semester—an assumption that can be partially verified through t-test comparisons on prior survey indicators—then we should expect all three groups to exhibit similar numbers of absences in the fall.

On the other hand, if arrests impede school attendance we should expect the following results: 1) youth who are arrested in the summer should have fewer absences in the fall semester than youth arrested in the fall because the negative effects of summer arrests, especially arrests that do not result in court cases, will dissipate somewhat by the time the fall semester begins. 2) youth who are arrested in the spring will have fewer absences in the fall than youth who are arrested in the fall and, most likely, fewer absences in the fall than youth who are arrested in the summer.

The first step in conducting this mini-staggered replication, once again, was to decide on a model of average absences in the fall of ninth grade. Variables that, net of arrests, explained some of the variation in absenteeism, were considered for inclusion. Inclusion decisions were based on how well variables predicted absences and influenced the coefficient for variables representing the semester in which an arrest took place. In addition to these covariates, two new predictors were created and deemed appropriate for the model—age at the beginning of the ninth grade school year and a count variable indicating which school year from 1993-994 through 1999-2000 school year in which the arrest took place. The school year values for this analysis ranged from two through seven.

The final model that was estimated is depicted in Table 12. It is important to note that no variables in the model actually significantly distinguished summer arrestees from fall arrestees or spring arrestees from fall arrestees. This level of homology is consistent with my expectations.

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However, some non-significant differences are evident and do support the inclusion of selection controls in the model.[60] These variables, such as prior achievement and offense type, are also included because their inclusion has an appreciable beta reduction or enhancement effect on the coefficient for at least of one of the seasonal dummies.

The results of the linear regression model depicted in model 1 of Table 12 suggests that, consistent with my expectations, being arrested in the fall causes an increase in fall absences relative to youth arrested in the spring. The estimated effect of arrests, 7.5 absences, is similar to the effect of arrests in the larger staggered replication analysis. Furthermore, the coefficient of –

Table 12
The Impact of the Timing of Arrest on Fall Absences among Youth Arrested in 9th Grade

	Model 1			Model 2		
Variables	B	Beta	p	B	Beta	P
Average prior achievement	-.33	-.25	.00	-.30	-.22	.00
Summer arrest	-4.49	-.10	.13	-6.00	-.14	.07
Spring arrest	-7.52	-.21	.00	-9.20	-.26	.00
Prior police station adjustments	1.64	.13	.04	2.64	.21	.00
Acting out	4.44	.17	.01	4.34	.18	.02
Property offense	-4.56	-.12	.09	-3.95	-.10	.19
Violent/weapons offense	-2.72	-.07	.29	-3.67	-.10	.20
Educational expectations	-1.51	-.08	.20	-3.03	-.15	.03
Age Fall of 9th grade	-3.14	-.09	.17	-4.29	-.12	.10
School year	-1.51	-.13	.05	-1.40	-.10	.15
(Constant)	87.64		.01	107.44		.01
Sample size	221			165		
R^2	.23			.3		

[60] For instance, fall arrestees scored higher on the average of the two eighth grade achievement tests (31.3) than summer arrestees (28.5). Furthermore, 34% of summer arrestees were arrested for property crimes compared to 24% and 26% among spring and fall offenders respectively. Spring arrestees are disproportionately arrested for drug offenses (47% versus 34% and 39% summer and fall offenders respectively.

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* p<.10 ** p<.05 *** p<.01

4.5 for the effect of a summer arrest on fall absences affirms the expectation that a summer arrest is not as harmful with respect to fall absences as a fall arrest. However, this effect does not quite attain statistical significance (p=.13).[61]

Model 2 more reliably estimates selection processes by restricting the sample to students surveyed within twenty months of their arrest. While reducing the sample by only twenty-five percent, this restriction results in staggered comparisons that are more decisively consistent with my expectations. Being arrested in the fall is now associated with nine more absences in the fall than youth who are arrested only one semester later, while the "summer arrest dissipation effect" is now six absences.[62]

Conditional Models of Absences

The "mini-staggered replication" described above demonstrates that even under conditions of very strict selection controls, being arrested is associated with a large increase in absences. This suggests that the arrests effects of similar magnitude reported in Tables 10 and 11 are also causal in nature. Thus, it is appropriate to ask which factors condition the arrest effects reported among larger samples. The analyses of moderating influences will focus first on the mini-staggered sample and, second, on the base sample comprised of youth arrested in summer, fall, or spring as well as the comparison groups arrested a year later.

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[61] A separate regression model that placed summer arrestees into the reference category determined that, while spring arrests are, as predicted, associated with three fewer absences in the fall than summer arrests, this effect is non-significant.

[62] This pattern of results suggests either that restricting the lag introduces a new set of unmeasured confounding selection factors—an unlikely scenario—or that the pattern of selection differences—more reliably measured in this model—dispose fall arrestees toward fewer absences or summer arrestees toward more absences. Under the latter and more plausible scenario, controlling for these selection factors removes a suppressor effect.

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In keeping with previous analyses, I first examine whether the number of arrests make a difference. The 259 respondents in the mini-staggered replication are divided into four groups: 40 youth who are arrested only in the summer semester (four of whom are arrested multiple times during the summer), 95 youth who are arrested once in the fall, 31 youth who are arrested multiple times in the fall, and 92 youth who are arrested only in the spring. Model 1 of Table 13 illustrates differences between these four groups net of selection controls. Youth arrested in the spring comprise the reference category. The three arrested groups appear to vary in the number of fall absences relative to those who are not arrested until the spring. Being arrested multiple times is associated with 11.5 additional absences while a single arrest is associated with only 5.5 additional absences. Being arrested only during the summer break is non-significantly associated with an increase in only three absences in the fall. While these differences are quite striking, a separate model (not shown) with the single fall arrestees as the reference group found that the differences between neither the multiple fall arrestees (B=6.0; p=.139) nor the summer arrestees and the single fall arrestees groups are statistically significant.

Next, I examine whether, beyond the number of events, the type of sanction makes any difference. This analysis utilizes the expanded sample in model 1 of Table 11. Arrests were aggregated across the semester of the first arrest for fall and spring arrestees in the treatment group, while these arrests were aggregated for both summer and fall semesters among the summer arrestees. This aggregated arrest information was used to derive three separate

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Table 13

Linear Regression Model of the Impact of the Number and Type of Sanctions on Absences

	Model 1: Fall Absences			Model 2: Post Absences		
Variables	B	Beta	p	B	Beta	p
Summer arrest(s)	3.10	.06	.37			
One fall arrest	5.52	.15	.03			
Multiple fall arrests	11.52	.19	.00			
Not arrested				-8.23	-.22	.00
Detention alternatives				-5.14	-.08	.14
Detained				-2.45	-.03	.60
Control variables						
8th grade achievement	-.32	-.24	.00	-.23	-.16	.00
Station adjustments	1.35	.11	.09	2.45	.17	.00
Acting out	4.55	.18	.01	4.46	.15	.01
Property offenses	-4.24	-.11	.12	-8.43	-.19	.00
Violent/weapons offense	-2.25	-.06	.38	-4.11	-.11	.06
Educational expectations	-1.51	-.08	.20			
Age in Fall	-3.37	-.10	.14			
School year	-1.40	-.12	.07			
Spring event				5.21	.13	.01
(Constant)	82.71		.02	28.31		.00
Sample size	221			302		
R^2	.23			.22		

* p<.10 ** p<.05 *** p<.01

"treatment groups"—youth who were detained at least once that semester (detained group), youth who were assigned to non-secure detention alternatives at least once (alternatives group), and youth who were arrested at least once but never assigned to secure or non-secure detention (released group). Each group is compared to youth in the non-arrested control group (the "10th grade arrestees) with respect to post sanction semester absences. Information on the type of sanction was provided mainly by the screening file and the school record (which indicates detention school enrollment). For cases identified only in the arrest file, I was unable to

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distinguish between youth assigned to secure and non-secure alternatives. However, a substantial minority of the released group was identified by the arrest file only.

Unfortunately, the statistical power to detect differences across the three treatments is depressed by the limited number receiving detention and detention alternatives. Among the 156 youth in one of the three treatment groups that also provided both an eighth grade achievement test score and a post sanction absences measure, only 29 were assigned to detention alternatives and only 15 were detained. This not only reduces the reliability of effect estimation (due to random errors) but it also makes it more difficult to disentangle the impact of the treatments from confounding selection factors. Nevertheless, Figure 2 (See Appendix A) gives a strong impression of differences across the three treatments in post-sanction absences relative to prior test scores. For instance, despite nearly identical eighth grade achievement average scores, the released groups exhibit six more absences on average than the alternatives groups and nearly five more than the detained group.

These differences, if not due to random error, could also be due to the influence of confounding factors. For instance, perhaps the released group are more likely to be arrested in the spring when absences trend upward. For these reasons, multivariates comparisons are warranted. Variables considered in initial models were selected from the final model of the expanded sample. Other variables that were important in other models of absenteeism were also considered. Most were ruled out as irrelevant.

One should bear the earlier caveats is mind when examining the results of Model 2 in Table 13—a linear regression model of the impact sanction type on subsequent attendance. To best illustrate the differences between types of treatments the reference category is comprised of

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the released group rather than the non-sanctioned group. The results of this model are somewhat surprising. First, the large negative effect of the coefficient for not-sanctioned suggests that even being arrested and being released with no recorded time in detention is associated with a large increase in absences. Even more surprisingly, being assigned to non-secure detention alternatives is still associated with five fewer absences than being released, even after controlling for acting out and the number of prior station adjustments. Youth sent to detention alternatives have slightly higher levels of acting out and numbers of prior station adjustments. This effect approaches statistical significance. However, it is important to note, that among youth with a prior absences measure (the spring arrestees) the 52 released youth have higher number of prior absences than the 10 alternatives youth and the 4 detained youth. Obviously, this difference can be still be due to chance.

The effect of detention is also in a negative direction. Models including only the three treatment groups confirm that the elevated levels of absences among the released group are not due to their having a larger number of arrests during the semester. In addition, differences between the groups in the timing of sanctions are small, and the timing of the sanctions during the semester (as measured by the order of month of the semester in which the arrest took place) surprisingly has no effect on the number of absences during that semester.

These findings with respect to differences among the sanctions beg further exploration. One question is whether these differences are short-term or whether they have longer term implications for both truancy and other outcomes. Statistical examinations of these questions are somewhat precarious given the limited statistical power resulting in the further refinements in the sample that would be required. However, graphical analyses of limited numbers of cases can

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also be illuminating if we begin with the premise that any patterns among cases have a sociologically rich explanation and likely do not merely reflect the presence of chance, idiosyncratic individual differences and circumstances.

Through this magnifying lens I approach the question about the longer term impact of different types of juvenile justice interventions. Figure 2 also displays the average number of absences recorded at the end of the semester following the semester in which the arrest (or comparison date) occurred. As the sample sizes displayed at each time point illustrate, the treatment groups suffer from differential attrition. Given that attrition—which results mostly from dropping out—is strongly, positively and often automatically related to absences, higher rates of attrition places downward bias on the number of recorded absences. Thus, appearances to the contrary, the figure does not lend support for assertions about the broader sample such as that being released causes absences to continue to climb through the following semester while secure and non-secure detention causes arrests to stabilize or even decline.

Still, the patterns of change among those that do manage to remain in school through the next semester and have absences recorded are interesting in themselves. For reasons that will be explored later in discussions of interviews I conducted with actual juvenile arrestees, those who do not experience detention are often more truant the next semester. On the other hand, the minorities of students in the other groups who manage to return to school while they are on home monitoring or after they are released from detention—sometimes released to a non-secure detention alternative—and who manage to avoid lengthy reincarceration, dropping out, or exclusion from school experience a leveling off of absences that semester.

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Before turning to the question of whether the truancy differences between youth receiving different types or doses of sanctioning translates into differences in course grades, I will explore other possible moderators of the impact of arrest on absenteeism. Utilizing the expanded sample, I tested the full range of promising moderator variables from earlier analyses. For the first time in this thesis, gender emerges as a moderator. Model 1 on Table 14 displays the interaction of gender and arrest net of covariates. This model suggests that the positive impact of arrest on absences is 7 days larger for females than males. Separate subanalyses (not shown) involving male-only (n=284) and female-only (n=73) samples find that impact of arrest for females is 12.5 additional absences ($p<.01$) while the impact for males is 7.3 absences ($p<.001$).

Other interactions were at least partially consonant with previous analyses. Warmer parent-child relations appear to soften arrests' effect on absences (See Model 2). Each unit increase in this measure is associated a 4.6 decline in the impact of arrests on absences. A similar though non-significant interaction was evident between arrest and perceived respect from teachers. However, home academic support and perceived teacher caring failed to decisively condition the impact of arrest.

In addition, one's level of support in the neighborhood may condition the impact of arrest on absences in the opposite way. As is shown in model 3 of Table 14, higher levels of neighborhood attachment are associated with stronger positive impacts of arrests on absences. Because high variance inflation factors, owing to collinearity, renders the estimate of the interaction effect highly imprecise, the sub-sample method was also employed to assess the

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Table 14
Conditional Models of the Impact of Arrest on Absences

	Model 1			Model 2			Model 3			Model 4		
Variables	B	Beta	p	B	Beta	p	B	Beta	p	B	Beta	p
8th Grade Achievement	-.23	-.16	.00	-.24	-.17	.00	-.25	-.17	.00	-.26	-.17	.00
Arrested	14.17	.37	.00	26.54	.69	.00	-50.42	-1.30	.02	11.96	.31	.00
Station Adjustments	3.02	.21	.00	2.86	.19	.00	2.82	.19	.00	6.22	.42	.00
Acting Out	3.70	.12	.01	2.98	.10	.05	3.50	.12	.02	2.88	.10	.05
Spring Event	6.82	.17	.00	7.29	.18	.00	7.70	.19	.00	7.09	.18	.00
Missing Variables	.24	.08	.08	.21	.07	.13	.19	.07	.16	.19	.06	.18
Property Offenses	-8.35	-.19	.00	-8.14	-.18	.00	-7.68	-.17	.00			
Violent/Weapons Offense	-5.39	-.13	.01	-4.45	-.11	.03	-4.23	-.11	.04	-1.87	-.05	.33
Age at Event	-1.86	-.05	.30	-1.79	-.05	.32	-1.82	-.05	.31	-2.10	-.06	.25
Life Satisfaction	-1.96	-.06	.23	-2.05	-.06	.22	-1.97	-.06	.23	-2.94	-.09	.08
School Year	-2.66	-.20	.00	-2.63	-.19	.00	-2.95	-.22	.00	-2.75	-.20	.00
Educational Expectations	-1.21	-.06	.21	-.56	-.03	.57	-1.38	-.07	.15	-1.05	-.05	.28
Neighborhood Attachments	-4.48	-.06	.23	-5.65	-.07	.13	-18.52	-.24	.00	-6.88	-.09	.07
Male	-2.78	-.06	.40									
Sanction * male	-7.31	-.19	.09									
Parent Child Relations				.56	.02	.73						
Sanction*Parent-Child Relations				-4.58	-.49	.04						
Sanction * neighborhood attachments							21.55	1.56	.00			
Sanction * station adjustments										-4.11	-.28	.02
(Constant)	84.16		2.87	81.80		.01	121.77		.00	92.76		.00
Sample size	358			358			357			357		
R^2	.32			.31			.31			.31		

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degree of moderation. These sub-sample analyses revealed that at low levels of neighborhood attachment (the bottom 46% of the distribution; N=156) being arrested barely has an effect net of the covariates. On the other hand, among the top 48% of the distribution of neighborhood attachment (N=171), the impact of arrest on absences is a robust 10.5 absences. A similar, though not as pronounced, pattern emerged with respect to neighborhood social capital.

The previous interactions were all sustained under conditions of a restricted lag as well. The final moderator, prior station adjustments, actually becomes more visible when selection processes are more reliably controlled. In the base expanded sample, the reduction of the effect of arrest associated with an additional prior station adjustment is 4.1 (p=.14). When this lag is reduced to 16 months or less and the lag restricted model is employed the effect is about the same, 4.26 but it attain statistical significance (p=.05). Comparing coefficients across logistic regression models is a tenuous procedure at best, however.

The Impact of Arrests on Grades: A Brief Look

Are the observed differences in absenteeism important with respect to other outcomes? On a general level, we know that absenteeism is strongly and automatically related to school dropping out. But how important are the differences in absenteeism observed among specific groups of sanctioned juveniles? For instance, does the fact that youth participating in detention alternatives attend school more often reflect greater engagement and improved performance in school or does it simply reflect adherence to the school attendance requirements of formal and informal agents of authority?

To address this question we can examine the impact of arrest on school grades. Because the purpose of this inquiry is to lend validation and importance to the key absenteeism results, I

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will present only select components of this analysis. The sub-amples in these analyses are almost identical to those in the absenteeism analysis because the same youth that provide prior and post arrest absenteeism data also provide grade data. The covariates in the models are also quite similar because grades and absences, not surprisingly, share some important predictors such as acting out and educational expectations.

Figure 3 displays the overall patterns of grade changes among youth who are arrested or whose comparison dates occur at various times during one's first year of 9th grade. Because no prior grades are available for youth arrested in the summer and fall, average normalized math and reading score in 8th grade are also presented. The four groups perform quite similarly on eighth grade achievement tests.

The overall pattern of results is consistent with those of absenteeism. Achievement clearly declines following arrest. The only major difference is that whereas absenteeism appears to increase most strongly after spring arrest, grades appear to be more affected by summer or fall arrests. Both the spring arrestees and their comparison group decline substantially in grades from fall to spring. Fall arrestees, by contrast, appear to diverge sharply from their controls in fall grades. However, information on prior grades of the summer and fall arrestees is not available. They may be as pronounced as they are for the spring arrestees. The correlation between 8th grade math scores and pre-sanction grades is .219—small enough that it is possible that similarity in eighth grade test scores does not preclude dissimilarity in grades at the same of sanction.

Table 15 displays summary results for models that mirror three key models of the impact of different types of arrest on absenteeism. For simplicity of presentation, the control variables

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are not presented, with two exceptions: prior achievement and the control variable with the largest coefficient. The remaining controls are virtually identical to the controls employed in the same model of absenteeism. The results for Model 1 demonstrate that among the small sample of spring arrestees with both pre-sanction and post-sanction grade point average, arrest has only a modest and non-significant negative effect on grades, net of prior grades. This is true despite the fact that the several of the control variables evidence no main effect on the outcome. Model 2 utilizes the expanded sample of Table 11—which includes fall arrests—and limits the event/survey lag to twenty months in order to reduce error in the measurement of selection factors. This model demonstrates that arrest significantly predicts declines in grades. When the lag restriction is removed, increasing the sample size from 214 to 319, the effect of arrest stays the same— -0.184 points on the grade point average scale—but the effect attains a greater level of statistical significance (p=.03).

Model 3 tests whether the results of the mini-staggered replication apply to grades as well. I selected the sub-sample with a maximum 20 months between the event and most recent attitude survey. The result show that youth arrested in the fall achieve GPA's that are .27 lower in the fall semester relative to youth not first arrested until the spring. Youth first arrested in the summer do not perform significantly better than those arrested in the fall.[63]

The fourth model assesses whether the observed declines in absences (Figure 2) of the two detention treatment groups relative to the released group following sanctioning translates into relative improvement in grades. Consistent with their predicted reduction of five absences,

[63] This model controls for eighth grade reading instead of math, because reading scores—based on the change in the reduction in the coefficient for arrest upon inclusion in the model—appear to be a more important covariate of arrest among this sub-sample.

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Table 15
Summary Results of Select Models of Grades in the Semester following Arrest

	B	Beta	p
Model 1			
Arrested	-.20	-.09	.20
Prior GPA	.78	.52	.00
Lack of depression	.16	.14	.06
N	149		
R^2	.34		
Model 2			
Prior math	.01	.16	.01
Arrested	-.18	-.11	.08
Prior station adj	-.14	-.20	.00
N	214		
R^2	.21		
Model 3			
Prior math	.02	.31	.00
Summer arrest	.07	.04	.59
Spring arrest	.21	.13	.05
Prior station adj	-.07	-.13	.04
N	222		
R^2	.18		
Model 4			
Prior reading	.01	.09	.09
Not arrested	.21	.11	.08
Detention alt.	.17	.05	.38
Detained	-.51	-.12	.04
Prior station adj.	-.16	-.22	.00
N	299		
R^2	.14		

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youth sent to a non-secure detention alternative experience a modest boost in grade point average of .17. This effect in not statistically significant, however, likely owing to the small share who received this treatment (p=.38). Detained students, on the other hand, exhibited a half a point decline in grade point average relative to those students who were released, despite the fact that their predicted absences declined by 2.4 absences relative to the released group. This provides preliminary support to the idea that forced attendance does not mean forced achievement.

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CHAPTER 7: EXPLANATION AND DEFENSE OF RESULTS

The statistical analyses in the previous two chapters sought to answer two central questions. First, does involvement with the juvenile justice system hinder participation in educational institutions? Second, does the alleged effect of juvenile justice involvement apply only to some individuals or under particular circumstances? Relatedly, does juvenile justice involvement actually fulfill its purported mission of helping some delinquent youth successfully function as students?

The analysis addresses the first question by comparisons of the school performance of two categories of youth in each of three grades: arrested youth (the treatment group) and non-arrested youth (the control or comparison group). The arrest and non-arrest groups in each grade are similar on age, demographic characteristics and delinquency, but only one group experiences an arrest during the time period for which school performance is measured. In addition to conducting separate comparisons across multiple school grades, which I call staggered replications, I enhance the validity of the results by utilizing an array of school performance outcome measures including achievement test scores, grade retention, dropping out, absenteeism, and grades. The first two measures gauge achievement in elementary school, while the remainder capture early high school achievement.

Two sentences succinctly summarize the main effects of arrest in the multiple staggered comparisons. During elementary school, first arrest and ensuing juvenile justice involvement has no main effect on math and reading achievement test scores, but it does increase the odds of

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repeating eighth grade. During ninth grade, being arrested dramatically increases the risk of dropping out, and substantially lowers attendance and grades for those who remain in school long enough to receive a report card.

The first unexpected finding that begs an explanation is that arrests do not significantly impact either seventh or eighth grade achievement test scores. I cannot cogently attribute this null finding to selection bias. After all, if anything, unobserved heterogeneity biases the results toward a rejection of the null hypothesis.

The first reasonable explanation is that achievement test scores capture mainly the enduring component of academic achievement. Educational researchers often note that achievement has both an intellectual aptitude component which follows a stable and predictable trajectory from year to year (subject to variation of course depending on learning experiences) and a motivational and behavioral component which can be quite variable from year to year. The achievement test scores utilized in this study are intended to measure academic skills rather than behavior or motivation. Given that detention is quite uncommon in the elementary school samples, most youth are not removed from school as a result of their juvenile justice involvement. The experience of an arrest may improve or worsen one's motivation or attendance in school, but it may take an event of much great disruptive force to actually alter one's intellectual performance trajectory from one year to the next.

The behavioral component of achievement, which more often fluctuates in response to changing life circumstances is better captured by grades than it is by achievement scores. Unfortunately, grades were not available during the elementary school years. However, the

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behavioral component of achievement is reflected in the test scores in two ways. First, it is not uncommon to see performances plummet from one year to the next or, less commonly, surge. Dramatic declines in test scores likely reflect weakening levels of interest, motivation and effort far more than changes in actual math and reading abilities. But disengaged youth, even if disproportionately present among the treatment groups, are not numerous enough to affect the overall results.

Second and more importantly, some students indicate their disengagement from school by not taking the test at all. The failure to take the achievement test likely reflects serious disengagement from school, rather than something like chance illness, given that make up tests are offered and promotion to ninth grade depends on test scores beginning in 1997. These students comprise a large and highly disproportionate share of the 7th grade and 8th grade arrested sub-samples. For instance 28% of the 8th grade arrested students, who otherwise would have qualified for the analysis of 8th grade math scores, were missing such scores as compared to only 11% of the comparison group. These differences are quite striking and suggest that school performance differences between arrested and non-arrested students in seventh and eighth grade merit further exploration. Given the presumed level of disengagement among those who skipped the test, it is quite likely that, in the absence of differential attrition bias, arrests would indeed have a sizable negative effect on test scores.

Grade retention is, in many ways, a better elementary school achievement indicator for this analysis. In contrast to achievement test scores, retention has strong academic and behavioral components. While retention did become, beginning in 1997, rigidly linked to eighth

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grade academic skills tests, it has always had a discretionary component. Many retention decisions are made by school officials, who evaluate and respond to students' performance and behavior. Furthermore, the retention measure suffers from much less attrition bias than test scores. Given retention's stronger connection to student behavior and institutional reactions as well as the relative absence of attrition bias, it is not surprising that logistic regression analyses of the base retention sample suggest that arrested students, net of selection controls, face a greater risk of repeating eighth grade. Unfortunately, arrested youth are more likely than controls to be subject in eighth grade to the automatic retention policy. This made it difficult to disentangle the impact of arrest from this confounding variable in smaller lag restricted samples. Likewise, smaller samples made it more difficult to detect arrest effects on 7th grade retention, which is much less common than 8th grade retention.

The second possible reason that strong arrest effects on test scores were not detected in elementary school is that a large share of the students, at the time of their arrests, attended elementary schools that were in the process of implementing the Comer School Development Program. This program espouses a multi-modal approach to human development and mental health, which may run counter to an exclusionary or neglectful response to adjudicated youth. I did not present systematic tests of this hypothesis in this text, because limiting the analyses to only youth attending Comer or comparison schools at the time of their arrests or comparison dates drastically reduces the sample size. Intriguingly, however, preliminary results suggest that the effect of an arrest on eighth graders is in a slightly positive direction for Comer students and in a negative direction for non-Comer students.

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The failure to detect arrest effects on grade retention in lag-reduced models is the second unexpected finding that merits further investigation. Reducing the average temporal distance between the measurement of the selection variables and the event whose effects are being measured certainly reduces measurement error of the selection factors. When sizable effects observed under conditions of high measurement error, are reduced under conditions of low measurement error, as appears to have occurred in the retention analysis, it is quite possible that selection bias was responsible for the initial observed effects. Moreover, one could, by extension, argue that persistent selection bias may account for effects observed on other variables like dropping out and absenteeism, even when measurement error is reduced through lag restrictions.

Although my analysis of the impact of arrests goes to nearly unprecedented lengths in minimizing selection bias, selection bias remains the most serious criticism that can be reasonably leveled at this research. It therefore merits careful consideration.

The selection bias interpretation of my findings reads as follows: arrested students face two-three times the odds of retention after eighth grade, four-five times the odds of dropping out after ninth grade, and they experience eight to ten more absences from school and a slightly lower grade point average in the semester in which they were arrested, not because of the impact of arrest per se, but because of the impact of the conditions and behaviors that led to the arrest. For instance, arrests can be precipitated by increases in truancy, drug use, delinquency or an exogenous disruptive event like moving or a family or gang-related conflict. These altered circumstances, if they lead to sustained changes in attitudes and behavior can also exert harmful

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effects on school performance—even if one is not arrested. Since I am generally unable to precisely measure delinquency and truancy at the time of offense (or the comparison date for the comparison students), a skeptic could argue that I cannot unequivocally rule out selection bias as an explanation of the findings I observed.

While I cannot completely rule out a selection bias interpretation of my findings, the foregoing interpretation finds little support in the theoretical literature and is consistently contravened by empirical evidence. The selection bias interpretation is predicated on the notion that the sizable disparity in dropping out between the arrested and comparison group reflects sustained changes in behavior among the arrested group that are not accounted for by individual differences measured during elementary school. The image of delinquency, arrests and educational exits as products of sudden and unpredictable discontinuities in life circumstances as opposed to stable trajectories contradicts prevailing criminological thought and a widely accepted model of dropping out as a gradual, cumulative process rather than a sudden event (Kelly, 1993).

The notion that delinquency is stable or at least follows predictable trajectories is a basic one in criminology. Perhaps, the most vocal champions of the notion of the stability of delinquency are Gottfredson and Hirschi (1990). Gottfredson and Hirschi's general theory of crime holds that the delinquent and deviant behavior in the teen years and throughout adulthood results from enduring traits—self-control, insensitivity, and aggression—that develop early in childhood. In their view, one's deviant propensities remain stable over time. Thus, the theory leaves little room for sudden changes in delinquency of the magnitude that could explain a large

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increase in high school dropping out following arrest net of prior delinquency and "analogous behaviors" (acting out).

Developmental theories, in contrast to the general theory of crime, are concerned with changes in delinquent and criminal behaviors and propensities over time. However, these theorists typically describe changes as patterned over the life course (Moffit, 1993). For instance, Sampson and Laub (1993a) describe how challenges to the social bonds with parents developed during childhood and greater time spent with peers helps account for observed modal increases in delinquency during adolescence. Laub's and Sampson's (1993) developmental model also accounts for dramatic shifts in individual behavioral trajectories through the concept of turning points. Turning points typically involve the assumption of new roles and responsibilities (e.g. fatherhood and employment) that reflect and sustain the formation of new social bonds and the severing of old social bonds. These changes in social bonds, in turn, result in changes in delinquency. Sampson and Laub (1993a) note that some "turning points" like incarceration may increase involvement in criminal behavior. While my analyses do not measure turning points directly, it is likely that I measured many of the individual traits and past experiences such as attitudes toward school and family support that are precursors to turning points. Hence, controlling for a comprehensive set of individual differences during the later elementary school years should preclude a residual "turning points effect" that accounts for the differential drop out rates of arrested and "soon-to-be-arrested" youth. The idea that ninth graders who are arrested in ninth grade and ninth graders who are arrested in tenth grade are on similar life trajectories is also supported by the fact that, among ninth graders with spring arrest

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or comparison dates, treatment and comparison youth exhibit very similar rates of absences in the fall.

Persistent skeptics may counter, however, that the fact that delinquency has been repeatedly shown to follow stable and predictable trajectories (Moffit, 1993; Loeber, 1982; Cohen & Vila, 1996; Sorensen, 2001) does not preclude that my particular findings are due to unexplained contextual changes. Though delinquency is patterned over the life course, individual fluctuations in delinquency occur and no theory is expected to account for all the variation that occurs in delinquency within individuals over time and between individuals.

While "random error" is considered an inescapable fact of social research, less widely explored is the degree to which the random fluctuations in circumstances and behavior within and across individuals varies between populations. Perhaps, the contextual circumstances of the inner-city delinquents whom I sampled are so erratic that individual differences at point A, as measured in this study, simply do not predict individual differences at point B very well. I already showed that this is not true with respect to academic achievement which is predicted very well by achievement a year earlier. Delinquency, on the other hand, may be more vulnerable to changes in contextual circumstances. For instance, research has suggested that rapid changes in contextual circumstance like school attachment (Najaka,.Gottfredson, Wilson, 2001), neighborhood characteristics (Ludwig, Duncan, and Hirschfield, 2001) and formal social control (MacKenzie & Li, 2002) can lead to changes in delinquency and arrest risk.

Thus, it is possible that some researchers consistently find evidence for the stability of delinquency because, in the aggregate, the contextual circumstances of their samples are highly

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stable (or follow stable trajectories). To the extent that I can demonstrate that the circumstance of my sample are also highly stable or predictable, I undermine the selection bias interpretation of my findings.

Recall that, in some of my sub-samples, the arrested and non-arrested groups differed in the number of disruptive and seemingly exogenous "stressor" events recorded on their surveys. Of course, such events would have to be quite common to have any importance in the drop out analysis. Among delinquents who completed a behavior survey in what should have been their 8th grade year, 32% reported that their families moved in the past year, while 13% reported that their mother got new boyfriends in the past year. Of course most of the residential mobility is within neighborhoods and therefore not highly disruptive. However, moving longer distances is also quite common. Among the 340 delinquents who were arrested in 9th grade and who evidenced a starkly elevated drop out rate in the 9th grade staggered replication analysis, 21% lived in a different census tract (according to the school record) at the time of their arrest than they did a year earlier.

While disruptive events are quite common in the sample, they do not necessarily lead to changes in delinquency. Perhaps, those who experience a set of disruptive stressor events just prior to an arrest experienced a commensurate number of disruptive events a year earlier and two years earlier. In other words, the lives of some of the sampled youth may be stable in their chaos, leading to expectations of stability of delinquent propensities and criminogenic conditions that also promote school failure.

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Some empirical evidence from my research supports the potential of the antecedent control measures I employed to adequately control for selection processes. To lend adequate statistical power to the assessment of the stability of key control measures over several years I needed to go outside of the staggered replication framework and draw from the entire sampling pool. The overall sampling pool contained 145 youth who were surveyed in 1992 when most were 5th graders and surveyed again in 1996 when most were eighth graders. Among this sample, several key selection factors do exhibit considerable stability over time. Acting out in 5th grade was highly predictive of acting out in eighth grade as illustrated by a sizable correlation coefficient of .326 ($p<.001$). Of course, I generally did not need to rely on 5th grade surveys to measure antecedent delinquency levels in eighth grade. The average event/survey lag was actually less than a year. The correlation between 7th grade acting out scores and 8th grade acting out scores (measured separately in the sampling pool for each cohort) generally exceeds .5.

These correlations, consistent with the unmeasured, unpredicted "turning points" explanation, are smaller among the staggered samples. For instance, the 75 youth who are expected (based on cohort) to be in 7th grade in 1995 and 8th grade in 1996 displayed a correlation between 7th and 8th grade survey measures of acting out of "only" .35. The smaller correlation than that observed in the sampling pool can be attributed to random sampling error given the sample size of only 75.

The negative stressor index is less stable than acting out, but still reasonably stable. The correlation between the 1994 index and the 1997 index for the 487 youth with measurements in both of these years is only .112 ($p=.01$). Furthermore, the correlation between 7th grade and 8th

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grade measurements for this index is between .2 and .3 in the overall sampling pool but quite low in the select small-sample cohorts in the delinquent sample. However, even if the low $7^{th}/8^{th}$ correlation of disruptive life events is indicative of actual instability in this indicator this gives little cause for alarm. First, the low correlation of this indicator probably reflects the infrequency of some of the events that comprise the index (e.g. parent's losing job, or parent's moving out). This same infrequency of stressful and life disrupting events relative to the frequency of arrests suggests that it is highly unlikely that unmeasured differences in disruptive events explains the observed differences between the arrested and non-arrested youth in the school performance outcomes. Second, this possibility is rendered further unlikely by the weak relationship between the negative life events index and several of the outcomes.

The third assuaging fact is that not only are negative life events not strongly related to several school outcomes but they are not strongly related to delinquency either. The arrests that occurred may indeed occasionally be due to an erratic change in behavior as opposed to a stable pattern of behavior. However, proximal unmeasured differences in acting out even when incommensurate with distal measured differences in acting out typically do not arise exogenously (e.g. unexpected death in the family). Rather, they take root in past psychological, family, neighborhood, peer, school and other conditions that are measured in this study. If two groups are comparable across these domains, as well as self-reported delinquency, at Time 1 it is likely that they are similar on these and other unmeasured sources of variation in delinquency when one of the groups is arrested one to two years later (15 months on average for the base dropout sample). Thus, by establishing comparability on an array of antecedent factors between

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arrested and non-arrested group in a particular grade and then statistically controlling for the few (sometimes random and generally fairly stable) factors that statistically distinguish between the groups, one goes a long way toward minimizing selection bias.

While nothing short of a random assignment may win over some critics, most selection bias proponents would find my evidence for a causal impact of arrests on absences and grades to be persuasive. To argue otherwise is to argue that students arrested in the fall of ninth grade are stricken with bouts of delinquency and other problems—that cause their attendance and grades in the fall to be lower than students who are arrested in the summer. Students arrested in the spring of ninth grade, on the other hand, are, by comparison, thriving in the fall, but then are stricken with the same affliction in the spring. Youth who are not arrested until the 10^{th} grade are largely spared these painful hits to their attendance in ninth grade, even when their absences or grades in the fall are the same as those who are arrested in ninth grade. While the existence of unmeasured confounding selection differences between those arrested only one semester apart is not completely outside of the realm of possibility, the burden of proof definitely rests with those who would advance an outlandish typology of delinquents that centers on seasonal or—even more outlandish—semester to semester variation.

If one accepts that the effect of arrests on absences is causal in nature then, given the strong relationship between truancy and dropping out, it is also quite likely that arrests caused drop out rates to increase as well. However, the small negative effect of arrest on course grades, is not commensurate with the large negative effect on attendance. The answer to that conundrum likely lies in the greater variability in absences as well as its greater sensitivity to

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behavioral changes. Most of the sampled students are in the extreme low end of the grade distribution. Among the 284 youth in the ninth grade absenteeism analysis with fall grade point average data, the average grade point average was .82. Despite the fact only about 1/3 of these youth were arrested before or during that semester, 68% of them averaged a D grade or below in their course work.

Although further inquiry into these dismal statistics is desirable, they clearly accord with an impression that most of the sampled students—both before and after arrest—either have little interest in school or are ill-prepared to perform beyond what is required to pass the majority of their classes. This same disposition was arguably demonstrated in the consistently low achievement test scores of most students. Students who perform the bare minimum or less and then are arrested may simply continue to perform at the bare minimum or less. Those performing just above the bare minimum who experience the disruptive force of an arrest may need to exert themselves just a little more than before in order to maintain their current performance levels. Thus, with respect to grades, many forces lend toward stability even in the face of life changes following arrest.

Absenteeism on the other hand, is much more a behavioral and much less of dispositional measure than grades. First and most obviously, arrests and subsequent court hearings can directly affect absenteeism through removing a youth from school. Second, absenteeism, unlike grades, can result in immediate sanctions leveled at a student including detention and suspension. Parents, judges and probation officers may receive information about a student's recent attendance levels but they do not typically receive recent information about grades.

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Consequently, absences are much more bound up with a youth's relationships with their families and institutional authorities, both of which may be affected by an arrest.

Interaction Effects

I now turn to the second question of whether the effects of arrest on school performance vary in predictable ways. Given that arrests may prompt a social support or control response on the part of families, teachers, and judges it is noteworthy that most of the main effects of justice system involvement are in a negative (harmful) direction. Do these results suggest a failure of these social control mechanisms? I did not address this question directly in the quantitative analysis given the paucity of data on the types and levels of social support and social control following arrest. However, I did address it indirectly by examining whether those who are in a position to receive more support and control following arrest are less likely to suffer academic harm following their arrests.

Factors that can affect the level of support following arrest are numerous and include the degree to which a youth is a "hardened" delinquent, attitudes toward teachers, relationships with parents, social support networks in one's neighborhood, the number and type of the sanctions one receives, and even the age and gender of the offender. I tested each of these interactions for each outcome and obtained inconsistent results.

The most consistently demonstrated moderating effects are also the least surprising. Youth who are arrested multiple times are typically more likely to exhibit harmful effects on school performance while those who were detained were far more likely to drop out. However,

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these results do not necessarily indicate the failure of social support systems. Perhaps, helpers face a tougher uphill battle with multiply arrested or detained students.

Youth with higher levels of prior station adjustments and self-reported delinquency may also, by virtue of their delinquent and defiant ways, draw less sympathy from potential helpers and, consequently, receive less social support following arrest. This is suggested by the finding that increases in station adjustments correspond to harsher impacts of arrest on 8th grade repetition and youth exhibiting higher levels of delinquency experience greater declines in eighth grade test scores. However, this finding is tough to reconcile with the separate finding that more station adjustments are associated with a lower risk of dropping out and fewer absences following arrests. While one may build a theory to resolve this glaring inconsistency, a more plausible and more troubling explanation is that at least one of these interactions resulted from random error facilitated by low statistical power.

Unfortunately, the more direct indicators of potential sources of social support following arrest demonstrated a similar lack of consistency in how they condition the effect of arrest. Arguably the most relevant indicator of social support availability, home academic support, tempered arrest's impact on dropping out but failed to moderate the impact of arrest on the other outcomes. Unexpectedly, positve parent-child relations temper the effect of arrest on absenteeism and, possibly, seventh grade test scores but appears to elevate the risk of retention among those who are arrested. Perhaps, some caring parents encourage grade retention following arrest as a "slow down and catch up" measure to prevent their children from falling further behind and spiraling out of control when they enter the more disorderly and chaotic

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environment of high school. In a similar vein, higher levels neighborhood social capital, consistent with the work of Sullivan (1989), appears to partially mitigate the effects of sanctions on dropping out while, at the same time, it may, along with neighborhood attachment, inexplicably intensify the negative impact of arrest on attendance.

Adding further complexity and ambiguity to this picture are the findings concerning the variable effects of arrests under different family arrangements. Most experts would expect that youth who live with neither biological parent and are typically cared for by extended family instead have less access to social support and less exposure to supervision both before and after arrest. Consequently, most would predict that such youth are more vulnerable to negative consequences of being arrested. In my sample, however, having no parent in the home was extremely common and was not associated with lowers levels of reported parental support and monitoring. Moreover, post-hoc t-test comparisons (not shown) reveal that the differences in perceived social support between differently parented kids, while generally not significant, are more likely to favor youth living separately from their biological parents. Accordingly, youth in no-parent homes actually experienced less harmful effects of arrest on dropping out.[64] The interaction is even stronger for eighth grade math test scores, predicting that being arrested may slightly improve math test score among youth with biological parents in the home. On the other hand, sanctions may yield more harmful effect on 7th grade reading scores for the no-parent groups than for those living with at least one biological parent.

[64] This finding reflects both higher rates of dropping among comparison youth who did not live with their biological parents and elevated drop out rates among treatment group youth who live with at least one parent.

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These results are quite surprising and anomalous. A full explanation would extend beyond the scope of this text, but some limited speculation seems warranted. First, youth who live with someone other than one or both biological parents typically do so because someone else is thought to provide better care-giving than the natural parent. So, the vast majority of the no-parent groups are probably living with parents that are more supportive than their natural parents. On the other hand, many among the natural parent group are presumably living with parents who are less effective than potential kinship caregivers. This difference could explain why some arrested youth may be better off living apart from their biological parents. Second, household living arrangements may be a factor in the treatment that youth receive from potential providers of social support and control following arrest. Perhaps living apart from one's parents is considered a mitigating factor by teachers, judges, extended family caregivers etc. leading to a more pro-social, restorative response. The contravening pattern of results observed with respect to seventh grade math scores, perhaps, reflects the recency of the family crises and disruptions leading to and continuing after the new living arrangements. By eighth and ninth grades, the new family situation may be less indicative of disruption as the new living arrangements attain greater stabilization.

Explaining Juvenile Justice Interactions

The foregoing attempts to reconcile inconsistencies in the moderating effects of social support variables are highly conjectural. Random error due to low statistical power may be an equally viable explanation for the variable effects of arrests under varying levels of presumed social support from teachers, families, and neighbors. But what of the effects of social support

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and control from the juvenile justice system itself? Formal control over juvenile behaviors, after all, is more likely than informal control to change following an arrest. This assertion finds confirmation in a recent study by MacKenzie and Li (2002) who found that offenders experienced no change in informal social control from family, the labor market and other sources while under intensive probation supervision. Consequently, the authors allege that the declines in crime they observed during probation resulted from the increase in formal control through increased monitoring and the threat of sanctions.

My sample of accused offenders doubtlessly vary in the level of monitoring they receive following their arrests. Behaviors under the watchful eye of the justice system may include not only delinquency and gang activity but also school attendance. As explained earlier, school attendance is often a condition of court supervision and juvenile probation.[65] Probation, supervision and other court dispositions are not a suitable moderating variable in this analysis, however, because the majority of youth are arrested only once in their first arrest's school year and most first arrests do not result in a finding of delinquency—a necessary condition of probation.

Nevertheless, many youth in my sample are subject to formal social control immediately following their arrests and their petition to juvenile court which generally occurs within 21 days

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[65] I did not pursue a systematic analysis of the impact of probation because of the unwieldy nature of available data, and because the experience of probation is not common during the measurement time frame among the treatment groups. Probation and court supervision are relevant only to outcomes measured at least three months after one's first arrest given that court supervision and probation do not commence until after a finding of delinquency, which may not occur until many months after an arrest unless a plea-bargain is achieved at a very early stage.

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after an arrest. This monitoring may affect school performance either directly or indirectly. Youth whose risk scores are high enough to result in assignment to a non-secure detention alternative like home confinement or electronic monitoring are forbidden to leave their homes except for pre-approved, fixed-duration activities like school and work. When faced with a choice between perpetual confinement in one's own home or spending at least part of one's day interacting with peers at school and enroute to and from school, I suspect that many choose to attend school. Likewise, indirect effects on school behavior may occur during monitoring when the threat of further sanctions leads youth to curtail their delinquency and, perhaps by default, spend more time at school. Re-arrested youth on electronic monitoring or home confinement or with pending court petitions face an elevated risk of detention at their screening hearing.

The results of my analyses of absenteeism accord with this account. I found that youth who were sent to detention alternatives at least once during the semester of their first arrest exhibited fewer reported absences that than those who were released without pre-adjudicatory supervision or detention. While this effect was not statistically significant or measured net of prior absences it is still impressive in light of the fact that, by design, youth who are sent to detention alternatives are more serious and/or frequent offenders than those who are released. If the "supervised sample" in this sub-analysis could have been expanded to include those under probationary or court supervision during that semester I suspect that this pattern of results would have been considerably stronger.

The final noteworthy discovery in the realm of conditioning factors is that the impact of arrest on absences differs for males and females. Net of prior test scores and other test scores,

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arrests negatively impact women's absences more than they do males. This difference does not necessarily suggest a gendered response to sanctions, however. Alternative interpretations are (1) that other unmeasured antecedent different between male and female arrestees account for the post-arrest differences and (2) since the small female subsample yields only one interaction effect out of five examined outcomes, this difference could be due to chance. These doubts gain further credence from the finding that the *change* in absences among the small group of 30 young women with complete ninth grade absences data, who are arrested in the spring of ninth grade, is of the same magnitude as that of the men. Further empirical research is necessary before developing a theoretical explanation for why arrests effects on school performance appear to vary by gender.

Do These Results Matter?

Now that I have demonstrated that arrest probably has a causal effect on school performance and this effect is inconsistently conditioned by levels of social support and control, the next major question is "So what?" This question has multiple layers. The first layer which I will address in the remainder of this chapter concerns the relevance of the findings for the research question. That is, do my empirical findings, assuming they are internally valid, suggest that juvenile justice involvement severs youth attachments to the educational institution? The second layer of the "so what?" question, which I will address in the concluding chapter, concerns the implications of the findings for the broader question of the mechanisms through which juvenile justice affects school performance and urban public educational institutions more generally.

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Do my observations of adverse, short-term effects of juvenile justice system involvement on multiple school outcomes imply that juvenile arrests can seriously jeopardize life chances? Alternatively, would the delinquent youth in the sample like have dropped out of school eventually anyway, even if they were never arrested? The second scenario, if true, may substantially diminish the theoretical importance of my findings and merits serious thought.

The foregoing challenge to my findings is an artifact of my research design. My sample was deliberately confined to youth who have been arrested at least once. My design provides little basis to speculate as to how the inner-city delinquents I sampled would have fared in school had they not been arrested. My findings do suggest that those who are arrested in ninth grade are more likely to drop out in ninth grade than youth who are arrested in tenth grade. But the vast majority of the sample, regardless of when they were arrested, failed to complete high school. Furthermore, they fail at rates far above their non-arrested peers. I can illustrate these points by examining enrollment status in the Fall of 2002 when the youngest cohorts of participants in the Comer study are expected to be in 11th grade and the vast majority are well beyond the high school years. Among the 686 males who meet the most basic criteria for the staggered replication analysis (arrested during or exactly a year after enrollment in seventh, eighth, or ninth grade), only 10.7% as of Fall 2002 were still enrolled in a mainstream or alternative settings, had graduated, or transferred out of the Chicago Public School System. Only about three percent of these youth were still under eighteen at the time. By contrast, among 4343 males from the

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sampling pool who show no sign of juvenile justice involvement, 64.6% exhibit these positive or neutral enrollment situations.[66]

A cynical response to the "so what?" question that flows from the above analyses is that my results do not show that arrests influences the risk of dropping out. Rather, they simply suggest that it influences when youth drop out. The argument that arrests do not elevate the risk of eventual dropping out seems highly implausible on its face, unless one believes that the arrestees—most of whom are arrested only once or twice—and the non-arrestees described above are as different from each other as their drop out rates suggest. Still, let us consider, for the sake of argument, that arrests serve only to accelerate dropping out. Would such a finding have any long-term implications?

Essentially, the question at hand is whether youth who drop out in ninth grade or sooner face an elevated risk of negative outcomes like unemployment and imprisonment relative to youth who do not drop out, say, until tenth or eleventh grade. This question is best addressed directly and empirically, but the best indirect evidence comes from research that suggests that, especially among minority males and females, the amount of time in school and skills acquired in school may be more important than the actual credentials in determining earnings (Ishikawa and Ryan, 2002). Perhaps, employment and success in the low-wage service sector depends heavily on reading and verbal skills as well as occupational coursework (Arum & Beattie, 1999)—all of which can increase with additional years of schooling and are predictive of incarceration risk. One's chances of pursuing and obtaining a GED should also be greater for

[66] The major source of incomparability between the two samples is that about 20% of the larger sample were under 18 in Fall 2002.

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youth who have more actual high school course work under their belts, although some evidence suggests that differences in earnings or employment between minority GED recipients and high school drop outs are spurious (Tyler, Murnane, & Willett, 2000) and accounted for by differential years of schooling (Heckman & Cameron, 1991). Perhaps, in Chicago, where dropping out is nearly normative, employers in the secondary labor market may have come to rely on years of education, even more than the GED, as a proxy for a youth's intellectual and social skill levels and dependability. Reaching eleventh grade may actually be a "credential" in the low-wage labor market. Dropping out in ninth grade, on the other hand, is an unambiguously negative signal for employers.

Thus, to the extent that incarceration and labor market success are tied to the timing of dropping out, my finding that arrest "speeds up" dropping out of school is far from trivial. And, to the extent that the accumulation of math and reading skills (which may be impaired by multiple arrests) and absences are bound up with both the timing of dropping out and long term employment prospects, the short-term effects of arrests on these dimensions are important as well.

While the long-term implications of the quantitative findings are far from clear, a better understanding of the reasons for the effects is within my grasp. Do youth reports of their actual experiences with school and the juvenile justice system accord with my conclusion that arrests matter? And do the reasons that youth provide for the effect of arrests accord with correspondence theory or a competing interpretation? These questions call for an account of the

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interrelationships between school, juvenile justice, and the lived experiences and developmental context of inner-city youth. The next four chapters heed that call.

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CHAPTER 8: INTERVIEW DESIGN, METHODS AND STRATEGIES

As is evident from the previous chapter, statistical tests of theories provide a very limited window into complex social phenomena. The quantitative analyses in Chapters five and six make a compelling case that arrests, under most conditions, adversely affect several measures of school performance, net of several covariates of both arrest and sanctions. However, the statistical analysis is incapable of addressing two crucial questions. First, do unmeasured factors explain both arrests and sanctions and render the association between arrests and school failure spurious? Second, what intermediary processes explain the link between arrests and school performance?

While the first question addresses the validity of causal inferences, the second issue is important on both theoretical and practical grounds. On a theoretical level, exploring social process cuts to the heart of social theory, since nearly all social theories are rooted in ideas about *how* a particular social behavior or phenomena is produced. Given the number and the intricacies of the intervening processes that underlie any statistical relationship in social science, a full understanding of social process cannot be achieved through statistical analysis alone. Attention to social processes also attains importance on a more practical level. Practitioners, policy makers, and other social engineers—need to understand the mechanisms through which dysfunctional behavior or a dysfunctional social system develops in order to pinpoint possible sites for intervention.

I address these core issues through qualitative interviews with a sample of the youth whose behavior informed the statistical analysis. As a result, the interviews are an avenue of

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both validation of causal relationships and exploration into the social processes underlying the impact of arrests and school performance. This chapter describes the mechanics of the interviews, while the next three chapters describe the substance of the findings. Chapter 9 brings the interview findings to bear on the issue of spurious correlation. Chapter 10 describes psychological and social processes that ensue from sanctions and have potential implications for school performance, but operate largely outside of the venue of the school. Chapter 11 focuses more on the social network and institutional dynamics following sanctioning centering on schools and juvenile justice.

Interview Design

Given that intermediary processes linking arrests to changes in school performance involve changes in psychological and interpersonal dynamics as well as youth's level of participation and integration into social institutions, the best source of information on these processes are clearly the youth themselves. However, exclusive reliance upon on the youth paints on incomplete picture. Individuals are not aware of all the social processes in which they participate, much less their causes and effects. For instance, arrested youth are likely to be incognizant of exchanges of information between school and juvenile justice actors, and sometimes between these actors and their parents.

Given my research objectives, the ideal research design would supplement and cross-validate youths' accounts of social processes with accounts and observations of school officials, parents, and official juvenile justice actors. Unfortunately, time constraints permitted systematic field interviews only with youth. Conversations with family members and a few education and

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juvenile justice professionals were incorporated into this research but not to the extent necessary to function as a reliable alternative or corrective to youths' accounts.

My chosen method for eliciting accounts of social processes was informal, semi-structured interviews in which youth provided personal narratives of their family and school experiences with a special focus on the social, psychological, and institutional dynamics they encountered leading up to and following sanctioning. I attempted to solicit from each youth who experienced changes in school performance following arrests a causal narrative of those changes. The analysis of the interview data aims to uncover patterns in the accounts, particularly among those who cite juvenile justice as a factor in their school performance.

Two challenges with this approach of illuminating causal processes are evident. First, an obvious drawback of asking youth for causal narratives is that retrospective explanations of behavior are not always valid. Much social psychological and sociological literature shows that causal stories often vindicate the storyteller. Second, given the diversity of lived experiences, the variability of factors included in explanations could make it difficult to highlight the most salient mediating processes.

Fortunately, researchers have developed some techniques to narrow the field of salient and valid explanations for causally associated events. These techniques do not merely rely on the narrative accounts of causal mediation. Rather, these techniques involve the systematic measurement of potential mediators, whether or not these mediators are stressed in the interviews. Causal relationships are inferred through an examination of configurations of mediators associated with particular outcomes.

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The first tool I utilize for this purpose, the case-specific selection design, is predicated on the idea that if a process mediates school performance declines following arrest, then it should be present among many or most of those who experience declines, but not present among those who experience no change, or who improve following arrest. Let me illustrate the utility of this approach through an example. If I were to select for my sample only youth experiencing a decline in educational performance following arrest and found that all of them reported problems with teacher relationships following their arrests I may construct a theory emphasizing teacher relationships as an explanation of negative effects. However, such a theory is more convincing if I can demonstrate that those whose school performance remained stable or improved following sanctions *did not* experience problems with teacher relationships following sanctions. A case-specific strategy, therefore, permits stronger assertions about the viability of different mediating theories.

Hence, in contrast to the common research approach of selecting and/or comparing cases on the basis of the independent variable(s), the case-specific selection design selects on the dependent variable. In this research, two case groups were selected—youth who experienced a pattern of declines in school performance following sanctions and youth who evidenced at least one instance of improvement in school performance following sanctions. I attempted to exclude youth who evidenced either trivial or no changes in school performance following sanctions. The inclusion of only youth who are likely to experience an effect of arrest is quite sensible; one cannot conduct a study of the mediation of a causal effect, if no sampled youth experience the impact.

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Sample selection proceeded as follows. First a random sample of 300[67] youth aged 18-19 in May 2001 was drawn from the pool of 1191 arrested youth from which the staggered replication was drawn. 18 through 19 was the chosen age range because youth at that age are young enough that juvenile justice and school experiences are still fresh in their minds yet old enough to reflect on how the pattern of juvenile justice involvement has shaped their schooling and life chances and, as a practical matter, to consent to participate in research.

The next major step was to select a sample of youth for interview solicitation that would ultimately yield ten interviews from each subsample. The research is limited to only twenty interviews owing to time and financial restrictions. Given that recruitment success rates in interview studies of at-risk youth samples gleaned from archival data have been as low as forty percent I aimed to select about fifty youth—apportioned approximately equally between the two groups for the solicitation sample.

The goal of selecting ten youth for each sub-sample thus required the classification of approximately 25 youth as "declined and another 25 as "improved". This determination was a two-stage process. First, I computed a set of variables representing changes in school performance following each arrest. These variables included changes in achievement test scores, absences, credits, grade point average, credits earned, and number of classes taken as well as enrollment status soon after arrest or release from detention and at the end of the semester or school year. These variables were also aggregated across semesters or school years (as appropriate) in which an arrest took place. Thus, these variables enabled me to examine changes

[67] The sampling pool was restricted to 300 cases, because the pool is large enough to ensure that adequate numbers of youth in each of the sub-samples, yet small enough that errors and inconsistencies in the data could be detected and fixed if necessary.

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in test scores and other performance measures after each individual event as well as the average change in tests scores across arrests for the same individual.

Next, the cases in the sampling pool, ordered randomly, were examined sequentially and manually in the data set. I classified about half of the cases according to patterns of improvement, stability, or decline.[68] The classification process resulted in several categories that were eligible for inclusion in the final solicitation sample. The "decline" sample consisted of 28 youth who experienced a major academic decline or a pattern of several modest declines following at least one arrest. Subsumed under the "improved" group (N=27) were youth who evidenced at least one major improvement or several modest improvements (9 cases), youth who evidenced very modest improvement after one or two arrests (12 cases) and youth who exhibit a mixed pattern across multiple arrests of both marked improvement and marked decline (6 cases).

Tracking and Recruitment Strategies

The next step was locating potential interview subjects and soliciting their participation. The objective of the recruitment strategy was to achieve a sample whose experiences are maximally representative of the patterns of academic changes following variable patterns of juvenile justice involvement. One could argue that the ideal recruitment strategy from this standpoint would be the intensive recruitment of a random sub-sample of the youth.[69] However,

[68] When classification was not immediately clear based on the above criteria I also examined other factors such as grade retention and transfers to alternative schools (although it is sometimes unclear whether these are positive or negative school outcomes). For borderline cases, I also considered some long-term outcomes such as eventual drop out, graduation, and transfer to GED program or evening school.

[69] Deliberative selection of a sample that is representative of the population along key dimensions is perhaps an even better way of bolstering external validity. However, such a strategy too easily leaves the sample vulnerable to accusations of researcher bias.

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this strategy may be ineffectual and impractical; such a strategy would not only face an uncertain response rate, it would also exceed the time and financial resources allotted for this research.

In order to balance concerns for sample generalizability with pragmatic considerations, I devised a two-pronged recruitment strategy. I simultaneously pursued a general recruitment sample and an intensive recruitment sample. The youth in the intensive recruitment sample were drawn from the general sample. A stratified random sample of eighteen youth—evenly split between "improved" and "declined" youth—were chosen for intensive recruitment. The plan was to conduct as many interviews as possible from the random sample and conduct up to ten interviews with the general sample on a first-come first-interviewed basis. In the end, only nine of the interviewees (five declined and four improved) were drawn from intensive interview sample, while the remaining eleven interviewees were in the general sample only.

General Sample Solicitation Strategies

The solicitation of the general sample consisted of two basic stages. The first, beginning in late March of 2002, involved letters and phone calls. The second stage, which began shortly thereafter and ran concurrently, involved locating incarcerated youth and is described below. The most recent address was gleaned from several sources including the Comer database, justice system records, the telephone book, and Internet directories. Addresses of known siblings and parents were obtained as well. Accordingly, several letters were often sent out in search of the same individual.

The recruitment letter was designed to minimize suspicion while maximizing enticements to participate. The letter clearly sought to situate the project outside of the Chicago Public School system or the criminal justice system. Accordingly, the letter minimized mention of the

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juvenile justice system, including it as only one of the institutions whose effect on youth is of interest to the researcher. The letter emphasized school experiences most. The drawback of that strategy, as I learned later, is that many of the youth were very bitter about their school experiences and were probably confused about why they of all people would be asked to participate in an interview about school.

The letter also mentioned that participants would be paid $30 for their participation. Despite this incentive and the large number of letters sent, only three of the twenty interview subjects were recruited through a letter (Shelton, Gabriel and Jay).[70]

While the letters prompted little response, they may have increased the receptivity of youth who were reached by telephone. Every youth contacted by telephone agreed to be interviewed. The telephone conversations, which emphasized the confidential nature of the interviews and the $30 compensation, most likely helped to establish trust and increase respondents' motivation.

The second stage of sample selection was to locate youth who were incarcerated. The State Department of Corrections furnished the names and locations of youth who were located in state youth and adult correctional centers. Likewise, Cook County public relations authorities provided information on youth located in Cook County Jail. One youth was surveyed in an adult prison while four were interviewed in jail.[71]

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[70] An additional volunteer reached by letter was rejected because the quota for interviews from the general solicitation sample had already been reached.

[71] Among the five youth in correctional facilities at the time of the interview solicitation only the one who responded to the letter was interviewed (Gabriel). Two were located too far away from Chicago while the remaining two were interviewed after their release and return to Chicago (Tyrece and Jay). Among the five youth residing in Cook County jail, four were interviewed (Petie, Hova, David, and Vince), while

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Intensive Recruitment Strategies

The intensive recruitment sample consisted of eighteen youth randomly selected from the overall solicitation sample. Of the nine youth recruited from this sample, six of them were recruited through the strategies described above—letters or phone calls or through jail administrators—except more effort was made to obtain the current phone numbers and addresses and to persistently follow up letters and phone messages with subsequent phone calls.

The remaining three interviews were the culmination of an extensive search process that sometimes went beyond conventional recruitment methods. First, I obtained most recent addresses for the youth listed at Cook County adult probation department. Armed with a list of recent addresses for the several youth in the intensive recruitment sample, I traveled to well over a dozen addresses throughout Chicago and solicited information from current residents and neighbors on the whereabouts of the youth or their parents. The odds were against finding several of the youth, particularly two who were fugitives. The three successful "cold call" visits, especially the one to a housing project, required an abundance of friendly persuasion and luck. See Appendix B for two "war stories" from the recruitment process. The anecdotes should garner some appreciation for the challenges of recruiting youth who do not particularly want to be found. Given the highly transient nature of the population along with their initial mistrust of me, a 50% recruitment rate of the intensive recruitment sample is a success.

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one was remanded to state prison before the scheduled interview could take place. Quite oddly, three of the four inmates interviewed in Cook County jail were facing homicide charges (all three profess their innocence). Such is one of the pitfalls of leaving sampling largely up to chance.

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Interview Sample

The interview sample appeared to be, in most respects, reflective of the sampling pool. 18 were African-American (including a youth of mixed black and white origins), one was dark toned and of Puerto-Rican origin, and one was white and of Mexican origin. Only one female was recruited and she denied ever being arrested. Hence, all first-hand accounts of experiences following juvenile justice involvement are those of males. The subjects ranged in age from 18 to 19, with a modal and mean age of 19.

As explained, the sample consisted of ten youth chosen because they appeared to fit a pattern of academic decline after arrest and another ten youth who evidenced at least one significant improvement after arrest. Before describing these two sub-samples of youth it is necessary to discuss the validity of the grouping process. After the interviews were completed I reviewed the narratives provided by the youths and compared them to the official school record. For two cases, I determined upon re-examination that the youth's academic narrative did not accord with the initial characterization. One youth's (Armand's) classification was switched from improved to declined while another improved case (James) was reclassified as neither improved nor declined—a categorization that disqualified him from many case-specific comparisons in the study.[72]

[72] The first of these youth, Armand, experienced an improvement in test scores after his first arrest. Several years later, he was arrested in the fall and dropped out toward the end of that school year. This mixed pattern of outcomes earned Armand a classification as both improved and declined. Upon re-examination, however, I determined that this classification was untenable, because the improvement was trivial compared to the decline. To illustrate, the change in reading scores was an improvement from 4th grade when Armand earned the lowest possible score of 1 to 5th grade when Armand improved to the abysmally low scores of 15. This hardly seems like an improvement worth analyzing. At the same time, Armand revealed in the interview that his arrest in seventh grade led him to flee his foster care placement

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While the youth I interviewed were very homogenous demographically, they were quite diverse with respect to academic and juvenile justice histories. Table 1 displays several characteristics of both sub-groups of the interview sample as well as the interview sample as a whole. The table also displays the same characteristics for the remainder of the sampling pool as a whole (n=279) in order to provide a rough sense of how "typical" the sample is. Of course, one should not expect the 20 youth to look like the sample as a whole. Aside from variation due to random sampling error, the sample was deliberately selected for exhibiting marked positive or negative declines in academic performance. It would not be surprising to learn that youth experiencing such changes following arrest differ from other youth. The table only serves to preclude inordinate levels of divergence from the norm.

One of the first notable statistics from Table 1 is that the twenty sampled youth exhibit an average of 3.70 juvenile arrests through June 1999.[73] This number is about average for the sampling pool. However, notice that the standard deviation among the interview sample is 3.5. This number reflects the diversity in the sample with respect to juvenile justice history. Of the

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and, consequently, miss most of seventh grade. In light of the gravity of the decline in comparison to the improvement, Armand was reclassified as a decline case.

The second reclassified case was James. He was initially recorded as an improved case because his math and reading scores improved by six and thirteen respectively following his arrest. However, subsequent to the interview, further examination revealed that the year in which the improvement occurred was his second year of eighth grade. An improvement of this magnitude after an additional year of learning the material is hardly an improvement at all. Furthermore, and consistent with this bleak interpretation, James was diagnosed as learning disabled beginning in the spring of his second eighth grade year. As a result of this closer examination, James no longer qualified for either the improved or declined group.

[73] This number is a slight underestimate given that arrests after 1997 that did not result in detention screening hearings are not included.

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Table 1
Characteristics of the Interview Sample and Sampling Pool

	Improved (*N*=8)		Declined (*N*=11)		Interviewed (*N*=20)		Non-sampled (*N*=279)	
Variable	*M* or %	*SD*	*M* or %	*SD*	*M* or %	*SD*	*M* or %	*SD*
				Demographic and Background				
Age at Event	14.27	1.66	14.45	1.94	14.37	1.77	14.10	1.69
Age at Last Survey	13.02	1.50	13.32	1.07	13.18	1.25	13.30	1.06
8th grade Math	28.00	12.26	26.33	14.47	27.17	12.82	29.68	14.95
8th Grade Reading	28.00	10.95	23.00	10.75	25.69	10.72	31.84	14.74
No biological parent (%)	33.3		63.6		50.0		23.7	
Retentions 5th to 8th	.33	.50	.36	.50	.35	.49	.25	.46
Single Parent (%)	44.4		27.3		35.0		37.3	
Working Parent (%)	55.6		63.6		60.0		45.9	
				Attitudes and Behavior Variables[a]				
Acting Out	6.00	2.28	5.05	2.92	5.45	2.65	5.17	2.78
Lack of Anger	3.36	.59	3.20	.65	3.27	.61	3.04	.84
School Attachment	3.60	.65	3.13	.68	3.37	.69	3.20	.81
Teacher Caring	3.67	.45	3.54	.51	3.60	.47	3.59	.62
Home Academic Support	2.93	.59	3.11	.57	3.03	.57	2.95	.56
Parental Control	3.34	.65	3.80	.62	3.59	.66	3.72	.79
Negative Stressor Events	.47	.50	.72	.47	.61	.49	.70	.58
Parent-Child Relations	3.58	.57	3.69	.68	3.64	.61	3.93	.71
Teacher Respect	3.47	.36	3.91	1.47	3.70	1.09	3.53	.84
Life Satisfaction	3.74	.49	4.23	.40	4.02	.50	4.00	.55
Soft Drug Use	1.22	.43	1.65	1.22	1.47	.97	1.85	1.15
				Juvenile Justice Involvement				
Total Arrests	3.33	3.61	4.00	3.63	3.70	3.54	3.52	3.10
Total Days in Detention	24.22	47.03	30.18	49.03	27.50	46.97	17.12	43.58
Total Station Adjustments	2.17	2.79	3.43	4.08	2.85	3.46	1.94	2.58
				Demographic and Background				
Age at Event	14.27	1.66	14.45	1.94	14.37	1.77	14.10	1.69
Age at Last Survey	13.02	1.50	13.32	1.07	13.18	1.25	13.30	1.06
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Single Parent (%)	44.4		27.3		35.0		37.3	
Working Parent (%)	55.6		63.6		60.0		45.9	

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Variable	Improved (*N*=8) *M* or %	*SD*	Declined (*N*=11) *M* or %	*SD*	Interviewed (*N*=20) *M* or %	*SD*	Non-sampled (*N*=279) *M* or %	*SD*
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Lack of Anger	3.36	.59	3.20	.65	3.27	.61	3.04	.84
School Attachment	3.60	.65	3.13	.68	3.37	.69	3.20	.81
Perceived School Caring	3.67	.45	3.54	.51	3.60	.47	3.59	.62
Home Academic Support	2.93	.59	3.11	.57	3.03	.57	2.95	.56
Parental Control	3.34	.65	3.80	.62	3.59	.66	3.72	.79
Negative Stressor Events	.47	.50	.72	.47	.61	.49	.70	.58
Parent-Child Relations	3.58	.57	3.69	.68	3.64	.61	3.93	.71
Teacher Respect	3.47	.36	3.91	1.47	3.70	1.09	3.53	.84
Life Satisfaction	3.74	.49	4.23	.40	4.02	.50	4.00	.55
Soft Drug Use	1.22	.43	1.65	1.22	1.47	.97	1.85	1.15
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Negative Stressor Events	.47	.50	.72	.47	.61	.49	.70	.58
Parent-Child Relations	3.58	.57	3.69	.68	3.64	.61	3.93	.71
Teacher Respect	3.47	.36	3.91	1.47	3.70	1.09	3.53	.84
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Variable	Improved (*N*=8) *M* or %	*SD*	Declined (*N*=11) *M* or %	*SD*	Interviewed (*N*=20) *M* or %	*SD*	Non-sampled (*N*=279) *M* or %	*SD*
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Total Station Adjustments	2.17	2.79	3.43	4.08	2.85	3.46	1.94	2.58

[a] Individual attitude and behavior variables are computed as the mean across all years of a youths' available survey data.

twenty youth, nine had only one arrest (two of these had at least one detectable station adjustment as well).[74] At the same time, eight youth exhibited four of more arrests. Three youth had nine or more. Improved youth are arrested less frequently than both the declined group and, modestly, the remainder of the sampling pool.

While many of the other differences between the interview sample and the overall sample as well as, of course, differences between sub-groups, could be due to chance, some are large enough to merit mention. The interview sample eighth grade reading scores, on average, are six points lower than the non-sampled youth. Interestingly, the math and reading scores of the improved youth are also slightly lower than average.

Both improved and declined youth are also more likely, according to the mode of their surveys, to be typically residing apart from their biological parents. This difference is mainly due to the characteristics of the declined group, a startling 64% of whom spent the majority of their surveys apart from their biological parents. Interviewed youth are also more likely to be retained and to have a mother working outside of the home. Both groups, especially and oddly

[74] Recall that the station adjustment file ends in February 1997. Thus, some station adjustments clearly went undetected.

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the improved group, also have higher acting out scores despite the fact that their acting out scores, relative to those of the larger sampling pool, are measured when the youth are younger.[75]

Overall, while only a few striking differences are evident on antecedent attitude and behavior measures across the categories pertaining to the interview study, the pattern of differences is striking. Surprisingly, the pattern of the differences between improved and declined youth suggest that youth whose school performance showed patterns of improvement actually reported less satisfaction, support, and control with respect to their relationships with their parents, although, perhaps, slightly higher satisfaction with school. The differences in the table provide very preliminary support to theories emphasizing institutional dynamics rather than familial or psychological explanations of the relationships between juvenile justice involvement and school performance.

In addition to sampling error and the expected biases associated with the targeted sample, some differences between my interview sample and the sampling pool may also be rooted in the peculiarities of my recruitment strategy. The recruitment strategy, which relied heavily on telephone calls and criminal justice agencies, netted youth of two primary types—those who live in homes with a stable guardian and a listed telephone number and serious or active offenders. One may suspect that youth whose telephone numbers are listed in the phone book or the juvenile justice record and who still live with the person listed as their guardian in the school or justice system records are in a more stable living situation than those who could not be found by phone. This notion is buttressed by the finding that three-quarters of the sample spent three or

[75] This assessment is based on the fact that acting was measured on an average of 2.75 for the interviewees surveys compared to 1.7 surveys on average the larger pool. Plus, the age at the last survey is 13.18 for the interviewees and 13.3 for the larger sample.

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four more years in the same elementary school during the Comer school years (1992 or 1993 through 1997). The same is true for only forty percent of those who were not interviewed.

Active offenders may also be more likely to have telephones if illicit drug sales (or other criminal activity) pay the telephone bills (only two sampled youth, Kevin and Damien, were employed at the time of the interview). However, the large number of unsuccessful follow-up telephone calls suggests that my sample suffers the unstable telephone service access, that characterizes the poorest segments of Americans, characterizes my sample as well.[76]

If the above is true it does not mitigate the fact that my sample consisted disproportionately of jailed youth, because they were easy to find and recruit. Their disproportionate presence likely biased the sample in negative ways, especially given that, of the five incarcerated participants, three were facing murder charges.

Interview Protocol

Setting

From March 26, 2002 through September 2, 2002 I conducted twenty face- to-face, semi-structured interviews. The non-incarcerated youth were generally interviewed in the convenient, discreet, comfortable, and generally quiet confines of their homes. Three interviews were conducted in restaurants. Occasionally, parents or other family members overheard parts of the

[76] In mid May 2003, about a year after the interviews were in full swing, I attempted to contact most of the interviewees to validate and verify some of the facts reported in this paper. Only two of the telephone numbers I called were still valid. Even the most stable informant, Julius, was reported to be no longer living with his family. While it may be the case that a current telephone book would lead to some of the youth again, an equally plausible explanation is that telephone service is irregular for the sort of youth I interviewed and their families. Perhaps, during my recruitment efforts, I found those youth whose parent (always the mother or grandmother) just happened to have telephone service at the time. And if I attempted to replicate this recruitment strategy a year later, a different set of youth would be located and recruited.

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interviews but privacy was granted for the vast majority of each interview. The five incarcerated inmates were interviewed in private rooms intended for consultations between lawyers and their clients.

Obtaining Subject Consent

Each youth was verbally informed that their identity and the identities of those they mention in the interviews would be held in strictest confidence, and that no means would exist to link the tape-recorded information to their identities. Youth are referred to in this text and all documentation from this project by a pseudonym. Each youth, including the five incarcerated youth, granted me written permission to do the following: (1) Tape record the interview. (2) Review for research purposes the youths' arrest and other juvenile justice records, as well as school records, and attitude and school climate surveys after completion of the interview, and (3) to securely maintain the youth's contact information and to contact the youth in the future should the need or the opportunity to conduct a follow-up interview with them arise.

Interview Format

The semi-structured interviews were guided by a checklist of topics, covered in a logical but not rigid order. In this way, the interviews assumed the format of a conversation, which was intended to boost the respondents' comfort level.

Interviews generally lasted about two hours.[77] Informants were asked questions covering a variety of topics. The first questions centered on the youth's school experience including

[77] Less time was spent with two youth—the woman who denied being arrested and who, therefore, had little or no useful information to offer with respect to educational and/or juvenile justice experiences

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schools attended, dates of attendance, and reasons for leaving. If youth did not mention their legal entanglements on their own, they were asked directly to discuss their school experiences following any arrests they may or may not have experienced. I probed for details pertaining to the social, psychological, and institutional dynamics all youth experienced following sanctioning.

Additionally, to probe whether youth experiencing differential outcomes following sanctioning had differentially satisfying experiences in the juvenile justice system and schools, I asked each youth to assess whether they were treated with fairness and respect by various members of the juvenile justice system including police, detention center staff, judges, and probation officers and by the school system including teachers, principals and school security.

Interview Strategies

Obtaining valid data in short order on sensitive subject matter from this population required overcoming several obstacles. Many of these youth find it difficult to trust strangers, particularly those who, like me, are from different race and class backgrounds as well as from a different age group. Furthermore, as the literature on the experiences of young minority males suggests and my interviews substantiated, many of these youth harbor a lingering sense of mistrust of authority figures, particularly those associated with schools or the justice system (Anderson, 1999). Therefore, it behooved upon me to disassociate myself from any connection to either of these institutions.

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following arrest as well as an evasive youth, Don, who had to leave with his father after an hour. Efforts to conduct a follow-up interview with Don were unsuccessful, so I spoke in person for an hour with his maternal grandmother instead.

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The first counter-image that I strove to project was that of a youth worker or a youth advocate. I explained that my interest in learning about the treatment of youth at the hands of school officials and the police and juvenile justice system was for the purpose of improving their treatment in these institutions. I emphasized that I wanted youth to share both what these institutions are doing right and what they are doing wrong. Adopting the persona of the youth worker, which also included driving a clunky car and dressing casually but respectfully, aimed to counter-balance the lengthy, legalistic consent forms which can easily trigger associations with the justice system.

An alternate persona—book author—serves as a foil to both the institutional authority image and the youth worker image in that it conveyed a sense of neutrality. I informed most youth that I will write a book about the experiences of young people like themselves and their interactions with institutions as they grew up. While most youth appeared to take this as a sign of my neutrality and their need to be honest, others may have felt pressure to make the story more interesting so it is featured more prominently in the book. I will discuss some examples of this latter problem further below.

Another strategy I used to solicit honest responses from youth was to display familiarity with their lives and those of others in similar situations. First, I informed each youth that I had access to his school and juvenile justice record. By demonstrating this access during the interview, I reminded them that I had a means to immediately "fact check" many of their claims. I often cross-checked facts during the interview. For instance, if a youth claimed that he attended school regularly during the semester after an arrest, I would examine the number of absences recorded for that semester in the school record and inform the youth, in a non-

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confrontational, non-accusatory manner, of any discrepancies between his account and the official record.

While this strategy likely minimizes outright prevarication, it introduces its own potential biases. Youth may experience such fact checking as invasive and associate me with other figures, including principals and probation officers, with the power to conjure up past "dirt" in their record. Resultant feelings of anger and embarrassment could lead youth to withdraw and withhold.

I used several strategies to minimize or neutralize these feelings. First, I concertedly attempted to treat all youth in an informal, respectful, and nonjudgmental manner at all times. Second, I sometimes prefaced questions that might elicit feelings of embarrassment with normative statements that conveyed the acceptability of negative responses. For instance, the question, "how did you feel about yourself after getting arrested" may imply a negative judgment. This negative judgment can be neutralized with a prefatory statement like, "some youth told me that being arrested was painful. How did you feel after your arrest?" The obvious hazard of such attempts to minimize social desirability bias is that they may bias the response in the opposite direction, leading to an exaggerated account of the extent of various hardships. The solution to this dilemma which I attempted to implement as much as possible is to present both socially desirable and undesirable responses as equally acceptable.

Of course I was not able to diffuse all feelings of suspicion and compel all youth to let their guard down completely. Two hours is often simply not enough time to establish trust. Youth varied considerably in their openness. Three youth gave mostly short abrupt responses that divulged little with respect to their feelings and vulnerabilities. Two of these youth, Robert

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and Adam, explained their reticence by pointing out that they had never been asked such questions before. By contrast, others, such as Shelton, Julian, David, and James, and three of the incarcerated participants—Gabriel, Hova, and Petie—indicated that they relished the opportunity to discuss their life stories and their feelings, even finding the experience to be therapeutic. As Simmel noted in a famous 1908 essay, (Simmel & Frisby, 1997) people are sometimes more willing to open up to a total stranger—whose disapproval of them would be of little import—than they are to their own families. Indeed Shelton's uncle, who was eavesdropping on our interview, subsequently reported to me that he learned many things about Shelton that he never knew. He also vouched for Shelton's sincerity.

Thus, with respect to eliciting honest and open answers, my status as a total stranger may have been an asset in some interviews but a liability in others. Some may have interpreted the interview not as an opportunity to vent and disclose but as a chance to project an image that represents how they want others to see them, rather than how they typically are. Furthermore, they may have seen one of the stated purposes of the interview—to collect stories for a book—as an invitation to bring their own fictional persona to life. While I did not suspect that any youth actually concocted an extended ruse for the purpose of the interview, I believe that youth varied along a continuum in the extent of bravado and grandiosity that they displayed. Those who displayed the most bravado may have been behaving in a characteristic manner as bravado is a means to maintain a fragile sense of self-worth and to deflect verbal or physical attacks from others (Anderson, 1990; Gilligan, 1987). At the extreme end of the continuum were two youth, Desperado and Joe, whose constant bragging about their criminal and sexual exploits and

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seeming invincibility left me wondering to what extent their accounts were tainted by a combination of delusions of grandeur and willful exaggeration.

Data Validation Strategies

Unfortunately, time and resource limitations precluded the use of two strategies for cross-validating the interview data. I was not able to conduct follow up interviews with each youth in which they could have affirmed prior statements and expounded on others. Nor did I systematically attempt to interview youths' parents or other family members. Rather, I conducted only a small number of follow-up phone calls with youth and had a small number of informal conversations with family members during the course of the interview visit or during telephone conversations. Thus, cross-validation strategies were scattershot at best.

Fortunately, I did have at my disposal a means of cross-validation that is not typically available to qualitative researchers. All surveyed youth participated in at least one self-report attitude and behavior survey as part of the Comer study. 13 of the 20 participants contributed three or more surveys, while seven filled out four surveys. This survey data combined with youths' school and juvenile justice records provided a helpful means to validate retrospective accounts of the elementary school years.

While I did not examine all the youths' surveys in depth, I selectively examined survey data when questions or doubts arose from the interview data. For instance, as a rough means to learn whether the bombastic or grandiose demeanor of Desperado and Joe was a means to conceal deep and abiding self-doubt (which may have been aggravated by justice system experiences), I examined how they responded to questions assessing their level of depression (including questions about feelings of worthlessness) and life satisfaction. This examination

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revealed that Desperado and Joe reported in elementary school the highest average levels of life satisfaction and the lowest levels of depression in the interview sample. This suggests that the youth were not feigning insolence and self-confidence for the purpose of the interview. Rather, the youth have maintained a high level of self-esteem for a long time, perhaps as a means to cope with a shortage of external validation. The survey responses of Joe and Desperado lent an air of legitimacy to the persona they displayed.

On the other hand, in other instances, survey responses did not accord with youth's recollections or my impressions. Such instances compelled me to scrutinize further both their narrative accounts and their survey responses. Generally, I was able to devise a logical explanation of observed discrepancies. When an explanation was not readily apparent, I reminded myself that day to day changes and exigencies can affect both the feelings and perceptions that drive survey responses as well the earnestness and sincerity that youth apply to the survey completion task. This is why social researchers generally limit their use of survey data to analyses of large groups rather than to individual respondents.

Analysis of Interview Data

The first step in analyzing the interview data was to transcribe the tape recorded interviews. Each of the interviews was fully transcribed, most of them professionally. Transcribers were instructed to change the names of any individuals identified by the youth in order to protect the confidentiality of the youth and the people to whom he referred.

The second step was to code the interviews. Given that a research assistant assisted in the coding process, it was necessary to devise a standardized coding scheme. However, since

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coding is an inductive process whereby some categories are derived from the data, only a limited number of pre-set categories were chosen based on my recollections of the interviews and the research literature. These included some basic descriptive characteristics and elements of the narrative like family structure, number of siblings, and the reasons youth gave for dropping out of school. I also coded for various potential mediating processes derived from the literature including labeling, defiance, and the various permutations of institutional exclusion (court visits, falling behind, school harassment, denied re-enrollment) and changes in social relationships (peers and family). In addition, I coded for known confounding explanations for the impact of juvenile justice involvement including prior problems with school, youth and family drug problems, and gang involvement.

As is typically the case in qualitative data analysis, with each new interview, sub-categories of the initial categories emerged and other classes of phenomena emerged that did not fit neatly into any of the extant categories, necessitating the creation of new categories. After coding a few cases on my own, I presented my research assistant with the expanded coding schema. I asked her to use the coding schema to both code the interviews already coded and to code the remainder of the interviews.

Coding consisted of a three stage process. First, the research assistant reviewed each transcript, manually coding sections of text with abbreviations corresponding to the coding schema. Second, the research assistant summarized the interview in spreadsheet form. The spreadsheet included an evolving list of categories and permitted comparisons across cases. I reviewed the manual coding for each interview and the spreadsheet, filling in gaps. In an interactive and iterative process the research assistant and I corrected errors and discussed new

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codes and modifications to old ones. Third, as patterns began to develop across the interviews, I developed separate documents corresponding to each of about 25 topics. Into these documents, I pasted excerpts from the interviews that were relevant to the topic and most illustrative in relation to a particular youth. I also included analytical commentary in which I contextualized the quotes and discussed emergent patterns. Thus, the coding process culminated in one document, the excel spreadsheet, that allows for comparison across cases, as well as a set of documents that permits the analysis of distinct topics.

While coding helps in the development of concepts and in the organization of ideas, it is merely the beginning of the analysis. The meat of my analysis consisted of deriving from the coded data the most salient mediating processes that explain why youth who are arrested often appear to suffer academically as a result. The first technique utilized to accomplish this goal was to construct from each interview an explanation for the changes that youth underwent. Next, beginning with the most commonly cited explanations, I examined each explanation's comparative prevalence among the declined cases and among the improved cases. The next step was to develop more refined interpretative theories which could address the particular conditions in which a social process is more or less likely to occur and to examine the factors that may condition the outcomes of a particular social process.

One vital tool in the refinement of causal theory is negative or deviant case analysis (Giordano, 1989). Negative or deviant case analysis is one of several methods subsumed under comparative methods and the logic of analytic induction (Ragin, 1987; 1994). The strength of this technique is that it capitalizes on the information provided by individual cases. Unlike conventional statistical analysis that focuses on inferences about a population, this technique

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purports only to make logical inferences and assumes that each case can make a valuable contribution toward that effort. For instance, imagine three cases A, B, C are compared with respect to outcome, Z which can assume the values of 0 or 1 and potential mediating processes X and Y. A and B exhibit an outcome value of 0 while C exhibits a value of 1. In practice, of course, more than three cases will be available, so A, B, C can be alternately thought of as representing separate subsets of cases or as individual cases.

Here is how the strategy can help one refine a causal explanation. A finding that mediating process X is present in both A and B may lead to the hypothesis that this process may lead outcome Z to take on a value of 0. Behavior rarely has necessary explanations and factors can be ruled out as necessary explanations fairly easily and decisively by finding cases that exhibit outcome 0 but not X. If X is a sufficient explanation of 0, by contrast, then all cases that exhibit 0 need not exhibit X but those cases who exhibit X should all exhibit 0. Case C is a deviant case if it exhibits mediating process X but also exhibits outcome 1. Deviant cases like C force a refinement of the causal theory as X alone is clearly not sufficient to explain the outcome. Further analysis may reveal that cases A and B share X and Y but that C only has X. A refined causal hypothesis is that X and Y are sufficient for outcome 0 to occur. X and Y can be said to form a causal configuration.

Unfortunately for researchers, causal reality is not nearly as neat and tidy as the example above. In practice, there would likely be cases sharing multiple combinations of several independent variables yet still exhibiting different outcomes. It may be the case that no causal configuration is uniquely found among all sampled cases exhibiting an outcome. Furthermore, the failure to sample and identify a deviant case does not mean that such cases do not exist.

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For these reasons, these techniques are most effective when larger samples are used. Indeed, Ragin has developed a Quantitative Comparative Analysis software program that identifies causal configurations uniquely associated with particular outcomes. This program is most useful when the sample is large and the number of potential mediating processes is small. Given that my study meets neither of these conditions, my analysis appropriates the logic of analytic induction but not the associated methodological apparatus. In other words, I use deviant case analysis as a heuristic device to bolster the validity of my interpretations. However, I do not attempt to develop a definitive set of causal configurations that produce variable outcomes. Rather, I concede that my sample is too small, and the pool of potentially relevant variables too large for this technique to be effective.

Before beginning to mine my twenty interviews for explanations of the impact of arrest on school performance, I must first seek confirmation that arrests, and not some alternative explanation, are responsible for observed changes in school performance. This task occupies the attention of the next chapter.

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CHAPTER 9: ARE ARRESTS AN INDEPENDENT VARIABLE?

The twenty interviews were structured to address two central questions. First, were the causal effects observed in the statistical analyses valid, and to what extent do they reflect the influence of other unmeasured factors and social processes that influence both arrest and school performance?[78] This leads to a second, and more central question: what are the intermediary processes underlying the causal impact of arrests on school performance?

Prior to answering the central question of the qualitative analysis, I devote a chapter to the first question. Assessing whether arrests caused changes in school performance is a lofty task by itself. A full understanding of the impact of juvenile justice involvement on school performance, and its importance relative to other determinants, requires an understanding of the complex array of forces that shape academic performance and educational attainment in the inner city. The sociology of education literature points to myriad mutually interdependent factors operating at the levels of the person, family, peer group, school, broader community and culture that influence youths' academic performance. Some of these forces may also result in arrests.

Adding to the complexity of this task is the fact that some of the same factors that may confound the relationship between arrest and school outcomes may also mediate the relationship between arrest and school outcomes. Quantitative researchers have attempted to distinguish between selection factors and mediators, in part, by examining their respective temporal relationships to the independent variable of interest. However, this task is much more difficult in

[78] The interviews can also speak to the validity of alleged moderating factors although this question was of secondary importance given the small sample and inconsistent statistical findings with respect to conditioning factors.

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the context of a retrospective interview. Time may cloud the distinction between the causes of juvenile justice involvement and its effects. With this in mind, I focus in this chapter on the factors and forces that are temporally proximate to the arrests and can produce changes in academic performance in the appropriate direction and with sufficient force to account for the statistical results reported in chapters five and six. While I do not claim to decisively disentangle temporal and causal order for most cases, I believe that an accurate and full portrayal of the system of institutional, interpersonal, and intrapsychic forces operating on my informants lends differential plausibility to various causal scenarios—only a minority of which place arrest at center stage.

More concretely, my overriding purpose in this chapter is to explore other factors besides arrest that could account for increases in dropping out, absences, and grade retention—that is, confounding explanations. Readers should bear in mind that a confounding social process and a mediating social process can look alike in all ways except that only the mediating process arrest is precipitated by the arrest . This chapter focuses on confounding processes, drawing mostly from interviews in which the social processes underlying school changes that occurred after arrests were not triggered by the arrests. In the subsequent two chapters, I draw mostly from interview data that illustrates how juvenile justice involvement can serve as a trigger for some of the same social processes. However, by focusing on explanations of changes in school performance this chapter is inevitably about both confounding as well as mediating explanation.

The Search for "Confounding Explanations"

My selection criteria were geared towards ensuring a high likelihood of sampled youth who experienced both negative and positive effects of arrest. However, despite my deliberate

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attempt to sample youth who actually experienced "the full impact" of arrests, only a small number of the narratives supplied by the youth clearly and decisively attribute changes in school performance to their arrests. Several youth in the declined group dropped out of school within a few months of being arrested while others experienced marked declines in test scores or increases in absences following arrests. Improvements in academic performance, on the other hand, were generally more modest in nature. Appendix C provides, along with a basic description of each case, a woefully abbreviated narrative of the educational changes that prompted classification and selection for the study.

Confounding explanations which were supposedly "controlled away" in the statistical analysis were abundant and untamable in the qualitative analysis. It was immediately clear in the interviews that the forces that give rise to involvement in the justice system are equally capable of producing sudden changes in school performance. Furthermore, while arrests and ensuing legal entanglements often did earn some salience in youths' accounts they were generally only one of a cluster of factors that hindered (and occasionally promoted) academic effort and motivation. Because arrests are a direct consequence of youths' social environments and/or behavior, youth understandably find it difficult to separate the effects of arrests from the morass of other forces associated with them. The role of legal sanctions can only be understood in the context of and in relation to these other forces. I now discuss some of these forces in brief detail.

Family

Most of the interview participants reported very stable family arrangements and positive relationships with their mothers or other close female caregivers like grandmothers or aunts.

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Most youth described their family life, both retrospectively and currently, in very positive terms despite a common experience of extended periods of father absenteeism.

In the case of Vince, James, Gabriel, LaShawna, Petie and Jay, however, family issues, rather than juvenile justice involvement, appear to lie at the root of their academic woes. Having spent much of their pre-teen years during the height of the crack epidemic, several youth or their significant others (Gabriel, James, Vince, Robert and Petie, for example), reported that their mothers (and sometimes fathers as well) had serious substance abuse problems.[79] Drug addiction in the home compounds problems of poverty and father absence and, in many cases, led to neglect of children's needs and behaviors. Vince was not alone in rationalizing his drug dealing as a means to compensate for his drug-addicted mother's parental failures. He claims, "[I was] making sure my family's straight. As long as my family's straight, I was all right."

Neglect by drug-addicted parents can influence school performance both indirectly and directly. Petie, whose mother "had to have her drugs before I had what I needed," wore cheap, used clothing which brought ridicule from his school mates and led to his participation in economic crime. Youth with drug-addicted parents also tended to report a lack of parental support for their studies and a lack of parental guidance and supervision. Petie explains, "…It wasn't really like no rules at home, me and my brother we made our own rules even though we had to obey our grandmother and the time she say come in or don't come in at all." Interestingly, Julius similarly lacked supervision due to his mother's repressive work schedule. Youth who lacked supervision, not surprisingly, devoted less time to school work and more time to deviant

[79] I suspect the same was true for the mothers of a few other respondents, although I did not ask them about it specifically. Joe, for instance, reported that his mother did not live with him because she "is having problems"

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pursuits. Some youth, faced with parents' drug problems and attendant problems like domestic violence, described regularly cutting school and getting into serious fights as early as second or third grade (Jay, Armand, David). These problems, in turn, often led to early trouble maker labels and banishment to special classrooms or schools more concerned with discipline than teaching.

While family problems were often severe, they generally developed gradually. At the same time, associated academic difficulties also drag on for years as youth are required to attend school through age 15. Thus, although family problems contributed to the development of emotional and behavioral problems, and consequent legal and academic difficulties, they typically cannot explain the observed abrupt changes in academic performance following arrest. The major exception is sudden changes in family circumstances such as the death of a loved one. Three youth, Shelton, LaShawn, and Jay, attributed declines in school performance that I had assumed were associated with their arrests to the deaths of loved ones. These youth reported that after their close relative died, they "didn't care about school anymore" and, except for LaShawn, became more involved in delinquency.[80]

Peers and Gangs

While families were generally of great importance in shaping youths' sense of identity and their motivation and ability to participate in both conventional and illicit behaviors, an even more proximate and salient determinant of both juvenile justice involvement and school performance in many accounts was the peer group. In order to understand how the peer group

[80] Vince stopped caring about school in sixth grade after he learned that the man he lived with was not his biological father. He also learned that his real biological father was a disreputable character who was killed when Vince was three years old. Thereafter, Vince sought to emulate his image of his father.

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can cause arrests and sudden changes in school performance, and hence serve as a confounding explanation of the impact of arrests, it is important to understand the uniquely pivotal role of the peer group in the inner-city.

Dominant views of peer group formation hold that peer groups form as a result of conscious decisions on the part of the youth and school actors (c.f. Padilla, 1992; Oakes, 1985). Most of the youth in my sample, by contrast, described their peer group less as a matter of individual choice and more as a matter of geographic destiny. Youth tended to hang around the youth that lived in their immediate vicinity. The exceptional cases, Julius, Damien, Desperado, and LaShawna, were insulated and distracted from neighborhood peer groups by extremely close-knit and protective families. Most youth however, particularly the nearly life-long residents of housing projects or stable neighborhoods, became tightly integrated into neighborhood peer networks.

The importance of geography makes sense in light of the prevalence of youth gangs in the neighborhoods from which my sample was drawn. Unfortunately, most of the high schools and some of the elementary schools the youth were mandated to attend drew from multiple neighborhoods, each dominated by a different gang. Gangs often vied for dominance within a school and such gang conflicts were often the most salient negative aspects of youth's recollections of their high school experiences.

> Tyrece: That place was terrible. It was like gang bang city in there. As soon as you get there, there are kids that will fuck with you. They throw pennies at the freshmen… They kicked me out of there.

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Under these conditions, membership or at least friendly affiliation with a gang was often safer than being a loner. These are lessons that two youth, Damien and James, learned the hard way. Damien, more effeminate and studious than the norm, was picked on incessantly by members of a youth gang in his school, while James was beaten up or chased by another housing project's gang nearly every day after school. Damien's response was to bring a gun to school in order to scare his chief tormentor into backing off. This action earned him an automatic expulsion from school and a one-month stay in the juvenile detention center, followed by transfer to an alternative school. Thus, peer conflicts and school exclusions were the principle sources of the academic trouble following Damien's arrest rather than the arrest itself.

Like Damien, James did not have the same level of protection afforded to official members of his neighborhood's gang or to his older brother who played on the basketball team. Despite these beatings he still attended school most days. His tendency to remain in school for the full class day actually left him even more vulnerable, because leaving school at unpredictable times is one form of protection against attacks. As high school progressed, and students from his neighborhood steadily left or were thrown out of school, James' thin layer of protection wore even thinner. Eventually, he decided that not even his education was worth being seriously injured or killed, so he stopped going to school. James's story, while not serving as a confounding explanation for his particular changes in school performance following arrest, (his arrest was in eighth grade and apparently had little influence on his academic trajectory) illustrates how gang-related events, such as a sudden rise in gang warfare or a drop below the threshold number of friendly, protective gang members enrolled in one's high school, can lead to a sudden change in "delinquency" and school performance.

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The reliance upon gangs and other peer groups for not merely companionship, respect, structure, guidance (Padilla, 1992) etc., but also for basic survival affords such peer groups extreme influence over youth's life choices. With respect to delinquency, arrest, and school performance, such close affiliations are a double-edged sword. While peer groups may provide the protection that help some youth survive the school experience, they can also foster anti-school attitudes and behavior. Youth who otherwise may have opted to attend school regularly and walk the straight and narrow find themselves compelled to participate in the activities of the peer group, whatever those may be.

Many of the youth, and disproportionately youth in the declined group, when asked to explain instances or patterns of slippage in their scores, grades, or attendance explained that they were hanging with the wrong crowd. Often, the "wrong crowd" were their friends since elementary school. A smaller number of youth gravitated towards delinquent peer groups upon changing neighborhoods or schools. In any case, the effects of deviant peer activities on their choices were pretty clear. For example, in the exchange below, Robert, describes his own decision to arrive at school late and leave school early to smoke "weed" as a group decision and as reflective of a larger peer culture.

> PH: Why did you leave school early again?
>
> Robert: 'Cause you hang, hang with my guys. We used to always leave. Play hookie. Lot of kids play that. Just don't wanna be in school, it'd be boring. Just go back outside.
>
> PH: So you always wanted to leave?
>
> Robert: Half the time—you just stay in there. Today, Fridays we leave and Mondays. Especially on Fridays. We always leave early. Go to a restaurant…smoke weed. That was that. …Bein' teenagers. That's what teenagers do.

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Robert, along with several of his friends, ultimately was dropped from the school for "fighting, absences, and disrupting classes." While Robert blames these behaviors, the official reason for his departure is listed in the school record as "Dropout - absences (more than 20 consecutive) over 16 years only." He learned of his "expulsion" one day from a list on the front door of the school. He describes even his exclusion from school in collective terms. "We knew we were going to get kicked out sooner or later," he noted nonchalantly, "We knew it was comin'."

Robert's account of his exit from school proximate to his arrest as a function of his peer group activities is not unique. Petie and Tyrece both provide uncannily similar accounts of their school exits, while Julius came close to a similar fate (see below). Tyrece reports, "I ended up just getting kicked out with the crowd, you know what I'm saying. Boot the whole crowd out."

Thus, serious truancy can be a response to individual detachment from school as well as normative pressures to skip school.[81] Youth who claim to be redirecting their lives in more pro-social directions seem more apt to acknowledge and renounce the authority of the adolescent peer groups. Shelton, an articulate young father readily bemoans the strong and destructive influence of his peers. Shelton was among the youth who pointed out that, his peer group affiliations occasionally led him to participate in activities that he did not want to partake in. For instance, codes of loyalty dictated that an attack on a member of the gang was an attack on the

[81] Youth like Robert reject the notion that their peers forced them to leave school against their will, because they have a genuine desire to leave school to begin with. The peer group simply facilitates this decision by providing validation of this desire and a social buffer against informal or formal sanctions. It is unclear the extent to which youths' retrospective assertions of personal agency reflect a desire to feel in control of their life trajectories and avoid confronting the truth that the youth were not free to decline invitations to leave school and engage in delinquency without suffering social sanctions.

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other members of the gang. Consequently, Shelton felt obligated to join in fights against other boys with whom he had no personal quarrel. Such fights generally not only make school a more unpleasant experience but they also carry an automatic suspension and frequently precipitate arrest. Group loyalties can even affect the school attendance of those who are not suspended, as those who are suspended sometimes pressure their friends to remain at home with them or to leave school early.

Peer groups associations not only lead to behavior that can get youth into trouble and earn them the label of a future trouble maker, but the associations themselves can also draw unwanted attention from school authorities. Consistent with labeling theory and the group hazard hypothesis (Erickson, 1973), school authorities sometimes impute to all individual members of a group the perceived characteristics of the group as a whole. Group labeling can result in youth being blamed or punished for acts that they did not do. Julius offers the best illustration of the influence of associative labeling as distinct from the influence of delinquency or arrests. Although Julius was a gang member, he reports that he was fairly well-behaved in school and was a favorite of his teachers. Nevertheless, he was subject to constant harassment from school authorities who were intent on ridding their school of trouble makers.

> Julius: So like since I was known to hang with this certain particular group, it's like they'll pull me. It happened continuously. They'll pull me in the office. And then they'll bring everybody down. They'll be like, "Yeah, you did it." And everybody admit it. …And they'll tell them I wasn't there. And that would just make – they'll just be like, "Oh, he wasn't there." Then I wouldn't get suspended or nothing. They'd just send me back to class or whatever. But since I hung with this particular group, I always ended up, you know, getting pulled in or whatever.

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Julius reported that he and his friends would be called into the Principal's office whenever something happened in the school. Though he was rarely formally punished, this process itself took its toll on his school work. As Julius' case will illustrate later, the juvenile justice system can play an important role in reinforcing this school labeling process. Thus, the effects of school-mediated "group hazards" are often bound up with the impact of arrests.

School Experiences

The foregoing discussions of family and peers illustrate how youths' close social relationships can affect their attention to their school work and their pursuits of illicit activities, which in turn can solicit reactions from the school in terms of grades and discipline. However, in many of the informant accounts, schools and their personnel did not merely assume a reactive role. Rather, the social and regulatory climate of schools along with the behavior of particular school personnel sometimes played an active, pivotal, and confounding role in explaining the academic performance changes following arrest that determined selection into this sample.

The stories that my sample provided about their school experiences are quite resonant with contemporary urban education research. The elementary and high schools that youth attended were beset by problems commonly associated with inner-city public schools in poor minority neighborhoods. Among the litany of complaints voiced by my sample are disorderly classrooms, daily fights, old and irrelevant textbooks, dilapidated equipment and facilities, and, especially in high school, authoritarian teachers, rigid school rules and routines, and school practices that demeaned individuality and youth culture, such as dress codes and school uniforms.

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While these factors all contributed to academic disengagement, most were, like emotional and family problems, more likely to cause a gradually increasing sense of frustration and boredom than to serve as a confounding explanation for the negative impact of arrests on school performance (except when arrest is preceded by a switch from a friendlier or more functional school environment to a less friendly and functional one). For this reason, I will not elaborate on youth's reactions to these conditions and how and why they may have varied across the schools they attended.

The aspects of the school experience that most merits further elaboration—because it can confound the impact of arrests—are youth's interactions with teachers and other school authorities. The harshest comments about the school experience were reserved for teachers and other school authorities, particularly those exhibiting apathy, meanness, or a tendency to "play favorites." A steady refrain from the interviews was that many or most of their teachers were "just there to get a paycheck." Tyrece, like many other students, spoke fondly of one or a few of his teachers but looked back on most with contempt.

> Tyrece: They didn't care if you learned or not. They'd be like, 'It's your choice whether you learn or not. You can talk, do, whatever. I'm still gonna get my paycheck.' They would actually say that….And that could mess a kid up. Kids think if the teacher don't care, then I'm not gonna care.

In addition to their pay checks, another numeric preoccupation for many teachers, especially in 8th grade, was the standardized test scores of their students. The pressure from teachers to pass these tests compounded youths' sense of failure when they did not pass. Five students in the sample—Joe, Don, James, Ton, and Petie—underwent the disappointment of

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learning that their reading scores precluded their timely promotion to high school. The stigmatization associated with grade retention can also make the separation from one's peer group even more difficult as it makes it harder to make new friends. Petie reports, "And then the next year the people who I was saying I'm going to holler at you all when I see you all in high school I was in their class and that was kind of embarrassing you know."

Finally, repeating the eighth grade curriculum fostered boredom and frustration. Don, already despondent over his mother's death, failed the eighth grade reading test, and was sent to a "transition school" to work on his reading skills. He had assumed that after he improved his scores he would be permitted to move on to a regular high school. He reports however, that he was not given enough opportunities to demonstrate his readiness for promotion and was not promoted once he was ready.[82] He recounts becoming so frustrated with the endless repetition of the transition center that he left school entirely without ever being promoted to high school. He places blame for his decision to drop out squarely on this school experience rather than on the three arrests he experienced during that school year.

Much research has demonstrated that feelings of boredom and frustration in school can lead students to act out in the classroom. School misconduct, in turn, can send students to the principal's office and/or out the front door of the school. Suspensions, which, by statute, frequently last a week or ten days in Chicago, can lead students to fall further behind in their classes. Perhaps even more damagingly, they may reinforce students' emergent beliefs that their presence in school is neither important for them nor desirable to school authorities. Because they

[82] Don's official records provide some support to his account. In the spring of his year at the transition school, he took the ninth grade standardized test even though he was not allowed to take regular high school courses.

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can lead to serious and sudden school declines, suspensions from school represent an important confounding factor that, unfortunately, was not available as a statistical control in the quantitative analyses.

Interestingly, in at least one case, detentions and in-school suspensions, widely considered a corrective to the perverse consequences of out-of-school suspensions had effects similar to those of out-of-school suspensions. Shelton laments the fact that detentions and in-school suspensions did not actually force him to stay in school. He claims that he almost always left detention early and he was never further penalized for doing so:

> Shelton: 'Cause once they send us to detention, we're gonna go to detention. We're gonna leave right up out the front door or either side doors. …What made it so bad, it was only one person, as far as security, one person in the detention hall, and there was a door that once you walk out that door you walk straight to the bus stop. And what made it so bad, that door was always unlocked.

Thus, focusing only on the student's behavior ignores the important role played by teachers and other school authorities—and the policies regulating their conduct—in perpetuating the cyclical relationships between school failure, deviance, and reaction to deviance. While zero tolerance disciplinary policies are a source of controversy nationwide, few jurisdictions have adopted as strict a regimen of penalties as Chicago. These policies permit suspensions and arrest for acts of ordinary adolescent deviance (redefined as "disorderly conduct" whenever it is convenient for law enforcement purposes). As a result, teachers in Chicago are given wide discretion over whom to suspend and for what acts. Teachers can also mandate parent-teacher (PT) conferences for nearly any act of deviance. These PT conferences sometimes are tantamount to a suspension because students are not allowed to return to school without an adult,

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and, particularly in unstable families, an adult may not be immediately available to accompany the students back to school.

While rigid disciplinary policies can endow teachers with greater control over their classrooms, they can also open the door to abuse. Teachers with personal grudges against students can use rigid disciplinary policies for minor offenses as tools to exclude youth from their classrooms or from their school. Several of the students described teacher behavior that, if true, can be viewed as a serious abuse of authority. Armand claims that his seventh grade teacher did not like him or was afraid of him because he had been in fights. Below are excerpts from the story of his exclusion from public school in seventh grade:

> Armand: Yeah, the teacher didn't like me that much. It was one day, my Grandmother was taking me to my classroom, 'cause I had came to school late. So then, the teacher was like don't be acting like a fool in here, the zoo you call your place home. …So I was like, aight. But I ain't staying today. So we had left. …So the next day, came back to school, my desk had real sticky stuff all over my desk, sticky stuff on my chair. So, the corner part of my desk didn't have nothing on it so I sat there, like lean. So the teacher was like, 'Armand, sit down!' Then I was like, I got all this sticky stuff on my desk. She just keep on yelling 'sit down!' I'm like man, I got all this sticky stuff on my desk man. And she like, 'Who ya talking to? I'm not your mother, I ain't your father.' Then I said something to her. And so she was like, 'That's it. Get the hell out of my classroom. Right now, go!' So after that I left. Then, um, she had said something that got me like kicked out of the public schools.
>
> PH: Did they expel you? Do you remember what they had expelled you for?
>
> Armand: For arguing with her.

Perhaps owing to his past fights, Armand was offered no slack for this incident and was transferred to an alternative school. A similar pattern of abuse appears to have been repeated there, as he was expelled from the alternative school for having a nail clipper.

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While Armand's story may seem at first glance a bit far-fetched, in relation to the other interviews, it is not completely unique. Rather, it stands out as an extreme permutation of a recurrent theme of abusive school authorities. Vince claims he was thrown out of one classroom because his teacher interpreted his nervous laughter after asking her a question as a mocking and threatening gesture. And in high school he reports that the assistant principal and the principal, after viewing him on video coming to school in a fur coat (he dealt drugs on the street, but claims he was focused and saintly in school), pulled and kept him out of his classrooms on virtually a daily basis for no reason and threatened to transfer him to another high school. Likewise, Kevin described a high school teacher who would engage him in petty battles of wills (e.g. staring contests) and sent him to the school counselor ten times for talking "smart" to her. He believes that this teacher was intent on preventing him for graduating and reports that he had to transfer out of her classroom in order to graduate. He claims:

> Kevin: I know she would have failed me, sure as I'm sitting here…. Because basically if you don't kiss up to her. It's like a job. The ones who kiss up to the boss, that who get the hours. That's who get the more pay. And the ones who kiss up to her, that's who get the better grades and that's who she like.

While Kevin's disciplinary problems never culminated in his dropping out (or being pushed out), those who are suspended frequently often do meet this fate. Students describe repeated detentions and suspensions as part of the process of accumulating a record. Students who accumulate enough strikes on their record may find themselves suddenly banished from school. I was not able to find any evidence of a "habitual offender statute" in the Chicago Public School system permitting exclusions on the basis of the number of students' prior disciplinary episodes. However, several interviewees attested that at least an informal policy to that effect

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exists in some of the schools. Tyrece, for example, left high school through this extralegal pathway. He asserts that no formal hearing was held regarding the school's desire to kick him out of school. "It was like, 'don't show your face no more or we're going to get you locked up,'" Tyrece alleges, adding, "He was like 'don't show your face among school grounds, I'm tired of you being in my office, I'm tired of giving you detention here and suspensions.'"

The above example alludes to another important reason why school experiences are an important confounding explanation of the effects of juvenile justice involvement: often times schools themselves initiate juvenile justice processing. As explained in Chapter 1, each public school in Chicago has sworn law enforcement officers stationed in the school. Not surprisingly, schools with more serious crime problems—like Tyrece's high school— have an even greater law enforcement and security presence. While arrests in school were not uncommon in the sample, they tended to be for minor offenses such as fighting and resulted in station-adjustments rather than formal charging.

However, when a drug or weapons offense is committed in the school an arrest is automatic, at least on paper.[83] Because the ensuing juvenile justice activities and sanctions are concomitant with school sanctions it is difficult to disentangle their separate impacts. Among the four youth who were arrested *and* charged for crimes they allegedly committed inside the school—Adam, Hova, Julius, and Damien—Adam clearly assigns more importance to school actors in shaping his subsequent school performance than to juvenile justice actors.[84] The other

[83] Kevin asserts that he witnessed leniency for some drug offenses, while Tyrece reports that gang members entering school with weapons were sometimes left alone.

[84] Youth who were arrested and subsequently thrown out of school for committing an offense in school (Hova, Julius, and Damien) were likely and unfortunately over-represented in the sample because I

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three youth describe a more complex interaction between the two sanctioning systems which will be elaborated in the next two chapters. As far as a proximal relationship between their arrest and a dramatic decline in school performance is concerned, these narratives provide collective substantiation of the social science adage: "correlation does not prove causation."

Adam's de-emphasis of the juvenile justice component of his school arrest reflects the fact that his arrest only led to participation in a brief, low intensity diversion program (drug classes). His arrest occurred when his favorite teacher found some cocaine on her desk twenty minutes after Adam went to her desk to ask her a question. Adam insists that he had never even seen cocaine before that day, but he was soon arrested and spent his two-week suspension in a residential drug treatment program in a hospital. Adam reports that despite the fact that his favorite teacher "seemed happy to see me" when he returned after his two- week stint in rehab, he harbored a grudge against her for the rest of school year. He reports, "I usually talk a lot and stuff but I stayed back … I just stay by myself for the rest of the year."

Conflicts with teachers and the resulting suspensions and/or arrests not only hindered performance and classroom participation but also frequently reinforced the very behaviors they were intended to reduce. The processes, described in chapter two, of defiance in the face of official sanctions were also seen with respect to school sanctions. Being suspended, particularly for petty offenses or as a result of false accusations, often leads to an acute sense of mistreatment and injustice and a desire to get back at the sanctioner (Sherman, 1993).

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deliberately selected youth who exhibited abrupt changes in school status or performance following their arrests.

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Desperado— the most charming but also the most brazenly criminal youth in the sample—was the most vocal example of such vengeful desires and tactics, even when his suspension was perfectly legitimate according to the rules of the school. He recounts that another youth's annoying harassment in class prompted him to initiate a fistfight—behavior that earned him an automatic suspension. His violent response to the provocation would probably be considered appropriate according to the norms of the dominant peer culture in the school and in his community (Anderson, 1987, 1999; Canada, 1995). Accordingly, he describes, in a manner that completely belies his charm, how his suspension was far from a moral corrective and, in fact, made a bad situation worse.

> DESPERADO: They suspended me for like two weeks. And that shit didn't mean nothing to me.
>
> PH: When you got suspended, was it hard to get back into the school?
>
> DESPERADO: No. Once my suspension was over, I'd just come back to school. I'd be madder than heaven; you know what I'm saying? Every time I'd get suspended, I'd come back with this rage, like 'what the fuck you suspend me for, bitch?' Shit, I would just be sitting there like, 'hell, now I *really* ain't doing shit.' And I'm waiting on them to say something, and I'm going to snap the fuck out of this mother fucker. I'd just come back, man, I'd come back to school as mad as hell. Yeah, shit, got my mom's fucking shit because I've got to stay home with her. You know, I come to school and now I got to deal with you mother fucker. 'Now I really don't like your ass; you know what I'm saying? You done got me in trouble and shit.'

Measuring the Prevalence of Confounding Explanations

The primary aim of this chapter is to address the question of whether the observed changes in educational performance were largely the work of the juvenile justice system or the result of confounding influences. Thus far my explorations of possible confounding

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explanations apply mainly to serious negative school outcomes such as premature school departure. Information on negative educational outcomes is provided not only by youth in the "declined" subgroup but also by some of the youth classified as "improved" or "mixed." Thus, in addition to the experiences of the eleven youth in the declined group, I examined also the premature school exit of Petie, a member of the improved group who dropped out shortly after his arrest at the beginning of his sophomore year, as well as the school failure and exits of Adam, Vince, and Shelton—all youth with "mixed" patterns of school performance following sanctions. In this way I maximize the number of observed declines and, correspondingly, the number of possible explanations.

At the risk of oversimplifying complex interrelationships, I shall attempt a grounded assessment of whether juvenile justice involvement played a palpable role in the declines of these fifteen youth or whether alternative explanations could possibly overwhelm any impact of legal involvement. For this purpose, I accepted youth denials of the effects of arrests at face value. On the other hand, I viewed participant's claims of a negative impact of arrest with greater scrutiny.[85] This inconsistent prevention of only false positives on the justice effects indicator leads to a conservative estimate of the prevalence of sanction effects. On the other, hand, declines were labeled as influenced by the justice system even if the effects of the justice system were only minor relative to other factors shaping school performance or if the same youth experienced academic declines following later arrests that were due to other causes.

[85] For instance, I classified Gabriel's declines as triggered exclusively by causes outside of the justice system—despite Gabriel's indications to the contrary—because his arrests were preceded by a long prior history of serious emotional and behavior problems, including gang involvement, which led to school failure and to his being shuttled from school to school. Put another way, Gabriel was in such serious trouble at home and at school that his arrests could scarcely have worsened his school situation.

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According to this balanced definition of a school decline influenced by the justice system involvement, the juvenile justice system played a minor or major role in eight of the fifteen youth's academic declines following arrest. For the remainder of the cases the role of the justice system was negligible or too difficult to assess. In the next two chapters, I will elucidate precisely why, how, and to what extent juvenile justice involvement affected the youth's academic performance.

Is juvenile justice involvement at least partially responsible for the observed academic improvements following arrests as well? The preceding discussion, consistent with my charge of explaining the statistically observed adverse impact of arrests on school performance, focuses mostly on the causes of declines rather than on the causes of improvements. This focus also reflects the fact that many of the observed improvements were disappointingly trivial, of ambiguous origins, or most likely not due to juvenile justice involvement and its aftermath.

Readers are again invited to refer to Appendix C for over-simplified explanations of focal academic changes following arrest. This Appendix belies the ambiguity and complexity of many of the explanations. For instance, Vince's case offers a whole pattern of school performance improvements following arrests that lack a clear explanation. His first arrest was two weeks prior to the start of sixth grade and was for residential burglary. He was placed on probation for that offense, and according to juvenile court records, he received probation until February of seventh grade. In spring of his sixth grade year his reading score improved an impressive 23 points from 13 to 36. His math achievement score in both fifth and sixth grade was the lowest possible. In the fall of seventh grade Vince was arrested for theft in November and mob action during Christmas vacation. The first was dismissed at a court hearing at end of March while the

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other was never even petitioned to court. Then in May of seventh grade, in spite of his two active court cases, his math score somehow surged a remarkable 92 points while his reading score held steady. Finally, he was arrested once in eighth grade just prior to the most formative moment of elementary school—the eighth grade standardized tests. On this test he earned the worst possible score on both the reading and the math.

Explaining inconsistent patterns of results such as these proved quite difficult. Youth, especially former avid marijuana users like Vince, understandably often have a hard time remembering why they performed particularly well or poorly on tests taken several years earlier. It is noteworthy that among three rounds of testing after arrest, Vince performed best in seventh grade after his juvenile court cases were closed and worst after a highly proximate arrest. However, since Vince gives his arrests no weight in the explanation of his school performance during these grades, I cannot reject the null hypothesis. That is, perhaps the attention-starved Vince just happened to be in a rare good mood when he took the seventh grade test. Armand and Kevin also exhibited small improvements in test scores following very minor elementary school arrests that both they and I were at a loss to explain.

Because test performance is subject to the day-to-day caprice of inner-city life as well as random variation it is a poor gauge of annual school performance changes for single individuals. (They function much better at an aggregate level). Absences, which are available beginning in ninth grade are a better indicator of sustained trends in school performance. Unfortunately, the quality of the explanation of changes in absences suffers when students' recollections of specific semesters is poor. For these reasons, I put more weight on more memorable indicators of school failure and success such as dropping out of school. Unfortunately, the only positive counterpart

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to dropping out of school is graduation and this is rare in the sample and the sampling pool. It is also not quite comparable, given that students can drop out of school after any arrest, but can only graduate after arrests in their senior year.

Despite, these impediments to adequate explanation of improvements in school performance, a couple of confounding explanations of scholastic improvements were evident and deserve brief mention. These explanations function as corollaries to the aforementioned explanations for declines. First, switching to a better school can affect school performance in a positive way. Even some of the most misbehaving students reported that certain teacher and school environments were more likely to bring out the best in them while others brought out their worst. For instance, Shelton described how moving to a new school led him to work harder in order to acclimate to the styles of his new teachers and the rules of his new school. In such cases, tangible indicators of improvement may lag behind unobserved increases in school engagement.

Another source of improvement in school that has nothing to do with arrest is a strong desire to escape a particular school context. This desire is acutely felt among students forced to spend another year in eighth grade after failing the eighth grade reading test the first time around. Accordingly the student who reported the strongest sense of shame in the face of his perceived disapproval from his family and his peers, Petie, also exhibited the largest test score gains on his second try. His math and reading scores improved 7 and 31 points respectively during the same school year in which he was arrested three times.[86] As I will discuss in the next two chapters,

[86] Among the whole interview sampling pool, the average math and reading gain for those arrested while in their second 8th grade is only 3 or 4 points.

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juvenile justice involvement and its reverberations throughout Petie's life, may deserve a good deal of the credit for Petie's improvement as well.

Because of the methodological difficulties described above, I cannot estimate as reliably the share of improvements that are explained fully or partially by justice involvement as I can for academic declines. The eight veritable instances of improvement following arrest that drew my initial attention appeared to be of two types. Improvements of the first type, experienced by three youth, lacked a clear explanation, because the improvement was minor and the youth was not able to remember the improvement, let alone the circumstances that may have contributed to it. The remaining five were, according to my best interpretation of the interview data, probably linked to juvenile justice involvement. This includes Petie's improvement described above. Hence, further discussion of improvement in school performance will be deferred to the next two chapters in which youth describe the various social processes that appear to mediate the impact of arrests on school performance.

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CHAPTER 10: UNDERSTANDING SANCTION EFFECTS: SOCIAL PSYCHOLOGICAL PROCESSES

The last chapter's discussion of youths' interactions with their families, peers, and school authorities converge upon a single point that applies to my staggered replication sample as well as my interview sample: at the time of their arrests, each sampled youth was firmly embedded in a complex social system. This system is constantly shifting in response to a youth's life course-related changes, ecological and structural forces, as well as exogenous shocks like a death in the family or a change of neighborhood. Shifts in the dynamics of the social system, sometimes dramatically and abruptly, may impede youths' motivation and ability to perform well in school. The social systems described by most of the youth would have probably lead most of them to eventually drop out or be locked out of school even if they never came into contact with the juvenile justice system.

None of the above should come as a surprise to scholars interested in inner-city youth and the sources of negative school outcomes. The preceding chapter's discussion of youths' families, peers, and school experiences confirms and extends many widely documented findings in the literature. However, it is also an essential prelude to the core and novel findings of this research. The novelty of my research is that it examines the effects of arrest—a partially exogenous shock—on the social systems that I described in the previous chapter. Through an understanding of this influence, we can learn how juvenile justice involvement contributes to the dismal educational prospects for delinquent inner-city youth.

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In this chapter, I direct my sociological spotlight towards the micro-level processes that ensue from involvement in the juvenile justice system. Arrests were theorized to affect the perceptions, expectations, and behaviors of students in relation to the school environment. On the other hand, juvenile justice involvement affects students' lives outside of school as well. School performance may suffer even in the absence of behavioral changes in school and institutional exclusionary practices or even in the presence of institutional attempts to prevent such failure.

This chapter discusses two social-psychological consequences of sanctions and their implications for school behavior. Specifically, this chapter discusses how juvenile justice experiences bear on youths' self-perceptions and attitudes toward authority. I situate the bulk of my findings within labeling and defiance theory. With the aid of deviant cases and cases drawn mostly but not exclusively from the "improved group," I provide a tentative assessment of the value of each theory for interpreting the effects of arrests on school performance.

The Effect of Arrest on Self-Image

As explained in chapter two, labeling theory suggests that arrest and legal sanctions can confer upon students the label of a troublemaker or deviant and lead others to treat the student as such. The adoption of a reputation and identity in accordance with these labels may be incompatible with the role of an "engaged, disciplined student" (Bowditch, 1993). The desire to maintain consistency between one's prevailing self-image and one's behaviors—what Maruna (2001) may call a cohesive self-narrative— as well as a reputation concordant with these self-perceptions may lead youth to behave in accordance with the deviant role instead of a more pro-social sense of self.

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While researchers have demonstrated that labels bestowed by justice system involvement like “convict” and “thug” can be a powerful force in shaping personal identities, behaviors, and life chances (Maruna, 2001), it is also clear that several conditions must usually be met in order for the justice system to bestow deviant labels on students that stick and sting over time. First, the label must be seen by the offender and members of his social system as beneath his moral character. If the official label is not seen as negative or the labeled youth is already “damaged goods”, the official labeling process is not likely to take hold in a firm way. Second, members of the social system should alter their behavior toward the students in a manner that indicates that they have imputed the negative label upon the student. If few actors in the educational institution even learn of a student’s arrest, he may escape stigmatization in that domain. Third, the student should, on a conscious or unconscious level, understand and internalize the label and act in accordance with it (Matsueda, 1992). The secondary deviance or school failure that results, in turn, serves to validate the label. Labeling does not have to affect both student and school perceptions and behavior in order to influence school performance. However, labeling processes are more influential if it affects both, given that the two sets of perceptions and behaviors are mutually reinforcing.

Are Arrests Stigmatized or Normalized?

The first requisite condition of labeling, that an arrest contributes negative defining information to a youth’s reputation is, according to my sample, violated with surprising frequency. Of course, the youth are the not the best source of information on how others perceive them following their arrest. However, given that, according to labeling theory the

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influence of external perceptions is mediated by youths' self-serving interpretations of those perceptions (Morash, 1983), my focus on the youths' perceptions is a forgivable offense.

The first reason why arrests reportedly often fail to sully youths' reputations is that arrests sometimes carry little stigma. As noted earlier, arrests are virtually a normative experience of adolescent male development in the context of impoverished urban schools and neighborhoods. To illustrate, among the males who attended Comer study schools for at least three years from 1992 through 1996,—the longitudinal sample of Cook, Hunt and Murphy (2001)—35% were arrested at least once during the elementary school years or high school years.[87] Equally strikingly, at least 23% of the male Comer sample spent time in the Cook county juvenile detention center or jail during this period. Further normalizing the experience of arrest and incarceration in Chicago is the routine use of arrests in the school disciplinary process (see chapter 1), a practice whose widespread use was verified by my interview respondents.

In light of these contextual circumstances, the common claim from my interviewees that being arrested was "no big thing" or "nothing" should not be viewed merely as an expression of defiance (Sherman, 1993) or as an attempt to "neutralize" their sense of shame or guilt (Sykes & Matza, 1957). Rather, routine arrests—that is, for the most common charges like marijuana possession, theft, and battery, are truly afforded little importance in these communities. Arrest is clearly not an event reserved only for hardcore offenders; it is the presumptive response for a

[87] This staggering rate is actually an underestimate of the rates in the population given that these youth on average experience more stability in their neighborhood and family environments than the youth who were not in this sample and because only about 80% of arrests and no station adjustments are measured beginning in February, 1997.

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wide variety of crimes and a wide variety of youth. Even 12 percent of the girls in the Comer longitudinal sample were arrested at least once.

In addition, not only are arrests for petty crimes quite common, but, according to my interviewees and other youth I met in the course of this research, false arrests are quite common as well. Fully half of my sample (ten youth) reported that police actually put drugs on them or otherwise framed them on at least one occasion. Without exception, the remainder of the sample (and the handful of relatives with whom I spoke) confirmed that fabricated arrests are standard or common police practice. Their estimates of the share of drug arrests—which comprise the majority of all arrests for my interview sample—that are "bogus" averaged fifty percent.

A high proportion of petty and bogus arrests has serious implications for the labeling process. If a large share of arrests and other punishments are for petty or non-existent crimes, then arrests, in themselves, become an unreliable gauge of student character. Before condemning a youth based on their arrest, those lending credibility to the petty and bogus arrest phenomena need more information on the specific circumstances surrounding the arrest. This information is generally not readily or widely available. Robert, who claims he was sent to prison after police arrested him for buying marijuana but decided instead to falsely charge him with drug selling, attests to the diminished status of arrests as a negative label:

> Robert: Gotta obey [the law]. But you don't gotta break no laws to go to jail. They will take you to jail for anything. They'll put drugs on you. Anything. They could put a gun on you.

Regardless of whether youth claims of being set up by the police are credible—and I have no reason to believe my sample is independently yet collectively ignorant or deceptive—the

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widespread belief in the regularity of this practice can function to erode the power and legitimacy of formal labels.

A second reason why the precondition of arrests leading to a negative reputation was frequently violated is that negative labels attached to a youth prior to their arrests are often so negative that arrests may convey redundant information. Prime examples of the "pre-packaged" are youth like Gabriel and Jay who at a very young age were officially labeled by their schools as emotionally and behaviorally disordered. Such labels often follow a pattern of chronic violence and disruption on the part of the student and result in transfers to other schools or segregation of the students into "special" classrooms. Such labels and students' consequent clustering with other similarly labeled students, not surprisingly, often serve to legitimate and perpetuate their oppositional and destructive behavior (Cohen, 1955; Padilla, 1992). The following quote from Gabriel was provided in response to a question about how arrests in general affected the way teachers perceived him. The manner in which his response quickly shifts the subject to his *school* arrests and then to his behaviors in school illustrates how the school behavior and sanctions that precede an arrest may far outweigh the arrest itself in conferring a negative label upon a student.

> GABRIEL: If you being arrested in school its going to affect you a lot because a lot of people are going to see who you are, what you do, how you go about yourself. So if they see I'm a person that gang bangs in front of the school, comes to school high, cuts school, gets into fights, beats up kids, I'm not going to be looked at like a normal student.

Thus, other students and teachers as well as Gabriel himself did not really need the police to inform them that Gabriel was a deviant. In fact, with respect to chronic miscreants like Gabriel,

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the students and teachers are more likely to be relaying this information to the police than vice versa.

Do Arrests Result in Social Rejection?

Not surprisingly, since juvenile justice involvement is not automatically viewed as "deviant", the second pre-condition, negative valuations and social rejection from members of one's social circle, is also often unmet. While most youth reported that their families were disappointed with them after legitimate arrests and often subjected them to increased punishment and control following their arrests, only two youth—Petie and Gabriel—reported facing overt labeling or rejection from their families following their arrests.[88] This finding has profound implications for the social embeddedness interpretation as well (see next chapter), which holds that arrests impinge on a youth's support network. Generally, families reportedly remained a vital source of social support, control, and guidance, even after youths' arrests. Lectures from parents did help some youth confront the consequences of their behavior and think more seriously about their futures—processes which can bear on youth's self-image in positive or negative ways—but these processes are better situated within reintegrative shaming or deterrence theory rather than labeling theory (see below).

Modally observed peer group reactions also do not fall neatly within the parameters of labeling theory. Given that arrests are so common, it is quite likely that many of the peers who sit in judgment of a youth have themselves been arrested. Furthermore, chances are that youth who have not been arrested and who are not well-acquainted with the arrested youth are not

[88] Both Gabriel and Petie were disparaged following their crimes and arrests by their grandfathers—a harsh response likely borne out of the frustration or resentment for having to take care of their unruly grandchildren while their daughters battled drug addiction.

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aware of the circumstances of a youth's arrest given that inner-city youth tend to be tight-lipped about their personal business, especially in large, unstable, impersonal settings like inner-city high schools. Hence, pursuant to my earlier arguments, peers often lack a compelling moral or factual basis to impute a negative valuation to an arrested youth. Desperado, who was quite marginally involved in his high school, is an extreme exemplification of the practice of secrecy and a possible illustration of the imperfect flow of information about arrests.

> PH: So did it affect how other kids perceived you when you got arrested?
>
> DESPERADO: Ain't too many mother fuckers know about me. Ain't too many that could tell you nothing about me. I'm a mystery.

Logic suggests that the youth who would most likely keep their distance from an arrested peer are pro-social peers who *are* well-acquainted with the youth and the circumstances of a serious arrest. However, the interviews uncovered no instance of this either. Apparently, arrests typically carry too little credibility and importance within the wider scheme of things to come between a youth and his friends. Tyrece suggests that pro-social peers may even make constructive use of a youth's arrest.

> PH: So did you find that once you got arrested that it was harder to be friends with the kids on a positive tip? Or that it didn't affect who your friends were?
>
> TYRECE: No, it didn't affect them at all, because they looked at me like I can learn from his mistakes, you know what I'm saying. 'I can know what not to do…stay on my right road,' you know what I'm saying? That's how they probably looked at me. They still – cause I did do the stuff I was doing, going to jail, and I still had the friends who was there in the first, beginning, being positive, still was there for me.

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Tyrece's account of peer reactions also runs counter to social embeddedness theory. The next chapter, however, will describe other, institutionally-mediated ways in which sanctions may influence social relationships with peers and teachers apart from labeling.

Because the primary interests of inner-city public school teachers and other school officials include the maintenance of student engagement, achievement and discipline in classroom and school, school agents arguably have a greater incentive than peers to identify and weed out delinquents. However, teachers, especially high school teachers, whose relationships with students are less personal, are also less privy to information about youth's arrests outside of school. They are likely to learn of an arrest only if it results in an extended absence from school or if they are contacted by a probation or parole officer requesting information on a youth's attendance (also apparently less likely to occur in high school). For these reasons, youth were often unsure of the share of their teachers who actually knew about their arrests.

Students vary in their reports of how teachers responded after learning they or other students had been arrested or in detention. Some interviewees believe that teachers share the belief that in their communities arrest by itself is not worthy of condemnation and ostracism. Robert likens school absence due to incarceration to being out sick. "It's the same thing," he insists. Jay suggests that teachers respond to arrests with concern.

> PH: When you got arrested, and the school knew about it, did it change anything for you at the school?
>
> JAY: Not really. It's just I got a lot of lectures from all the teachers. It didn't really change anything, because that wasn't a good neighborhood. I wasn't the only one sitting there getting arrested and stuff. It was a lot of other people getting arrested too; they had to just talk to the teachers.

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In the same fashion as youth's parents, teachers, like Jay's, often did reportedly alter their behavior toward students after their arrest in an apparent attempt to prevent future arrests. Whether such teacher treatment unintentionally confers negative labels depends on the circumstances. Braithwaite (1989) draws a distinction between reactions to deviance that label and stigmatize youth and those that induce feelings of shame as a means to foster, both a desire on the part of the deviant to reintegrate into society, and society's acceptance of their reintegration. Braithwaite refers to the latter type of response as reintegrative shaming. Responses to deviance that display disapproval of the act, while emphasizing an offender's essential goodness or normalcy and his or her importance to the community will help ensure that "a deviant identity...does not become a master status trait that overwhelms other identities" (Braithwaite and Mugford, 1994, p. 142). On the other hand, if Jay received a lecture in which a teacher repeatedly called him a thug, the lecture would likely fail to promote reintegration.

Teacher responses that appear to facilitate reintegration will be discussed more fully later in the next chapter's section on social embeddedness. Of relevance at this juncture are examples in which apparent attempts at reintegration may have inadvertently promoted negative labeling. For instance, several students (Tyrece, Petie, Shelton, Jay) described how teachers provided extra help to them when they returned to school after spending weeks in juvenile detention. Without extra assistance many students in my sample would stand little chance of passing. Indeed, most youth in the declined group did not receive the required assistance. At the same time, such special treatment violates inner-city high school norms, of impersonal or even icy relationships between teachers and students. Special treatment may also draw extra attention to students and their plight as "ex-offenders". Students are well aware that the reason they are being treated

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differently from other students is that they have been in trouble with the law. As a result, a youth's status as a law breaker may become a more salient part of his identity. Note, however, that Shelton, Petie, and Jay all experienced an improvement in school performance during their period of "special attention."

The discussion of labeling theory thus far has focused on the impact of arrests that do not, by themselves, indicate that one is deviant. However, many arrests are indicators that one has run afoul of the norms of low-income communities and is deserving of social rebuke. As Shaw and McKay (1929) emphasized long ago and others (c.f. Anderson, 1999) more recently, conventional values predominate even in the poorest neighborhoods. To the extent that arrests call attention to and substantiate one's participation in acts that violate the norms of poor neighborhoods, they are indeed often looked upon unfavorably, prompting negative reactions within the school and the larger community.

Certain conditions appear to make juvenile arrests more censurable or stigmatizing within the school context. First, arrests before 8th grade are less common and, consequently, more deviant. Second, arrests that occur at school call immediate attention on the part of school officials and peers to a youth's status as a law breaker. A student's memory of being paraded through the hallway in handcuffs is not likely to fade fast, especially in schools or grades in which such arrests are infrequent events. Gabriel vividly and bemusedly recalls his ritual "perp walks" through his elementary school.

> GABRIEL: The police would come to the school, guide me out the principal's office, cuff me up. I'm cuffed up looking out at the students smiling, laughing. You know, thinking it's funny, you know.
>
> PH: The students are?

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GABRIEL: Yeah. Because I got to walk down a hallway, then down the stairs, and then another hallway, then finally out the building.

As mentioned earlier, Gabriel needed little help from the police in establishing his reputation as a deviant. He received earlier feedback to that effect from his family and his teachers. However, memories of interactions with teachers, family, and peers can gradually fade over time or be refashioned into constructive or congenial events. Arrests, on the other hand, are more difficult, though far from impossible, to assimilate into a positive self-narrative because they yield vivid memories and are endowed with nearly universal significance as symbols of criminality.

Acceptance and Resistance of Negative Labels

While the first two preconditions for a "labeling" effect to occur following arrest—a negative meaning endowed to the arrest and confirmatory treatment from significant others—did sometimes occur, their occurrence did not ensure a change in self-image. While negative appraisals from others exert a powerful influence over the development of adolescent self-image (Matsueda, 1992), especially at younger ages, positive self-conceptions can be quite resilient. Various psychologists and criminologists have discovered a variety of techniques that humans use to protect the "self" from negative valuation and associated feelings of guilt following socially unacceptable behavior. Bandura (1979) describes various means through which "self-evaluative consequences can be disassociated from censurable behavior". These techniques, which mirror Sykes and Matza's (1957) techniques of neutralization, include minimizing or denying the act or the consequences of the act and blaming others, including the victim, the

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sanctioners, or one's co-offenders. In a more recent elaboration of such techniques, Maruna (2001) describes how offenders protect the self by depersonalizing their behavior—referring to their behavior as something that happened to them, as an "it", as an act of many or a "we," or as an act of the generalized other or the "you."

For my purposes, explicating the particular techniques that youth employed to avoid accepting negative labels is not as important as assessing whether or not their self-protective behaviors effectively shielded them from self-valuations that could hurt their school performance. Despite the "objective facts" about their academic and juvenile justice records, some students maintained healthy delusions about their capacities (Maruna, 2001) and inflated expectations about their futures. Joe appeared to be one of the most resistant to negative labels resulting from his legal entanglements or other failures.

> PH: Did anybody treat you differently after [your first arrest]?
>
> JOE: Naw, I ain't never get treated different.
>
> PH: Anybody ever call you names or anything?
>
> JOE: Everywhere I went, I held it down.
>
> PH: Held it down? What does that mean?
>
> JOE: I get my respect.
>
> PH: So you never had anybody call you a gang-banger or try to put you down, call you a criminal?
>
> JOE: Naw, 'Cause don't nobody know I'm a criminal or whatever, I'm a gang-banger or nothin' 'cause around, well certain people know, but like my elders—I respect the people. So I wouldn't put myself out there for them to say something like that.

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> PH: Did getting involved with the system affect you in any way? In terms of how you felt about yourself?
>
> Joe: Naw. nope….I still feel that I could be whatever I... like when you'se a shorty, you always be like you wanna be this, you wanna be that. When I was little, I still think I still do what I wanted to do. I still could do that. I still think that I'm perfect. Can't nobody tell me nothin'. Can't nobody make me believe nothin' different.

Joe apparently feels his positive contributions to his family and his community are outweigh any negative evaluations from others. He dwells on his positive qualities rather than his negative ones and reframes his failures as successes (e.g. he bragged about rising through the ranks of the gang hierarchy while he was in jail). Perhaps, relative to many of his peers, Joe's ratio of positive to negative output is quite high. To other observers (Maruna, 2001) such a positive outlook toward the self, even if a "cognitive distortion," can be behaviorally adaptive because it disassociates the core self from negative behaviors or a negative identity allowing for a high self-esteem even in the face of highly cogent disconfirming evidence.

Other observers, however, like Gilligan (1987), who studied serious violent offenders, may interpret Joe's claims of unassailable confidence as attempts to conceal deep-seated feelings of shame. Unfortunately, since offenders who harbor feelings of deep shame often try to conceal them at any cost to themselves or others (Gilligan, 1987), it is impossible for me to ascertain the actual affect of negative labels on self-esteem for such offenders.

Of course, not all offenders are violent offenders and not all violent behaviors—such as collective violence—take root in individual shame. Some of my interviewees readily admitted or evidenced feelings of shame in response to their arrests. These feelings did not typically appear to be strongly associated with the crime or its victim (although most of their crimes had

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no real victim). Rather, the feelings of shame were associated with the effects of one's arrest on loved ones and one's own self-conceptions. Many interviewees conveyed a deep sense of regret for the feelings of disappointment, embarrassment, and worry that their arrests and incarceration engendered in their loved ones. Youth were sometimes forced to ponder, "How could *I* do something like this to my mother." Consistent with much research on informal social sanctions and shaming theory (Sherman and Smith, 1992; Hirschi, 1969; Braithwaite, 1989), those in my sample with strong social bonds to familial positive role models appeared to experience the most acute sense of shame following arrests.

Youth may face an even greater challenge to their self-concept if their first-time or petty offense leads to their being lumped together with youth with far bleaker prospects. In such situations, of course, youth could employ a number of defenses to challenge this association (e.g. illegitimacy of sanction, blaming momentary lapses in judgment). However, situational factors can sometimes overwhelm these defenses. For instance, incarceration largely disconnects a youth from his activities, family, romantic partners and friends – all of which usually serve to affirm a youth's identity. In short, incarcerated youth are denied easy access to information that could readily counteract the negative label and its concordant treatment by correctional authorities. At the same time, such youth are compelled to associate with youth who manifest different values. Research on total institutions (Goffman, 1968; Haney & Zimbardo, 1998) like prisons has demonstrated the tremendous and rapid flexibility of individual identities and subjectivities in response to complete contextual transformation. Youth who experience periods of incarceration that number in years, rather than weeks or months, accordingly experience more

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dramatic and enduring changes in self-image than those who experience the more typical pattern of short, intermittent periods of confinement.

How can youth preserve their pro-social identities when cut off from external affirmation of this identity? The short answer is that, in addition to maintaining external ties as best they can, they also seek affirmation of their pro-social identities in prison. Three youth in my sample demonstrate how the detention center failed to challenge their pro-social self-conceptions and may even have strengthened it. Both Julius and Damien had a clean arrest record at the time of their focal arrests (although Julius was an active gang member). However, their offenses were serious enough to warrant extended stays in juvenile detention. For reasons described earlier, being placed in detention was a challenge to their self-esteem. Both Damien and Julius coped with this challenge first by finding cues (e.g. verbal and reading skills) that set them apart from the other youth in the Center. This process was partly reaffirmed through playing roles that placed greater social distance between them and the typical confined youth. For Julius and Damien, this role was that of tutor or mentor to the other youth in the center. Julius describes in detail how he developed this role and how it started him on his path toward reform.

> Julius: Now, in the detention center, I mean, it's like the weirdest things, well to me the weirdest things affect you. Because there were people actually there, you know what I'm saying, that didn't know enough to learn like they were supposed to or whatever. To the point where we used to be in the detention center rapping and doing little basic stuff. And they asked me to spell simple words. And I be like, "Well, you know how to gang bang, but you don't know how to this and that. And you don't know how to read and write." And I was like, I mean, it kind of give me a little wake up call, because it's like well, okay, at least I was able to get this far. And you all wasn't even – didn't even have a chance to learn how to read. This is all – all you all know is gangs and whatever. …so everybody would talk about fashion, who had these new shoes on or whatever. But it kind of gave me a wake up call, because it was like it kind of let me know that I was lucky to be brought up in an environment where I had a mom to care enough to make sure I learned how to read, write, and do math, and all that stuff. Because even though

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> the work was so easy to me, for lots of people in there it was kind of a challenge, because all they knew was crime. So, I mean, at the time of the moment, it's like I kind of felt good that I was helping, because like when I went in there it was like a lot of people was asking me questions on how to do this or how this is and how that is, because I had knew. And like mostly everybody who was on my deck, they wasn't – they was just not as educated. So they used to look at me like, well. So it's like I didn't make friends through being in the gang, because I was the opposition in there. So it's like I made friends for what I knew….And that's how I became cool with a lot of people. And it's like it was a nice experience. It was not being in jail. But it was, I mean, to know I could help people and help people understand and help people with problems or whatever, that was pretty cool. Because the guards in there, they don't really care. They're just there to watch you basically. So if you be like, "Well, how you do this?" they're going to be like, "Get away from me."

Julius reports that his desire to distance himself from "real criminals" in the detention center continued after his return home. He also felt the desire to redeem his image in the eyes of his mother. Accordingly, he asserts that he stopped hanging with his gang and spent his evenings under the watchful eye of his mother.

Julius also reports, and his school record confirms, that his attendance rate doubled in the semester after his return from the detention center (see next chapter's discussion of probation officers for further explanation). It is important to note that Julius and Damien may have been able to assume a mentoring role at the detention center only because they possessed skills and knowledge that were of value to the other youth at the detention center. Most youth confined at the detention center do not manifest the same level of intellectual functioning and cultural capital, so these experiences may be highly atypical.

To illustrate the moderating importance of intellectual and cultural capital, the third youth, Adam, was also reportedly able to maintain social distance from youth in his youth prison due to his close ties to his family. He also received all A's on his report card while he was

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incarcerated, making the honor roll each semester. This experience motivated him to re-enroll in high school immediately after his release from prison and to work hard initially to catch up. He reports, "Them A's made me feel smarter, you know." This feeling did not last long, however, as his academic work, considered superior in prison, was barely satisfactory by the standards of his public high school, and his high school friends, to whom Adam was inseparably tied, provided no reinforcement for his new interest in school.

To summarize, my interviews mitigate against labeling theory as an important mediator of the negative impact of juvenile justice involvement on school performance. By the time of their first arrest, many of the youth, in both the "improved" and "declined" groups, were already conditioned to view arrest as a normal part of adolescence in their communities, and many of the potential dispensers of negative labels are conditioned to take arrests lightly as well, except in the rare cases when detailed information is available on grave circumstances surrounding an arrest. Furthermore, most youth possess the ability to tune out, filter out, reframe, minimize, or otherwise "neutralize" feedback that challenges their self-image. The labeling process, therefore, is likely to impact school performance only when it acts on school actors' evaluative and disciplinary behavior directly, when youth actively seek out negative labels or when youth are immersed in a total institution. I found no clear cases of negative labeling by teachers or other school officials increasing in response to an out-of-school arrest and then leading to declines in school performance.[89] Among my sample the labeling process actually bears more on the

[89] Vince claims that harassment from teachers increased following his arrest but his account suggests that it was severe enough prior to his arrest to have resulted in the same exclusionary fate. Other youth described their teachers as more watchful or stern following their arrests but these reactions, assuming

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accounts of youth in the improved group as, given that such youth are more sensitive to negative labels and are also more motivated to deflect negative labels by working even harder and behaving better with the aid of supportive families and teachers.

The Effect of Arrest on Attitudes toward Authorities

The next social-psychological interpretation I considered is that youth who judge their treatment at the hands of the justice system as unfair or excessively harsh may develop antagonistic attitudes toward justice authorities. Negative experiences with the police, judges etc. may lead youth to regard all authorities with suspicion and resentment including school authorities. Antagonistic relations with school authorities, in turn, frequently culminate in school exclusion or failure (Myers et al, 1987; Jordan, Lara, and McPartland, 1996)

Like labeling theory, defiance theory finds only partial support in my qualitative analysis. Consistent with defiance theory, many youth reported that their arrests and ensuing interactions with the justice system left them angry and disillusioned. My respondents provided enough anecdotes of perceived disrespect, abuse and unfairness at the hands of the justice system to easily fill a book. Although every stage of justice processing provided instances of reported abuse, the arrest stage was the most fruitful. Drug arrests, the most common class of arrests, often resulted from police harassment and improper search and seizures. Moreover, allegedly, police frequently fabricated and distorted narratives, witness statements, and even evidence in order to secure a conviction. Public defenders and judges were described as equally unmoved by youths' claims of innocence or improper arrests and equally unwilling to challenge injustice.

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they involve the attachment of negative labels, never figured prominently into any accounts of post-arrest school failure.

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Opportunities for abuse and injustice continue and sometimes increase following conviction or incarceration. Youth often encounter guards at the detention centers, prisons, and jails whom they characterize as verbally and physically brutal and exceedingly inflexible in their enforcement of rules. Inflexibility is the most frequent complaint about probation and parole officers as well, although reviews of these authorities were, relatively speaking, quite mixed.

As youth proceed through the stages of the justice system and accrue more arrests, they also accumulate more experiences of abuse. In addition, the accumulated experiences of their friends, families, neighbors, and ethnic group often served to confirm and compound their own mounting hostility towards the justice system. However, the experience of abuse or injustice does not automatically trigger a process of defiance and resistance. Official actions are judged against not only legal parameters but also against individual and normative expectations of official conduct. As the frequency and severity of unfairness and abuse in a community increases, the community's expectations of fair treatment may diminish. This principle helps explain why the youth I interviewed sometimes spoke nonchalantly about law enforcement and judicial practices that would shock or outrage your average white middle class person.

> DESPERADO: Yeah, people who – mother fuckers just standing there. Oh, you standing here, huh? Let's go. Put your ass in that car, take you for a ride. Mother fuckers don't come back. You sitting up there, where'd my dog go? He got in the car with the police. He ain't came back yet? No. Mother fuckers don't come back from that shit.

While all respondents learned at a very early age to diminish their expectations of privacy and courtesy in their dealings with the criminal justice system, youth vary in their expectations of and in their emotional and behavioral reactions towards criminal justice authorities. The same official actions can prompt variable responses.

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Thus, even more important than learning the particular facts about official misconduct and inequity, is learning how youth felt about their arrests and whether these feelings affected their attitudes and behaviors in the educational realm. Nearly every youth I interviewed recounted that their opinions of the criminal justice system were sour prior to their first arrest and grew worse following their arrests or other street encounters with the police. First-hand experiences with the police were pivotal in shaping their attitudes, because youth trusted their own perceptions of the police more than the embellished "war stories" of their peers or fellow inmates as well as the countervailing positive images from the mass media.

In contrast to interactions with the police, youth interactions with public defenders, judges, and probation and parole officers were highly predictable, even mechanical. Consequently they were less often reported as sources of youths' hostility toward the justice system. The most common exceptions are the correctional, probation, and parole officers who scrutinized youths' behaviors too closely and punished them for minor misconduct. For instance, Petie recounts his anger after he learned that his probation was revoked for his failure to report to his probation officer that his phone had been disconnected.

> Petie: I call her, I'm like 'what's happening, why do you have me on a detain list?' She's like, 'the only reason why I was going to detain you was because your phone got cut -- your phone was disconnected and you didn't let me know.' I'm like, 'oh *that's* it?', I'm like 'okay all right.' I'm like 'so did you all violate me yet?' She's like 'yeah you've been caught -- you got caught in boot camp with controlled substance,' or something like that. I'm like 'okay, all right thank you.' Hung up on her.

The questionable treatment that youth experience from the police on the street is often gentle compared to what youth encounter in correctional facilities, where justice agents operate largely outside of public scrutiny. While some staff or guards are described as "cool" and as

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valued providers of edible or smokable contraband, an overwhelming number of comments were condemnatory. The following excerpts from Tyrece's lengthy denunciation of juvenile detention center staff are illustrative of respondents' typical attitudes toward correctional authorities:

> TYRECE: Plenty of people getting their heads rammed through glass doors. You know the glass doors thick as hell, they about this big.
>
> PH: They're banging the kids' heads through glass, the guards? …Why would they do that?
>
> TYRECE: Well, at the [Detention Center] you had certain rules, you know what I'm saying. You can't talk in the TV room…you've got to ask permission to use the bathroom, you know what I'm saying. You're told… "get in [your room], you stand under the clock," like a little ass kid. All type of bullshit, man. Some people weren't having it, you know what I'm saying. Some people like, well who the fuck is you talking to, you know what I'm saying. And they're not trying to hear what the staff talking about…They're going to do their time and go on home and ain't nobody be telling them what they got to do while they're here. …But some staff was looking at it like, shit, you on my time, you going to do what I say to do or I'm going to kick your ass, basically. ….I seen a lot of staff just riding on people for no reason, fucking with people for no reason. Having a friendly conversation and you come and make the mother fucker stand up and get under the clock for no reason.
>
> PH: Just for talking.
>
> TYRECE: Just for talking, right, you know what I'm saying. That shit, that's why a lot of people was doing shit they had no business doing, because the staff wouldn't treat them any kind of way, so they now repaying the person back, you know what I'm saying. Fuck up for the whole unit, make the whole unit get locked down or some shit like that.

Incarceration not only subjects youth to further abusive treatment, but it also gives them time to reflect on their experience and puts them into contact with more seasoned veterans of the criminal justice system. These reflections and interactions with prison "elders" can help youth integrate their memories and thoughts of the justice system into an articulated opposition to the justice system as a whole. This critical perspective, in turn, can channel their burgeoning

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resentment more directly at criminal justice authorities. One incarcerated youth, Gabriel, who claimed to be suffering from regular harassment from a prison guard at the time of our meeting, expressed a coherent defiant orientation that his older prison "mentors" likely helped him refine in prison.

> PH: These arrests…did they affect your life in any way?
>
> GABRIEL: The only way they affected me life is, I could say that, I mean it scared me in a way. Every time I, I sat there and I'm like, man. These police talking shit or whatever. They think they're hard core. They're, they're just regular people just like me or whatever. The only thing they made me think was I hate police. You know, I hate cops. I hate them because the way they treat people. They got that little, little code there that We Serve and Protect and all that shit. But it ain't about nothing. That's, that's just their little, their little way of getting away with a bunch of bullshit. You know what I'm saying?
>
> PH: It's a bummer.
>
> GABRIEL: So yeah. So that's why, that's when they used to harass me and talk shit to me, the only thing it really changed in me is what I see in them; what I thought they were. When I was a little kid, I, 'man, I want to be a cop. I want to be a lawyer.' You know, 'I want to this. I want to do that.' And once I see how they is, that ain't, man, that ain't… they're fucking criminals themselves.
>
> PH: And the lawyers too or it's the cops?
>
> GABRIEL: No. Lawyers… well there's some crooked ass lawyer because my PD… you know what PD's are? They say they're public defenders. They're not public defenders, they're called, I call them penitentiary delivery because that's what they do. Deliver your ass to the penitentiary. And then I didn't even know the fact that when they deliver us to the penitentiary, they get paid thirty-something thousand a year for hiding us in that penitentiary….

Gabriel's perspective flows naturally to a repudiation of the profit motive of the criminal justice system—a critique not unlike that offered by many sociologists and critical criminologists. Of course, relative to Gabriel, most of my sample had far more limited exposure

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to justice system agents and, consequently, less refined and cohesive views on criminal justice authorities. Still, most had at least one direct negative encounter with the criminal justice system.

School Spillover Effects?

Do such encounters and resultant negative attitudes affect dispositions toward school authorities, particularly among youth experiencing pronounced declines in school performance following their juvenile justice experiences? As mentioned earlier, some theorists of adolescent attitude and personality development have asserted that youth form global opinions toward authorities based on their assessments of particular types of authority. Research has attempted to confirm this in part by demonstrating that attitudes toward teachers and police are two dimensions of a single construct—attitudes toward authority (Nevitte & Kanji, 1999).

I cannot offer an authoritative assessment of whether youth interactions with criminal justice authorities shaped their attitudes toward their teachers, and vice versa, since I was not able to witness the development of these attitudes. Rather, I must rely on youth's retrospective accounts of their feelings and actions before and after their arrests. The majority of the youth in both camps reported that negative interactions with justice system agents were not, by themselves, sufficient to alter their dispositions toward authorities in general and school authorities in particular. Two youth, Armand and Robert, both alleged victims of false arrests, responded in the same reflexive and somewhat defensive manner to a question about whether they still respected their teachers following their negative encounters with the justice system. Both stated, "I respect my elders."

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This terse statement is quite revealing. It suggests that these youth naturally associate teachers with the older family members, neighbors, ministers etc. that collectively comprise one's "elders." This association is not surprising given that teachers are often the first adults outside of one's family with whom most youth come into regular contact. Many low-income youth, particularly those who are starved for attention, develop very close attachments to their teachers. Furthermore, it was not uncommon for youths to reports that their own parents attended their school and had some of the same teachers. This form of generational closure reinforces the social bonds that develop between teachers and students.

Thus for some students, my question about whether negative interactions with police affected their attitudes toward their teachers would be as strange as asking whether these negative interactions affected their attitudes toward their parents. Teachers are often viewed as part of a youth's social circle or at least part of their community. Police, to a larger degree, are viewed by youth as outsiders to the community, as agents of the dominant society (Anderson, 1999).

However, in Chicago Public Schools the line between school authorities and criminal justice authorities has grown increasingly blurry. Many teachers, through summoning the school's police officers at the first sign of trouble, provide entrée into the juvenile justice system. Other teacher behaviors may also invoke a less direct association with criminal justice practices and authorities. For instance, teachers' rigid enforcement of the school's zero tolerance disciplinary codes may remind students of their strict probation officers. Likewise, teachers' harassment and deprecation of students, arbitrary exercise of disciplinary authority, or leveling of false accusations, may trigger associations with the police on one's block. Such associations

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may become more transparent and automatic as teachers are observed associating with actual probation officers and police officers during the course of their duties.

Thus, it seems reasonable to suspect that the experience of mistreatment and unfairness in the criminal justice system can adjust the lens with which youth view school disciplinary behavior. The pivotal question is whether youth who have endured mistreatment from the justice system are more likely to perceive mistreatment at school and to respond more adversely to perceived mistreatment than youth who have not suffered abuse by the justice system. One could argue that youth who have never been involved in the justice system should actually be more disturbed by excessively harsh or unfair treatment from teachers, since they have less exposure to authorities who behave this manner. Indeed the interview participant who fixated the most on his experiences of teacher mistreatment, Kevin, had the least amount of involvement with the justice system.[90]

Unfortunately, my interview data is ill-equipped to address whether mistreatment by the justice system prompts differential associations in response to teacher mistreatment. To begin with, it is difficult to devise an adequate definition of "mistreatment." One could focus on the number of negative experiences or even on the ratio of negative experiences to positive experiences. Given that negative events are more influential than positive events (Tyler & Huo, 2002), and that the most negative events are the least subject to diverse interpretation, I choose to focus on the experiences of egregious misconduct or excessive punishment. I classify my sample according to whether or not a youth experienced acute mistreatment at the hands of the

[90] LaShawn, the student who denied being arrested, also devoted considerable critical attention to incompetent or corrupt teachers.

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justice system. I define acute mistreatment as the alleged experience of either arrest or incarceration (through probation or parole revocation) for something one did not do, probation or parole revocation for something extremely minor, or police brutality (being hit hard enough to leave a wound). While subjective interpretations of the legitimacy of even these behaviors varied slightly in my sample, they clearly violate normative standards of fair and humane treatment in their communities and would seem to be prime candidates for evoking a defiant response.

I find that four of the eight youth in the improved group experienced acute mistreatment (as defined). Three of these claim frame-ups and the other youth, Adam, claims a false accusation from a teacher leading to an arrest, police brutality, as well as "false" accusation in an assault that led to armed robbery charges and two years of imprisonment. James, who experienced "no change" also had no acute mistreatment. By contrast, 7 of the 11 youth in the "declined" group purportedly endured one or more episodes of this form mistreatment. Six of these youth claimed that police planted drugs on them. Of these six, three of them reportedly were beaten by the police as well. The other youth, Jay, was reportedly beaten by the police on multiple occasions and had his probation revoked after tensions with his mother caused him to move out of her apartment.

This rough comparison provides preliminary support for the notion that serious mistreatment is associated with declines in school performance following arrest. The next logical question is whether those who experienced acute mistreatment, were more likely to report changes in their perceptions of and reactions towards school authorities following their arrests, and whether these changes preceded declines in school performance. It is clear that the youth

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who were framed, not surprisingly harbor strong resentment for the police and also tend to report negative attitudes towards their teachers. It is unclear, however, whether the experience of wrongful arrests exacerbated youth's attitudes towards their teachers.

Only three youth in the entire sample acknowledged or drew any connection between their indignant feelings towards the justice system and their demeanor towards their teachers. These connections, however, were not very persuasive and may have been of little consequence for their school performance via the mechanism of strained relationships with school authority figures. Gabriel, who was never framed but who encountered police disrespect at each arrest, believes that his negative experiences with the police led him to view his teachers in a different light.

> PH: When you started hating the police, did it affect how you acted toward anyone else? Teachers, parents…
>
> GABRIEL: No but it, it opened my eyes to how somebody can… how can I say it? Portray an image that they're not in. You know what I'm saying? That they're not like, act like okay. Well I'm a good teacher. I'm this, this, and that but on the side they're really ain't no good teacher. You know what I'm saying? Once they pull the student to the side they can say you're going to fucking do this or I'm. You know what I'm saying? Well I'm going to fail you or whatever. .. So that's why I look at people not just, oh what they're trying to be or what they want to be. It's like I look at them for really what, what's there.

The second youth Jay, in contrast to Gabriel, did not resent the police for beating him three times and does not believe that the police practice of putting drugs on kids to effect arrests is unfair. Rather, he views these behaviors as the "normal" and just consequence of one's own behavioral choices. With respect to police frame-ups he states, "So if they put something on me then they got their man anyway. They got me for what I was doing just when I wasn't doing it

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then." Without a hint of resentment or even resignation he declares, "The game is cold, but it's fair."

On the other hand, Jay sees little fairness or justice in his treatment during his three years of incarceration. He could find no rationale to justify some of the stringent rules governing inmate conduct and the swift and severe punishments for violating the rules. His aversion to the iron fisted nature of the authority exercised in prison grew so strong that he claims it tainted his views on all authority figures, even his mother.

> PH: Did [getting involved with the system] affect your relationships with authority figures?
>
> Jay: Aw yeah, a lot. I can't stand for nobody to tell me what to do. Now they can ask me to do it, like, but like, 'You better go in there and do that right now,' I can't... I can't stand authority figures.
>
> PH: Do you think the experience of being arrested, the experience with police...
>
> Jay: Yeah, that had a lot to do with it, because they was controlling me, telling me what to do. Like, 'You fittin' to go here now.' When I was in the Audi home, 'You fittin' to sit in this TV room all day and you can't talk.' Stuff like that. 'You talk and I'm putting you in the hole for 40 minutes, 45 minutes. And then it's bathroom time; you gotta raise your hand to ask to go to the bathroom. That's why I can't even deal with authority figures anymore.
>
> PH: So, when you went back to school, did it affect how you treated the teachers?
>
> Jay: Yeah, it affected all authority figures.
>
> PH: Even the parents?
>
> Jay: Yeah, my mother, that's why I couldn't live with her 'cause she told me what to do and I wouldn't do it. I'd leave; I'd go somewhere.

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Tyrece, whose comments (excerpted above) about his resentment of jail staff resembled his friend Jay's, also believes that the experience tainted his view of teachers. He says, "Because you kind of look at it like, if one of them are like that, all of them are like that." Gabriel, Jay, and Tyrece each apparently experienced increased difficulty assimilating into the school environment as they become older and increasingly embedded in a deviant lifestyle. However, I have ample reason to doubt that their problems with school authorities and ultimate failure in school can be traced to their experience of criminal justice maltreatment. Each of these youth had a history of serious problems following rules that predated their involvement with the justice system by several years. Jay and Gabriel were both put in segregated classrooms because of their behavioral problems, while Tyrece reports that he had a temper and was "not too good of a person at that point in time when all that happened."

Thus, while it's possible that justice system maltreatment made their relationships with school authorities even worse, it seems even more plausible that the foregoing claims from Jay and Gabriel represent the human tendency to blame current problems on contemporaneous factors, rather than on distal developmental factors.

While I find little evidence that defiance of criminal justice authorities spills over into defiance of school authorities, I do find ample evidence that defiance affects school performance indirectly through increasing delinquency. Defiance processes seem even more salient in youth explanations of deviance amplification than labeling or criminal embeddedness. Defiance theorists (Sherman, 1993) and procedural justice theorists (Tyler, 1990) trace deviance amplification to displaced shame and eroded legitimacy of the law and legal authorities. My informants' accounts of the defiance process broaden this motivational framework. My

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respondents highlighted two complementary motives for increasing delinquency in response to official misconduct and abuse. The first is the reduction of strain. The experience of being falsely accused and arrested tends to arouse intense feelings of frustration (Agnew, 1992). These feelings can become so severe that one way to avoid them is to actually commit more crime (see Padilla, 1992). In that way, a more equitable balance will be reached between the acts one commits and the punishment one receives. And one's next arrest will not arouse unbearable feelings of strain. Rational choice/deterrence theorists may explain this behavioral response to false arrest not in terms of strain reduction but in terms of the changing calculus of criminal behavior. A false arrest may lead actors to view the costs of offending and non-offending as similar (i.e. "damned if I do and damned if I don't"). The following quote which refers to a youth's arrest and subsequent suspension for breaking up a fight in which he had no strong personal stake can be seen as illustrative of both perspectives.

> Shelton: right, [the police] reported it. Ya know, I got suspended for breaking up the fight, which, in fact, if I had known I was gonna get suspended for breaking up a fight, I'd a threw the punch and made it real.

The restoration of a semblance of balance and equity underlies the second motive for deviance amplification—revenge. Committing more crime is a way of getting back at the police who mistreated them. Gabriel explicates this motive as well as its ramifications for his school performance.

> PH: Did these arrests affect you at all in school?
>
> GABRIEL: In a way. Yeah. Because for the fact that getting me arrested made me hate the cops and made me want to do what they don't like. And, and as I did what they

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> didn't like, I was doing bad things and them bad things were like gang banging, selling drugs. And as I did those, I started cutting school.

An indignant response to sanctions may affect school performance not merely through increasing fatalism, anger, and delinquency but also by affecting one's timely release from prison and one's compliance with institutional rules and the terms of home confinement, probation, or parole. Release from prison on parole (which is dependent on one's good behavior) and successful completion of home confinement, probation, or parole all require fairly stringent compliance with an inconvenient and sometimes demeaning set of rules. An offender's incentives to obey them are greater if the punishment is viewed a process of restoration and as the payment of a debt to society (Braithwaite, 1989; Maruna, 2001). Offenders who view their punishment as wrongful or excessive may come to feel that they have no legitimate debt to repay and will unlikely see an intrinsic motive in complying with these rules.

While no offenders actually verbalized this idea, it can be offered as a compelling, though partial, explanation for some failures to comply. Adam was among the most committed to school of all my interviewees. Yet, his decision to skip a few classes violated his parole and resulted in his return to prison for a year. Tyrece, compared to Adam, was less committed to both school and probation. He reports returning to jail six times on his most recent adult case for failing the mandatory drug tests as part of his probation. What these two disparate youth hold in common is arrests that were, by their accounts, "bogus". Other alleged victims of false arrests like Joe, Desperado, and Armand, were equally lax in their compliance with their probation. However, their probation was not revoked because of the hands-off nature of their probation

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officers and other dynamics of their probation officer/client relationships. I will discuss the role of these institutional agents in greater detail in the next chapter.

In summary, I find little evidence that rebellious orientations originating in the criminal justice system are imported into schools. Those who draw this connection had serious problems respecting school authorities to begin with. Defiant reactions to sanctions do appear to worsen school performance, however, to the extent that involvement in further delinquency leads to disengagement and exclusion from school. Thus, my qualitative analyses do lend defiance theory, unlike labeling theory, some importance as an explanation of declines in school performance following arrests. However, these processes, unlike those discussed in the next chapter, do not directly implicate school institutional actors and practices.

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CHAPTER 11: UNDERSTANDING SANCTION EFFECTS: SOCIOLOGICAL PROCESSES

The last chapter explored the social psychological consequences of arrest in relation to schooling. I focused in particular on how arrests influence self-image as well as attitudes and behavior towards authority. I find little evidence that the processes of labeling and defiance associated with criminal justice sanctions spill over into the school, much less that they follow a pattern suggesting an institutionalized response.

The next theory I consider, social embeddedness, focuses more directly on institutional roles and reactions than labeling and defiance theories. Recognizing that social institutions such as families, schools, and neighborhoods allocate access to social networks, social embeddedness theory calls attention to how involvement in one institution, say juvenile detention, may influence participation in other social networks. Negative labels and ostracism may change one's status within or access to the benefits of a social network, but so may forced, temporary isolation from the network due to incarceration or home confinement.

While social embeddedness theory focuses on any social network with relevance to schooling and any social processes following sanctions that may condition participation in these networks, institutional exclusion theory focuses mostly on one institution (schools) and one type of institutionalized response (exclusion). For this reason, institutional exclusion theory is treated separately. This chapter brings my interview data to bear on both of these theories of the relationship between juvenile justice involvement and school performance.

The Effect of Arrests on Social Networks

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To reiterate, social embeddedness theory holds that certain sanctions, particularly incarceration, can disrupt the social networks that sustain youths' participation in school. At the same time, the experience of arrest and incarceration can solidify or intensify one's participation in criminal networks, which devalue school participation.

Some of my findings with respect to embeddedness are the logical counterpart to my findings with respect to labeling. Consistent with the modal pattern that only under rare specified conditions did teachers, families, and peers alter their treatment of youth following arrests, youth also typically reported that their social relationships did not change, quantitatively or qualitatively, as a result of juvenile justice involvement. Peer bonds can be extremely strong, sometimes even taking the form of surrogate family relationships (c.f. Padilla, 1992; Anderson, 1999). Furthermore, the friends of juvenile arrestees, as well as their family members, often experience arrests themselves, occasionally at the same time. It follows that most arrests do not change at least the affective dimension of these relationships. Families are typically described as unwavering in their support of youth except when a youth brought their deviant behavior and social conflicts home with them (e.g. Gabriel)—an eventuality that most youth took great pains to avoid.

The obvious circumstance in which social relationships should change is prolonged incarceration, since confined youth are uprooted from their social context and forced to navigate a new social climate. Among my sample, only four youth spent an extended period (two or more consecutive months) in detention, jail, or prison prior to re-enrollment in school, and they varied

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in their assessments of the extent to which it disrupted or severed their social ties.[91] Of course, maintaining one's peer relationships is generally challenging while incarcerated. Incarceration, a time of great social need, is also viewed by some as a chance to sort one's true friends from the fake ones. Incarceration gives one's friends the opportunity to demonstrate their loyalty and devotion or, conversely, reveal their falsity or "phoniness." Petie illustrates this dynamic in his depiction of how the failure of his peers to deliver on expectations of financial support during his incarceration strained peer relationships.

> Petie: When a nigger get locked up it's like all his buddies think he gone for life or something, you know what I'm saying? It's like they forgot about him, and that's bad. Because I know the first time I got locked up for that seven months, my buddies only sent me some money on the books one time. And that was ugly because when I got out, I wanted to do something; you know what I'm saying? But I didn't get out on that, I went to boot camp. But like I said, I kind of got my man together, I'm like leave them niggers alone, them niggers ain't nothing.
>
> PH: So you wanted to get back at them for not helping you out?
>
> Petie: Yeah, and I suppose to have a little authority over the block, you know what I'm saying? And they going to treat me like that, send me something one time like I ain't nothing. They supposed to send me something every week.

Not surprisingly, visits from one's peers are also quite rare. This appears to hold not only for those locked away in remote prisons but also for young inmates in the Cook Country jail and detention centers. Tyrece reports that during his first stint at the local juvenile detention center, which lasted six and a half months, he didn't receive any visitors, because the friends and family that wanted to visit him weren't allowed to do so due to stipulations that those under 21 must be

[91] Several other youth spent lengthy periods incarcerated following their exit from mainstream education. This chapter will give secondary consideration to the impact of these experiences on social networks as well, despite the fact that these impacts bear little relevance to their school outcomes.

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accompanied by an adult; his grandmother was too sick to escort them (his mother was murdered).

Does incarceration introduce youth to a new set of criminal friends and contacts that are sustained upon release? My small number of interviewees consistently responded to this question in the negative. While the dynamics of social relationships behind bars are too complex to receive adequate treatment in this text, two key features of these relationships emerged from the interviews. First, pairings and groupings in prison, except for the special trust that one may share with one's cell mate ("cellie"), are largely instrumental in nature. According to my interviewees, although one may associate with a particular group and maintain cordial relations for protection, one does not typically let these friendships get too close, lest one's vulnerabilities be exposed and exploited. Second, youth from certain neighborhoods and gangs do not need to make new friends in juvenile detention or the Cook County Jail because many of their friends are incarcerated at the same time. Robert asserts that, even in his downstate prison, he was able to associate with inmates from his North Lawndale neighborhood. Third, given that friendships are largely neighborhood-based and many youth maintain life-long friendships from their neighborhood networks (especially those that have not moved since childhood), bonds formed in prison, which draw from diverse neighborhoods and cities, lack any institutional sustenance outside of prison. Adam, who spent two high school years in the same youth prison and several months in Cook County Jail cites his neighborhood ties as the reason why he never let his prison contacts compete with his home-based networks. And through his disassociation, Adam, like Julius and Damien, tried to actively avoid socialization into a deviant subculture.

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P.H.: Did you have any problems with the other kids [in the youth prison]?

Adam: I didn't say much to nobody…I spoke with them, play ball and stuff, but I ain't trying to make any friends. Just play ball and we have to eat together and all that…they were used to that type of stuff when I got in there…it was like my first time ever going down…some people said they had been down there three times…it was like the first for me… …Cause I know I wasn't going to be down there for life…stress myself out for the whole year.

P.H.: …Why would you not want to make friends? …To help you cope, for protection, and what have you…?

Adam: I don't know, I just…it was just like different people and I ain't know them … I've been seeing the same all my life….I ain't been around no different people from all--Rockford, and Peoria, Waukegan, from all over…Different cities and towns and stuff… We weren't talking about the same, didn't have much in common with each other.…

Two deviant cases require special consideration with respect to the effect of incarceration on social relationships. First, Jay reports that his lengthy incarceration taught him he was a "thug" and habituated him to associating with like-minded young men. Regarding the effect of incarceration on his choice of friends he reports:

Jay: It changed it a lot because I like to hang it with thugs instead of, like, just any old person. Like, I don't like hangin', I still don't like hangin' around no preppy guy... somethin' like that. I like thugs. Like somebody I can cope with, you know?"

Sensing that Jay conflates incarceration with his abiding deviant and rebellious propensities, I pressed him on whether he would have been friends with these youth if he had never been arrested. He responded, "probably so, I probably would've probably still chose 'cause I grew up in the ghetto. I probably still would've chose, yeah." Thus, it is unclear the extent to which extended incarceration, over and above sustained involvement within delinquent activities, influenced Jay's taste in friends.

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The other deviant case, Joe, described how he fell into the role of gang leader while he was in jail at the tender age of 17. He reports that in jail, positions in the gang hierarchy are stable while the people who occupy the position are constantly being released, transferred to other divisions, or "sent down" (a slang term for being transferred from jail to prison). Joe reports that since he was a gang member in good standing who "knew the [gang] literature", his lengthy period of incarceration allowed him to climb the ranks until he eventually achieved the position of block security chief. He was allegedly aided in this role by his physical stature and the "movement" privileges that granted him access to other parts of the jail. Thus, Joe's account of his sanctioning experience, if credible, fits well with the notion that sanctions can embed youth more firmly into deviant subcultures.[92]

The failure of confinement experiences to embed most of my respondents deeper into criminal networks may reflect three important conditions. First, for many youth, contact is too brief or trivial to affect their established social networks. Second, many youth are so firmly embedded in their peer networks at the time of their sanctions that these networks can outlast a youth's incarceration. In some communities, local detention and incarceration is so prevalent that one can maintain a connection to one's peers and community while incarcerated in these local institutions. Third, criminal embeddedness may also be atypical because criminal sanctions

[92] While Joe's account provides a gripping narrative, it falters as an illustration of criminal embeddedness and its possible role as a mediator of the impact of sanctions on school failure. First, Joe's narrative, which colorfully depicts him laying on his bunk, issuing and enforcing orders to men in their 20's and 30's, and receiving outside food from the guards on a regular basis, seems to lack credibility in the same fashion as the stories of his sexual exploits. Given Joe's apparent proclivity toward embellishment, it is impossible for me to trust his account of deep immersion into jail gang life. Second, irrespective of whether his jail experiences were factually retold, Joe offered no indication that he became more embedded in the gang following his incarceration. In fact, less than one year after his incarceration Joe reported that that he didn't gang bang and instead focused most of his energy on the pursuit of sexual conquest and gratification.

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may prompt countervailing social processes. Specifically, the challenges of confinement may also prompt an outpouring of social support and social control from pro-social networks—who may function to counteract immersion into criminal networks.

Case-specific Comparisons of Social Support

Of the conditions mentioned above, I find that the third deserves special consideration within the case-specific comparative framework. Youth who declined most in school performance following arrests are distinguishable, not by intensified involvement in criminal or antisocial networks, but by the absence of a social support network which could help them cope with the challenges to school functioning posed by juvenile justice involvement.

The potential challenges encountered by youth involved with the justice system are manifold, and they intensify as youth become further enmeshed in the system. Many of these adversities have been discussed in the preceding chapters. They include negative labels, anger about mistreatment or unfairness, and simple anxiety about one's immediate and long-term future. Youth who are detained and incarcerated often must cope with severe material, social, and emotional deprivation as well as the fear and actual infliction of verbal and physical abuse by guards and inmates. Some youth also reported that they picked up "bad habits" like fighting while incarcerated and learned to be "better criminals." Finally, youth, who are released from juvenile detention and correctional facilities, may encounter difficulties re-enrolling in their old school or acclimating into a new school, catching up on course work, meeting the requirements of probation, parole, or home confinement while staying engaged in school, and coping with the stress associated with all these tasks.

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One persistent theme that emerged from the interviews is that considerable social support and control is generally necessary to minimize and overcome these difficulties. My quantitative analysis offered only inconsistent support for the importance of social ties to various presumed providers of social support following arrest in conditioning the impact of sanctions on school performance. My interview data helps to illuminate these inconsistencies by revealing circumstances in which social ties can be helpful with respect to school performance and those in which they are not helpful.

The simultaneous imposition of so many school-related challenges often calls for financial and social resources beyond what is readily accessible to a youth. In some cases, such as a juvenile's pre-arrest embeddedness in criminal networks, it may actually call for a complete transformation of one's social context. Of course, youth and their families vary in terms of the amount of resources at their disposal and their ability to mobilize additional resources and alter their social context. My interviews focused intensively on the nature of social support youth received following sanctioning. In my analysis, I attempted to assess the level and type of social support received from peers, family, teachers, and service providers. I use social support and informal social control interchangeably, because the same activity (e.g. tutoring) can be a source of both social support and social control and because both social support and social control affect delinquent and school behavior through the same mechanisms (e.g. social bonds).

In order to avoid oversimplifying these dimensions of support, I did not attempt to quantify these dimensions and conduct statistical comparisons. Rather, I focus my analyses on the cases whose experiences are most revealing and discuss the patterns I observed among these cases.

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The cases of greatest interest are those that present substantial improvements or declines in school performance proximate and subsequent to involvement in the justice system. If social support is an important mediator of the impact of arrests on school performance, then youth in the "improved" and "declined" groups should differ in the level and/or type of social support they received. The social support received by youth who do not fit into either category is also corroborative of the importance of social support if it does not resemble too closely the social support offered the other two groups.

For the purpose of this case-specific comparison I gave special attention to five youth—Julius, Tone, Petie, Shelton, and Robert—who experienced a bona fide and substantial improvement in their school performance following an arrest. The other nine youth--Jay, Tyrece, Adam, Damien, Armand, Don, Petie (again), Shelton (again), and Joe—experienced a genuine and dramatic decline in school performance following their arrests. The remaining youth either experienced an arrest whose aftermath was subjectively so innocuous (James and Kevin) that no mobilization of social support was necessary to maintain a social equilibrium, or, inversely, a pattern of delinquency and school failure so acute and established at the time of their arrests that almost no amount of arrests or social support could slow the inevitable results of a lifetime of academic disaster (Desperado, Vince, Hova, David and Gabriel) or, lastly, the subjective need for social support as a result of arrest is unknown (LaShawn).

A full understanding of the role of social support requires distinguishing among general types of social support. Emotional support, also referred to in common parlance as "moral support," can be defined as social interactions with a person that are intended to maintain or elevate their hope, motivation, and mood—to keep their spirits up. Emotional support is very

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important to the maintenance of emotional stability, especially in relation to court visits and incarceration. Emotional support can help prevent a youth's pending court case from ruining his concentration at school. Emotional support is even more important while a youth is incarcerated. Letters, phone calls, packages, money, visits, etc. from loved ones all serve to remind an inmate that he is valued and loved—despite the contravening messages that other inmates, guards, and society at large may convey—and that he has much to look forward to upon his release. Several of my informants suggested that without emotional support, an inmate can fall into deep despair—a condition that certainly does not bode well for one's academic engagement upon release. Tyrece, who persisted in his patterns of delinquency and truancy following his release from detention, but who feels he has gotten his life together recently, testifies to the importance of emotional support during incarceration.

> PH: When you got arrested and got locked up, did it make your relationship with your family less close or more close?
>
> TYRECE: No, that made it more closer because they're there for me 110 percent. …Supporting me each and every way. I feel that's what made me a better person, too, though, you know what I'm saying. Because some people, when they get locked up, their family abandon them, you know what I'm saying, don't send them no money, write them no letter, accept their phone calls? Won't come and see them, none of that shit. And that type of shit make people go crazy. Some people come home, just be fucked up in the head. You do two years without a phone call or visit, no money. Making your way through the jail off other peoples, you know what I'm saying. That will fuck a person up. But if your family there to help you 110 percent, supporting you each and every way, that's not anywhere it can go wrong. I don't care if you is locked up, you always going to be all right, because you got your family. That's how I think I came out to be straight, because my family was always there for me.

Thus, while my respondents confirm that close ties to one's family can increase the pain of separation during incarceration (Lindquist, 2000), they affirm the importance of these social

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ties for maintaining a healthy outlook and meeting one's basic needs while incarcerated. While emotional support may be essential to maintaining one's sanity and morale during and after incarceration, sanity is only one ingredient in the recipe of school success. Indeed, I failed to detect any differences between the two groups described above in the level of emotional support they reportedly received from their peers and loved ones.

A second form of social support that is more instrumental to successful academic participation following juvenile justice involvement is academic support. Academic support can be defined as interactions that, generally and purposefully, promote a youth's success in school. Academic support can be direct, such as tutoring, monitoring, conferring with teachers, and sanctioning school successes and failures (e.g. poor attendance). Or it can be indirect, such as the resolution of interpersonal or institutional conflicts or crises that interfere with school participation. Academic monitoring and the sanctioning of academic failure are not always effective in the short or long run. For a behavior to be considered academic support it should, as a rule, be helpful rather than counterproductive. Interactions that provide academic support can also dispense emotional support if they offer encouragement and positive attributions that enhance academic motivation and self-efficacy.

A variety of actors can deliver academic support as defined above. The actors that drew the most attention during the interviews were family, teachers, and probation officers. In most accounts, the academic support provided by these actors appears to be mutually reinforcing. That is, a family's efforts to provide support are more effective if they are reinforced by a youth's teachers and probation officer. By the same token, a probation officer's efforts to promote academic participation through either incentives or punishments are likely to be

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ineffectual unless they are endorsed and reinforced by a youth's family and teachers. Of course, the efforts of all these actors receive a boost if they are supported by the peer group and the youth themselves.

In practice, there tends to be little coordination between these potential providers of academic support and control. And, concordant with the evidence presented thus far, at least one of these actors is often wholly deficient in or detrimental to the provision of social support. My interviews suggest that it is highly unlikely that youth find all, or even a majority of their teachers, family members, and probation officers to be supportive during and following their juvenile justice involvement.

Consequently, accounts of social support are often characterized by conflicts of intentions and methods among actors within a youth's social network. I find that the entire "improved" sub-sample described above found at least one person in his social network to be especially helpful, while most could cite at least one person who was not helpful. The social networks of the improved groups were more productive overall as well as more harmonious in their provision of academic support in comparison to the social networks of the declined groups.

The case of Julius, presented in fine detail below, illustrates three important points with respect to social support. First, the unified force of parents, teachers, and probation officers can successfully facilitate an improvement in a youth's academic performance. Second, immersion into a new social context may be necessary to extricate troubled and in-trouble youth from the negative influence of the peer group. Third, the combination of formal control and informal support and control can be effective as long as punitive powers are exercised sparingly.

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Julius is a 19-year old, very personable, aspiring rap artist. He grew up and still resides with his mother and his younger sister in a large apartment complex on the North side of Chicago. There are no other family members with whom he and his immediate family maintained regular contact during his "at-risk" years (early and mid-teens). During eighth grade, he started hanging out with the "wrong crowd" and became involved with fighting and some drug activity. His mother, who worked long hours, was unable to supervise him after school. In an attempt to escape the negative influence of his peer group and the unremitting group labeling process in school that resulted [see reference to Julius in preceding chapter's section on labeling], he claims that he deliberately chose a high school outside of his neighborhood. There too, however, he fell in with gangs and was again singled out for harassment by school police and officials.

Everything changed for Julius in October 1998, toward the beginning of his junior year, when he was arrested for seriously injuring someone in a fight outside of the school, an offense for which he was eventually expelled. Following his arrest, he spent close to three weeks in juvenile detention. As described in the previous chapter, Julius experienced detention as a "wake up call" and he was able to carve out a pro-social niche within the network at the juvenile detention center. This experience motivated him to try to embed himself in a more pro-social peer network. He claims:

> Julius: I stopped hanging with my gang a lot, too. I started hanging with my two best friends, because they was athletes. Everything that came out of them was positive energy, everything. So I just started hanging with them real tight, real hard. And they had kept me – that kept me out of a lot of trouble.

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Julius's efforts to turn his life around were buttressed by several other people. His mother kept him in the house every night and lectured him frequently and in a spirit of concern. His teachers gave him extra help and small assignments to help him catch up, admonished him about hanging out with the wrong crowd, and some threatened to report his behavior to his probation officer.

His probation officer also played an important role in Julius' academic recovery. She dutifully monitored his school attendance and cut him absolutely no slack. He reports, "She was just super fair. I mean, no bending, no this, no bending that, you know what I'm saying?" Her rigid demeanor wore on him. He states, "I think she just thought I was going to be another one who went right back to jail and was just going to be on probation, this and that." His dislike of his inflexible probation officer helped motivate him to perform well, and dispelling her perceived stereotypical characterization of him ultimately gave him satisfaction. His truancy was cut by more than half during the semester after he returned. And each time his probation officer threatened to send him back to jail for missing school, his teachers, who liked him and could see that he was trying, fabricated excuses for him. He proudly reports that his probation officer was pleasantly shocked by his faithful compliance with the terms of his probation.

After his expulsion went through at the end his junior year, Julius was transferred to an alternative school.[93] This experience, in his view, embedded him further in pro-social networks. Alternative schools, which, unlike regular schools, are not firmly associated with a particular neighborhood, provided him further distance from his gang associations (a continued source of

[93] Julius skipped the final week of school, because he was told that the principal was going to have him arrested for what he claims is yet another false accusation by a principal that was unwaveringly committed to his exclusion.

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harassment following his detention). Furthermore, alternative school provided Julius with smaller classrooms and more one-on-one interactions with teachers. As a result, after years of repeated failure, he was finally able to understand math. Julius believes that his alternative school provided him something he never would have been able to achieve in regular school—a diploma.

Julius asserts that the changes to his social networks that followed his release from detention and his transfer to the alternative school were marked and sustained. He insists that, a year after graduation, he still went to lunch with teachers from his alternative school.

While Julius' improvement does mark him as an outlier, it is equally useful to view his experience as lying at one of end of a continuum of social support and social control in the wake of a serious arrest. Julius was uniquely fortunate that his mother, teachers, and probation officer acted in concert to counteract the influence of his gang-involved peers and promote improved school performance.

The remaining youth in the "improved" sub-sample delineated above credit some of their improved school performance to altered social relationships. The transformation of their social milieu, however, was not as dramatic, sustained, nor as complete as that of Julius. For instance, Petie reported that his family provided him extra support after his stay in juvenile detention and that his teachers tried to give him extra help, at least initially. Likewise, his probation officer, whom he shared with several peers, monitored his school attendance and even his behavior on the street—attention/supervision that he greatly appreciated. However, his family was not nearly as strict, so these positive changes were no match for the normative pressures and temptations of the housing project that led him astray from school. His teachers withdrew their support at the

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first sign of relapse and he proceeded to fall even further behind. He reports, “Same stuff continued, like it was going in a circle or something.”

While an outpouring of social support and social control following sanction could be a necessary condition for successful reintegration into school following juvenile justice involvement, it does not seem to be sufficient. The extent, duration, and success of social support and social control efforts are all contingent on a youth’s receptivity to them. As any drug treatment professional or drug addict will attest, no program will succeed for an addict that does not want to be helped. Gaps between the amount of help provided and youth’s receptivity to the help seem to close quickly given that even the most well-intentioned helpers are likely to withdraw their support when the target of their help shows a lack of interest in, or even hostility towards, their efforts. While respondents did often credit others with instilling in them the desire to change (or the fear of not changing), they typically attributed their behavioral changes or, more commonly, their failure to change, to personality, motivational, and volitional factors. In this regard, the self-narratives of personal change were similar to those described by Maruna (2001).

Shelton described how he resolved to change his ways “once they closed them bars” for the first time at the juvenile detention center. His efforts at personal reform and academic catch-up were assisted by the extra help offered to him by his family, teachers, and probation officer. However, when his mother died, he “stopped caring” and was thrown completely off the track of school improvement.

PH: Did [your family] try to help you? Like, prevent you from getting arrested again?

Shelton: Yeah, they tried.

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PH: How?

Shelton: As far as, trying to help me get myself back in school, stop hanging around the people that I was hanging around. But as usual, by me being hardheaded, I didn't wanna listen to nobody tell me nothing. Just did it anyway.

Like Julius and Shelton, Joe exudes a certain affability and charm that likely increased the desire on the part of family, teachers, probation officers, and others to help him. However, he reports that he was perfectly happy with himself and his situation and, therefore, had little desire to change in response to anyone's help.

PH: What made it hard for you to change?

Joe: Ain't nothin' make it hard. I just didn't wanna change at the time. I didn't want change. I preferred things the way they was. Why? I don't know. Shit just happens. I like, I was livin' that life. I like to goof off. Shit, I was havin' fun.

Unfortunately, sometimes personal motivation and deeper embeddedness in pro-social networks are incapable of preventing academic decline following sanctions. Of all my interviewees, Damien spoke the most passionately about the mobilization of his family-based social networks during and after his stay in juvenile detention. However, this support was powerless to prevent his expulsion from school and transfer to an alternative school, which dashed his long-standing hopes of going to college.

The Effect of Arrest on Educational Exclusion

With respect to school performance outcomes, school policies and practices have the power to override individual motivational, attitudinal, and interpersonal factors. In a climate of

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zero tolerance, schools have the authority and the means to deny virtually any youth access to public education. For this reason, institutional exclusion deserves separate consideration.

In contrast to labeling, defiance, and social embeddedness theories, which focus on micro-processes at the interpersonal and psychological level, institutional exclusion theories focus on features of the school and juvenile justice institutions and how these bear on the school participation of youth who are involved with both institutions. The limited professional and academic literature on this question focuses on the policies and practices of both institutions with respect to the education of delinquents as well as the interaction, or lack thereof, between these two institutional domains.

Of course, the foregoing theories also highlight the importance of institutional reactions to the youth. The key difference is that institutional exclusion theory calls attention to the ways in which the responses of institutional actors are patterned by features of the institution or are themselves features of the institution. That said, it is often difficult to distinguish between individualized, discretionary responses of institutional actors and "institutionalized responses." For instance, individual acts of labeling and harassment of returning offenders by teachers and school security guards in order to push them out of school may be viewed simply as individual discretionary responses to student behavior. Alternatively, these discretionary responses may flow from institutional pressures to boost test scores and attendance rates and may, thusly, have become a ritualized practice receiving tacit support from managerial authorities. Likewise, the concerted efforts on the part of some probation officers to fill parental and positive role model voids could reflect institutional protocols and policies that were not examined in this research.

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Thus, differences between institutional exclusion and the other theories may be a matter of emphasis nearly as much as a matter of substance.

The institutional features that are of concern to institutional exclusion theory are those which impose a set of constraints on the student offenders as well as the teachers, principals, probation officers, and other agents who are charged with ensuring that an offender continues to receive an education, and who are empowered to deny educational privileges to offenders. Consistent with my discussion of this theory in Chapter 2, the institutional exclusionary processes related by my respondents can generally be classified as one of three types—push, pull, or *defacto*. Although, here too, the distinction between them is not always clear.

Given that Chicago actually passed an ordinance in 1997 allowing schools to exact the ultimate price for arrests off school grounds, I, at the very least, expected the less aggressive forms of "push out" to be quite prevalent in the declined sub-group. The near absence of overt exclusion in my respondents' accounts was quite striking. No interviewees were expelled from school or sent to alternative schools for crimes committed outside of school. Schools also seemed to have a virtual open-door policy with respect to students returning from detention or youth prison. Of the 12 students who tried to get back into school following release from detention or youth prison—several on multiple occasions—only one reported resistance on the part of the school. And this student's experience is a far cry from that suggested by the exclusionary policies and rhetoric in Chicago. Unlike the other students, Jay had never been to a regular high school at the time of his release from youth prison. Thus, the school in which he attempted to enroll, which I'll call Carver, had less of an obligation to accept him. Carver denied

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him admission on the grounds of overcrowding, although Jay believes that the prison's name emblazoned on his school transcript had something to do with the decision.

This exclusion was not a major setback for Jay, however. Undaunted, he and his mother proceeded to another high school, which was also in the vicinity of his home. The school agreed to enroll him a few weeks later, after Christmas vacation. He reports, and his school record corroborates, that he received a full load of credits for the fall semester even though he attended public school for only a few weeks. The school gave him full credit, mostly because he was enrolled in school that semester in the youth prison—not because continuity existed between the curricula in the two school systems.

In addition to asking youth about their own experiences of school exclusion following arrest or incarceration, I also asked them about the experiences of family, peers, and acquaintances. Most of the youth were unable to think of anyone who was excluded from school or denied readmission into school specifically because of his or her involvement with the justice system. The only exception, Kevin, recounted that his brother was denied re-enrollment into high school after his stay in jail despite his sincere desire to return to school after he was released. The school refused to admit him, because he had already been dropped for having too many absences and because he lacked sufficient credits to complete high school in a timely fashion (he was 17 at the time).

While overt exclusion appears to be extremely rare, more subtle push processes appear to be commonplace. I covered these in some detail earlier in terms of the consequences of negative labels and defiance in the school setting. The most common exclusionary techniques appear to be harassment and punishment for minor behaviors. My sense is that these exclusionary

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methods result from a perception that a youth is a trouble maker in the context of the school setting. A youth's juvenile justice involvement outside of school does not necessarily create this impression, but it can serve to reinforce it. A key question is whether individual exclusionary responses to this information from teachers, principals, school security officers etc. are consistent enough to qualify as an institutional response. My data provides too few examples of exclusionary school responses to externally-imposed formal sanctions in order to suggest routinization. However, my interviews do suggest that, across schools, the techniques for purging schools of unwanted students are similar:

> Shelton: As far as, how they normally get the kids out, every little thing that, even if it's not a big issue, they make a big issue out of. And that's gonna go on they record. You only get so many tries before they kick you out of there. So after they, they gonna find any little thing, any little thing, even if you not doin' nothing. If they even see you drop somethin' on the floor and you don't pick it up, they'll mess with you for that. They'll mess with you for arguin' with the teachers, even if you know you right and the teacher wrong. Some teachers and some police officers have grudges against some students. You know, for what, I have no idea. You know, why, I have no idea.

To the extent that such efforts involve coordination among principals, school security, and/or teachers it accords with institutional exclusion theory more than with labeling and defiance theory. The fact that the accumulation of disciplinary infractions in order to provide an official justification for bureaucratic action is pervasive in various bureaucracies including the work place and prisons lends credibility to the idea of the institutionalization of "informal" school exclusion. The presence of sworn police officers in the school clearly helps schools build such "rap sheets" on students.[94]

[94] Tellingly, one student I interviewed, Vince, assumed that I would know from his school record that he was considered by school authorities to be a gang leader (based on the videotape of other students admiring his fur coat).

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While this line of inquiry could have benefited tremendously from interviews with principals and school police officers, I got the sense from student interviews that schools typically did not take advantage of law enforcement information available to them about the crimes their students committed outside of school grounds (although police often go to schools during the course of their investigations of off-campus crimes or to execute warrants for such crimes) or their statutory authority to exclude students on the basis of off-campus misadventures. Perhaps these struggling schools keep their doors open to delinquents because they are already suffering from declining enrollments. School personnel already have their hands so full with in-school disciplinary issues that what happens outside of the school receives low priority. In other words, in-school mischief is so common and so problematic for school functioning, that schools do not need to rely on indicators of risk from the criminal justice system. In fact, as I have discussed, criminal justice involvement by itself may not be a very reliable indicator of risk. The former school principal of a school once plagued by violence, whom I interviewed for other research (Hagan, Hirschfield, and Shedd, 2001), reported that she found kids returning to school from jail or prison to be more focused and more disciplined because, "they didn't have the time to waste." She also felt that juvenile justice involvement was hardly grounds for special consideration in her school given that 50% of her students (her estimate) were somehow involved in the system.

While I did not find much evidence of push processes, pull and de facto processes were a different manner. Many mandates of juvenile justice involvement, including arrest, court visits, and detention, involve forced absence from public school. Arrest and court visits were rarely blamed for academic problems, given that most students in the sample, especially from the

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declined group, were absent at least a dozen times during the semester anyway. A few more absences that semester did not make a difference in minds. Spending several weeks or months behind bars may bring different results, however. Given that knowledge in several courses like math is cumulative, it is nearly impossible for a student—only marginally functional in the first place—to miss several weeks of class without becoming irretrievably hopelessly lost. Some students respond to this problem by delaying re-enrollment until the next semester. If a student repeats this process during a subsequent semester, however, he will fall so far behind in course credits that he is on a pace to remain in high school until his early twenties. This realization prompts many students to disengage and drop out, and prompts schools to refuse to readmit those who have already dropped out. In this way, juvenile justice involvement contributes to the process of dropping out of school and exclusion from school.

While juvenile justice was seldom cited as the most important pull factor leading students to fall behind in school, it was often listed as a major reason:

> TYRECE: When I was getting arrested, I would just say fuck everything, you know, I don't care what happened. I don't care if I don't learn, don't care if I do learn, you know what I'm saying. I miss a lot of days in this class. Miss a lot of days in this class. Miss a lot of days out the whole school, period, yeah.
>
> PH: Why would you miss days?
>
> TYRECE: Don't want to go. Want to hang out, kick it with the friends, smoking weed….
>
> PH: Did you go back to [your high school] after you got back from the [detention center]?
>
> Shelton: Yeah, after that I started going back, you know trying to continue some of my classes, you know trying to make up for lost time that I had already lost, but sometimes it

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> would just got the point where I took too long. I was gone for so long and it was already too late.

It is important to note that most of the students I interviewed were ambivalent about or bored with school before they were incarcerated. While some students from the improved group (Adam, Shelton, Julius) struggled to catch up after falling behind, others (Hova, Tyrece) seem to have viewed the daunting tasks that successful re-enrollment would require as a convenient excuse to finally shut the door on school.

Juvenile detention and incarceration does not function merely to keep youth out of school. On the contrary, many youth spend a greater share of their time in school during their confinement or incarceration than they did beforehand. However, in virtually a consensual fashion, my respondents indicated that education behind bars does not substitute for the real thing. In the context of the juvenile detention center, where even talking—much less normal youthful rowdiness—is forbidden by guards, school provides the only opportunity that youth have to release their energy. Outside of the immediate control of the guards, many or most detained youth reportedly spend their classroom time goofing off. According to my respondents, most teachers in the detention center have given up on the task of teaching, opting instead to hand out workbook assignments and/or to participate in the "social hour". Similarly, Petie offers a typical description of the school in the Cook County Jail that he recently stopped attending:

> Petie: School ain't nothing here man because they ain't got no guard, them boys be in a class making all types of noise doing anything they really want, basically anything they want to and them teachers don't be getting a word across. They don't be saying nothing; You know what I'm saying?

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Petie reports that he signed himself out of the jail school so he can sleep an extra few hours "on the deck" instead of having to wake up early for the wasteful classroom experience he describes above.

A few youth did report having teachers while incarcerated who commanded attention and respect (See Ayers, 1997). Furthermore, Joe reported that the detention center taught him how to use a computer, because, in contrast to his public school, his detention center teacher assigned students practical or fun activities, such as Internet activities, that actually gave students an incentive to learn how to use the computer. Joe describes his teachers at the detention center as "cool as hell." Likewise, another youth, Hova, remarks that, while in jail, he finds it easier to focus on schooling because of the absence of other stimulating distractions. Using words that belie their meaning he reports, "Ain't got nothing to do in jail but to learn." Hova experienced school exclusion even in jail, however. His involvement in a gang fight led to his transfer to a maximum security division that offers a GED class only once a month.

No matter how much students learn in the detention center or jail alternative schools it is unlikely to help them in the long run because these schools' curricula lack continuity with the curricula in the regular public schools. Students of all ability levels are often placed in the same classrooms. Hova complained that in his youth prison, his teachers were "teachin' like we kindergartners or some shit. It was them mother fuckers in my classroom that was slow as hell. They couldn't do shit." Consequently, with the possible exceptions of remedial students incarcerated at a very early age, students almost inevitably fall behind during prolonged periods of confinement.

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As students get older and fall further behind in regular school, the General Equivalency Degree becomes a more viable option. Correctional institutions often encourage youth aged sixteen and over to the pursue a GED over a high school diploma by offering such youth more GED preparation and testing opportunities training than high school coursework. As Robert explains, "That's [the GED prep] the only thing that's good in there." In this way, the correctional system endorses an inferior educational credential.

While the pursuit of GED preparation during incarceration may close the door on regular high school, it can also represent a positive educational opportunity for incarcerated youth. Relative to a high school diploma, the GED offers these youth a tangible and relatively attainable goal, with clear benchmarks. Accordingly, GED training is also more individualized than high school course work. Certain criminal justice settings may also be conducive to GED preparation. Some youth experience youth correctional schools as schools of last resort which provide the stable sustenance, surroundings, and routines that they never experienced on the outside.

The particular criminal justice settings that may offer the most stability and structure are boot camps and youth prisons. Among my sample, Joe, Tyrece, and Adam prepared for the GED while at boot camp. Likewise, Joe (during a separate stint) and Jay prepared for the GED while in youth prison. Not surprisingly, the schools in boot camp are quite orderly by comparison to the schools in jail and the detention center. Joe, Tyrece, and Adam reported that they made considerable progress toward their GED while in boot camp. Jay reportedly excelled on his practice GED test as well.

Joe, the self-described "goof-off", believes the boot camp experience instilled in him a discipline and confidence about his educational abilities that he never knew he had:

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> Joe: When I was in boot camp, I had my head straight. I ain't even smokin' no squares or nothin'. So let me go to school. You gotta be in your shit. You gots to. So I was doing that shit. I wasn't goofin' off or nothin'. You know they give us a little time to goof off, so when they give us time to goof off, that's when I goof off. But other than that, you know, I wasn't goofin' off….I'm calling home every Wednesday. Calling home like yeah, [imitating a supportive relative] "You takin' the test?" I'm so happy. So we took the practice test. You know I score the mother fucka—290.
>
> PH: So you passed?
>
> Joe: No, on the practice I scored 290. But on the real test, that shit got so frustrated. I took it but I got stuck on the math, 'cause that shit was hard. So I scored a 39 on the math, when I need to score a 40 or a 41. And I would've passed. I just missed it.

Unfortunately, Joe never had the chance to retake the GED test. Adam, Tyrece, and Jay were also never able to take advantage of the intellectual capital they accrued while incarcerated. In a bittersweet fashion, Adam and Jay were both released from custody just prior to their scheduled test dates (Tyrece missed passing the test by a few points). When school events and criminal justice events are in conflict, criminal justice events nearly always take precedence. The fact that these youth were not provided the opportunity to take their GED prior to their scheduled release represents de facto exclusion, and underscores the conflicting institutional logics of the criminal justice and educational institutions, which I discuss more in the concluding chapter.

I certainly should not generalize about the viability of GED programs in correctional facilities based on the experience of four youth. After all, these could be unusually motivated and skilled students who somehow managed to progress in poorly run programs. I am confident, however, that their positive experiences speak to the relative effectiveness of the program rather than their own individual traits. I am able to largely rule out the "individual

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traits" explanation of their progress, because these same individuals met with miserable failure in obtaining their GED's while released. Joe reports that he would show up to his GED program high on marijuana and would shoot dice in the alleys.[95] He was "locked up" soon after. Adam had to drop his GED classes after his parole officer denied him "movement" for missing a drug counseling appointment. Tyrece has been in and out of jail on probation violations, but he is currently in a GED program thanks to a chance street encounter with a minister he met during one of his jail stays. Jay cannot sign up for GED classes because he, having spent the bulk of his teen years incarcerated, is having intractable difficulties obtaining the social security card he needs to procure the requisite state identification. Only one other student, Damien, is currently pursuing education or training of any kind. Tragically, life on the outside offers too many distractions, temptations, conflicts, and obstacles to sustain my respondents' participation in GED classes for more than a few weeks. Boot camps and youth prisons, while depriving them in so many ways do provide a simpler, more structured lifestyle conducive to GED preparation.

One of the potential obstacles to participation in both GED programs and regular public school is monitoring by a probation or parole officer and related monitoring associated with home confinement or house arrest. With respect to school performance, this monitoring is a double-edged sword. On the one hand, Court "programs" are explicitly designed to encourage or permit school participation. School provides one of the few allowable means through which those on home confinement or house arrest can engage the outside world. Furthermore, school attendance is a frequent condition of parole or probation. Accordingly, probation and parole officers sometimes take an active role in ensuring that a youth re-enrolls in school. As Robert

[95] Petie and Desperado reported similar failed experiences with GED programs outside of school.

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explains, “Your probation officer will put you in school. If she come up there, they have to put you in school. No matter what. Cuz. They’re saying he has to be back in school or he goes to jail. They’ll put him in.” As we saw from the discussion of social embeddedness, mentoring and monitoring from one’s probation officer can improve school attendance. (Whether forced attendance actually improves school *performance* is another matter). On the other hand, the failure to attend school can and does result in revocation of probation or parole and in (re)incarceration in a juvenile or adult facility. Thus, even youth who vastly improve their school attendance above their often abysmal pre-involvement attendance levels, may face incarceration if their improved attendance rate does not live up to the realistic or unrealistic standards of their probation or parole officer. Incarceration can, as I have already shown, effectively dash a youth’s hopes of obtaining a high school degree.

It is impossible for me to say, based on my interviews, how often this particular pull factor, the revocation of probation or parole due to poor school attendance, is operative. Two findings in the interviews are illuminating, however. First, probation and parole officers vary considerably in their monitoring style. They range from extremely lax and forgiving of serious violations, to inattentive to and forgiving of all but serious violations, to extremely hawkish and unforgiving of any violation.

Of the eight youth in the improved group, six had probation officers while still in school. Three of these youth, Petie, Julius, and Adam, reported that their probation or parole officers regularly monitored their school attendance. Only Adam actually suffered exclusion as a result of this monitoring. Two other youth in the improved group, Vince and Shelton, reported that their probation officers helped them enroll in special programs, but did not vigorously monitor

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their attendance in regular school. The other youth, Robert, reports that his probation officer told him to go to school, introduced him to Boys and Girls Club, and that he complied with both, but she never actually followed up on his attendance patterns. His pattern of cutting and disrupting classes and fighting increased after his Boys and Girls Club program ended and eventually culminated in his exclusion from school due to "excessive absences."

The declined group displays a different pattern with respect to probation monitoring. Of the eleven youth in this group, eight had a probation officer while still in school. Of these eight students, only Desperado reports that his probation officer monitored his continued enrollment or attendance in school. He reports that he complied with his probation officer's orders only when he believed that he faced a serious threat of incarceration should he not comply. Consistent with his defiant attitudes towards authority, Desperado's descriptions of his interactions with his probation officer suggest, that, through his partial compliance, he was able to preserve his proud defiance by "getting over" (Anderson, 1999) on those who sought to control him.

> Desperado: They kept talking, we're going to lock you up. I was like, all right, I'm going to go. And just like they kept talking, I kept talking, too, shit, talking that game. Yeah, I'm going to go. And as soon as they find out I'm in school, they say all right, we ain't going to lock you up. All right, I'm dropping out; you know what I'm saying? Shit. It was, I don't know, it was kind of like a little game I was playing. You know, it felt like a game to me, man, because I was seriously laughing at them mother fuckers, man.

Thus, Desperado deliberately sabotaged any attempt to improve his school performance through mandatory school enrollment. Four other members of the declined group described efforts on the part of their probation officers and other juvenile court agents to control their behavior and to help them. These efforts consisted of curfews (Damien), evening reporting

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programs (Don), and orders to enroll in a GED program (Hova and Joe). Despite these efforts, these youth, like the rest of the declined group on probation, said that their probation officers did not seriously monitor their school attendance and did not revoke probation for nonattendance or drop out. Thus, the case-specific comparison suggests that the presence of extra support and diligent monitoring from probation officers, as well as youth's receptivity toward that help, are two factors distinguishing youth whose academic performance is maintained or improved under probation from those who, following arrest, continue their downward and outward slides.

On the other hand, my findings also suggest that probation violations due to truancy are not a major cause of the impact of juvenile justice involvement on school performance. Perhaps, most probation officers were too hampered by large caseloads to notice or did not consider school nonattendance legitimate grounds for revocation. Also, offenders never have near perfect attendance while on probation or parole.

The experience of Adam is probably an egregious anomaly, but it still warrants mention. Upon his release from youth prison, Adam promptly enrolled in high school. He attended regularly but left school with his friends on three occasions. His parole officer learned of his lapse and reported it to his judge without discussing it with him. His judge responded by sending him back to prison for a year. Adam described the shocked reactions from other inmates as they learned that his incarceration was for "missing three days of school."

Criminal justice monitoring can be a pull factor through several other means. Most commonly, violations of other rules of probation or parole or violations of home confinement often lead to the removal of youth from school in order to be re-incarcerated. Some youth preemptively stop attending school following such violations because continued attendance

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would mean arrest by the police officers stationed in the school. Jay describes why he stopped going to school after he cut off the electronic monitoring band around his ankle.

> Jay: I cut my band off, and I was on the run then. Then, I had to stop going to school and stuff. I couldn't go to school, cause…you know [the school] got a police station in it. So, if I did go to school there, they'd have me right there. I wouldn't even be able to go nowhere.

Jay's also describes how an earlier experience of home confinement resulted in his expulsion from a different school:

> Jay: And then, I got out on house arrest, this was when I got kicked out of [school], 'cause the teacher tried to hold me in detention. And I had a band around my leg and I couldn't, I can't just stay, I just can't stay in the school. I had a band around my leg. They tying me up to get home, and if I ain't there at this destination, I go back to jail. "You may wanna keep me 'cause I was messin' up but you can't keep me. You wouldn't let me out the door" And I was tryin' to get passed him. So I hit him with a chair, and they let me, he finally let me out the door.

Although Jay's behavior earned him an expulsion from school he did not violate his house arrest or his probation. Amazingly, Jay reports that his judge understood his predicament. In both Jay's mind and that of the judge, the criminal justice monitoring and disciplinary apparatus trumps the school's disciplinary system. It would be very useful to learn what the consequences for Jay would have been had he chosen to stay at school detention and was caught violating the terms of his house arrest.

As mentioned, probation officers often help students re-enroll in school. However, the chosen school is not always the preferred school of the student. Joe reported that his probation officer prevented him from re-enrolling in his high school, "Thompson", following his stay in the

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detention center. While she told him that his school did not want him anymore, he strongly believes that his probation officer wanted to prevent his re-enrollment at Thompson in order to separate him from the negative influence of his peers. While she was willing to help him enroll in a regular high school, he opted to enroll in a GED program instead because it was a "a quicker way to get out of school and stuff." As evidenced above, Joe, in contrast to Julius, did not become embedded in a pro-social network, nor did he escape his peer networks as a result of his enrollment in a GED program. Joe acknowledges that he probably would not have done much better in a regular high school.

> PH: Do you feel that there are environments that would've been better for you than Thompson?
>
> Joe: It don't matter what she put me at, because wherever you go, it's always gonna be some trouble that you can get in.

Perhaps, Joe, like Julius, would have benefited from an alternative school environment, which provides individual attention. However, Joe, unlike Julius, was not interested in changing his social network. One must remember that individual orientations, attitudes, and choices can shape both the delivery and the outcomes of both supportive and exclusionary responses.

In summary, my interviews provide support for social embeddedness and the role of institutional actors like teacher and probation officers, but mostly in terms of maintaining or improving school performance. I found very few examples of criminal embeddedeness and overt school exclusion following justice system involvement. By contrast, juvenile justice institutional pull processes were evident and consequential for several members of the declined group and implicate schools to the extent that they fail to mitigate against these exclusionary processes.

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The next and final chapter will elaborate on the relationship between juvenile justice and educational institutions.

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CHAPTER 12: SCHOOLS, JUVENILE JUSTICE, AND THE *HABITUS*

The empirical analyses in the preceding chapters could stand on their own as a meaningful and cohesive sociological analysis. I have systematically examined whether involvement in the juvenile justice system affects school performance and educational attainment in the inner-city. The results suggest, not only that juvenile justice involvement should be added to the long list of verified causes or antecedents of school failure and dropping out in the inner-city, but, perhaps, that it merits a high importance ranking on that list. However, some caveats must accompany this assertion. First, my findings may be unique to delinquents in Chicago and my analysis did not systematically weigh the importance of juvenile sanctions against the school performance predictors featured most prominently in the literature. Second, given the poor scholastic histories and the plethora of other impediments to ongoing school engagement and achievement that inner-city "delinquent" youth encounter, it is quite likely that most of the youth in my sample who dropped out of school shortly after their legal trouble would have eventually dropped out anyway. However, my findings are still important insofar as juvenile justice involvement affects the amount of schooling one receives and is a tipping factor in the drop out or push out decision of the many students that teeter on the fence but have not quite "dropped" or "been pushed over" yet.

I also explored some of the conditions under which and the mechanisms through juvenile justice exacts its "collateral consequences" on inner-city schooling. In this way, I avoid the over-simplified, one-dimensional, mechanistic model of social relationships often rendered by quantitative research that does not accord with or capture the complexities and contradictions of

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the human social experience. Simply put, my quantitative and qualitative results offer evidence that juvenile justice involvement affects different people differently under different circumstances. The dimensions on which the effects seem to pivot most decisively seem to be a youth's cultural capital, self-concept, and level of functioning, as well as a youth's access to academic support and monitoring from parental figures, teachers, and probation officers. Excessive and excessively punitive monitoring from teachers and probation officers, as well as perceived severe mistreatment by other criminal justice authorities, appear to promote the academically harmful effects of juvenile justice involvement. My qualitative findings bear on the validity and applicability of labeling theory, deterrence theory, defiance theory, criminal embeddedness theory, as well as the literature on exclusionary institutional dynamics faced by adjudicated youth. My research not only tentatively adjudicates between several competing perspectives on the effect of legal sanctions on individual attitudes and behavior, but it also superficially addresses issues of concern to policy makers and practitioners such as the advantages of boot camp, youth prisons, and alternative educational settings, relative to detention centers and jails with respect to promoting learning as well as the obstacles youth face in earning a degree (other than the "third degree") from any of these institutional experiences.

Some may question the basis on which I can draw any theoretical or policy implications from only twenty interviews. In my defense, I offer two assertions. First, while I spoke at length with only twenty youth, I discussed with them, on average, about three of their juvenile arrests as well as their adult arrests when applicable and relevant. Youth also detailed or summarized the experiences of family members, friends, fellow inmates, and classmates. In short, most of the youth I interviewed contributed more than merely one data point to each topic of interest.

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Rather, they were participant observers who could offer a grounded or informed opinion, replete with many real-life examples, on many issues of central concern to this research. Second, note that this work was not designed as a definitive test of any particular theories. It is better viewed as an excursion into the largely uncharted territory of the relationships between educational and justice system institutions and involvement within them. My overarching intention in this work is to develop a blueprint for future study in this area, as well as to advance some grounded ideas about these institutional interconnections.

While my empirical findings bear on the interpretive perspectives first outlined in Chapter 1, they also provide a fuller understanding of the impact of sanctions on school performance that extends beyond the confines of those theories. In this final chapter I weave my empirical findings into a new conceptualization of the relationship between juvenile justice and school performance that draws from the literature linking individual educational aspirations and decisions to social structural developments. My earlier empirical analyses, in the words of Macleod (1995), "merely set the context for beginning to wrestle with the real issues of how social structures capture agents' minds and control their attitudes and how individuals resist and succumb to the inertial pressure of structural forces" (p. 137).

Arguments of a social structural and institutional (macro) nature are, by necessity, somewhat speculative in nature. My rationale for shifting from a highly empirically-based and systematized mode of discourse to a more loose and reflective one is rooted in the scientific method in social science research (Stinchcombe, 1968). I have attempted to actualize or at least mimic all the major steps in the scientific method. First, I derived scientific hypotheses from a body of theories. These hypotheses were tested using a quasi-experimental methodology with

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supplementary interviews to aid in interpretation and qualification. The next step in the iterative, cyclical scientific process is to assimilate the research findings into, or discuss them in relation to, the original theoretical framework that generated the research hypotheses for the purpose of confirmation, refinement, extension, or even refutation.

The first theoretical refinement suggested by my research pertains to the concept of institutional exclusion. One of the most striking and unexpected findings of this research is that schools, at least through 2000 in Chicago, according to former students' accounts, typically do not seem to use juvenile justice involvement as certification of one's status as trouble maker or as a convenient excuse to purge trouble makers. Despite the fact that politicians in Illinois and Chicago have passed laws that explicitly aim to limit access of delinquent youth to mainstream schooling, I found little evidence that schools have "taken advantage" of this opportunity to rid themselves of this population. Rather, I find evidence that schools tends to allow youth to remain in school after committing serious offenses and that they permit such offenders to return to school each time they are released from the custody of the justice system. While my qualitative data does not permit me to generalize, a moderate portion of my sample reported that teachers actually provided them extra help upon their re-enrollment in school—hardly evidence of a systematic effort to exclude delinquents.

While the juvenile justice system may not have promoted the specific targeting of arrested or adjudicated youth for abandonment and exclusion, my findings suggest that it may have affected the role and function of schools in inner-city communities in other ways that have implications for school performance. First, the criminal justice system has incorporated school attendance into its system of monitoring and punishment. Ostensibly, court-mandated school

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attendance aims to re-engage youth in the educational process and promote their attachment to conventional values and lifestyles. However, the origin of such policies within a juvenile justice system increasingly modeled after the adult system should spark questions of whether and how such policies promote internal criminal justice objectives of control and punishment and external criminal justice objectives of the system's increased legitimacy and prominence in society. Given that mandatory school attendance is typically not accompanied by any services like tutoring or counseling that would be necessary to reduce the boredom, failure, and frustration that school attendance typically brings delinquents, the main value of mandatory school attendance appears to lie in the more efficient management of potentially unruly youth.

Moreover, mandatory school attendance also promotes school exclusion to the extent that imperfect school attendance is a legitimate means through which detention center, jails, and prisons can compel their alumni to return. Given the often chaotic nature of Chicago public schools and inner-city life in general, and the normatively low rates of school attendance, near perfect school attendance for probationers and parolees seems completely unrealistic—a rule whose frequent violation should have been predicted by its architects and proponents. Such a rule may reduce the intractable truancy problem that plagues Chicago Public School, but it also threatens to circumvent some teachers' and administrators' goal of helping errant students (at least those deemed deserving of help) within the parameters of youth culture and the school's established institutional framework. This tension is clearly manifest in my informants' reports of their teachers actually lying to probation officers and school police officers in order to prevent students from, literally, being locked out of the classroom.

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My interviews highlight other ways in which the logics, norms, and goals of the juvenile justice and educational systems collide to the detriment of dually involved students. These tensions are not merely evident in the interaction between institutional domains, but also within the institutional domains as both educational and criminal justice institutional agents have set up shop in both educational and criminal justice facilities.

While in some situations, like protective teachers' lying to probation officers and school police officers, these tensions are resolved in students' favor, more times than not the institutional interests of criminal justice prevail, pulling students further away from schooling and deeper into the criminal justice system. The interviews detailed a number of ways in which the two institutions clash. First, they operate on different calendars. Criminal and juvenile justice activity evolves largely around the court calendar and sentencing and release schedules. Thus, the justice calendar varies for each individual. School calendars, by contrast, are fixed for everyone in a particular program. Unfortunately for youth, individual justice calendars do not take into account a youth's school calendar. Youth court dates may occur on exam days and court continuances may cause a youth to miss an extra month of school while waiting in detention or jail. Release dates also typically do not correspond to the school calendar as youth are often released in the middle or toward the end of the semester--completely forsaking the concept of cumulative learning--or just prior to when one is scheduled to take a GED test. Such conflicts would be less likely to occur if juvenile justice institutions, aided by "indeterminate sentencing" schemes and a genuine service orientation, were still mandated to consider a youths' rehabilitative needs first and foremost. The primacy of fixed legalistic parameters (e.g.

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sentences, probation terms) and managerial and actuarial efficiency over individual educational needs is a casualty of the criminalization of the juvenile court (Feld, 1999).

In the context of correctional institutions where the criminal justice calendar and managerial concerns reign supreme, the educational institutions embedded within them have no choice but to assimilate and adapt to correctional priorities. In the context of the Cook County detention center and jail, this accommodation apparently occurs at the expense of any meaningful education. Given that the average stay in detention is about three weeks, course curricula are divided into three-week courses. Whereas in mainstream schools care is generally taken to impart substantive subject matter at a level of difficulty appropriate to a student's assessed level of ability, such an effort is apparently beyond the mandate and scope of the detention center, jail, and some youth prisons. In all these institutions, students of widely varying abilities are placed into the same classrooms and the curriculum is depicted by many of my respondents as "too easy."

Rather than continuing the hopeless struggle to provide a substantively meaningful education to a highly transient youth population of inconsistent levels of prior knowledge, the juvenile detention center appears to have shed its pretense of being an educational institution in a traditional sense of the term. According to the curriculum director of the school, the individual courses—irrespective of subject matter—are supposed to cohere around rehabilitative themes like valuing education and peer pressure. While such attempts to infuse rehabilitation into conventional curricula may seem harmless on the surface, they reveal the absurdities that can result when one attempts to fuse educational and correctional imperatives. The correctional framework casts students as inmates to be managed and "rehabilitated." Teachers who view

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youth as inmates may find it difficult to simultaneously heed the charge of liberal pedagogy—to nurture their individual intellectual growth and autonomy.

Within this context of the nearly complete subordination of educational to penological concerns, it becomes more understandable why, according to most of my interviewees, many inmates and teachers within correctional facilities have given up on the learning/teaching enterprise altogether. The view of confined youth as inmates first and students second is not only a possible cause of this educational failure but also a convenient excuse. Flaws in the schooling system can always be justified as promoting safety, efficiency, or even as just punishment for criminal behavior. The Director of curriculum at the detention center points to such overriding considerations in order to justify the practice of placing more advanced students in the same classrooms with remedial students. He explains:

> I had a student tell me, 'you know this curriculum is too easy.' And I say, 'the crime you committed wasn't too easy. We are not here to make the education environment a rewarding one for you. We seek to make it 'functional.'

In the presence of conflicts and contradictions between the norms, conceptions, and meanings which prevail in educational institutions and the fiscally, legally, and culturally superseding criminal justice institutions, those charged with providing an education to delinquents must make a decision that falls somewhere between two poles. At one pole is the choice to define one's response to delinquents largely within the parameters of the educational paradigm and to resist changing to accommodate the special constraints imposed by the mandates of the criminal justice paradigm. At the other pole is the choice to attempt to reconcile the conflicts between the two institutions and assimilate the new realities of

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criminal justice institutional dominance into the structure of schooling. While my interviews suggest responses leaning toward both poles they also suggest that both resistance and accommodation can be detrimental from the point of view of the students who are attempting to stay in school and out of detention, jail, and prison.

The resistance choice, that is, the failure of schools both inside and outside of criminal justice institutions to accommodate to criminal justice constraints can have clearly negative yet avoidable consequences. For instance, GED programs within correctional settings like boot camps could conceivably make the changes necessary to ensure that youth who have undergone the necessary preparations have an opportunity to take the test before they are released. Likewise, GED programs on the outside could take into account students' progress while incarcerated, so their GED training can resume after their release. Furthermore, the Office of Juvenile Justice and Delinquency Prevention (OJJDP) has outlined a number of ways in which educators in public and correctional schools can work together to promote a seamless transition from one institution to the other (Stephens and Arnette, 2000). These tools include exchanging information vital to proper curricular placement, individualized curricula, joint pre-release planning and service coordination, pre-release visits to the receiving school, and continuous, consistent transitional services through both settings. My interviews suggest that, in the absence of such services (formally or informally provided), students are often unable to make an effective transition from confinement back to school.

Promoting a seamless transition between the two institutions, however, is not without its pitfalls. In a political context dominated by fear, distrust, and punitiveness, the policy task of providing a smooth transition can easily be couched as a means to ensure school safety rather

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than to promote re-entering students' educational progress. In Chicago, a transitional program charged with aiding the reintegration of youth from detention and correctional facilities into alternative and mainstream schools was instituted in March, 2001, beyond the range of my empirical research. While the program is labeled a transitional program, it is clear from the web-site of the Chicago Public Schools that the educational needs of ex-offenders is not foremost on the minds of its planners. Rather, the intended functions of the program are 1) to screen out students who, based on their ability "to perform academically and socially" are "identified as not being ready to return to a regular school environment" and 2) to reintegrate juveniles ages 12 to 17—those who are deemed "ready"— into regular schools, "without disrupting the learning environment and school climate." In each school, the task of reintegration is to be performed by "teams of counselors, student intervention specialists, probation officials, and youth investigators at their schools" (Chicago Public Schools, 2001).

Notably absent from these teams is anyone explicitly charged with providing the one-on-one tutoring and mentoring my interview participants repeatedly insisted would have been helpful in getting them back on a legitimate educational track.[96] In short, this program seems to embody the criminal justice system's emphasis on assessment, correction, and control more than the educational system's supposed emphasis on providing a substantive education.

In such a context of accommodation, OJJDP's recommended "best practice" of information sharing for the purpose of catering curricula to individual needs can also be

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[96] The external programs with which the transitional program are "closely aligned," according to the descriptions provided, largely provide behavioral instruction or counseling rather than actual academic support.

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subverted toward exclusionary ends. Depending on their orientation, principals or other school officials may use information obtained from criminal justice sources not as a tool of curricular assessment but as "intelligence" which is to be shared with school police officers for law enforcement purposes.

While neither completely ignoring the special needs and monitoring apparatus of delinquents nor completely accommodating them is unequivocally helpful or harmful, certain institutional actions are definitely more helpful or harmful than others. The modal story in Chicago appears to be that, except for occasional teachers, the school system provides no special help for delinquents and lends them the same level of care or contempt as the rest of the student body. In other words, schools resist changing to specifically accommodate this population.. At the same time they, with a few exceptions described earlier, tend to cooperate with probation and police officers even if such accommodation results in the exclusion of students. Julius' cases illustrates how teachers can go out of their way to stifle the exclusionary efforts of criminal justice agents at the same time other agents in the school—the assistant principal in Julius' case— can go out of their way to accommodate them. In the main, the school-related institutional interactions that students described following their juvenile justice involvement are either neglectful (benignly or malignly) of their needs or harmful to their academic progress.

Just as school institutional actors can decide to accommodate or resist the various challenges and constraints imposed by the embedded nature of criminal justice in America, inner-city students, both individually and collectively, can choose, within limits, to readily accommodate or vigorously resist the structural constraints associated with the relationship

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between schooling and juvenile justice. The experience of being arrested and suffering academically may become assimilated into a student's views of the social structure.

Such changes in perceptions may influence their school-related behavior. Various prominent scholars of social reproduction contend that important decisions about schooling are conditioned, not only by objective features of the social structure and one's position within it (e.g. race, class, geographic location) but also by individually and collectively defined perceptions of that social structure. For instance, MacLeod, who also interviewed urban youth about their school experiences contends that the perception of America's social opportunity structure as an open one, as manifest in one's belief in the "American Dream," plays a central role in determining how much effort youth devote to school—America's official pathway to social mobility. To the extent that juvenile justice experiences affect one's perceptions of the social structure, then one may expect them to influence school engagement as well.

My failure to consider these questions up until this point reflects the fact that none of the sources of my theoretical postulations and interpretations considered such questions. Theorists of the impact of criminal sanctions largely describe the consequences of sanctions in the micro-context, not considering the dialectical relationship between patterned individual experiences and the larger social structure. My second source, my interview respondents themselves, while not completely unaware of structural barriers such as unemployment and discrimination, generally failed to articulate any perceptions of the social structure, much less assign them importance in explaining their school behavior.

My respondents' de-emphasis of perceived structural realities in explaining their behavior does not necessarily indicate their lack of actual importance. First, as any introductory-level

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sociology instructor will attest, 18 and 19 years olds often insist on relating even the most macro-level concepts in individualistic terms. However, a failure in articulation does not necessarily imply a failure in understanding, even among those without even a high school education. Second, the failure to overtly discuss perceptions of social structure reflects the fact that social structural realities are seen as a given, that is, as so stably ingrained into one's consciousness and daily life that one does not think to describe them as a cause of their behavior.

This idea of the submerging of social structural realities into one consciousness has been crystallized in sociology thanks in large part to the work of the French intellectual Pierre Bourdieu (1977) and his concept of the *habitus.* To Bourdieu, the habitus is where objective, structural realities and subjective dispositions and perceptions converge. According to Boudieu (1977), the habitus consists of "the system of internalized structures, schemes of perceptions, conception, and action common to all members of the same group or class" (p. 86). Although the habitus is a property of social groups it can also be seen as the aspects of individual human consciousness that are given to us by virtue of our group memberships via the mechanisms of shared experiences, histories, and associations.

According to Bourdieu, as well as Macleod (1995), an important component of the habitus is perceptions of the opportunity structure. Macleod argues that structurally-conditioned differences across social groups in these perceptions help explain differential expectations, aspirations, and consequent school efforts across social groups. However, in addition to its perceptual and conceptual components, the habitus also influences human cognition and behavior through means that are not readily accessible to the conscious mind. The habitus includes traditions and subtle habits that people acquire by virtue of their location in the social

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strata. People generally do not question such habits; they just do them. Actions that are performed by force of habit no longer possess any real motivation except for inertia and the comforts of routine. When asked, in the context of a research interview for instance, about their motivation for a socially habituated behavior, actors may be at loss and may even feel compelled to invent one.

For this reason, the habitus is a very slippery phenomenon to study empirically. Still, its importance for theories linking social structure to educational behaviors behooves me to consider its role in the juvenile justice/school performance relationship. My work suggests several ways in which criminal justice experiences and realities may have become part of the habitus of severely disadvantaged inner-city minority students. First, the extant evidence, my statistical data and my interviews, reveals that being targeted by the police, often without just cause, is a normative experience among young black and Hispanic, males. Second, participation in the common behaviors of marijuana smoking, cutting school, and, in some cases, fighting can draw very harsh reactions from the police, even resulting in imprisonment. Third, incarceration is a common experience of older youth as well as adults. And it is not viewed as strictly limited to criminals and "thugs ". Fourth, in the schools attended by the most disadvantaged students exiting school following juvenile justice involvement is quite common. Among youth who get involved with the justice system and their close family and peers, the experiences of falling behind in school or losing interest in school while one is incarcerated forms part of the school habitus. Regardless of whether juvenile justice *causes* school dropping out, which I took great pains to prove, the proximal association between entering the juvenile justice system and exiting the school system likely also helps constitute the habitus of these groups.

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Given that juvenile justice involvement is pervasive in certain communities and given its observed capacity to close the door on legitimate social opportunities, some of the aforementioned elements of the emergent criminal justice habitus may clash with aspects of the habitus shared by wider collectivities such as the high premium that segments of the black community place on education, and that the working class places on work (Macleod, 1995). Pervasive juvenile justice involvement is just one of many aspects of the collective experience of young black males that diminishes their educational and labor market prospects. However, given that discrimination on the basis of arrests and convictions has been formalized within schools and the work place, juvenile justice involvement may be seen as particularly damaging to one's future prospects. Valuing education at the same time one is becoming aware of one's diminished educational potential can, of course, be assaultive to one's self-esteem (Macleod, 1995). Willis (1977) describes how working class British youth, faced with their educational failure and knowledge of their objectively low chances of advancement respond by rejecting mental labor in favor of manual labor, which is seen as more masculine. This option may not be available to comparable black youth in the inner-city, however. Young black men failing in the poorest black schools, especially those with criminal records, gain little by exalting the value of work, given that jobs that express masculinity and pay a decent wage are also not accessible to them.

How do inner-city youth cope with their failure to succeed by the standards of the dominant society? Other scholars have suggested that one response is to form and join peer groups that reject conventional values and define alternative criteria for success (Cohen, 1955; Macleod, 1995). Some of the redefined criteria for success may actually be an inversion of the dominant criteria. That is, success in conventional society may be defined

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by following the rules and performing well in school, while within some peer subcultures, status and approval may be earned by defying rules and school authorities.

If the factors that lead to a rejection of dominant values—such as lack of access to conventional means of attaining the middle class model of success—are sufficiently prevalent, then subcultural values can actually become the dominant values within certain contexts. This principle is demonstrated in the concept "acting white," which refers to a view allegedly prevalent among black urban adolescents that working hard in school (Fordham and Ogbu, 1986) and/or using proper English (Fordham, 1999) falls outside of the scope of black cultural identity. Various scholars have described the values of inner-city culture that compete with mainstream values for dominance in the socializing institutions of ghetto youth. Among adolescent males, salient values and avenues to status include self-respect, masculinity/virility, physical strength and agility, toughness/bravery, loyalty, and highly visible displays of monetary success (Anderson, 1990; Canada, 1995; Anderson, 1999; Messner & Rosenfeld, 2001). Demonstrating these values and skills is not just a means to earn immediate gratification and validation from one's peers. It is also a part of the individuation and status attainment process that all adolescents undergo enroute to becoming what their community, or at least their salient referents, defines as an "adult."

Many criminologists have noted that adolescent peer groups, especially youth gangs, and criminal activity, particularly fighting and drug sales, provide the context and means through which the focal concerns delineated above are expressed and rewarded. Inner-city schools, by contrast, which emphasize docility, sobriety, academic skills, and enforce rules prohibiting any flashy clothing or other status symbols (e.g. certain jewelry, cell phones) suppress all of these

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values and exclude those who exploit the ample opportunities to demonstrate them. While some have argued that schools help nurture and sustain deviant peer subcultures by demeaning working class identities and providing youth something to rebel against (Macleod, 1995; Cohen, 1955; Stinchcombe, 1964), in the context of the modern ghetto, conforming to the peer culture or the street culture is not rebellion at all. The "rebels" are sometimes those who, like my informants, Damien and James, attempt to succeed by conventional standards.

The juvenile justice system, more so than the public school system, does nurture and reinforce the values of the street culture and provides opportunities for success on its terms. First, prison culture places an even greater premium on toughness and gang affiliation than street and school culture. As Joe's story attests, established gang hierarchies exist within jails and prisons and the transient nature of the population means that opportunities to attain a status promotion via toughness, peer loyalty, criminality and other criteria that gangs value abound. Second, correctional institutions, in contrast to schools, do not exclude youth for fighting, using drugs, disrespecting authorities etc. Rather, they respond by punishing them, elevating their security status, and keeping them in the institution *longer*. Such treatment can enhance one's reputation for toughness and resilience and, I suspect, embed them further into prison culture.

The reputation that one establishes in a carceral setting, given the permeability of the prison and the ghetto, may follow a youth to the street after his release. Moreover, the mere fact of having served time has implications for status on the streets. Several of my interview respondents indicate that incarceration carries considerable social import within their wider adolescent social network. For instance, it can certify status as tough or as committed to a

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deviant and rebellious lifestyle, which helps define one as "cool." Since this point is somewhat controversial I adduce four supportive quotes:

> Petie: It's just like when you get locked up, you get out, you feel like, you feel like man, "I've been there and did that. I ain't scared of you,"…something like that, you know what I'm saying, "You want to fight, what's up," You know what I'm sayin? I don't even know…something like that or something.
>
> Tyrece: Shit, they probably look at you like you're grown or you're cool, you know what I'm saying. Like you got locked up, you cool. That shit ain't cool; you know what I'm saying?
>
> Hova: They be like, man, you'd be talkin'... 'I'd been locked up before,' they like brag about it and shit, thinkin' it's cool.
>
> Jay: Yeah, [arrests] stopped me from going to school a lot. That's what it mostly did. stop me from going to school, and then, but then I was a role model. I wasn't a role model, but everybody looked up to me up at school, like, all yeah, "That's Jay right there; he cool right there. Yeah, we gonna roll with him Let's walk home with him. Let's go where he going."

To labeling or defiance theorists, the above quotes may simply suggest that incarceration labels a youth as deviant, and many youth, deprived of conventional means of status attainment and, perhaps, angered by mistreatment by authorities, respond by embracing the deviant label. Labeling and defiance interpretations, however, fail to capture the larger story. The feelings of coolness, toughness, or maturity that youth attribute to their incarceration do not result from negative labeling. As is evident from my earlier discussion of labeling theory, few youth report that parents and school authorities altered their treatment of youth in a way that would lead to self or peer attributions of "coolness." Nor do youth apparently lend much attention or weight to the reactions of authority figures outside of their families.

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To fully understand the function of the justice system in the social landscape of the inner-city, one must distinguish between the concepts of label and acquired status. To some degree the distinction is valuational in nature; labels are generally negative while acquired statuses are generally positive. However, a more fundamental distinction can also be identified. Labels, analogous to those we wear on our clothes, are fabricated or constructed. As active human social psychological creations, they can be resisted, removed, hidden, and they generally fade over time. While labels are sewn onto individuals, statuses, on the other hand, are etched into the social fabric and are, by nature, much more enduring than labels. While labels can be passed within small social groups, statuses are allocated by social institutions. Institutions often develop formalized rituals or ceremonies for allocating or removing status, including job/exit interviews, weddings and other rites of passage. Individuals may control the statuses to which they aspire and achieve, but they generally have no control over the content of the status or the status allocation process. Statuses, of course, are very important to the life course as they structure almost every social relationship and set and mark the course of one's life trajectory. Status hierarchies and allocation processes also form part of the habitus of a social group.

Has juvenile justice involvement become an important status symbol or a status allocator in low-income communities? If so, what are the ramifications of this for inner-city youth's involvement with other social institutions, like school? I cannot provide any definitive answers to these questions from my research. However, my interviews provide some clues. As is evident from the preceding four quotes, juvenile justice involvement is a status symbol to the extent that it indicates that one is manly, cool, or tough. Thus, it signals that one has acquired some of the characteristics that are esteemed within the peer culture.

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At the same time, juvenile justice involvement has become so prevalent that it may affect the values and norms of this peer culture. In communities that are devoid of organized sports, valued extra-curricular activities, jobs for youth, and other routes to individuation and status, the juvenile and criminal justice systems provide a set of common yet variegated rituals and experiences. These common experiences form the basis of status definition. Youth can compare their level of participation in different programs, and the number and nature of their arrests, probation violations, court appearances, detention experiences etc. Out of these organizational entanglements and rituals, new community-based criteria for success and new statuses can develop. One perverse consequence according criminal justice a status allocation functions is that some youth may actively seek out justice system involvement. For instance, Vince attributes his decision to sell drugs to his desire for "attention." By his own design, his behavior brought the attention of the police and of bystanders and peers who saw the police chasing him. Tyrece also underscored the role of the justice system as a status allocator when he stated that he knew many people who felt that they could "make it better" in prison:

> Tyrece: But a lot of people, I hear this out of people mouths telling me that they can make it better in jail, you know what I'm saying. They feel like they can make it better in jail. And I know people who get locked up just for that, you know what I'm saying, they can stay in jail, you know what I'm saying?

Life course theorists describe how society structures the life course through providing status transitions and associated rituals. Some status transitions like marriage and parenthood are common across all groups while others are largely confined to particular social groups. For instance, among many middle class people, going away to summer camp represents the first time away from one's family and the establishment of some autonomy. This autonomy is further

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nurtured though other status-laden rituals like obtaining a drivers license and high school and college graduation. Some of the youth I interviewed describe their involvement in the justice system in the same manner in which middle class youth may reflect on their experiences of summer camp and obtaining a drivers license.

The aspect of incarceration which my respondents fixated on the most was separation from family. While this was a great source of pain, it also forced them to learn to survive without their family's direct help. They had to learn how to go without certain luxuries, how and when to fight, and new ways to cope with stress and hardship. Accordingly, some of the youth described how the experience toughened them up. Petie, who, as reported above, felt that incarceration decreased his fears and inhibitions, states:

> Petie: I believe because that was my first time I got to spend, spend locked up being away from my family; you know what I'm saying? Then I see can't nobody come visit, don't nobody write me or nothing. I got to thinking like man it didn't do nothing but make me stronger; you know what I'm saying?

Thus, incarceration is clearly a turning point in the life course that has implications for the processes of individuation and social skills development. However, the presence of "life skills" curricula in the juvenile justice system aside, the experience of incarceration often fails to function as a turning point enroute to desistance (Sampson and Laub, 1993a). The deprivations and conflictive relations that are endemic to the carceral experience fail to prepare youth to enter the worlds of school and work. Rather, they most effectively teach youth how to navigate and survive in the social world of prison and, according to some of my respondents (Jay, Desperado), to be better criminals on the street.

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While juvenile justice, like summer camp, teaches young black males many important life lessons, it does not, in my preliminary estimation, rise to the level of an established rite of passage into adulthood or adult criminality . Youth who leave the juvenile justice system, generally return to school, and many do not go back. Fewer still will end up serving extended time in adult prison. On the other hand, the common sequential, loosely age-graded pattern of school disengagement, juvenile justice involvement, school drop out and exclusion, deeper justice involvement, and imprisonment likely occurs with such regularity—a part of the lower class habitus—that it helps endow school and justice involvement with particular status meanings. Under structural conditions of compulsory education only through age 16, exclusionary school practices, pervasive involvement in the juvenile justice system, and mass incarceration, schooling is associated with childhood, while incarceration is increasingly associated with adulthood. For some youth with very poor labor market prospects who anticipate prison in their futures the juvenile justice system may have supplanted schools as the institution whose status rituals mediate their transition into adulthood.

However, structurally speaking, adulthood holds out the prospects of a great deal more than frequent incarceration. Even those interview participants who experienced prolonged spells of incarceration retain some aspired or actual attachment to the conventional labor market. Accordingly, most of the youth, now approaching their twenties, recognize the importance of education, deeply regret their earlier disengagement from it, and currently want nothing more than to obtain their GED's (though most have, for the time being, given up the pursuit of this elusive commodity).

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Another anecdote helps to illustrate the extent to which the criminal justice system helps youth "grow out" of school. Petie vividly and emotionally related to me the story of one of his "boys" whom I'll call Stan. Stan was "like a straight A" student who also played on the basketball team. However, while he was "good in school" he was also "wicked on the streets." An active gang member, he was arrested for possessing a pistol. With his arrest came anticipation of roles as future defendant, prisoner, and even ex-convict. To manage the defendant role effectively, he hired a lawyer. And to make prison life more tolerable and to ease the transition out of prison (and apparently the financial handicaps of felony convict status) he began to save money. Speaking in terms of the collective "you" (Maruna, 2001), which indicates that he sees Stan's behavior as normative and relevant to his own life, Petie explains Stan's rationale:

> Petie: You won't get out of jail broke. …Or to take it to the jail with you so you won't have to worry. You know you ain't got no family that's going to send you nothing, you'll have some already.

To raise the money needed to manage his new roles in a self-reliant fashion, Stan dropped out school and joined the illegal drug trade. Thanks to his investment in a lawyer, he was able to beat his case. However, Petie notes that he was not allowed immediately back into school. Rather than opting to wait until the next semester, he chose instead to pursue his GED. One may speculate that Stan's taste of adult life, as served up by crime and criminal justice, marked a status transition of sorts and explains why he did not pursue his education more aggressively following his acquittal.

Thus, my argument is that the juvenile and criminal justice system endows status within certain peer networks and helps shape both the timing of status transitions and the criteria for

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status attainment among inner-city youth. The status structure that the juvenile justice system helps reinforce and configure does not reward the same behaviors and future aspirations that the school system explicitly encourages. Thus, the deeper one's entrenchment in the justice system and the more one's perceived future is bound up with the criminal justice system, the less relevant and salient are the dictates of the student role. However, contrary to the expectations of labeling and subcultural perspectives, I find no evidence that juvenile justice involvement encourages the rejection or inversion of conventional standards of success. Rather, the juvenile justice system provides an alternative route to status for those who are largely bereft of other means. However, it is by no means the preferred route. Most of the students I interviewed, contrary to the "acting white" thesis, valued school at least in principle, tried to at least keep pace with their classmates even after their arrests, and spoke admiringly of those who were actually able to successfully overcome daunting and compounding obstacles to school success.

Again, I must stress that, except for my assertions concerning the impact of arrest on school performance, my major arguments are only loosely grounded in empirical evidence. Rather, they derive from the logical application of extant empirical and theoretical work as well as anecdotal evidence collected from twenty youth and a few other key informants. Still, it stands to reason that, if the justice system has become nearly as prominent as other social institutions in disadvantaged communities such as schools, the labor market, and the Church, then it should wield a comparable level of influence. Given the well-documented failure of criminal justice institutions to achieve benevolent social aims—a record largely confirmed in my research—one should not expect this influence to be a positive one. The critical perspective I have offered about the severity and reach of this influence—that rituals associated with justice

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system involvement are gradually supplanting high school graduation and entrance into the legitimate labor market as normative status transitions in some communities—should be grave enough to draw the attention of sociologists across the sub-disciplinary spectrum. I hope that future researchers, practitioners, and inner-city youth prove me wrong.

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Appendix A. Figures

Figure A1 . Class Absences Before and After Arrest

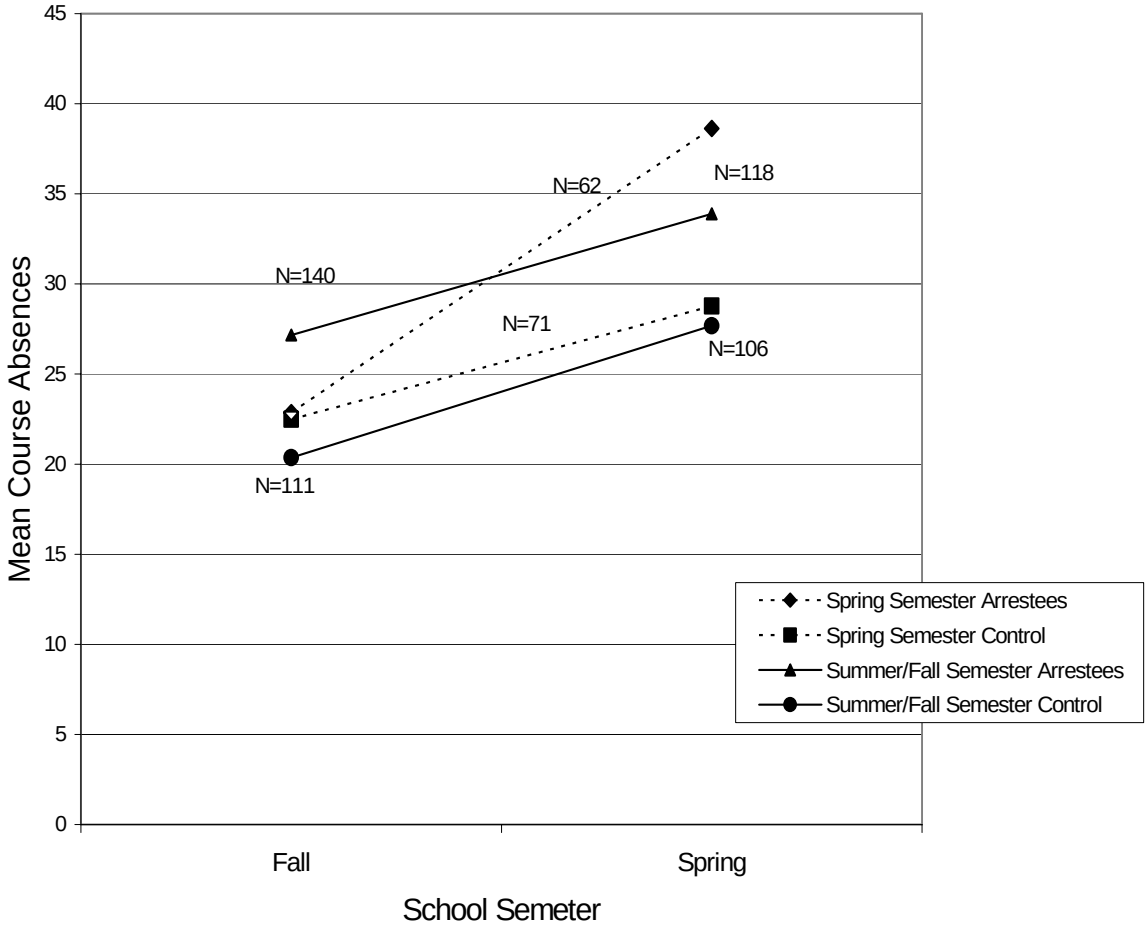

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Figure A2. Class Absences Before and After Arrest Across Intervention Groups

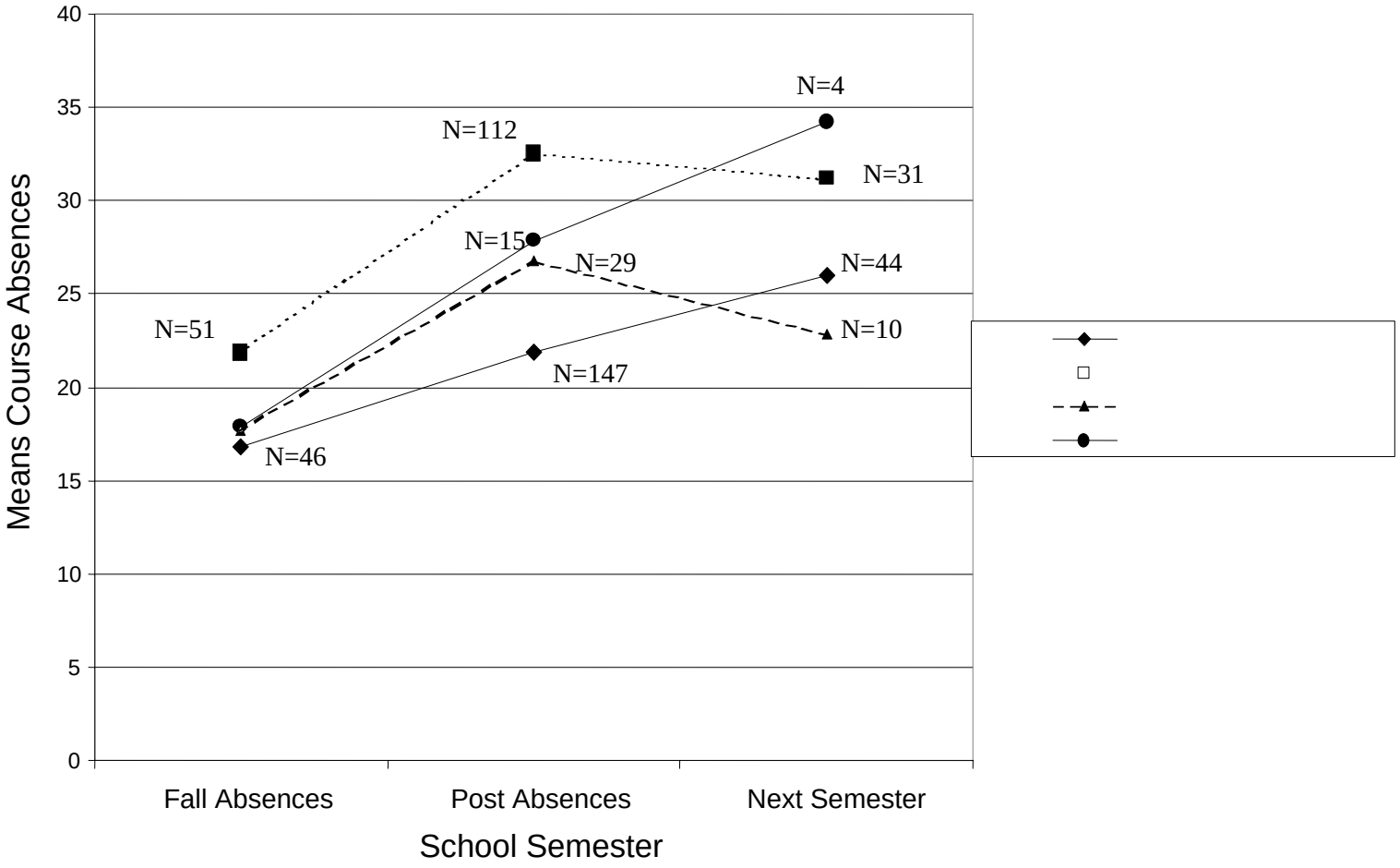

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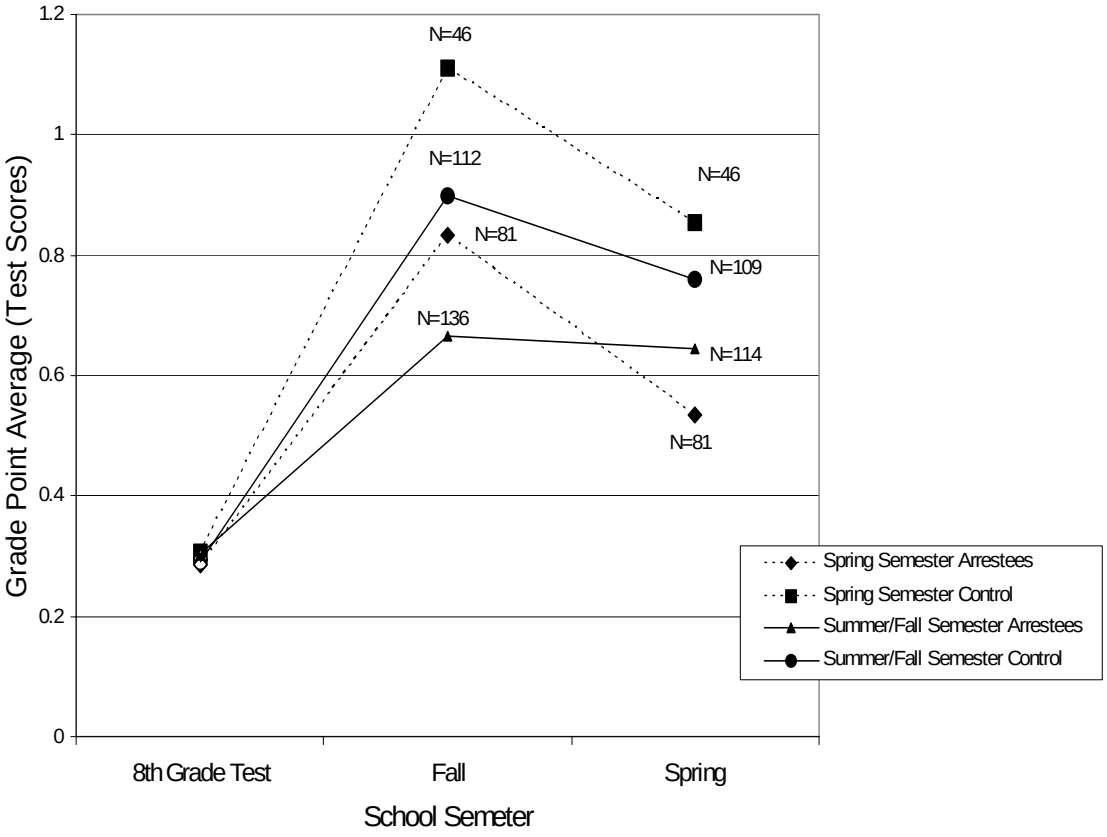
Figure A3. Average Test Scores and Grade Point Average Before and After Arrest



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Appendix B. The Challenges of Interview Recruitment: Two Anecdotes

Two examples should illustrate the challenge of tracking and recruiting some of the hard-to-locate youth. On one visit to a housing project, my attempts to wait for a tardy youth, James, to return home were thwarted by a gang of youth who aggressively insisted that I pay them five dollars so they could "watch my car" while I waited. James later told me after our interview that he fled my first interview attempt because my reminder over the telephone that the interview would be "taped," raised suspicions about prurient intent.

On a separate visit to a different housing project where, according to the Probation department, a current probationer, Tyrece, lived, I was told by a managerial employee of the Chicago Housing Authority that the youth I sought, Tyrece, no longer lived there. (This report later turned out to be false). Unconvinced, I soon asked a youth in the courtyard where I could find Tyrece and the youth referred me to a couple of youth across the street from the courtyard. Next, I spent the next thirty minutes convincing a series of curious and skeptical youth, two of whom pretended to be the youth I was looking for, of my benevolent intentions. Eventually, I was lucky enough to speak to a girl claiming to be the youth's younger sister who gave me their phone number. It turned out that Tyrece was actually present in the courtyard when I arrived but took off running as soon as he learned I was looking for him. I took extra care at the start of our meeting to ensure that the person I was interviewing was indeed Tyrece.

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Appendix C. Brief Profiles of the Interview Respondents

	Arrests[a]	Race/Ethnicity and Sex	Improved/ Declined Pattern	Basic Narrative of Focal Educational Change(s) subsequent to arrest
The Improved Group				
Adam	7	Black M	Both	Test Score Increased after 8th grade arrest; technical violation of parole ended high school career and still stifles GED preparation.
James	1	Black M	Neither (Originally improved)	8th grade test score gain associated with retention; Fear of gang violence in high school led to eventual dropping out.
Julius	1	Black M	Modest Improvement	Probation officer monitoring and teacher support improved attendance; completed alternative school.
Kevin	1	Black M	Modest Improvement	Test score improved a little following early and minor arrest; graduated high school despite teacher harassment.
Petie	5	Black M	Modest Improvement	Probation/teacher support along with personal drive improved test scores; high school dropped him for truancy.
Robert	2	Black M	Major Improvement	Attendance improved substantially following arrest possibly due to probation officer support; high school dropped him in 11th grade for truancy, fighting, and class disruptions.
Shelton	1	Black M	Both	Probation/teacher support improved 8th grade test scores; high school dropped him for truancy after mother's death.
Tone	1	Black M	Modest Improvement	Attendance and grades improved; Credits mother's guidance and justice system deterrence.
Vince	11	Black M	Both	Math score improved following one arrest for unclear reasons; Harassment from high school authorities, which increased after later arrest, allegedly led to dropping out.

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	Arrests[a]	Race/Ethnicity and Sex	Improved/ Declined Pattern	Basic Narrative of Focal Educational Change(s) subsequent to arrest
The Declined Group				
Armand	3	Black M	Major Decline (Originally Both)	Went "on run" in 8th grade after violating probation; never went back to school.
Damien	1	Black M	Major Decline	Decent student expelled following gun arrest in school; completed alternative school.
David	12	Puerto-Rican M	Major Decline	Serious truancy and marijuana; thrown out of 8th grade for writing “gang-bangin stuff on a piece of paper.” Never returned; incarcerated repeatedly afterward.
Desperado	5	Black M	Major Decline	Passionately hated school and almost never attended; Never complied with Court’s orders to attend school.
Don	4	Black M	Major Decline	Despondent because of mother's death and frustrated with "transition school"; dropped out before high school.
Gabriel	2	Mexican M	Major Decline	Serious emotional and behavioral problems; frequent school discipline; unstable household; Dropped out in 9th grade.
Hova	1	Black M	Major Decline	Expelled following gun arrest in school; not interested in academics.
Jay	9	Black /White M	Major Decline	Conflicts between juvenile court demands and school demands; went “on-run” several times, dropped out of high school after Stepfather died.
Joe	5	Black M	Major Decline	Goofing off in class, smoking pot; probation officer actively prevented re-enrollment in high school; failed GED.
Lashawn	1	Black F	Major Decline	Denies arrest for fighting; Attributes dropping out to brother's death.
Tyrece	1	Black M	Major Decline	Arrests along with criminal activity caused him to fall behind. Truancy, tardies, and fighting led to extralegal "expulsion" from school.

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