ABSTRACT

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To what extent can government actions reduce crime? I address this question using a combination of aggregate and individual data on federal grants, police employment, incarceration, and arrest records. I begin with a study of the aggregate impact of the Community Oriented Policing Services (COPS) program that provided grants to states and localities to pay up to 75 percent of the cost for new police hires for three years. In the first chapter, coauthored with William N. Evans, we show that each officer paid for by grant funds increases the size of the force by 0.70 officers. We argue that the size of COPS grants can be used as an instrument for the size of the police force in regressions where crime is the outcome of interest. These models indicate that police added to the force by COPS generated statistically significant reductions in auto thefts, burglaries, robberies, and assaults.

In the second chapter I show that not only did COPS grants temporarily increase city police employment, local governments exhibited an asymmetric response to changes in grant support, permanently increasing their police force even after the three year grant

expired. Using a stylized model of policing I identify the immediate deterrent and long term incapacitative effects of police officers on crime rates using variation in the size and timing of active and expired UHP grants. My results suggest that larger police forces reduce violent crime primarily through increased incapacitation of offenders. Deterrence plays the largest role in reducing auto theft.

In addition to larger police forces, longer sentences have increasingly been used by governments as a means of reducing crime. In the final chapter, I use individual data from Maryland to estimate the incapacitative effect of sentence enhancements. I find that offenders who receive sentence enhancements would on average be arrested for 2.8 criminal acts and be involved in 1.6 index crimes per person if they were released instead. This is measure of marginal incapacitation is substantially lower than existing impacts of average incapacitation, which have been incorrectly used by policy makers to justify the imposition of sentence enhancements.

GOVERNMENT REGULATION OF ILLICIT BEHAVIOR

By

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Dedication

To Matt, my sounding board, confidant, and favorite economist

To my parents, Marc and Vera, and my brothers, Marcus and Christopher, who have supported and believed in me through this entire process.

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Chapter 1: Introduction

Sustained economic development is impossible without secure property rights. This statement should not be controversial, as the positive connection between the ability of an individual to capture the returns to their efforts and their effort level was established by Adam Smith. Governments are defined as an entity that has a monopoly on legitimate violence, and collects taxes in exchange for protection. Leading scholars in institutional economics have focused on the relationship between rule of law and economic development (Rodrik, Subramanian and Trebbi 2002; Acemoglu, Johnson and Robison 2001), and Hurst (1956) identifies four shifts in how the US legal system enforced property rights that put the United States on the path to long term economic growth. Theoretical papers in appropriative conflict (Grossman and Kim 1995; Gonzalez 2005; Besley 1995) have quantified how rational actors will allocate their resources away from productive investment towards non productive defense and productivity destroying appropriation. In <u>Structure and Change in Economic History</u>, North (1981) highlights the role that good institutions play in a functioning market economy using the example of expectations involved in the purchase of a bag of oranges. One of the "rules of the game" involved in that transaction is that a person buying a bag of oranges will pay the seller and not just turn and run. Government protection of property rights implies that the government makes some attempt to ensure that individuals who attempt to encroach on the property rights of other will be unsuccessful.

By construction, a government's monopoly on the legitimate use of force gives it a comparative advantage in making sure individuals follow the rules of the economic and legal institutions in place. Theory on the connection between regulation of illicit behavior and productivity is supported by empirical evidence such as Field (2006). The problem of illicit behavior is not just an issue in developing countries. Between 1986 and 2005, the total cost imposed on crime victims was an average of 1.7% of GDP[1]. Understanding and defining the most efficient ways in which governments regulate illicit behavior is an important and understudied question in economics.

Prior to the 1930s, reliable national statistics on illicit behavior are unavailable. Without a clear record of the history of illicit activity, the popular press would occasionally alarm the public with declarations of crime waves. In 1930 the Federal Bureau of Investigation began surveying law enforcement agencies, compiling the Uniform Crime Reports in an attempt to create a public record of eight harmful illicit behaviors: Murder, Manslaughter, Assault, Rape, Robbery, Burglary, Larceny, and Vehicle Theft. These eight behaviors have become known as index crimes. Eighty years later, the Uniform Crime Reports are still arguably the best available source to measure the efficiency of government attempts to control illicit behavior on a large scale.

According to the Uniform Crime Reports, in the late 1960s the United States experienced an unprecedented increase in property and violent crime. This increase in crime is typically associated with the burgeoning drug culture, social upheaval and the adolescence of the baby boom. Crime rates reached a plateau in the mid 1970s before increasing again sharply in the mid 1980s. Violent crime rates fell dramatically through the 1990s, although since 2005 homicide rates in the Uniform Crime Reports have begun

[1] This is based on index crime counts as reported in the Uniform Crime Reports, GDP estimates from the Bureau of Economic Analysis, and average cost of crime estimates in Miller, Cohen and Weirsema (1996). Recent cost of crime estimates (Rockoff and Linden 2006; Cohen, Rust and Steen 2006; Cohen, Rust, Steen and Tidd, 2004; Miller, Levy, Cohen, and Cox 2006) produce even large costs to victims.

to rise again. In the following chapters, I examine the effectiveness of two different government policies intended to reduce crime; policing and sentence enhancements.

The first attempt of governments to respond to this increase in illicit behavior was to increase the severity of punishment through increased use of incarceration. The 1973 Rockefeller drug laws in New York State were the first sentence enhancements specifically targeted at illicit behavior connected to substance abuse. Federal action followed. Between 1984 and 1994 the federal government passed three "omnibus" acts, the Comprehensive Crime Control Act of 1984, which overhauled the structure of federal sentencing process in favor of predetermined sentencing guidelines and created the Office of Justice Programs to coordinate federal grants, the Crime Control Act of 1990 which created the Byrne grants to states for general criminal justice expenditure, and the Violent Crime Control and Law Enforcement Act of 1994 (VCCA), which created the Community Oriented Policing Services (COPS) grant program.

At the same time that legislatures were increasing incarceration on both the extensive and intensive margin, there was also a shift in how local law enforcement agencies operated. Policing agencies had been organized according to a model developed by O. W. Wilson.[2] The "Professional Model" of policing was capital intensive, with the goal of having the fewest number of police officers responding to illicit behavior in the largest possible area in the smallest amount of time. Foot patrol was discourages in favor of officers in patrol cars being directed by a central dispatch officer. The professional model of policing was challenged in 1979, by Goldstein who proposed instead "Problem Oriented" Policing. Unlike policing under the Professional Model, Problem Oriented

[2] Wilson's textbook Police Administration contains a series of leading yes or no questions regarding police acitivty, in which the correct answer is always the capital intensive one.

Policing is labor intensive and requires that police officers not only leave their cars and "walk a beat," but actively engaged the community at large in reducing illicit behavior.

The increased use of incarceration and policing were intended to reduce crime by increasing the ability of governments to detect and capture individuals who engaged in illicit behavior and increase the severity of punishment the government inflicted on guilty parties once captured. The combined result of government policies between 1984 and 2001 has been a dramatic increase in the number of individuals incarcerated. These combined policies have had a negative impact on crime, but government spending on crime control imposes costs on society. Incarceration often separates parents from their children, and reduces their ability to provide financial support for their dependants. Government attempts to prevent illicit behavior can have perverse effects on the efficiency of legal institutions. The census counts incarcerated individuals as residents of the jurisdiction in which they are incarcerated, not where they actually live, distorting political representation. As Tonry (1998) points out, law enforcement in the United States is closely tied to racial tension, and the African American community is disproportionately impacted by increases in government attempts to reduce crime. Increased police presence may not be welcome, and may be interpreted as government intrusion and maintenance of an unfair status quo, instead of an attempt to provide increased safety and protection.

I begin my study of how governments control illicit behavior with federal grants for police. In the first chapter, "COPS and Crime," my coauthor William N. Evans and I contribute to the public finance literature on intergovernmental grants by quantifying the extent to which federal grants distributed through the COPS program supplemented, rather than supplanted, new police hires. Using these grants as an instrument for changes in police force size, we find that contrary to much of the existing research in criminology which predicts an ambiguous relationship between police and crime, a 10% increase in police employment will result in a 9% reduction in violent index crimes, and a 4% reduction in property crime.

In the first chapter, the relationship between police and crime is treated as a black box. Two forces, deterrence and incapacitation, can exist inside that box. In the second chapter, I present evidence that the hiring grants had a permanent impact of police force size, evidence that not only is the federal government able to influence the allocation of locally provided public goods, but that temporary earmarked grants can have permanent effects on local government expenditure. Following the procedure used in Kessler and Levitt (1999), I estimate that roughly 2/3rds of the reduction in crime attributed to one additional police officer is due to increased incapacitation, although there is some heterogeneity across crime types.

Most of the crime reduction attributed to increased police presence in COPS and crime is due to increasing the flow of offenders from the general population into incarceration. In the final chapter I estimate the impact of government actions that decrease the flow from incarceration to the general population, namely sentence enhancements. I focus explicitly on incapacitation, using individual level data that allows me to separate incapacitation from deterrence in a way that is not possible with aggregate data. I also take advantage of quasi experimental variation in sentence length that allows me to identify marginal benefit of increased incapacitation due to a replicable government policy, which is an order of magnitude less than the mean criminal behavior of all incarcerated inmates one year after or one year before incarceration.

I hope that my research highlights the contributions that economists can make to the study of crime control. Governments must balance protection of property rights with the costs associated with surveillance and punishment. As Spellman (2000) points out, economists are inclined to specify implicit assumptions and directly address causality in a way that researchers in other fields do not. The following chapters are not the final word on how effective government regulation of illicit behavior, but they represent a step forward in quantifying the causal impact of policing and incarceration on crime.

Chapter 2: COPS and Crime

2.1: Introduction

In 1994, Congress passed and President Clinton signed into law the Violent Crime Control and Law Enforcement Act (VCCA). The VCCA was the largest crime bill in the history of the country, authorizing a total of $30.2 billion in federal funds for various law enforcement and crime prevention programs. A cornerstone of the VCCA was the establishment of the Community Oriented Police Services (COPS) office that authorized the Department of Justice to provide grants totaling $8.8 billion in fiscal years 1994-2000 to local police agencies for various community crime prevention programs. The bulk of the funds under COPS were set aside for the Universal Hiring Program (UHP) that provided grants to local police agencies to pay 75 percent of the cost of new police hires. Federal funding for the COPS program, which was initially set to expire in 2000, has been re-authorized annually. As of the end of fiscal year 2004, a total of $11.3 billion in grants were distributed by the COPS program[3] with roughly $5 billion of these funds being used to hire 64,000 new police officers under the UHP program.[4]

Since its inception, the COPS program has been the center of some controversy. Reports by the General Accounting Office (GAO, 2003) and the Inspector General of the Department of Justice (1999) suggest that grants for officers were, in many cases, used for hires that would have occurred anyway or diverted for other purposes. Some reports suggest that because of these diversions the COPS program never came near its goal of putting an additional 100,000 officers on the street and Davis et al. (2000) questioned whether the COPS program increased the number of officers all. Finally, there is limited

[3] http://www.cops.usdoj.gov/Default.asp?Item=35.
[4] http://www.cops.usdoj.gov/Default.asp?Item=53.

and conflicting evidence about whether crime fell in towns that received COPS grants (Zhao, Scheider, and Thurman, 2002; Zhao and Thurman, 2004; GAO, 2003; GAO, 2005; Muhlhausen, 2001). Citing these concerns about the program, the Bush Administration for FY 2005 reduced the appropriations to just under $20 million and eliminated COPS hiring grants.[5]

In this paper, we use panel data for 2,074 cities and towns over the 1990-2001 periods to examine in detail a number of issues surrounding the COPS program. In the first half of the paper, we use the variation in timing and size of grants to test whether receiving a hiring grant increased police force size. This portion of the paper has its antecedents in the large literature in public finance on the impact of intergovernmental grants on the size of governments (Bradford and Oates, 1971, Hines and Thaler, 1995, Gamkhar and Oates, 2001). Our results suggest that the COPS grants did temporarily increase the size of the police force, but that some of the funds are used to supplant hires. For every four officers granted through the program, federal funds paid for three and the police force increased by a little over two. We also extend some of the previous work from political science and examine the determinants of the size of COPS grants. We find that hiring grants are correlated with the pre-COPS levels of crime and the size of the police force, but grants are not correlated with pre-grant changes in the crime rate. Given our fixed-effects model, COPS grants can therefore be thought of as an exogenous change in the size of the police force. This type of variation allows us to examine the impact of both increases and decreases in police forces on crime.

[5] http://www.whitehouse.gov/omb/budget/fy2005/pdf/budget/justice.pdf. In the FY2006 budget, three additional large grant programs which provided money for local law enforcement, the Edward Byrne formula grant program, Local Law Enforcement Block Grant program, and State Criminal Alien Assistance Program were eliminated.

A number of authors have examined the relationship between police force size and crime, but much of this literature uses cross-sectional data. Summaries of this literature by Cameron (1988) and Sherman (1992) conclude that there is little evidence that a larger police force reduces crime. Other researchers have criticized the cross-sectional research noting that since the size of police forces are determined in part by the level of crime, cross-sectional models are possibly subject to omitted variables bias (Fisher and Nagin, 1978; Sherman, 1992; Marvell and Moody, 1996; Levitt, 1997). In recent years, a limited number of papers have used quasi-experimental variation in the size of the police force to determine the impact of police on crime.[6] Even with these recent efforts, there is scant evidence that more police reduce crime. In this paper, we hope to exploit the change in the size of the force generated by COPS grants to re-examine this question.

Using our city-level data set, we find that additional officers granted through the COPS program produce statistically significant drops in burglaries, auto thefts, robberies, and aggravated assaults. We also find similar reduction in murders, but the p-value on the t-test that the coefficient is zero is 0.10. We find an implied elasticity of crime with respect to the size of the police force of -0.26 for property crime (t-statistic of -1.72) and -0.99 for violent crime (t-statistic of -3.2).

[6] Levitt (1997) found that more police hired in the periods just prior to a mayoral election generated statistically significant reductions in murder, and large but statistically imprecise reductions in robbery, aggravated assault, burglary, and auto theft. McCrary (2002) demonstrated that once he corrects for an error in the way Levitt implements weighted-least squares, the standard errors on the estimates for specific crimes increase until the results are no longer statistically significant. Levitt (2002) uses the number of fire fighters as an instrument for the number of police and in this work, the author finds that police have a statistically precise negative impact on murders and auto theft, and negative and statistically insignificant reductions in rapes, robberies, burglaries and larcenies. Klick and Tabarrok (2005) and DiTella and Schargrodsky (2004) find that criminal activity tends to decline in the areas that have a visibly larger police presence during terror alerts, but there is the possibility that crime fell because criminal opportunities also declined during terrorists alerts. Corman and Mocan (2000) use monthly crime data in an attempt to avoid the simultaneity problem, but their model is dependent on the assumption that there is no omitted variables bias in month to month variation in police deployments.

2.2: The Violent Crime Control and Law Enforcement Act of 1994

The VCCA (PL103-322) had three main components. The first was a major increase in federal government involvement in crime prevention primarily through expanded federal jurisdiction and enhanced penalties for certain crimes.[7] A second major component of the law was various immigration initiatives.[8] Third, the VCCA provided substantial grants to state and local law enforcement agencies. The Attorney General was given the power to distribute $8.8 billion to private and public law enforcement agencies through the newly created COPS office. The Clinton administration's stated goal was to add 100,000 police officers to the streets nationwide by the end of fiscal year 2000.

The COPS office distributes three types of grants. About 70 percent of all available funds were originally earmarked for the UHP program that provided funds to hire new police officers.[9] A smaller share of funds were set aside for the Making Officer Redeployment Effective (MORE) program grants designed to reduce the administrative tasks performed by uniform officers and increase their time on patrol. Both UHP and MORE grants are matching grants where the COPS office provides up to 75 percent of the cost of the designated item or officer. UHP grants are capped with a maximum grant

[7] The substantive criminal provisions include: bans on certain assault style weapons, expansion of the types of federal crimes subject to the death penalty, a three-strikes law for certain violent federal crimes, stiffer penalties for certain gang and drug trafficking offenses, greater flexibility in charging minors accused of violent offenses as adults, a requirement that states establish registries of sexually violent criminals, prohibiting the sale of firearms to people with domestic violence restraining orders against them, and the strengthening of federal standards for firearm dealers.

[8] These initiatives include the authorization of an extra $1.2 billion for border control and $1.8 billion to reimburse states for incarceration of illegal aliens convicted of felonies.

[9] The COPS office has a distribution requirement of 85 percent of the available funds. Depending on the year, between 80 and 90 percent of those funds were earmarked for hiring grants. The UHP was actually created by merging together two different hiring programs. AHEAD grants were targeted at cities with populations larger than 150,000 people, and smaller cities received FAST. Available funds were divided equally between these grants. Although these grant programs were combined one year later, total funds were still divided in this way.

of $75,000 per officer over three years, while one-time MORE grants have no maximum size. A third group of grants distributed by the COPS office are commonly referred to as "Innovative Grants". These grants include the Distressed Neighborhood Program (DNP) grants, Cops In Schools (CIS) grants, the Small Communities Grant Program (SCGP), and individualized grants to combat both gang problems and domestic violence. DNP grants were given to 18 selected cities and paid 100 percent of the salary and training costs of a new officer.[10] CIS grants provided up to $125,000 over three years for the salary and benefits of a new trained officer whose sole responsibility was to patrol a local school. Grants could be renewed for two additional years,[11] and reapplication was possible. SCGP grants were introduced in 1998 to help agencies in cities smaller than 50,000 people retain granted officers for one year after a hiring grant expired. Some grants were rejected, but the COPS office actively solicited grant applications. Once an agency received a grant, the amount of paper work required to receive additional grants was minimal- applications were often less than 300 words.[12]

The VCCA states that COPS grants must be used to expand the police force and not supplant officers that would have been hired anyway. Police departments were required to demonstrate that they were now hiring officers above and beyond the number allotted to them by the local government. Most auditing was done from Washington DC, either through email or phone calls.

[10] DNP and individualized grants tended to be larger on average than UHP, MORE, CIS, or SCGP grants, but these vehicles were used infrequently.
[11] In practice, hiring grants were never renewed.
[12] In one instance, a grant of 83 officers was awarded to the Montgomery County Police Department with a one-page application. This was more typical during the first years of the grant program. Interview with Officer Nicholas Tucci, Director of Grants and Budgeting, Montgomery County Police Department

2.3: Previous Literature on the VCCA

Given its size and scope, there is surprisingly little research on the operation and impact of the VCCA. Much of the existing literature has questioned the degree to which hiring grants have expanded the size of the police force. Since money is fungible, many expressed concern that grants would be used to supplant existing funds, notwithstanding the legal directive of the grant. The concern that intergovernmental grants may not increase spending in intended categories is not limited to this program. Following the typology in Wilde (1971), UHP grants are incremental closed-end matching grants that lower the price local forces must pay for a limited number of additional police officers. A standard model of local public finance such as Bradford and Oates (1971) has the unambiguous prediction that UHP grants should have increased the size of the police force. However, a number of authors argue that the difficulties in constructing the counterfactual of what spending levels would have been in the absence of the grant make it difficult to enforce that money is spent in intended areas. McGuire (1978, p. 27) argues that there are a "…variety of steps a recipient, especially a local government, can take to transform a conditional or categorical grant into fungible resources." In fact, McGuire (1979, p.35) argues that "Possibly the greatest opportunity for defeating intended conditional effects occurs in cases where grants are supposed to apply only to increases in local output over current levels", which is exactly the structure of UHP grants. Not surprisingly, there is a large literature in public finance that examines the degree to which matching grant increase recipient spending in intended categories. This literature is summarized in Gramlich (1977) and Hines and Thaler (1995) and recent examples include Baikker and Staiger (2005) and Baker, Payne and Smart (1999).[13]

The possibility of diverting resources to areas other than new hires is pronounced in the case of UHP grants. First, agencies received a lump sum check from the COPS office and were then expected to hire officers with those funds. Second, most police agencies were expanding over the 1990s so it would be easy to re-classify hires that would have occurred anyway as COPS hires. Third, although the COPS office monitored agency-hiring practices, this often consisted of little more than a phone call to the agency to ask the names of the officers hired under the COPS program. The limited evidence to date suggests that the program did not expand the size of the force as the law had intended. In an audit of 147 grant recipients, the Inspector General of the Department of Justice found evidence that in 41 percent of their sample, grants were used to supplant funds rather than expand the police force. Some question whether the COPS hiring grants expanded the police force at all.[14] A report by Davis et al. (2000) notes that between 1993 and 1998 the number of local police increased by about 87,000, which is consistent with historical trends in the size of police forces.

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[13] The literature on the diversion of matching grant funds is similar to the large literature on block grants the 'flypaper effect.' A standard textbook model of public finance suggests that intergovernmental grants are equivalent to increases in constituent income, so economic theory predicts these grants should not increase spending by an amount more than an equal increase in income (Bradford and Oates, 1971). Research on intergovernmental grants does not consistently support this theory, instead finding evidence that ‘money sticks where it hits.’ In their review of the literature, Hines and Thaler (1995) notes the bulk of these studies demonstrate that while local spending increases by about five to ten cents for every dollar increase in income, unrestricted block grants increase spending anywhere from 50 cents to a dollar. The results, however, are not completely one-sided. A number of recent papers including Knight,(2002) are consistent with the original Bradford and Oates model.

[14] The Washington Times, May 9, 1999. "Phantom COPS" B2; The Washington Times, July 27, 1999. "IG Report dashes Clinton plan for 100,000 new police officers; Funding not same as deployment, audit says" A4; The Washington Times, May 19, 2002. "COPS is a Crime" B5. The Washington Times, October 9, 2000, "Where are the 'cops' when you need them": A15

One aspect of the COPS program that has received some research attention is whether the program impacts crime rates. Zhao, Scheider, and Thurman (2002) used data over the 1995-1999 period to examine the impact of COPS grants on crime rates. In their sample of 2,438 cities with populations in excess of 10,000, the authors found that larger per capita hiring grants reduced both violent and property crime rates. However, in their sample of 3,662 cities with populations less than 10,000, the authors found that hiring grants increased both crime rates. In both models, the authors estimated a two-way fixed-effect specification that controlled for year effects (common to all cities) and county-specific effects, plus a control for 1994 crime rates. The paper had a number of shortcomings including the lack of any pre-COPS data in their sample, the use of time effects common to all cities and the lack of city-specific controls (General Accounting Office, 2003; and Muhlhausen and Rector, 2002). In response to one of these criticisms, Zhao and Thurman (2004) estimate similar models to their original papers but instead include city-specific fixed-effects. In this more recent paper, the authors still find a negative impact of receipt of a COPS grant on crime for larger cities. In a more recent effort, the GAO (2005) analyzed the impact of COPS grants on crime. That study used data from the same period and cities from the same population group as ours, plus, the authors noted in the report that they based their estimation strategy on an earlier version of this paper. They used our two-stage least-squares methodology and a very similar procedure to control for time effects. The GAO report choose to use dollar values of grants as the instrument for the size of the police force but their implied elasticities of the size of the force for each index crime were similar to the estimates here. The GAO report did not however cluster standard errors at the agency level so their coefficients are more precise than the ones presented here. [15]

2.4: Data

Our data comes from four sources. Data on crime and police agency size is taken from the FBI's Uniform Crime Reports (UCR) from 1990 to 2001. Data on COPS grants are from the records of the COPS office provided to us for this project. Annual demographic information at the county and Metropolitan Statistical Area (MSA) level is taken from the Census, and annual data on employment and wages at the county and MSA level is taken from the Bureau of Economic Analysis (BEA).

Data on grants from the COPS office was merged with the UCR crime data using an 8-digit agency identification number used by the FBI, called an ORI.[16] In the UCR data in our period of analysis, there are 3,029 agencies and 36,238 observations.

We were unable to match a non-trivial number of grants to agencies in the UCR. The large number of grants that could not be matched back to an agency in the UCR was also noted by Davis et al (2000) in their analysis of the COPS programs. The 2,683 agencies we were unable to match appeared to be primarily small towns that would not appear in our sample of cities with 10,000 people or more, or they were grants to

[15] We have concerns about what model is actually reported in this more recent paper because the authors report a coefficient on the city-specific 1994 crime rate, a time-invariant city-specific variable, even though they claim to have included city-specific fixed-effects.

[16] The COPS program reports information on 26,920 grants given to 11,357 agencies by the end of fiscal year 2001. However, 2,858 agencies in the COPS data set had ORIs that were inaccurate or did not match any ORI in the UCR universe. Using addresses and agency names, we were able to match an additional 205 agencies that received COPS grants to their UCR reports. The unmatched agencies appeared to be primarily small towns that would not appear in our sample of cities with 10,000 people or more, or they were grants to jurisdictions that did not report data to the UCR. To verify this point, we looked up populations counts from the 2000 Census for the 100 unmatched agencies with the largest UHP grants and found that only four were cities or townships with populations in excess of 10,000. The large number of grants that could not be matched back to an agency in the UCR was also noted by Davis et al (2000) in their analysis of the COPS programs.

jurisdictions that did not report data to the UCR. There are 1,865 unmatched grants to local police agencies in 542 cities. Of the largest 100 UHP grants to these cities, only four are to agencies serving populations in excess of 10,000 people in 2000. The average unmatched SCGP grant is $8,769, compared to the matched average of $17,959. The average matched CIS award is just under twice the size of the average unmatched grant, $65,414, compared with $34,128. Matched UHP awards averaged 3.4 officers, and the average grant was for 1 officer. The average unmatched UHP grant was also for 1 officer, with a mean grant size of 1.3 officers. Unmatched grants are very similar in magnitude to grants to agencies smaller than 10,000 people, an average of $36,000 for CIS grants, $13,100 in SCGP grants, and 1.3 UHP officers. This grant comparison strongly suggests that our unmatched grants were likely to fail our population requirement, and their exclusion does not bias our sample.

The UCR contains monthly data on arrests and reported offenses for eight index crimes (murder, manslaughter, assault, rape, robbery, burglary, larceny and vehicle theft), plus basic information about agency employment as of October for a census of all law enforcement agencies operating within the jurisdiction of the United States of America. Not all jurisdictions report data to the FBI, but during our time period of analysis between 88 and 96 percent of the US population is covered by agencies that submit reports to the UCR (Maltz, 1999).

Agencies submitting data to the FBI for inclusion in the UCR include state, county and local police, sheriffs' offices, law enforcement agencies on Indian reservations, college and university police forces, plus many other special jurisdictions. Given the focus of the COPS program, we only use data for local police forces in cities with at least 10,000 residents.[17] For each city in our sample, we construct reported annual crime rates (defined as crimes per 10,000 city residents) for seven of the index crimes listed above.[18] We use reported crime rather than arrests as the key covariate because the latter variable may be mechanically related to the size of the police force. The UCR data provides the size of the sworn police force (those officers allowed to carry a weapon and make arrests) and civilian employees employed as of October of the reporting year. When measuring the size of the police force, we report both of these numbers per 10,000 people in the jurisdiction.

The UCR data is essentially unedited by the FBI and there is tremendous heterogeneity across cities in the quality of the reporting. As a result, the data requires thorough 'cleaning' before use. There are a number of coding errors in the crime and employment data vastly under or overstating crime for a particular jurisdiction. Some states have years in which almost all agencies drastically underreported crimes including Alabama in 1990, Florida in 1996, Wisconsin in 1998, South Carolina in 1991, and South Dakota in 1996 (Maltz, 1999).[19] To identify potential outliers in the data, we regressed violent and nonviolent crime rates in each jurisdiction on a quadratic time trend. If any agency has an observation where the predicted and actual values of the dependant variable for either crime rate differed by more than 50 percent in absolute value, we visually inspected the time series for all index crimes for that agency.[20] In some cases, it

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[17] Levitt (1998a) documented that small police agencies shut down at night. A hiring grant may allow these agencies to employ a night shift, mechanically increasing the number of crimes recorded by the agency.
[18] Manslaughter, as recorded in police reports, is accidental death. Because accidents are not preventable crimes, papers estimating the impact of public resources on crime typically delete this from the analysis.
[19] Some of the error may be due to adoption of new computerized reporting systems in the early 1990s. Helen Balok, of Fort Colins, CO Police Department.
[20] Less than 5 percent of the observations have more than 50 percent absolute difference between predicted and actual values. The 50 percent rule also captures nearly all of the underreporting in the particular states listed above (e.g., Alabama in 1990, Florida in 1996, etc.)

appeared as if agencies used 999 or 99999 instead of a missing value code, and in these instances, we simply edited the data. [21] In other instances it is clear that a keypunch error occur but it is not obvious how to adjust the data. In these instances, we excluded those years of data from our regressions. Any agency that had four or more years where the predicted and actual values differed by more than 50 percent, we deleted the entire time series. We also delete data for 24 agencies that ever report employing no police officers. Finally, we follow the FBI's procedures for constructing national crime estimates and delete data for Illinois in estimates of rape.[22] Eliminating agencies with unreliable crime data reduced the data set to 26,737 observations from 2,360 agencies.

Finally, we deleted 26 agencies that did not have any observations before 1994 when the first COPS grant was awarded. Our final sample contains 23,335 annual observations from 2,074 agencies from 1990 to 2001. Our sample represents 65 percent of the cities in the U.S. with populations larger than 10,000 people, 82 percent of the population living in these cities, and 44 percent of the US population was policed by these agencies in 2000.

Crime rates increase dramatically with city size and because of this we have constructed five sub-samples based on population: jurisdictions larger than 250,000 people, between 100,001 and 250,000 people, between 50,001 and 100,000, between 25,001 and 50,000, and 10,001 to 25,000 people.[23]

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[21] The Hillside Township, NJ, Police Department monthly rape reports for 1993 are as follows:
Jan: 1 Feb: 1 March: 2 Apr: 999 May: 1 June: 0 July: 1 Aug: 1 Sep: 0 Oct: 2 Nov: 1 Dec: 0.

[22] Illinois has a broad, gender-neutral definition of rape that does not correspond to the definition used by the FBI. As a result, Illinois police agencies do not report any rapes to the FBI. There are only 4 Illinois agencies in our sample.

[23] For the few agencies whose populations grow or decline outside of those boundaries in our time period, they are placed in the population group that they fall into the majority of the years.

Table 2-1 reports basic characteristics of the agencies included and excluded from our analysis. Excluded cities, especially those in the smallest, largest, and 100,001 – 250,000 size group, are significantly smaller than those included in the analysis. The smallest size group and cities between 100,000 and 250,000 thousand people were significantly smaller than included cities, and in all cases there is a statistically significant difference between the number of officers per capita in included and excluded cities. This difference is not surprising, since agencies with relatively fewer employees are much more likely to report sporadically to the UCR.

Because annual demographic data is not available for all cities, we use MSA-level data for any agency serving populations of 100,000 or more, and for smaller cities, we use county-level data. Information about the percentage of the population between 15 and 24 years old and the percentage of the population that is black was obtained from the U.S. Bureau of the Census. From the Bureau of Economic Analysis (BEA) Regional Economic Information System,[24] we downloaded data on per capita income, the average salary of paid jobs eligible for unemployment insurance (UI), and the number of jobs eligible for unemployment for UI. We divided this last number by the number of people aged 20-64 to obtain a proxy variable for the employment to population ratio that we call "jobs per adult". All dollar values are converted into real 2000 dollars using the Consumer Price Index.

Table 2-2 displays some basic characteristics of the cities in our sample and the COPS grants these agencies received. In the first three lines of the table, we show the number of cities in each population group, the number of officers, and the number

[24] http://www.bea.gov/bea/regional/reis/.

officers per 10,000 people in the 1993. Small cities dominate our sample and the largest cities have 50 percent more police per capita than the next largest cities.

In the middle third of the table, we report some descriptive statistics about UHP and DNP hiring grants. Recall that the COPS office paid for 75 percent of the cost of UHP hires and 100 percent of DNP hires. To allow us to aggregate these two different grants, we constructed a variable that measures the *Paid officers granted* per year, calculated as follows:

*Paid officers granted = 0.75*UHP Hires + DNP Hires*.

The broad scope of the hiring grants provided by the UHP and DNP programs is evident. Nearly all cities in our sample received hiring grants, but the fraction decreased with city size. All but one city in the largest size group receives a hiring grant, and 89 percent of cities with populations between 10,000 and 25,000 people received a grant. The bulk of the officers granted went to the 61 largest cities, with hiring grants providing funds for roughly 5,550 new officers per year. By comparison, funds for 4,400 officers per year were provided to all the other 2,013 cities in the sample. On a per capita basis, the hiring grants were more heavily concentrated in the largest and smallest cities, with the grants adding about one and a quarter officers per 10,000 people in cities that received a grant. The hiring grants program peaked in 1998, when the COPS program provided funds for about 5 percent of police officers nationwide. The size of the program began to shrink in 2000, and in 2001 only about 2 percent of all police officers were COPS officers.

MORE grants were not distributed as widely as hiring grants. Over 90 percent of the largest cities received at least one MORE grant, but that fraction declines rapidly as city size falls, with only 14 percent of the smallest cities receiving MORE grants. For those agencies that received a grant, the size of MORE grants per capita is U-shaped. Conditional on receipt of a MORE grant, the average per capita dollar amount of a MORE grant is similar to a hiring grant.

In the next two sections of the paper, we examine whether COPS grants increased the number of sworn officers employed by departments and whether the grants decreased crime. We then turn our attention to the flow of grants, allowing the magnitude of increases and decreases in hiring grants to vary. In all cases, we use a within-group model that examines whether outcomes change as agencies acquire grants over time. One concern with this model is that the number of sworn officers was increasing and crime was declining rapidly during the 1990s. Adding year effects to models can capture time series patterns common to all agencies, but if agencies that were experiencing the greatest decline in crime were receiving the largest grants, then the within-group model would overstate the impact of receiving a COPS grant. Likewise, if grants were distributed to those cities with the fastest growth in officers per capita, then the model would overstate the impact of grants on hiring.

In Table 2-3, we examine this issue in detail and test whether the size of COPS and MORE grants are correlated with the level and time trends in crime rates and the size of the police force. For this table, we aggregate total officers granted per 10,000 over the 1994-2000 period and all MORE dollars per person over the same period and regress these cross sectional values on pre-treatment levels and trends in key variables such as 1993 demographic characteristics, the number of crimes and officers per 10,000 people in 1993, the percentage change in these two variables from 1991 to 1993,[25] plus dummy

[25] We use 1991 rather than 1990 for these percentages because crime rates peak in 1991 and start to fall afterwards.

variables for the city size groups listed in Table 2-1. The estimates from this model are similar in concept to the work of Choi, Turner and Volden (2002) who used data from a cross-section of 853 cities to examine what predicts whether a city applied for funds. The authors found that cities with larger populations, a lower fraction minority population, higher murder rates, higher unemployment rates and cities with elected mayors are more likely to apply for grants. Using a smaller sample of 261 successful grant applications, the authors found that the fraction of the request granted was higher in cities with a lower fraction minority, larger populations, a higher fraction unemployed and in cities that are in states with a 'vulnerable' Democratic senator in the 2000 election.

There is a statistically significant positive relationship between crimes per capita and the total amount of officers granted by the COPS office. Translating these gradients into elasticities, both values are roughly 0.4, meaning that a ten percent higher crime rate or ten percent larger police force translates into 4 percent more hiring grants per capita. The fact that the COPS program distributed larger grants to cities with higher crime rates or already larger police forces should not contaminate our econometric model because we can control for these differences by adding city-specific fixed effects to each model.

The hiring grants are unrelated to any pre-existing crime trends or trends in police hiring. The covariates are measured as fraction changes, so a 20 percent change in crime between 1990 and 1993 is recorded as 0.2. The coefficients on the trends in crime and police hiring are small and statistically imprecise. The magnitude of the coefficient on trends in crime suggests that moving from the 25^{th} to 75^{th} percentile city in crime growth, a 40 percentage point change in growth rates, would generate a loss of 0.06 officers per

10,000 people a change in grant size of only 1.2 percent of the sample mean.[26] Likewise, grants were not directed towards agencies whose growth was being outpaced by their population, nor were they directed at agencies that were already stepping up their hiring. Holding other factors constant, hiring grants are negatively related to population, which is to be expected since funds were set aside for smaller towns. However, cities with greater population growth received larger grants.

These results are not sensitive to alterations to this model. For example, in probit models where the outcome is whether a city received a grant, we find that crime growth rates or employment growth rates are not statistically significant predictors of the outcome. Our results are also not sensitive to the time period over which we measure change, nor are they sensitive to how we construct our change measure. For example, in some models we regressed levels of crime in the 1990-1993 period on a linear trend and included the coefficient on that trend in the regression. This coefficient is statistically insignificant. The coefficient on percent changes in crime from 1985-1991 is 0.047 with a standard error of 0.082. We also limited our measure of crime to violent crimes and found similar results. Because the 1993 levels of crime and police employment are mechanically correlated to growth rates in those variables before 1993, it is possible that our inability to find a significant relationship between crime rates and grant size is due to strong covariance between these variables. Estimating a model of total officers granted per 10,000 residents without crime and employment levels increases the coefficients (standard errors) on the crime and employment growth variables to 0.76 (0.73) and 0.66

[26] There is some concern that the estimate is too imprecisely estimated to say anything definitive about whether grant size is correlated with trends in crime. We do not believe this is the case. Using the upper bound on the 95 percent confidence interval for this estimate suggests that even a city with a 40 percentage point increase in crime produce a decrease in the average grant size of about 0.51 police per 10,000 people, which is about 10 percent of the sample mean.

(0.88) respectively. In this case, the same 40 percentage point increase in crime rates considered above will decrease average grant size by 0.3 officers per 10,000 people, which is only 6 percent of the sample mean.

We find similar results for the size of MORE grants. The only notable difference is that we precisely estimate that a 10 percent increase in the black population in 1993 is correlated with 4 percent decrease in total MORE grant receipt. We did not find evidence of a relationship between the size of the black population and hiring grants. In addition, we do not find any relationship between population size and MORE grant receipt, which we did for hiring grants.

The fact that funds do not appear to be distributed in a systematic way commensurate with expected future need is not surprising. The anecdotal data suggest and our conversations with numerous police agencies confirm that there were few barriers to any agency successfully applying got a UHP grant. The slow pace with which funds were originally dispersed actually lead the COPS office to encourage agencies to apply. One agency we spoke with had originally requested funds to hire new officers and was subsequently called by the COPS office and asked why their request was so small. Later in the conversation, the COPS office asked the budget director how many more police they though the department needed. The agency then received a check a few days later for that exact amount. What seemed to separate agencies getting larger or smaller grants was simply their moxie in asking for funds.

2.5: Did COPS Increase Police?

2.51: Econometric Model

The goal of the COPS program was to place 100,000 more police officers on the streets by the end of fiscal year 2000. To date, the COPS office has distributed funds to hire 64,000 new officers under the UHP program.[27] The office also argues that funds from MORE grants have allowed local agencies to deploy 42,000 full time equivalents.[28] However, audit data summarized above suggests that in some cases, COPS funds were never spent or the grants were used to supplant hires rather than to expand the force. In order to use COPS hiring grants to estimate the effect of number of police on crime, we must first establish that these grants caused non-trivial changes in the number of police. To date, there has been no systematic statistical evidence estimating the extent to which the COPS program expanded hiring. In this section, we estimate models similar to those used by researchers who are concerned with whether earmarked grants are spent on their intended purpose.

The unit of analysis in our work is a city police agency (i) in a year (t). We control for the permanent differences in characteristics in agencies by including a complete set of city-specific fixed effects. The basic econometric model we employ is therefore a within-group estimator where we examine whether the number of sworn officers employed increases when agencies receive grants, holding constant the relative size of police forces.

The fixed-effects model described above will not provide consistent estimates of flypaper COPS if the sizes of the grants are correlated with unmeasured trends in the size

[27] http://www.cops.usdoj.gov/Default.asp?Item=53.
[28] http://www.cops.usdoj.gov/mime/open.pdf?Item=319.

of the police force. The COPS hiring grants were distributed during a period when police forces expanded by 17 percent nationally. If agencies with the fastest growing forces also received the largest grants, not controlling for trends in hiring would possibly overstate the hiring effect of grants. We could control for some of these trends in hiring by adding year effects to the model, but these dummy variables would only capture the trends in hiring common to all agencies, an assumption that is unlikely to be correct since we have over 2,000 agencies in our sample. We have therefore adopted a procedure that allows for heterogeneity in year effects across cities based on their size and similarity in pre-treatment trends in crime rates and the size of the police force. Specifically, for each police agency, we estimate a model of the crimes per 10,000 people and police per 10,000 people for the 1990 to 1993 period on a linear time trend. For each of the five city-size groups (g) listed in Table 2-1, we grouped the crime and police growth rates into M_g groups based on the ranking of growth rates for each outcome. For each city size group g, a city could fall into one of M_g^2 'cells'. For example, a city could fall into the fastest-growing crime rate group but the lowest-growing officer growth rate group. We picked the largest M_g such that each of the M_g^2 cells would have at least two cities. Across the five city size groups, from largest to smallest populations, the value of M_g^2 that we use are 9, 16, 25, 36 and 100,

The basic first-stage regression we estimate is of the form:

(1) $$POLICE_{it} = X_{it}\theta_1 + \textit{Paid officers granted}_{it-1}\beta_1 + u_{1i} + v_{1gjkt} + \varepsilon_{1it}$$

where *POLICE* is the number of sworn officers per 10,000 people in agency i in year t, X is a vector of time-varying economic and demographic characteristics for the city, *Paid officers granted* is measured in the previous calendar year, u_{1i} is the agency fixed-effect,

v_{1gjkt} is the year effect for a city in size group g, growth rate group j and police growth rate k in year t, and ε_{1it} is the idiosyncratic error. Employment is measured as of October of a given year and there can be large delays between the receipt of a grant and an agency's actual hiring of an officer; therefore, we choose to lag the grants by one year.

Recall that UHP grants are the overwhelming type of grant received, and these grants only pay for 75 percent of the cost of new hires. Recall also that *Paid officers granted* is measured as the number of new officers paid for by COPS grants, so if an agency received money for 4 new officers per 10,000, then *Paid officers granted* is 3. With this specification, if grant recipients are meeting the letter of the law, then the coefficient on β_1 should be equal to roughly 1/0.75 = 1.33.

2.52: Results

The basic first stage results for all 2,074 cities in our sample are displayed in Table 2-4. In these models, we include as covariates the log per capita income, the log salary of jobs covered by unemployment insurance, the jobs/adult ratio, the fraction black, the fraction between 18 and 24 years of age, agency fixed-effects and year effects that vary by the city size and the pre-COPS growth rate cell groupings. Standard errors allow for arbitrary covariance in errors within a city.

In the first row of Table 2-4, we report the coefficient on lag officers granted per 10,000 people and in the first column, we include no other COPS grants as covariates. The results from this exercise are consistent with the previous flypaper literature in that most of the money for new cops seems to have stuck. The coefficient on this variable is roughly 0.7 with a small standard error, indicating that 70 percent of the money delivered to agencies went to expanding the police force.[29] However, the results indicate that police agencies were far from following the letter of the law. A city that received a grant for 4 cops per 10,000 people was paid enough money for 3, and according to the coefficient estimates in column 1, added on average 2.1 more officers to the force. We can easily reject the null that the coefficient is 1.[30] Therefore, while receipt of a COPS grant expanded the police force, it did not increase the number of police to the degree the program was designed. In fact, the size of the force expanded by about half as much as the law intended.

In column (2), we examine whether the estimates are sensitive to adding additional covariates that measure receipt of other COPS grants. Specifically, we add lagged measures of MORE, CIS and SCGP grants per capita. Receipt of a MORE or SCGP grant has no statistically precise impact on the size of the police force, but there is some indication that receipt of CIS grants adds police to the force. Controlling for these additional COPS grants does not appreciably change the coefficient on lagged paid officers. In the third column of Table 2-4, we include 1, 2, and 3 year leads of the announcement of a COPS hiring grant. The size of a COPS hiring grant that is about to be announced does not contain any information about the size of the contemporaneous police force.

The number of civilian employees and police officers in any agency are highly correlated. Going from largest to smallest city groups, these five correlation coefficients

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[29] When we estimate the model with contemporaneous officers granted as the key independent variable, we find similar results- for each paid officer granted, the police force expands by 0.685, with a standard error of 0.072. Because police employment is measured as of October and crime is measured January to December, we lag the grants by one year for the remainder of our study.
[30] If we estimate a model using *officers granted*, as opposed to *paid officers granted*, the coefficient on our key independent variable falls to 0.517, approximately 75% of our baseline model. The standard error on *officers granted* is 0.058.

are 0.95, 0.46, 0.49, 0.64, and 0.60. One might be concerned that the large coefficient on the paid officers granted in the police regressions is picking up the fact that agencies that would have grown faster anyway are the ones receiving grants. If this were true, given the strong correlation between civilian and non-civilian employees, we would expect to find a large coefficient on the officers granted coefficient in the civilian employees regression as well. As a specification check, we estimated a similar model but with police department civilian employees per 10,000 people as the dependent variable. These results are reported in the final two columns of Table 2-4. The coefficient on lag paid officers in the civilian employee's regression is small and statistically imprecise. Looking at column (1) and (4) of Table 2-4, the impact of the COPS hiring grants is 16 times larger in the officers versus civilian employees regression. The results in column (5) of the table indicate there is some evidence that MORE and CIS grants increased civilian employment.

If every officer granted only increased the size of the force by 0.7 officers, an important question is then where did the rest of the money go? Recent studies have argued that firefighter employment is correlated with police employment, due to the tendency of local governments to have a relatively constant preference for public expenditure for all "first responders" (Levitt, 2002). We supplement the data used in this analysis with information on the number of fire fighters per capita for all cities in excess of 100,000 people for the years 1992 to 2001 from the Annual Survey of Governments. Prior to 1994, the correlation coefficient between fire fighters and police per capita in these large cities is 0.65. Table 2-5 presents the results of models similar to equation (1) above, where the dependant variable is not total police employment, but the number of city fire fighters per 10,000 people. The coefficient on lagged officers granted in the largest cities is 0.132 with a standard error of 0.155, and 0.285 with a standard error of 0.145 for cities between 100,000 and 250,000. Additionally, the coefficient of lagged hiring grants on police officers plus firefighters per 10,000 residents in both city groups is statistically indistinguishable from both 1.0 and 1.33. These results are at least suggestive that local governments used funds from UHP grants to increase fire fighter employment. It is also likely that cities that received COPS hiring grants may have been more likely to apply for federal grants to hire additional fire fighters.[31]

2.6: COPS and Crime

2.61: Econometric Model

As we noted in the introduction, there is limited evidence suggesting that a larger police force will reduce crime. As previous researchers have demonstrated, cities with greater crime rates tend to hire more officers making it difficult to isolate the causal impact of police on crime. The results from the previous section demonstrate that for most city sizes, the police force increased by about 0.7 officers for every officer paid for by a COPS hiring grant. Our results in section 2.4 indicate that number of police officers granted was not related to trends in crime. These two facts suggest that the COPS program can potentially be used as quasi-experimental variation in the size of police forces to examine the impact of hiring more police on crime.

The basic equation of interest is one that relates the size of the police force to levels of crime, or:

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[31] We would like to thank Michael Herman, of the Federal Emergency Management Agency, for insights on the relationship between federal grants for fire fighters and police officers.

$$(2) \qquad CRIMERATE_{it} = Xit\theta + POLICE_{it}\ \beta + u_i + v_{gjkt} + \varepsilon_{it}$$

where *CRIMERATE* is some measure of reported crimes per 10,000 people in city i, in year t. Even with the year and detailed time fixed effects in the model, we anticipate that OLS estimates (of β are) inconsistent because sudden shocks to crime (ε_{it}>0) may encourage departments to hire more officers, thereby understating the possible crime-fighting benefits of more police. COPS hiring grants change the size of a police force, but are unrelated to changes in the crime rate. We estimate a reduced-form relationship between COPS grants and crime rates as

$$(3) \qquad CRIMERATE_{it} = Xit\theta_2 + \textit{Paid officers granted}_{it\text{-}1}\ \beta_2 + u_{2i} + v_{2gjkt} + \varepsilon_{2it}$$

Equations (1) and (3) can be used to obtain consistent estimates of the (parameter β by) noting that the 2SLS estimate (of β in) this exactly identified model is simply the ratio of (estimates $\beta = \beta_2 / \beta_1$.)

In the top half of Table 2-6, we present the estimates of equation (3) for the seven index crimes plus aggregates of property and violent crime. Crime rates are measured in crimes per 10,000 people and means of crime rates and police per 10,000 for the year just before the COPS program was started are listed in the bottom of the table.

Reading across the first row of results, we find that receipt of a COPS hiring grant generates reductions in all crime rates. These reductions are statistically significant in the case of burglary, auto theft, robbery, and assaults, and of marginal statistical significance for murder. The average COPS hiring grant was about 1 officer per 10,000 people. Dividing the coefficients by mean crime rates, we find that the average COPS grant reduced burglaries by 2.2 percent, auto thefts by 3.3 percent, robbery by 5 percent, murders by 3.2 percent, and assaults by 3.6 percent. Aggregating index crimes into property and violent crimes, we find that an average hiring grant will generate a statistically significant reduction in violent crimes by 3.7 percent and a reduction in property crime of 1 percent. This last result is only statistically significant at the 10 percent level, and the magnitude is driven primarily by a small effect of hiring on larcenies, by far the largest crime rate in the property crime group.

In the second panel of estimates in Table 2-6, we report 2SLS estimates of the equation (2) using the officers granted as an instrument for the number of police. These numbers are equal to the division of the reduced-form estimates by 0.689. Below the 2SLS estimates, we scale the parameter values so as to turn them into elasticities, representing the percentage change in crime generated by a percentage change in police. We find that the elasticities for burglary, auto theft and larceny are roughly -0.6, -0.8 and -0.08, respectively, with a total elasticity for property crime of -0.26 with a t-statistic of -1.74. The elasticities for robbery, murder and assaults are fairly large, and the aggregate elasticity for violent crime is -0.99 and this estimate is statistically precise at conventional levels. These elasticities are incredibly similar to the estimates in Levitt (1997, 2002).

Many jurisdictions that received hiring grants also received other types of COPS grants. The 2SLS models linking police and crime are only consistent if there is no other factor directly impacting crime that is correlated with the size of the hiring grants.[32] To

[32] If for example, MORE and hiring grants are correlated and MORE grants help reduce crime by making police more efficient, then the 2SLS model would overstate the impact of expanding the police force.

examine this question, we estimate models similar to the reduced-form models in Table 2-6, but we include MORE, CIS and SCGP grants. All values are recorded as dollars received per person, and we lag the values one year to allow time for the grant money to be spent. Because these additional grants will impact crime throughout some avenue other than new police hires, the 2SLS model makes little sense in this context so as a result, we only report reduced-form estimates where regress crime rates on grants.

In previous research, authors have examined the impact of MORE type grants on crime by adding a covariate that measures MORE funds per capita received during a particular period. This type of specification may ignore the particular characteristics of these grants. MORE grants were one time grants to complete a specific project, but our data set does not tell us what that project was, or even if the grant money was spent by the police agency.[33] These grants allowed a law enforcement agency to make an otherwise infeasible financial investment that would save officers' time, allowing them to spend more time on the beat.[34] If a MORE grant is used to make a capital investment, which the COPS office suggests is the primary use, which capital investment will continue to affect crime for many years. To examine this possibility, we include in our models the first three lags of MORE spending per person.

2.62: Results

The estimates for these reduced-form models are reported in Table 2-7. In general however, the results in Table 2-7 are similar to those in Table 2-6. Focusing on the MORE grants, we find the coefficient on this variable is statistically precise and negative in equations for burglaries, auto thefts, larcenies, robberies, rapes, property

[33] The Montgomery County Police received a MORE grant that was spent by the department of corrections.
[34] This point was emphasized by Carol Jiacinto, Director of Fiscal Affairs for the Prince George's County Police Department

crime and violent crime. In many cases, the coefficients on the lags of the MORE grant increase with the length of the lag, suggesting that the technology investments are durable and it may take some time to integrate into police work. The mean size of MORE grants are about $1 per person per year, meaning that in the first year after a grant, receipt of an average sized grant reduced burglaries by 0.5 percent, auto thefts by 0.8 percent, larcenies by 0.3 percent, and robberies by 1.5 percent. We find no evidence that CIS or SCGP grants reduced crime at all – although most coefficients are negative, the standard errors are typically larger than the parameter estimates. Therefore, there is not enough power in our model to say anything conclusive about these types of grants.

Our results hold up well to a series of specification checks. Because crime rates fell dramatically over the 1990s, our biggest concern is that failing to adequately correctly control for this decline overstates the benefits of the COPS program. Table 2-8 displays the results of eight major alterations of the model. In Row (1), we display the baseline results from the reduced-form models in Table 2-6, in which we include agency specific fixed effects and time effects that vary by population group and pre treatment trends in crime and police employment. In the second row, we exclude any control for pre-treatment trends and only include year effects that vary by city-size group. As expected, this increases the magnitude of our coefficients. In addition, our estimate of the effect of hiring grants on murder becomes statistically significant. Because property crimes, specifically larceny, are more than 3 times as prevalent as violent crimes, there is a concern that trends in larceny are dominating our matching process. In order to address this, in Row (3) we match agencies based on pre-grant trends in each individual crime. This does decrease the magnitude of our coefficients, but receipt of COPS grants is associated with a statistically significant decline in burglaries, car thefts, robberies, and assaults.

The size and scope of drug markets vary regionally, we include fixed effects that vary by year, city size, and census division in Row (4), which increases both the magnitude and precision of our estimates. In Rows (5) and (6), we used crime rates in the 1990-1993 period to construct city cell groups. The period is a short one but it is a period when crime was declining. Cities that experienced the largest increases in crime in the late 1980's had the sharpest declines in the 1990s. To broaden the years of data used to construct the cell groups, we group agencies according to their crime trends from 1985 to 1990 (Row 5) and from 1985 to 1994 (Row 6). Controlling for 1980s crime trends causes the sign of the larceny coefficient to reverse, but the categories that were statistically significant in row (1) are statistically significant in Rows (5) and (6). In Row (7), we address the spatial aspect of COPS grants by controlling for hiring grants given to other agencies operating in the same county in year t-1. This does not affect our estimates, and in fact slightly increases the magnitude of our reduced form estimates.

As we noted above, because of problems with the UCR data, our sample only represents 65 percent of cities with populations in excess of 10,000 people. Our results could be driven by the fact we have a selected sampled. As we note in Table 2-1, our sample is much more complete when we consider cities with populations in excess of 250,000. In that city size group, we have data for 97 percent of cities and the same fraction of population. In Row (8), we estimate reduced-form models for only the largest cities. Three results are of note for this subsample. First, because crime rates in these largest cities are substantially higher that for the full sample, it is no surprise that the reduced-form coefficients are now larger. Second, the same crime rates that showed statistically significant reduced-form results in Row (1), burglaries, robberies, auto thefts and assaults, are statistically significant in this sample as well. Third, receipt of UHP grants generates statistically significant reductions in both the aggregate violent and non-violent crime rates.

In the final two rows, we address concerns that these hiring grants are the result of a spurious decline in crime rates over the 1990s by including not only agency specific fixed effects, but agency specific linear time trends. This increases the explanatory power of the model considerably for a panel data set. For example, the R^2 on the non-violent and violent crime models increase from 0.93 and 0.90, respectively, in Row (1) to 0.95 and 0.94 in Row (10). Because this leaves little variation to be explained in our model, it is no surprise that the magnitude of our coefficient estimates falls dramatically. Although receipt of a UHP grant is estimated to reduce auto thefts, robberies, murders and assaults, none of these estimates are statistically significant. Because much of the variation in crime rates and hiring grants occur in the largest cities, In Row (10), we present reduced-form estimates for cities with populations in excess of 250,000 people where we also include agency-specific time trends. In these models, the t-statistics for the *Officers granted* variable has a t-statistic of -3.3 in the auto theft equation and -1.9 in the assault model. In the aggregate crime equations, we can reject the null that the coefficient on *Officers granted* in the non-violent crime model is zero at the 95 percent confidence level, but the t-statistic on the corresponding coefficient in the violent crime model only has a t-statistic of -1.44.[35]

[35] We only present results for seven of the eight FBI index crimes. In police reports, manslaughter is unpreventable, accidental death so n increase in the size of the police force should not have a direct effect

2.7: Conclusion

The COPS program has been criticized by a number of groups with a chief concern being there is no evidence the grants were used for their intended purpose, and evidence on the effect of these grants on crime rates is mixed. We find that these grants tend to increase the size of the police force but the effect is only on half of what is expected if local police agencies were following the letter of the law. For every 4 police officers granted throughout the UHP program, grants paid for 3 and police departments hired 2. We find no evidence these grants are used to expand the number of civilian employees working for the police department. Some of the lost dollars may have been used to hire fire fighters, suggesting intragovernmental competition for grant revenue. Grants not designed for hiring police tended not to increase the size of the force.

One of the primary functions of local governments is the provision of public safety. In 2000, local governments spent $36 billion on policing activities. Local jurisdictions have also shown a willingness and ability to spend more on police over time: between 1990 and 2000, real spending on local police increased by over a third and the number of sworn officers increased by almost 20 percent. Despite this increase in spending, there is surprisingly little evidence that more police generate lower crime rates. Deciphering the impact of police on crime is complicated by the fact that crime rates and police levels are simultaneously determined. In this paper, we have attempted to use the unique structure of the COPS program to break the simultaneity problem and examine the impact of more police on crime. These grants increased the size of the police force in a

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on accidental deaths. When we estimate a model of incidents of manslaughter per 10,000 people, we find that the coefficient on *Officers granted* 0.12, with a standard error of 0.11. We feel this is strong evidence that COPS hiring grants were distributed in a way that is uncorrelated with changes in public behavior or attitudes towards risk, which might otherwise bias our results in favor of finding that police deter crime.

non-trivial way, and the size of the grants was not correlated with changes in crime rates. Therefore, if an increase in the number of police decreases crime, we should see a reduction in crime in cities that received grants.

We find statistically precise negative drops in crime in the years following receipt of a COPS hiring grant in four of seven index crimes (auto theft, burglary, robbery, and aggravated assault), and the magnitudes are in line with other research that uses quasi experimental variation in the size of the police force. We also find that COPS grants that allowed agencies to invest capital in new policing technology generated small but statistically precise drops in the same four index crimes plus larcenies.

Did the costs of the COPS program outweigh its benefits? Based on the 1997 Law Enforcement Management and Administration Survey, a high estimate of the marginal cost of hiring an additional officer is \$54,000.[36] This implies that the total cost of the

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[36] The cost of an officer is more than their base salary. Using data from the 1997 LEMAS survey, we estimated the following equation:

$$TOTEXP_i = \alpha + FTO_i\,\beta_1 + PTO_i\,\beta_2 + FTC_i\,\beta_3 + PTC_i\,\beta_4 + POP_i\,\beta_5 + \varepsilon_i$$

Where $TOTEXP_i$ is the total expenditure of agency i, FTO_i is the number of full time officers employed by agency i, PTO_i is the number of part time officers employed by agency i, FTC_i and PTC_i are the civilian equivalents, and POP_i is the number of people in agency i's jurisdiction. Total expenditures include the sum of all wages, salaries and benefits paid to employees in FY 1996, operating expenses, and investment in new equipment. Equipment investment is defined by the LEMAS survey as purchases "with a life expectancy of five years or more," so in order to get annual marginal cost, we divide that number by five. Agencies that indicated that they did not include benefits in their gross wages and salaries were assumed to pay an additional 30% in benefits, slightly more than the 15-20% suggested by the LEMAS questionnaire. The coefficient of interest, β_1, varied between \$46,000 and \$54,000, depending on our assumption of the magnitude of these non-reported benefits.

To calculate the total cost of the COPS program, we multiplied 54,000 by the number of paid officers granted to our sample between 1994 and 2001 and by 0.7, a high estimate of the fraction of paid officers who were hired. The total cost of the COPS hiring programs is therefore the cost associated with all officers hired by the program plus 30% of the monetary value of each grant, which "leaked", and is simply a cost for the federal government. Recall that 0.75 paid officers were granted for each "officer" granted by the UHP program, and we accordingly use the coefficients of paid officers granted on crime in order to calculate the benefits of the COPS program. The total benefit of the COPS hiring grants is therefore

$$\sum_c SC_c \sum_t \sum_i \beta_c \textit{Paid officers granted}_{it}$$

hiring grants in our sample is approximately $4.4 billion, an amount consistent with the COPS officer's expenditure reports. In order to get a low estimate of the benefit of the COPS hiring grants, we will use the coefficients of officers granted on crimes from model (4), in which we control for pre-existing crime-specific trends. A back of the envelope calculation, based on cost per crime estimates in Miller et al. (1996), suggests the minimum net benefit associated with the COPS hiring grants is $3.4 billion. Assuming that the increase in police officers attributable to COPS hiring grants had no impact on larcenies or rapes, the minimum net benefit is estimated to be $2.9 billion. In our sample, we find that the costs incurred by the COPS office and local governments by UHP and DNP grants are outweighed by the monetary benefit of the resulting reductions in crime.

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where *Paid officersgranted*$_{it}$ is the number of paid officers granted to agency i in year t, β_C is the coefficient of officers granted on crime c, and SC_C is the social cost per crime of crime c, as calculated by Miller et al (1996). All values are in real 2000 dollars.

Chapter 3: Temporary COPS and Permanent Police: the Asymmetric Impact of the Universal Hiring Program

3.1: Introduction

Understanding how local governments respond to reductions in earmarked grants is an increasingly relevant issue. The National Governors Association reports the elimination or reduction of 150 non-defense, non-homeland security federal grant programs in the 2006 Fiscal Year.[37] The reduction in intergovernmental grants affects almost all areas of public good provision, including health spending, education, and transportation.[38] Federal assistance for police protection has been hit especially hard. The Universal Hiring Program, which at its peak provided funding for more than 5% of the nation wide police force, was eliminated in the Justice Departments FY 2003 budget request. In the FY2006 budget, three additional large grant programs that provided money for local law enforcement (the Edward Byrne formula grant program, Local Law Enforcement Block Grant program, and State Criminal Alien Assistance Program) were eliminated. How have these reductions in federal support affected the provision of public goods?

In this chapter, I evaluate the ability of the federal government to generate permanent changes in local government spending. Specifically, I will focus on how city police employment was affected by increases and decreases in intergovernmental grants

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[37] LaPaille, C. (2005). The Presidents Proposed Fiscal Year 2006 Budget. National Governors Association Backgrounder

[38] According to reports by the National Governors Association, a total of almost 16 billion has been cut is cut from federal assistance to state provision of Medicaid and Medicare. State bonuses for reducing out of wedlock births, as well as the 627 million dollar Community Services Block Grant program have been eliminated. Funding for forty eight education grant programs, including the State Grants for Safe and Drug Free Schools program, has been eliminated, and funding for 16 additional programs has been reduced, for a decrease in federal transfers of 4.7 billion dollars. Grants for adult employment training have been reduced by 146 million dollars. Federal aid for highways has been reduced by 17%, and the size of the Airport and Airways Trust Fund, which distributed grants to airports, has been reduced by 14%.

made through the Universal Hiring Program (UHP), which were distributed by the Department of Justice Community Oriented Policing Services (COPS) Office from 1994 to 2004. I find that every officer granted by the COPS office to local police agencies through the Universal Hiring Program (UHP) increased the size of the local police force by 0.6 officers while the grant was active. My estimates suggest that a government that received a temporary UHP grant permanently increased the number of police on the streets, and that recipient cities continued to see reductions in crime due to the UHP program even after federal funding was rescinded.

In the previous chapter, my coauthor and I present evidence that UHP grants were effective at reducing crime, but make no statement about the mechanism driving this relationship. The crime reduction due to increased police presence is treated as a black box. By distinguishing between temporary and permanent changes in the police force, I am able to partially disentangle crime reductions due to increased deterrence of criminal behavior and increased incapacitation of criminals. This methodology is similar to that used by Kessler and Levitt (1999) to distinguish between the incapacitative and deterrent effect of sentence enhancements. A temporary grant encouraging local governments to hire police can decrease crime through two mechanisms, deterrence and incapacitation. An increase in officer presence may cause potential offenders to not commit crimes by raising the probability of being arrested and punished. If larger police forces arrest more individuals per capita than smaller forces then crime rates will also fall due to increased incapacitation. Using a stylized model of policing I take advantage of the dynamic nature of UHP grants to disentangle the crime reductions into incapacitative and deterrent effects. In cities that receive a UHP grant, the changes in crime over time suggest that increasing the number of police will reduce violent crime primarily through increased incapacitation. Deterrent effects are relatively more important for property crime reduction.

<u>3.2: Existing Research on Asymmetries in the Flypaper Effect</u>

Approximately 0.7 new city police officers were hired for every paid officer granted by the COPS office. Contrary to the theoretical predictions of Bradford and Oates (1971), we find empirical evidence suggesting that city governments do not treat revenue from closed ended matching grants as fungible. Many of the postulated theoretical explanations for the apparent "flypaper effect," summarized in Hines and Thaler (1995), focus on repeated game frameworks. In this context, the local government's permanent revenue source, taxes, is temporarily augmented by an earmarked intergovernmental grant. Chernick (1979) explicitly incorporates this idea. Alternatively, the flypaper effect is generated by the forward-looking nature of local politicians, who maximize their utility by temporarily increasing public expenditure without raising taxes (Filimon, Romer, and Rosenthal 1982; Wyckoff 1991; Singhal 2006). All of these potential explanations assume that local and federal governments interact for more than one period, and are rational, forward-looking agents.

Properly specifying how local governments respond to decreases in grant revenue is critical when intergovernmental grants are modeled in a dynamic framework. Traditionally, economists assume that treatments effects are symmetric; if a local government responds to a federal grant for hiring police by increasing their police force, the local government will respond to the termination of that grant by reducing their police force an equivalent amount. When this assumption is unjustified, estimates of the impact of temporary intergovernmental grants on public expenditure will be biased towards zero.

The literature on asymmetric flypaper effects presents no strong consensus on the equivalence of local government response to increases and decreases in grant revenue.[39] Evidence of asymmetric responses are found in Gramlich (1987) and Volden (1999), who found that local governments raise additional revenue to make up for lost grants. If total local expenditure increases when a grant is received, but does not fall when the grant ends, then temporary federal grants have caused a permanent increase in local government expenditure. In contrast, Stine (1994), Gamkhar and Oates (1996), Goodspeed (1998), Gamkhar (2000) found that reductions in grants are correlated with reductions in the provision of the earmarked good. One explanation for the inconsistent findings is that heterogeneity in institutional form of the local government or the grant may affect the way revenue is allocated.[40] It is also possible that the endogenous nature of the federal grant making process creates the illusion of fiscal symmetry.[41] UHP hiring grants were distributed by civilian COPS office employees, who are unlikely to play a role in local government planning. I cannot rule out the possibility that the distribution of UHP grants was correlated with local preferences, but I am able to empirically asses the relationship between crime, police hiring, and future UHP grants in detail.

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[39] See Gamkhar (2002) for a summary of existing literature of asymmetries.

[40] Melo (2002) identifies heterogeneity in the existence and symmetry of the flypaper effect across institutional form. Sub-national entities in Columbia are more likely to permanently increase local spending when they are more dependent on the national government for revenue. McGuire (1979) and Gamkhar (2002) highlight the benefit of modeling local government responses to grants which do not fit the classic lump sum form.

[41] For example, Gamkhar (2000) finds no evidence of asymmetries in local response to federal highway grants. However, if federal politicians distribute these grants only when they are needed, the conclusion of Knight (2002), it is likely that grant revenue decreases only when the local government no longer wishes to spend money on the earmarked good.

3.3: The Universal Hiring Program

The Violent Crime and Control Act of 1994 (VCCA) was passed after one of the more dramatic increases in violent crime in United States history. Under this act, the Department of Justice was charged with counteracting rising crime rates by encouraging local governments to hire more police. The Universal Hiring Program was one of several grants programs used by the Department of Justice to subsidize the salaries of new police officers. Through the UHP, the Department of Justice paid local governments 75% of a new officer's salary for three years, with a maximum of $75,000 provided per new officer. For most city governments, the $75,000 cap was a binding constraint. According to the 2004 International City-County Management Association's annual Police and Fire Personnel, Salaries, and Expenditures Survey an entry level police officer's annual base salary ranged from $41,793 to $51,661, implying that the minimum marginal cost of a police officer ranges from $125,379 to $154,983.[42] If the maximum UHP grant of $75,000 in 1994 was continually adjusted to remain constant in real terms, it would have covered 71% of the minimum starting officers' salary over three years. But failure to change the nominal terms of the grant meant that the real value of the UHP grants gradually diminished over time. Anecdotal evidence suggests that the cap on these UHP grants may have prevented some agencies from applying for them, and may have affected the way they spent these grants.[43] Choi, Turner and Volden (2002) found that city managers, who serve terms of an unspecified length, were significantly less likely to

[42] This minimum cost excludes benefits and necessary capital expenditures. Recall that estimates of the total marginal cost of a police officer in the previous chapter are approximately $54,000 in 1997 dollars, or approximately $60,582 in real 2004 dollars.
[43] COPS in Schools (CIS) grants had a maximum payment of $100,000 per officer over three years and was therefore a more attractive grant program to the Prince George's County Police Department, where the prevailing wage is higher than the national average.

apply for UHP grants than four-year city mayors. The authors attribute this to the fact that not only do UHP grants not cover their promised 75% of an officers salary for three years in most cities, the UHP grants cover only 10% of the total cost to a local government of employing an additional officer for 10 years.[44]

Accepting a UHP grant and complying with the terms of that acceptance involved an increase in local government spending, in most cases beyond the official 25% matching rate. In the previous chapter, my coauthor and I find evidence that these grants did increase the size of city police forces, but for every "paid" officer granted by the COPS office, the police force increased by only 0.7 officers. Because the UHP program "paid" for at most 0.75 percent of an officer, this means that for every four officers granted under the UHP program, only two new officers were hired. While this is significant leakage of grant money, this is a larger increase in police employment than would be expected if money were completely fungible- the flypaper effect seems to be at work.

The COPS grants represented a small increase in the national police forces, roughly 5% at its peak, but the social benefit to crime reduction due to these grants represented a total net benefit to society of at least $2.9 billion. Despite this, funding for all COPS grant programs was eliminated from the Federal government's 2006 fiscal year budget. Prior to this complete elimination of funding, a shift in federal attention away from preventing street crime to preventing international terrorism was in place. In fiscal year 2002, $2.3 million were cut from all hiring programs grants.[45] In 2003, the Department of Justice did not request any federal budget allocation for the UHP program,

[44] Regional variation in police officer salary, as well as reelection laws may provide interesting variation in the perceived generosity of UHP grants, to be explored in future research.
[45] http://www.usdoj.gov/jmd/2002summary/html/ojp_cops.htm.

stating “the objective of funding 100,000 police officers [has been] met”.[46] Based on phone surveys of a nationally representative sample of 1,270 law enforcement agencies, the Urban Institute concluded that while most agencies intended to maintain the UHP funded positions after the grants expired, small districts in particular were uncertain in 2000 about the feasibility of replacing the lost revenue.[47]

3.4: Data

I am interested in measuring the dynamic effect of federal grants on city police employment. As in the previous chapter, the data used in this chapter are drawn from four different sources. Information on the size and timing of UHP grants was obtained from the COPS office. This data set contains information approximately 26,920 grants distributed to 11,357 law enforcement agencies between 1994 to 2001, and includes the start and end date of the grant, the number of officers granted, the type of program, the dollar value distributed to the agency, and the Originating Agency Identifier (ORI), a unique identification number assigned by the FBI to all local enforcement agencies. Table 3-1 displays the number and average size of new UHP grants that were awarded to the cities in my sample each year. I construct a measure of “active” UHP grants by summing the total number of officers granted to agency i in year t.

$$UHP_{it} = \sum_k UHP_{itk}$$

[46] http://www.usdoj.gov/jmd/2003summary/html/ojp-cops-bs.htm
[47] Koper,C., Maguire, E., Moore, G., and Huffer, D. (2001) Hiring and Retention Issues in Police Agencies: Readings on the Determinants of Police Strength, Hiring and Retention of Officers, and the Federal COPS Program. Report to the National Institute of Justice.

The number of officers granted to agency i in year t is approximately equal to 0.75 of the "paid officer granted" variable used in the previous chapter. I measure the permanent effect of UHP grants by summing the total number of UHP officers that have ever been granted in year t to agency i, minus the currently active grants.

$$EXPUHP_{it} = \sum_{0}^{t} \sum_{k} UHP_{isk} - \sum_{k} UHP_{itk}$$

Information on city police employment, crime rates, and population of the police jurisdiction is taken from the FBI's Uniform Crime Reports (UCR), which is publicly available on the ICPSR website. Annual county level observations on demographics- specifically the fraction of the population that is Black, and the fraction of the population between 18 and 24, are taken from the Census, and annual county level observations on per capita income, employment, and average wages are taken from the Bureau of Economic Analysis.

These data sets were combined in three steps. First, information on COPS grants were merged with the UCR data by ORI code. A non-trivial number of grants in my data set have ORIs that either are clearly in error (AR0ZZZZ), or do not seem to exist in the UCR data. Reasons for this identification error by the COPS office include the agency not reporting to the UCR,[48] or that two or more agencies merged into one during the 1990s.[49]

[48] Of the 23,170 agencies in the Law Enforcement Agency Identifiers Crosswalk, 4,439 do not appear in UCR statistics

[49] The UCR data itself does not record agency mergers, but anecdotal evidence of police department mergers and fragmentation suggest this may be a possible explanation.

The next task was to combine this data set with demographic and economic indicators from the Census and BEA. The UCR data contain a FBI county code which is different from the Census FIPS county codes. These two data sets can be combined, however, using a crosswalk maintained by the Bureau of Justice Statistics and the National Archive of Criminal Justice Data. Using this crosswalk, demographic data from the Census was added to the police statistics. The final step to combining the data is accounting for the difference in BEA and Census reporting areas. The Census creates separate identifiers for major cities, such as Honolulu HI and Manassas Park, VA, while the BEA produces one observation for all jobs within a county. To address this problem, I changed the BEA county codes to match with the Census "city" and "county" codes, essentially duplicating BEA observations. Between 1994 and 2001 1,851 out of the 2,074 cities in my sample received three year UHP grants. The high frequency with which individual grants were extended makes the UHP program a particularly attractive venue in which to examine the asymmetric effects of intergovernmental grants.

3.5: Temporary Grant, Permanent Change?

3.51: Econometric Model:

Do temporary UHP grants cause a permanent change in police force size?" I attempt to answer this question with a variation on the first stage econometric model used in the previous chapter. I model the number of police officers employed as a function of the number of officers currently subsidized by active UHP grants and the number of officers that were subsidized in the past by UHP grants that have since expired. If the Department of Justice was able to permanently increase city expenditure on police through the UHP program, then I will find a positive correlation between expired grants and current employment, even when I control for the size of active grants.

Between 1985 and 1993 crime rates in the United States increased sharply, particularly violent crime rates. In 1993 that trend reversed itself, and the number of crimes committed per capita in 2001 was the lowest on record.[50] It is well documented that crime fell fastest after 1993 in the cities where crime rates had been increasing the fastest.[51] This macroeconomic fact poses a potential empirical problem for estimating the impact of UHP grants on police employment. In the previous chapter, we develop a fixed effects model in which each of the 2,074 cities in our sample are placed into one of 186 groups according to their average population and 1990-1993 growth rates in crime and police employment. Including a year fixed-effect for each of these groups allows us to difference out common changes over time in the legal and criminal environment, such as prison population. I use a fixed effects OLS regression of the form:

$$(1) \qquad Police_{ijt} = \alpha_i + \delta_{jt} + X_{it}\gamma + UHP_{it-1}\beta + EXPUHP_{it-1}\theta + \varepsilon_{it}$$

Where $Police_{ijt}$ is the number of sworn officers employed per 10,000 residents by agency i in year t. Each agency has its own time invariant fixed effect α_i, and a time varying "group" effect δ_{jt}. I also control for the fraction of the county that is Black, the fraction of the county that is between the ages of 18 and 24, the log of per capita income in that county, the log of the average wage in the county, and the fraction of the county population that is employed (estimated by the number of jobs in the county divided by

[50] See Levitt (2004) for a summary of possible explanations for why there was such a sharp break in the trends in crime.
[51] See Rapheal and Ludwig (2003)

the number of people between 18 and 65). The error term ε_{it} allows for arbitrary correlation within an agency across time.

The independent variables of interest are $UHP_{i,t-1}$ and $EXPUHP_{i,t-1}$. $UHP_{i,t-1}$ measures the total number of UHP officers granted to agency i per 10,000 people in city i in year t-1. Initially, UHP officers granted is lagged one year to allow the agency time to find an employee to hire. $EXPUHP_{i,t-1}$ is the number of UHP officers that agency i has received through grants that have expired by t-1, again per 10,000 people in city i in year t-1. Measuring grants in this way captures the flow of grants an agency receives, as opposed to the stock measure used in the pervious chapter. Typically, research on grant asymmetry has relied on changes in the level of grant money to identify response asymmetry. This type of formulation, where changes in the level of grant revenue are the independent variables of interest is not appropriate in this case. Cities can (and did) receive more than one UHP grant, and each UHP grant was intended to supplement the hiring of NEW officers. Consider the following example; in 1994 the Capital City (population 10,000) police department received a grant that paid for the salary of 1 officers for 3 years. In 1997 Capital City receives a second 1 officer, 3 year grant. There would be no apparent change in the level of federal funding in 1997 even though Capital City would now be expected to employ two more officers over its 1994 force size. I can identify the change in grant status using the active and expired approach. From 1994 to 1997, UHP_{it} would equal 1, and from 1998 to 2000, $EXPUHP_{it}$ would be one. If Capital City only increased spending on police when that spending was subsidized by the Federal government, $\theta=0$. If the UHP grant program caused Capital City to permanently increase the size of its police force by two officers, β will be equal to θ. If $0<\theta<\beta$, this indicates that the temporary grant has caused a permanent change in the police force, because the cutback in spending is less than the initial spending increase. The first case is evidence of a symmetric flypaper effect, while the other cases are evidence of an asymmetric response to intergovernmental grants.

3.52: Results

I find evidence that local governments responded to UHP grants in an asymmetric way. Table 3- 2 presents my estimated values of β and θ for the 2,047 cities in my sample. I estimate that for each UHP officer granted to city i, the police force increased by approximately 0.6 officers during the three year period when the grant was active. I estimate the UHP grants permanently increased the police force by 0.6 officers per officers granted. I cannot reject the null hypothesis that the temporary and permanent effects of UHP grants are the same (p value=0.715). My estimate of the permanent effect is less precise than that of the temporary effect. This reduction in precision could be interpreted as evidence of heterogeneity in the response of cities to decreases in grant funding. However, I observe roughly one half (3,994 vs. 8,345) the number non-zero values of $EXPUHP_{it-1}$ as UHP_{it-1}, which will mechanically increase my estimated standard errors by a factor of 2.

One potential source of heterogeneity in the permanence of UHP grants could be in the size of the grant itself. Recall that the number of officers granted per capita through UHP varied across city size, with the smallest and largest cities receiving approximately 0.3 more officers per 10,000 people than mid sized cities. As noted by the Koper et al.(2001), the ability of a local government to replace expired grant funds may be inversely related to the size of the expired grant. If this is the case, I would expect that the estimated value of ($\beta - \theta$) should be larger in the largest and smallest cities, implying that local government in these cities have a more symmetric response to intergovernmental grants than others. Columns (2) through (6) display estimated values of β and θ for cities in each of the five population groups. I cannot reject the null hypothesis that the permanent and temporary responses the UHP grants were the same for all cities except those between 50,000 and 100,000 people, which as a group received the smallest UHP grants per 10,000 residents. Surprisingly, I estimate that the permanent response to UHP grants is larger than the temporary response. While a UHP grant is active, cities in this category increase their police force by approximately 0.7 officers per person, or one for one with the number of paid UHP officers granted. After the grant expires, the police force actually increases by one full officer per officer granted, which was the original intent of the UHP grant.[52] My estimates suggest that the assumption that cities respond in a symmetric way to increases and decreases in grant revenue is not appropriate in this case. In fact, when the intergovernmental grant is small enough, local governments may overcompensate for the declines in grant revenue.

The Department of Justice distributed several types of grants to local governments through the COPS office. If law enforcement agencies that received UHP grants were more likely to receive another COPS grant, the coefficient estimates in presented in Table 3-2 may be biased. In columns (1) through (6) of Table 3-3, I present the regression results when I include lagged values of all other grants distributed by the COPS office. MORE grants, which were used to invest in policing technology or hire supplemental

[52] Another potential explanation could be that it took cities of this size longer to find potential police officers to fill their compliment, although it is not clear why this would be true only for these mid sized cities.

staff, are not associated with increases in the number of sworn officers. Both COPS in Schools and DNP grants are associated with statistically significant increases in force size. In fact, the size of the police force appears to increase almost one for one with the number of DNP officers granted. Because they are both used to hire more police officers, in the previous chapter my coauthor and I aggregated these two grants into a count of "paid officers granted." DNP grants were million dollar grants given to only 18 cities deemed by the Department of Justice to be in particular need of new officers, so the exogenous variation in paid officers granted is driven primarily by changes in UHP grant levels. Each of the cities that received a DNP grant also received UHP grants. A finding that UHP grants did not increase police employment independently of their correlation with DNP grants would be problematic for the our previously drawn conclusions about COPS grants, police employment, and crime. I argue that the robustness of our estimate of the temporary and permanent effects of UHP grants on hiring is evidence that these DNP grants are not driving our first stage estimates in the previous chapter.

In the final column of Table 3-3 I estimate the impact of COPS grants on civilian police employment. Evidence that active UHP grants increased the number of civilian support staff should be interpreted as law enforcement agencies using UHP funds as general revenue. The same conclusion is not necessarily true for expired UHP grants. If civilian support staff and sworn officers are complimentary inputs in a law enforcement production function, then a permanent increase in the number of sworn officers will increase the marginal return to hiring an additional civilian employee.

The elimination of federal funding for the UHP grant program precludes me from making out of sample predictions as to whether the impact of the UHP grants extended beyond 2002. I observe 28,191 officers being granted to local city governments, and grants for 16,075 officers. This is a large number of expired grants, but my window of observation does not extend past the absolute end of funding for the UHP program. In this sample, the hiring decision by local governments after a UHP grant ended is similar to the dynamic situation described by Chernick (1979). When the program was still being actively funded, police chiefs may have expected that continuing to employ UHP funded officers would increase the probability of receiving more UHP grants in the future.

3.6: How do COPS Reduce Crime?

3.61: Econometric Model

I find evidence that temporary grants used for hiring new police officers do appear to cause lasting increases in the number of police per capita in the recipient cities. Increasing the number of police on the streets can reduce crime through two different mechanisms. An observed increase in the frequency of patrols may cause a potential criminal to reevaluate his perceived probability of being punished for a crime. Following the Becker model of criminal behavior (Becker 1968), if the likelihood of being caught goes up, the cost of committing a crime will increase, and that individual will be less likely to engage in criminal behavior. This behavioral change is called general deterrence. A larger police force can also reduce crime through increased incapacitation if larger police forces make it more likely that repeat offenders are caught and removed from the general population. In this case, increasing the number of police officers in period t will lead to reduced crime in period t+1 because more offenders who would have committed crime in period t+1 are incarcerated instead of on the streets. Being able to distinguish crime reductions due to deterrence and crime reductions do to incapacitation can have important policy implications, as there are different opportunity costs associated with the two mechanisms.[53]

In this section I present a highly stylized two period model where crime rates are a function of the population, the fraction of individuals engaging in crime, and the number of police officers. In order to disentangle crime reductions due to deterrence and crime reductions due to incapacitation, I assume that a small increase in the number of police officers per capita will have an initial deterrence effect as the presence of more officers induces potential offenders to change their behavior. This initial deterrent effect will be constant as long as police intensity is constant and will be followed by an incapacitative effect as the increased number of police per capita results in higher arrests per capita.

My assumption about the timing of these two effects is important, and is supported by empirical and theoretical arguments by Sherman (1990) about the responsiveness to crime after police "crackdowns." Included in the definition of "crackdown" is a sudden increase in the number of officers on foot patrol, which was the intention of the Universal Hiring Program. Sherman surveys existing studies of crime rates after temporary increases in police activity, and argues that "over short term periods with a large enough number of offenses, it seems reasonably plausible for police to interpret crime reduction as a deterrence effect," although after police arrest individuals with a high rate of offending, incapacitation begins to play a role.

Assume that in period t, some fraction f of the population decides to engage in criminal activity. The more police there are in any given period, represented by p, the

[53] A detailed discussion of the literature on deterrence and incapacitation is left to the next chapter.

higher the probability that a criminal will be arrested, a(p). I assume for simplicity that all individuals who are arrested are incarcerated for the next period. That fraction f is determined by the probability of being punished for criminal behavior, and is therefore a decreasing function of the number of police officers, f(p). If there are r residents in the city in period t, the crime rate in period t can be described as:

$$Crime_t = r_t f(p_t)$$

The number of criminals who are arrested an incarcerated in period t is simply the probability of being arrested for any given crime times the number of crimes committed.

$$Arrests_t = a(p_t) * r_t f(p_t)$$

I make two simplifying assumptions regarding $a(p_t)$ and $f(p_t)$. First, I assume that $f(p_t)>a(p_t)$, ie: police do not apprehend all criminals, and secondly the first derivatives of a(pt) and f(pt) are equal in magnitude and opposite in sign- that criminals decrease the probability of committing crime one for one with the probability of being arrested. The number of residents who are not incarceration in period t+1, and therefore the pool of potential criminals, is therefore

$$r_{t+1} = r_t [1 - a(p_t) f(p_t)]$$

and the crime rate in period t+1 is

$$Crime_{t+1} = r_t\left[1 - a(p_t)f(p_t)\right]f(p_{t+1})$$

In the beginning of period one, the city receives a one-period UHP grant, which is used to subsidize the hiring more officers. This increase in police will have an instantaneous negative affect on crime, and assuming that not all criminals are arrested, a positive impact on arrests.

$$\frac{\partial Crime_t}{\partial p_t} = r_t \frac{\partial f(p_t)}{\partial p_t} < 0 \qquad \frac{\partial Arrests_t}{\partial p_t} = r_t\left[\frac{\partial a(p_t)}{\partial p_t} f(p_t) + \frac{\partial f(p_t)}{\partial p_t} a(p_t)\right] > 0$$

The UHP grant in period t will also have an affect on crime in the next period.

$$\frac{\partial Crime_{t+1}}{\partial p_t} = r_t\left[1 - a(p_t)f(p_t)\right]\frac{\partial f(p_{t+1})}{\partial p_{t+1}}\frac{\partial p_{t+1}}{\partial p_t} - f(p_{t+1})r_t\left[\frac{\partial a(p_t)}{\partial p_t} f(p_t) + \frac{\partial f(p_t)}{\partial p_t} a(p_t)\right]$$

The second period affect of a period t change on police officers on crime in period t+1 is composed of a deterrence effect, the first term, and an incapacitative effect, the second term. The deterrence effect operates through the change in the probability to offend due to the increased number of police officers who were hired in period t, and are still around in t+1, and the incapacitative effect represents an additional reduction in crime due to the increased number of arrests which took place in period t, incapacitating some of the individuals who would have committed crime in period t+1 if they were free. Both reduce crime, and the relative magnitude of these two effects depends on the permanence of the city's response to a temporary grant, $\frac{\partial p_{t+1}}{\partial p_t}$. Note that this is value is equivalent to the ratio of the previously estimated values of and θ and β.

I estimate the reduced form relationship between hiring grants and police officers as

(2) $$Crime_{ijt} = \alpha_i + \delta_{jt} + X_{it}\widetilde{\gamma} + UHP_{it-1}\widetilde{\beta} + EXPUHP_{it-1}\widetilde{\theta} + \nu_{it}$$

Information about the mechanism through which police reduce crime can be computed by comparing the estimated values of $\widetilde{\beta}$, β, $\widetilde{\theta}$, and θ.[54] The permanent impact of UHP grants on crime, $\widetilde{\theta}$, should give some information about the relative importance of deterrence and incapacitation to the crime reductions due to the Universal Hiring Program. I expect the absolute magnitude of $\widetilde{\theta}$ to be larger than the estimated value of $\widetilde{\beta}$, reflecting the fact that $\widetilde{\theta}$ represents both a deterrent and incapacitative effect of increase police presence. The relative magnitude of the deterrent and incapacitative effects can be roughly estimated by comparing the estimated values of $\widetilde{\beta}$ *(θ/β) and $\widetilde{\theta}$ - $\widetilde{\beta}$ *(θ/β). If the latter is larger than the former, then the permanent reductions in crime are due primarily to increased incapacitation. If the former is larger, than increasing the number of police on the streets primarily reduces crime through deterrence.

I estimate the "temporary" impact of police officers on crime as an average effect over the first three years, and the "permanent" impact as the average effect of the proceeding three years. It is almost certain that my estimated "temporary" effect contains some incapacitative effect, but I assume that incapacitation is relatively more important

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[54] Recall that because of scaling, the estimated value of $\widetilde{\beta}$ should be three quarters of the reduced form estimates in the previous chapter.

in the "permanent" effect of officers.[55] As a result, my estimates based on equations (1) and (2) should be interpreted as rough approximations of the relative magnitudes of the two effects, overestimating the relative magnitude of deterrence.

This decomposition would be more problematic if local governments responded to UHP grants in a symmetric way. If this were the case, $\tilde{\theta}$ would represent only an incapacitative effect, since θ would equal zero. That incapacitative effect would actually be decreasing over time as previously arrested individuals were released from incarceration.[56] The aggregate data do not allow me to observe whether or not the immediate and unobserved incapacitative effect in $\tilde{\beta}$ is larger or smaller than the observed incapacitative effect in $\tilde{\theta}$.

3.62: Results

My estimates of the temporary and permanent effects of UHP grants on crime are displayed in Table 3-4. As in the previous chapter, I find that UHP grants caused statistically significant reductions in robbery, aggravated assault, burglary and car theft. UHP grants are also associated with reductions in murder, rape and robbery, but the coefficients are imprecisely measured. City police departments were granted 1.4 UHP officers on average, implying that an average UHP officer grant temporarily reduced burglaries and car thefts by 3%, robberies by 6%, and aggravated assaults by 4% in the next year. Agencies that had UHP grants in the past had an average of 0.96 expired granted officer years. Dividing the coefficients in row (2) by the mean crime rates, I find

[55] This assumption would be violated if potential offenders did not learn about the increase in force size until after the new police officer had made arrests, as is the case with undercover officers. As currently defined, potential offenders have three years to observe a new officer and change their behavior.
[56] Sherman (1990) also raises the possibility that even after police presence is reduced, a "residual deterrent" effect may remain if potential offenders incorporate the knowledge about reductions in police presence more slowly than increases.

that after expiration, the average UHP grant permanently reduced murder by 8%, robbery by 14%, aggravated assaults by 10%, rapes by 6%, and burglaries by 5%, and car theft by 2%. With the exception of car theft, the magnitude of the permanent effect of UHP grants on crime is larger than the temporary effect. The estimated permanent impact of UHP grants on rape and murder both increase by a factor of two and these estimates are significantly different from zero at the 10% confidence level. Reductions in car theft, while large in magnitude, are now statistically insignificant.

In the final two rows of Table 3-4, I present my estimates of $\tilde{\beta}$ *(θ/β), the change crime due to the deterrent impact of log term increase in police presence, and $\tilde{\theta}$ - $\tilde{\beta}$ *(θ/β), the reduction in crime due to incapacitation. For all crimes excluding car theft, the magnitude of incapacitative effect of increased police presence on crime in later periods is larger than the deterrent effect. After 6 years, incapacitation will drive crime reductions following a permanent increase in police per capita (p value = 0.02). In addition, I estimate that the incapacitative effect of increase police presence is larger than the deterrent effect for robbery (p value = 0.008), assault (p value = 0.04), and rape (p value = 0.08).

The relative magnitudes of the deterrence and incapacitative effects across crimes do correspond to a priori assumptions about nature of different crimes. Because I define the immediate impact of a police officer as an officer who has been on the force three years or less, I do not fully separate out deterrence from incapacitation, and my results, like those of Kessler and Levitt (1999) should be interpreted as approximations. The reductions in crime due to incapacitation are more than twice that due to an induced behavioral change for murder, robbery, aggravated assault, and rape, all violent crimes that are less likely to be committed by first time, marginal criminals. For property crimes, the deterrent effect is relatively larger in comparison, and for the deterrent impact on car theft actually larger than the incapacitative effect. These results are consistent with criminology literature (Nagin 1998; Spelman 2000; Doob and Webster 2003) that generally concludes that attempts to reduce crime through deterrence are not as effective as increased incapacitation.

When I include additional COPS grants in my regression equation, the magnitude of my estimates decreases by a small amount, and the precision of my estimates is not affected. These coefficient estimates are presented in Table 3-5. Again, this robustness test provides further support to our assumption in the previous chapter that variation in UHP grants, as opposed to the infrequently used by large DNP grants, is driving our reduced-form and 2SLS estimates of the impact of police on crime rates.

3.7: Conclusion

Between 1994 and 2004, the Department of Justice distributed \$5 billion to local law enforcement agencies in the form of Universal Hiring Program (UHP) grants. These closed-end matching grants provided up to 75% of a new officer's salary for three years, with a maximum award of \$75,000 per officer. In this chapter, I extend the previous analysis of COPS grants by relaxing the assumption that decreases in UHP funds have the same effect on police employment as increases in UHP funds. This analysis is particularly relevant for fiscal policy, as the UHP grant program is one of many intergovernmental grants that has been eliminated or reduced in recent years. I find evidence that local governments increase police employment in respond to these earmarked federal grants, contrary to theoretical predictions but consistent with existing research on the COPS grant program, and that police employment does not decreased when the grant is terminated. For every two UHP officers granted to a police department, one additional officer is hired. After UHP funding ends, police employment keeps pace with population growth. My finding that temporary grants induce permanent increases in local government spending is consistent with local governments responding to changes in intergovernmental grants in an asymmetric way. Heterogeneity in the permanent response to UHP grants across city size suggests that smaller grants per capita are more likely to induce permanent changes in the provision of local public services.

Federal manipulation of the bundle of locally consumed public goods distorts the ability of local governments to maximizing heterogeneous preferences. This is not optimal unless there are sufficient externalities to the particular good (Oates 1972). In the case of law enforcement, it is possible that high crime rates in one area will spill over to another (Sah 1991), and in the previous chapter my coauthor and I found that the benefit of the COPS hiring grant program outweighed it's costs in terms of contemporaneous crime reduction. I find evidence that even after the UHP grants paying for 16,722 officers expired, local governments maintained those positions.

The taxpayer cost of a permanent increase in police is higher than the cost of temporary officers, since a permanent increase requires a transfer of fiscal responsibility from the federal government to cities. This does not necessarily mean that local governments should have reduced the size of their police force after UHP grants expired. The benefit of having a higher number of police per capita in terms of crime reduction is increasing over time, as the initial deterrence impact of increased police patrol is augmented by the incapacitative effect of increased arrests and incarcerations. The additional ability to incapacitate more offenders over time is particularly important for violent crimes. The long term effect of the average UHP grant, which includes both a deterrent and incapacitative effect, is an 8% reduction in the murder rate, a 14% reduction in the robbery rate, a 10% reduction in the aggravated assault rate, and a 6% reduction in the number of rapes per capita. When UHP grants are active they reduce crime primarily through deterrence, and as a result the effects are not as large.

The Universal Hiring Program induced local governments to temporarily increase the number of police it employed. When federal funding for these relatively small increases in police per capita expired, local governments did not reduce hiring in a symmetric way. The permanent increase in police employment was not necessarily inefficient. I find that increases in the number of police per capita have an increasing ability to lower crime rates. The additional incapacitative effect stemming from employing more police on crime rates is potentially twice as large as the deterrent effect.

Chapter 4: More Time, Less Crime? Estimating the Incapacitative Effect of Sentence Enhancements

4.1: Introduction

Over the past 20 years, the number of people imprisoned in the United States has grown from 682 thousand people in 1984 to over 2.1 million in 2004,[57] a twenty-year growth rate that is 10 times that of the US population as a whole. This sharp rise was due to an increase in the flow from the general population to prisons and jails, as crime rates rose through the late 1980s and jurisdictions increasingly substituted incarceration for fines, as well as a decreased outflow from incarceration to the general population, as mandatory minimum sentences and "three strikes" laws were adopted. Sentence enhancements such as these increased the severity of punishment a convicted offender received by lengthening of time they were incarcerated. There exists a general consensus that the publicly provided criminal justice system as a whole (policing, sentencing, and incarceration) reduces the amount of crime in society[58] but concerns regarding the timing and the application of sentence enhancements, coinciding justice policy changes, and exogenous environmental factors have resulted in little consensus on how changes in sentencing have contributed to the crime drop of the 1990s.[59]

Understanding the relationship between incarceration and crime rates is further complicated since incarceration theoretically has two distinct effects on individual criminal behavior: deterrence and incapacitation. A deterred offender is able to commit crime but chooses not to. An incapacitated offender would choose to commit crime but is

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[57] http://www.ojp.usdoj.gov/bjs/prisons.htm
[58] Doob and Webster (2003), Levitt (2004)
[59] Zimring and Hawkins (1995), Spelman (2000), Doob and Webster (2003), and Levitt (2004) provide an extensive and critical review of research on incapacitation and deterrence.

unable to. The relative contribution of these factors to the decline in crime has important fiscal implications for governments. If the threat of longer sentences deters criminal behavior, the state does not have to increase spending on arrest, prosecution, and incarceration. Empirical evidence that the threat of increased punishment deters offenders is mixed at best,[60] and must be weighed against the ability of other social programs to alter behavior.[61] Even if the threat of a sentence enhancement does not induce potential offenders to alter their behavior, sentence enhancements can reduce crime through increased incapacitation, as offenders are prevented from re-entering society. Measuring the social benefit of increased incapacitation requires an estimate of how many crimes these offenders would have committed had they been "on the street." Unlike crime reductions due to deterrence, crime reductions resulting from increased incapacitation come at a price. The government must make a non-trivial fiscal outlay on prison construction and inmate supervision.[62]

The number of crimes prevented by a sentence enhancement through incapacitation equals the number of crimes incarcerated offenders would have committed if they were on the streets during that enhancement. The empirical task is to create a plausible estimate of this number. Self reports of criminal behavior immediately prior to incarceration, which Zimring and Hawkins (1995) characterize as the "window period", can be misleading measured of the incapacitation effect because these numbers reflect the potential criminal behavior of the average incarcerated inmate. Sentence enhancements

[60] A review of the literature by Doob and Webster (2003) highlights the contentious nature of the debate over sentence enhancements and deterrence. Becker (1968) suggests that rational criminals would be deterred by harsher punishments, but McCrary and Lee (2006) demonstrate how high discount rates result in "rational" criminals being undeterred by increased sanctions.

[61] Donohue and Siegelmen (1998), for example, suggest that early childhood interventions are at least as cost effective as increased incarceration at reducing crime.

[62] Greenwood et al. (1996) estimate that the effect of California's three strikes law on the prison population could cost the state $5.5 billion per year.

only increase the incapacitation of inmates who are about to be released, and the stock of incarcerated offenders is likely to be significantly more criminal than the exiting flow from that population.[63] Comparing the behavior of two individuals, one who receives a sentence enhancement and one who does not, is equally unsuitable since sentence enhancements are typically given to the "worst" offenders who usually have the highest risk of recidivism.[64]

An ideal way to estimate the effect of sentence enhancements on crime would be to randomly assign offenders to short and long prison sentences. In that case, the average criminal behavior of the "lucky" offenders receiving the shorter sentences can be interpreted as the criminal activity the "unlucky" offenders were prevented from engaging in during their sentence enhancement. Once the unlucky offenders are released, the difference between their criminal behavior and the contemporaneous criminal behavior of the lucky offenders provides insight on the effect of sentence length on criminal behavior.[65]

I approximate this ideal experiment using quasi-experimental variation in sentence length created by a change in Maryland law that reduced the recommended sentences for a specific group of individuals between 23 and 25 years old. In Maryland, a recommended sentence is calculated for each convicted offender based on his criminal history and the crime for which they are convicted using a guidelines worksheet developed by the Maryland Sentencing Commission. Prior to July 1st 2001, an offender's juvenile record was included as part of their criminal history until that offender was 26

[63] This is noted in Donohue and Siegelman (1998) and Cavanagh and Kleiman (1990).
[64] Greenberg (1985) highlights the problems inherent in using cross sectional data to study crime in a critique of Hirshi and Gottfredson (1983).
[65] This point will be further illustrated in section IV d.

years old. I find that each additional delinquent "point" officially assigned to an offender increased incarceration by a mean of 222 days. After observing the number of delinquent points offenders sentenced after July 1st 2001 would have qualified for under the prior regime, I then estimate the sentence enhancement offenders with juvenile histories would have received if they had been sentenced under the old guidelines. Under the assumption that the change in sentencing is unrelated to changes in underlying criminality, the offense rate of the lucky former delinquents between when they were released and when they would have been released is the number of crimes that the unlucky 23-25 year-olds would have committed if they had been released early. I find that offenders are arrested at a mean rate of 2.8 times per year, and are involved in approximately 1.5 index crimes per year while they would have otherwise been incarcerated. The difference between the magnitude of my estimates and past research highlights the importance of using economic reasoning, in this case distinguishing marginal versus average effects, when evaluating policy decisions.

The chapter proceeds as follows: In section 4.2 I describe existing research on incarceration and crime, and discuss how the estimates of incapacitation in this paper differ from previous work. Next I describe the Maryland sentencing process and the 2001 law change in detail. A discussion of the data set I use and the econometric model follows. In section 4.6 I present my estimates of the number of crimes sentence enhancements prevent by incapacitation. I discuss the costs associated with sentence enhancements in section 4.7, and offer some concluding discussion in section 4.8.

4.2: Existing Research on Incarceration and Crime

Incarcerating convicted offenders imposes substantial costs on society. Federal, state, and local governments bear the direct financial cost of incarceration, a combined $61 billion on corrections in 2003 alone.[66] Incarceration also imposes indirect costs on society. The stigma of being an ex-con can act as barrier to employment which can reduce general productivity through decreased worker-firm match quality,[67] and children of incarcerated adults are potentially more likely to be impoverished.[68] Incarcerated inmates are at high risk of contracting sexual transmitted infections, including HIV[69] and Hepatitis C, that can be transmitted into the general population upon release. Increased use of incarceration can also decrease the deterrent value of imprisonment by increasing the fraction of the population that has gone through the prison system and therefore decreasing the stigma associated with having a criminal record.[70]

When thinking about the social costs of sentence enhancements, it is important to differentiate between the social cost of incarceration and the social cost of increases in the length of incarceration. Recent work by Kling (2006) highlights the lack of research on the social costs of increased incarceration relative to the social cost of being incarcerated. Kling finds that incarceration length has little to no effect on post-incarceration earnings, and it seems likely that the marginal social cost of being

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[66] http://www.ojp.usdoj.gov/bjs/glance/tables/exptyptab.htm
[67] This observation follows from the Becker (1971) model of discrimination. Pager (2003) finds evidence of negative effects of incarceration on future employment of a similar magnitude to racial discrimination.
[68] Recent work by Oliver et al. (2006) highlights the link between black male imprisonment and black child poverty.
[69] Recent work by Johnson and Raphael (2006) suggest that the high incarceration rates among black Americans can explain a large fraction of racial disparities in HIV infection.
[70] Nagin (1998) and Blume (2002) suggest that as the fraction of the population which has been incarcerated increases, the stigma of being an ex-con diminishes, reducing the deterrent effect of incarceration.

incarcerated for an additional year is significantly smaller than the average social cost of becoming incarcerated.

The costs of incarceration must be weighed against its social benefits – namely, reduced crime. Increasing the severity of incarceration reduces crime through general deterrence if potential offenders reduce their criminal activity to avoid punishment.[71] Reviews of the literature on incarceration and deterrence by Nagin (1998) and Doob and Webster (2003) find little evidence that longer sentences induce potential criminals to stop offending. Deterrence is only one mechanism through which sentence enhancements can affect crime rates. By removing a "slice" of an offender's criminal career,[72] longer sentences can reduce crime through incapacitation as criminals are prevented from engaging in criminal activity. The number of crimes a one-year sentence enhancement prevents through incapacitation is the number of crimes an offender would have committed during that period if he was on the street instead of in a correctional facility.[73]

Attempts to estimate the number of crimes prevented through incapacitation empirically have generally relied on one of two approaches. One method is to measure the criminal activity of inmates immediately prior to their incarceration, obtained through arrest records or self reports, and use this number to estimate the average hypothetical offense rate of all incarcerated offenders.[74] Estimates of the number of crimes prevented by incapacitation per offender per year achieved in this way vary widely, from 187

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[71] Inmate surveys conducted by Tunnell (1996), and Waldorf and Murphy (1995) suggest that criminals give only passing consideration to the severity of punishment prior to acting.
[72] Blumstein (1983)
[73] This statement assumes that no crimes are committed in correctional facilities. While this is not the case, I follow the literature in focusing only on crimes visible to the non-institutionalized population.
[74] This approach is based on the model of incapacitation and the criminal career developed in Avi-Itzahk and Shinnar (1973) and Shinnar and Shinnar (1975).

(Zedlewski, 1987) to 12 or fewer (DiIulio and Piehl, 1991 and 1995, Cohen, 1983). Spelman (2000) translate some of these estimates into elasticities, and suggests that doubling the current prison capacity could reduce crime by between 10 and 30%. Donohue and Siegelman (1998) argue that existing research suggests a “best estimate” of 15%.

An alternate strategy to estimate incapacitation is to take advantage of events that affect the sentence length of offenders and are arguably unrelated to other factors which may influence crime rates. Levitt (1996) identifies cross-state variation in the timing of prison overcrowding litigation, and finds that the changes in state crime rates and state prison populations after such laws are passed suggests that doubling the number of people incarcerated would reduce crime by as much as 30%. In a later paper, Levitt (1998b) relates crime rates in large urban areas to changes in arrest rates in the previous period for similar crimes (crimes more likely to be committed by the same offender), and finds that a detained offender would commit between 5.1 and 8.2 crimes per year if not incarcerated. Marvel and Moody (1994) use Granger causality methods to estimate that an incarcerated offender would commit on average 17 crimes if not incapacitated.

Each of these approaches has strengths and weaknesses. Assuming that self-reports of criminal activity are reliable, it is easy to disentangle deterrence and incapacitation with individual level data. The researcher can see directly how many crimes the incapacitated offender chose to commit when not incarcerated. However, one well known drawback of individual studies that sample the incarcerated population (the stock of prisoners) is that the mean pre-incarceration criminal activity of all incarcerated inmates is higher than the post-incarceration activity of the marginal inmate who would be incapacitated by any given policy change (the flow of exiting prisoners). Sentence enhancements lead to increases in incapacitation only for offenders who would otherwise have been released. Since a large fraction of the about to-be-released incarcerated population in Maryland, as in most states, is deemed by parole boards to be at low risk of recidivism, estimates of incapacitation based on all incarcerated inmates likely overstate the benefit of sentence enhancements. In addition, offenders tend to be incarcerated after pronounced increases in their rate of criminal behavior.[75] If offenders with the largest pre-incarceration spikes are also the ones most likely to be included in a stock based sample, this would exacerbate the upward bias.

By identifying situations in which the marginal offender faces either more or less punishment, estimates of incapacitation using aggregate data are better able to identify the change in crime that a change in incarceration policy causes. However, the sizes of deterrent and incapacitative effects resulting from a change in policy can be difficult to separate using aggregate data, since offenders enter and exit the pool of individuals who are at risk of offending at different times. Kessler and Levitt (1999) point out that since the sentence enhancements created by California's three strikes law would not increase incapacitation until the end of the sentences offenders would have received prior to three strikes, the drop in crime due to deterrence should precede the drop in crime due to incapacitation. Kessler and Levitt (1999) identify the changing relative magnitude of deterrent and incapacitative effects over two year periods, but cannot fully separate the two. If any offenders would have been incarcerated for less than two years prior to the three strikes law, the change in crime rates measured by Kessler and Levitt (1999) would include both the deterrent effect of harsher punishment and the incapacitation of repeat

[75]Zimring and Hawkins (1995) pg 83

offenders. The nature of the policy change itself can also confound attempts to isolate deterrence from incapacitation. For example, a crackdown on car thieves, through increased police presence would discourage potential robbers at the same time that it incapacitates car thieves.

4.3: Sentencing In Maryland

Maryland is one of seventeen states where guidelines are used to determine the sentence of a convicted offender. Judges in Maryland are not penalized for deviating from the guidelines, but a large majority of sentences they impose do fall within the recommended range the guidelines provide.[76] An adult convicted of a crime in a Maryland circuit court is assigned numeric offense and offender scores. These two scores correspond to a row and column of either a violent, property, or drug offense guidelines matrix, that identify the upper and lower bounds of a recommended sentence.[77] Information about the individual's offender and offense scores and minimum and maximum recommended sentences are entered into a guidelines worksheet, that is given to the judge prior to sentencing. Parole boards receive identical copies of the guidelines worksheets given to judges when deciding whether or not to release an incarcerated offender.

The offense score of someone convicted of a drug or property offense is wholly determined by the Maryland criminal statute. The most severe drug offense, being a drug "kingpin", is a category II offense, while the most severe property offenses, arson or first

[76] Annual reports by the Maryland Sentencing Commission indicate that between 1999 and 2002, roughly 50% of imposed sentences fell within the recommended range. Since 2002 the percentage of sentences that fall within this range has been above 70%.
[77] Copies of the sentencing guidelines, as well as the old and new guidelines worksheets, are available on the Maryland Sentencing Commission's website http://www.msccsp.org.

degree burglary, are category III offenses. Less severe offenses, such as passing a bad check for less than $500 or possession of drug paraphernalia, are category VII offenses. The offense score of an individual convicted of a violent crime includes additional information about the offense. The Maryland criminal statute determines the number of "seriousness" points assigned to the offense, ranging between 1 and 10. Use of a firearm results in two "weapons" points, any other weapon contributes one weapon point. If the victim of the crime dies or sustains a permanent injury as a result of the crime, two "injury" points are awarded. If the victim is temporarily injured, one injury point is awarded. Finally, a "vulnerability" point is awarded if the crime victim was under the age of eleven, over the age of sixty five, or physically or mentally disabled.

The offender score is calculated as the sum of four elements – the individual's prior adult criminal history, their juvenile criminal history, their history of parole violations, and their relationship to the criminal justice system at the time of the offense. Adult histories can be either major (5 points), moderate (3 points), minor (1 point), or none (0 points). An offender who has ever been found in violation of their parole receives one "parole" point. An offender who was under correctional supervision (incarcerated, on probation, etc.) at the time of the current offense receives an additional "relationship" point. Finally, an offender who was below a specific age threshold at the time of the offense and has not remained crime free for more than five years, receives up to two "delinquent" points. One delinquent point is assigned if the offender had two or more past findings of juvenile delinquency or one confinement. A record of multiple confinements results in two delinquent points being added to the offender score.[78]

In 1998, the Maryland Sentencing Commission became concerned that their treatment of former juvenile delinquents was too harsh relative to other states.[79] An individual's juvenile record was a major factor in their punishment for crimes they committed as adults until they turned 26 years old. The Sentencing Commission decided that it was unfair that a 25 year old man should be held accountable for his actions at 17 in Maryland, but not in a similar state. In order to rectify this inequity, the Commission determined that delinquent points would only be assigned to offenders aged 22 years or younger at the time of offense. The revised guidelines, in which offenders aged out of their juvenile histories at 23 instead of 26, went into effect on July 1st, 2001. This meant that 23, 24 and 25 year old offenders who would have had one or two delinquent points added to their offender score prior to July 1st, 2001, instead received zero points thereafter.

In order to illustrate the potential effect of this guidelines change, consider two similar former delinquents, each convicted of felony drug possession.[80] The adult history of each offender qualifies them for three offender points, and each has two past findings of juvenile delinquency. What should the expected impact of that additional delinquent point be? Following the sentencing guidelines for drug offenses, someone with an offender score of three convicted of felony drug possession, offense score III – B, would receive a recommended sentence of between three and seven years. A similar offender

[78] When an individual is convicted of more than one offense or more than one criminal event in a single sentence, a new guidelines worksheet is filled out for each offense. The final recommended sentence range is the sum of the minimum and maximum recommended sentences for each criminal offense.
[79] As noted by Dr. Charles Wellford, Maryland Sentencing Commission member
[80] Felony drug possession is the most common offense in Maryland.

with one additional delinquent point would receive a recommended sentence of between four and eight years. Following Bushway and Piehl (2006), the difference in the midpoints is one year.

Several other changes in the guidelines went into effect on July 1st 2001, including a change in the penalty for crimes against the elderly, and the inclusion of victim information in the worksheet. As long as these additional changes affected former delinquents and non-delinquents equally, this should not bias my estimate. This assumption can be verified by examining the effect of the guidelines change on the sentences of males in different age groups.

4.4: Data

My estimation strategy has two stages. First, I estimate the additional number of days that unlucky former delinquents were incarcerated due to the addition of their delinquent points to their offender score. I have received a data set from the Maryland Sentencing Commission on 39,961 sentences of males between the ages of 18 and 35 who were sentenced between 1999 and 2004. In approximately 18% of these sentences the offender was between the age of 23 and 25 at the time of the offense. Each observation includes the name, race, and date of birth of the convicted offender, as well as information on the elements of his offender and offense scores, the jurisdiction in which he was sentenced, the date he was sentenced, and the sentence passed down by the judge. The dependent variable of interest in this stage of the analysis is the actual sentence - the number of days an offender is incarcerated- which is not necessarily identical to the sentence passed down by the judge. In order to obtain actual sentence length, this sentencing data is linked to incarceration data from the administrative records from Anne Arundel, Prince George's and Washington County Departments of Corrections and the Maryland Department of Public Safety and Correctional Services, which oversees the Baltimore City jails and Maryland state prisons. I do not observe the intake and release dates for sentences served in the other 20 local jurisdictions. Approximately 39% of Maryland residents lived in one of these jurisdiction in 2000[81] and 64% of convictions between 1999 and 2004 occur in one of these four jurisdictions.

The second step in the analysis is to measure the criminal behavior of offenders after their sentences are completed. I link the sentencing and incarceration data to the Maryland Arrest Repository data, obtained from the Maryland Department of Public Safety and Correctional Services. The Arrest Repository or "rap sheet" data record every arrest made by a law enforcement agency in Maryland, and tracks individuals using a fingerprint-supported state identification number. The rap sheet data allow me to add information on dates of arrest, reasons for arrest, and arresting agency for each individual who has a Maryland arrest record.

Creating a complete set of sentence, intake, release and arrest dates requires that I exclude a non-trivial fraction of my affected sample. Using full name and date of birth I was able to link 3,345 out of 5,847 sentences to incarceration dates and individuals with adult arrest records and incarceration dates. Table 4-1 displays some descriptive characteristics relating to the sentences of 23-25 year-olds from the Maryland Sentencing Commission data that are included and excluded from the final sample. The sentences that I exclude from my analysis are clearly different from the sentences I include. Since my data contain the universe of state prison sentences, but only jail sentences in four large jurisdictions, over 80% of the sentences of individuals between the ages of 23 and

[81]http://quickfacts.census.gov/qfd/states/24/24510.html

25 at time of offense who are sentenced to state prisons are matched to incarceration length, but less than 50% of individuals of the same age sentenced to jails are linked to incarceration length. As a result, the mean sentence length, offense score, and offender score associated with linked sentences are higher than those for sentences that are excluded. Given these differences, my incapacitation estimates may not generalize to some offenders, such as those who are only incarcerated in local jails during their criminal careers or offenders who are less than 22 years old.

The excluded offenders are more likely to be white, since Prince George's County and Baltimore City have the largest African American populations in Maryland, and sentences not linked to incarceration spells are also more likely to be related to property offenses. I was able to link 83% of all Maryland sentences to individuals in the state rap sheet data, meaning that I could not locate arrest histories for 17% of individuals convicted in circuit court. Interestingly, offenders who are excluded because I was unable to find their Maryland arrest histories have similar characteristics to those for whom I do not have an actual sentence.

I am able to identify the number of delinquent points a 23-25 year-old sentenced after July 1st 2001 would have received under the old regime using the Maryland Sentencing Commission records. Table 4-2 compares the race, offense scores, offense types, and observed incarceration spells of former delinquents and non-delinquents in my sample. Unlucky former delinquents appear to have spent almost two additional years incarcerated relative to offenders without delinquent records, whereas lucky offenders appear to serve slightly less time, if any, that non-delinquents overall. Comparing of mean observed time served is potentially misleading, since right censoring (i.e., incomplete incarceration spells) essentially creates an upper bound on the difference between sentences of lucky delinquents and non-delinquents who are sentenced towards the end of the sample. An alternative way to illustrate the changing effect of delinquent points on incarceration is to compare the mean sentence passed down by a judge. The second row of Table 4-2 presents mean sentences passed down by judges, with serious offenses excluded.[82] Former delinquents sentenced under the old guidelines received sentences of 96 months on average, compared with 37 month sentences given to non-delinquents. Under the new guidelines, former delinquents received sentences of 50 months on average, only five months more than non-delinquents.

Former delinquents sentenced under the new guidelines regime were much more likely to be convicted of drug offenses than those convicted under the old guidelines regime. Just over 43% of unlucky former delinquents were convicted of drug offenses between 1999 and 2001, compared with 67% of lucky former delinquents.[83] There are two likely reasons for this. I am more likely to be able to identify drug offenders as former delinquents if drug offenders move in and out of the legal system at a higher frequency than other types of offenders. Alternately, if prosecutors see drug offenders as less dangerous than those convicted of violent or property offenses, they may be less likely to assign these particular offenders official delinquent "enhancements."[84] This is potentially an area of concern if there is heterogeneity in the effect of the delinquent "enhancement" on sentence length across offense types. In Appendix A I examine this possibility. In this particular sample, I find no compelling evidence of a heterogeneous

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[82] I do not present median values because they represent information about a particular individual.
[83] This pattern is not evident in former delinquents between 20 and 22 years old.
[84] This was pointed out by Hope Corman, and is similar to the phenomenon of "DRG creep," identified by Simborg (1981)

effect. In all other respects, the demographic characteristics of the four groups are generally similar.

It is possible that the sentence "disenhancement" that delinquent 23-25 year-olds received after July 1st 2001 induced some of those delinquents to commit crime. By lowering the severity of the punishment they would receive, 23-25 year-olds with delinquent records who may have chosen to not commit crime prior to July 1st 2001 may have done so once the new guidelines were in place.[85] If this were the case, lucky former delinquents would be on average less criminal than unlucky former delinquents. The addition of these marginal offenders would result in the average offense rate of lucky 23-25 year-olds to be lower than unlucky 23-25 year-olds, leading me to underestimate the number of crimes averted by incapacitation.[86] This policy change was not publicized. A Lexis-Nexis search of major and local Maryland newspapers between 1998 and 2004 found no mention of this guidelines change. However, despite this evidence I cannot entirely rule out the possibility that the former delinquents were aware of this rule change, and that some of the individuals used to estimate crimes averted through incapacitation could have potentially been deterred if the guidelines had not changed. This assumption will be formally tested in future research.

[85] This is a logical extension of the Becker model of criminal behavior.
[86] If these additional offenders were perceived to have a lower risk of recidivism by parole boards, they would be let out earlier, causing me to overestimate the size of the sentence disenhancement they received, artificially increasing the number of days these offenders would be at risk of offending. At the same time, the number of crimes these offenders committed post release would be lower than expected.

4.5: Estimating the Delinquent Enhancement

4.51: Econometric Model

The 2001 Maryland Sentencing Guidelines revision provides a compelling source of quasi-experimental variation in sentence length. The effect of this legal change on the amount of time served by 23-25 year-olds with juvenile histories is displayed in Figures 1a and 1b, which plot the mean observed and completed incarceration spells of delinquent and non delinquent 23-25 year-olds who were released in six month increments from January 1999 to December 2004.[87] The exclusion of juvenile history from the offender scores of 23-25 year-olds after 2001 resulted in a distinct change in the actual time served by these offenders. The null hypothesis that non-delinquents and former delinquents served equal sentences is easily rejected under the old guidelines (t=-5.04), but not under the new guidelines (t=-0.85). Prior to July 2001, 23-25 year-olds with delinquent points were incarcerated for longer periods of time than non-delinquents. After July 1st, 2001 the average number of days served by 23-25 year-olds with juvenile records appears to be identical to the number of days served by non-delinquents.

Figures 2a and 2b display the probability that a 23-25 year old is incarcerated for any period of time and the mean observed incarceration period given that the 23-25 year old is incarcerated. 23-25 year-olds with juvenile histories are more likely to be incarcerated than 23-25 year-olds without juvenile records under the old guidelines. While the percentage of delinquent offenders who are incarcerated, depicted in Figure 2a, declines over time, it is evident that changes in the intensive margin, show in Figure 2b, are driving the break in trend at the date of the guideline change in Figures 1a and 1b. In

[87] The exclusion of incomplete sentences results in the visible trend downwards over time. For the purpose of illustration, I exclude the sentences of individuals who have offense scores which are greater than 12.

three of the six months after the guidelines change, all 23-25 year-olds appear to be incarcerated at the same rate, but in the other four six-month intervals the difference in the percentage of former delinquents and non-delinquents is only slightly smaller than the difference under the old guidelines. Therefore I expect to find that the guidelines change had only a moderate impact on whether or not a 23-25 year old with a delinquent history would spend any time in jail or prison relative to how long they are incarcerated. Examination of the sentencing guidelines for drug, property, and violent offenses reveals some intuition for why increases in offender score would affect incarceration on the intensive but not extensive margin. Someone convicted of murder will be incarcerated even if it their first offense, while someone convicted of possession of drug paraphernalia will typically not be incarcerated for that offense, even if they have a long criminal record. [88]

While being sentenced after July 1st 2001 is arguably unrelated to other characteristics affecting criminal behavior, there are drawbacks to simply comparing the incarceration lengths of delinquent 23-25 year-olds before and after the law change. Estimates of the reduction in time served by former delinquents due to the regime change can be biased by changes over time in the legal and illegal economy, detention center capacity, or public attitudes towards incarceration. To control for changes in these unobserved variables, I will employ a difference-in-difference strategy. I estimate the decrease in incarceration time due to the exclusion of delinquent points as the difference in time served by delinquent 23-25 year olds before and after the guidelines change, minus the difference in incarceration length of non-delinquent 23-25 year-olds before and

[88] DiIulio (1996) points out that the most controversial aspect of California's three strikes law was the increased weight of an individual's criminal history over the immediate offense in determining whether or not to incarcerate someone.

after the guidelines change. Because they received identical offender scores under the old and new guideline regime, the change in the incarceration length of non-delinquent 23-25 year-olds provides a natural comparison group for delinquent 23-25 year-olds.

This difference-in-difference approach will provide an unbiased estimate of the effect of the guidelines change on time served if the changes in unobserved factors that influence incarceration length affect both non-delinquent and delinquent 23-25 year-olds equally. This seems plausible given that, as shown in Figures 1a and 1b, the year to year variation in average time served by former delinquents and non-delinquents appear similar. As noted earlier, the impetus for the 2001 guidelines change was equity; the Sentencing Commission felt that former delinquent and non-delinquent 23-25 year-olds should not be treated differently in sentencing, given that they are not treated differently in other parts of the criminal justice system. Some evidence in support of the idea that, after age 23, delinquents and non-delinquents are similar can be drawn from Figure 3, which displays the average number of arrests per year of former and non-delinquents convicted at 23-25. Arrest rates for the two groups are generally equivalent after age 23.

Sentence length measures a duration of time. As such, it can take on integer values that are no less than zero. In addition, the distribution of sentence lengths is skewed right, with a few particularly long sentences causing the mean sentence to be larger than the median sentence and the standard deviation of sentence lengths to be higher than the mean sentence. In order to accommodate these aspects of the dependent variable, I model the effect of the guidelines change on incarceration length using a censored negative binomial model.[89] There is some intuitive appeal to this specification-

[89] Using a negative binomial model in this case is equivalent to assuming that the duration of incarceration follows a gamma distribution, which can have either negative or positive duration dependence. See

it implies that, ceteris paribus, "worse" lucky former delinquents would have received longer sentence enhancements under the old guidelines than lucky former delinquents with minor adult records convicted of less serious offenses.

Approximately 23% of the 23-25 year-olds in my sample serve no time in jail or prison. I previously argued that a judge's decision to incarcerate an offender is different from the decision of how long the incarceration will be, particularly in regard to offender score. In this case, the resulting mass point at zero can lead to spurious overestimation of the overdispersion parameter.[90] I therefore model the probability of being incarcerated and length of incarceration given imprisonment separately.[91]

I assume that, for all convictions that result in incarceration, actual sentence length S_i* (measured in days) follows a negative binomial distribution, with

$$P\left(S_i^* = y\right) = \frac{\Gamma(\delta + y)}{\Gamma(y+1)\Gamma(\delta)}\left(\frac{\delta}{\delta + \lambda_i}\right)^{\delta}\left(\frac{\lambda_i}{\delta + \lambda_i}\right)^{y}$$

where

$$(1) \quad \lambda_i = exp\left(\omega + \beta^N Facts_i + \theta_{OG} OldGuide_i + \theta_D D_i + \theta_{OGD}\left(OldGuide_i * D_i\right) + \varepsilon_i\right)$$

I model the actual incarceration spell, S_i*, of male 23-25 year-olds in Maryland as a function of the facts of the case, $Facts_i$; a dummy variable, $OldGuide_i$, that equals one if

Cameron and Trivedi (1998) pg 109. The benefit of using this estimation procedure relative to a standard censored duration model is that the desired marginal effect, the extra number of days an individual would have been incarcerated, can be calculated in a straightforward manner.

[90] Gurmr and Trivedi (1996)

[91] I use the Stata module to estimate censored negative binomial regression as survival model developed by Hilbe (2005). Unless the probability of not being incarcerated is unrelated to the length of incarceration, this will produce inconsistent estimates of the effect of the guidelines change on incarceration length. Attempting to control for sample selection by including the inverse mills ratio derived from the first stage probit model as an independent variable in the negative binomial regression, analogous to the Heckit approach, does not result in any statistically or economically significant change in the magnitude of the marginal effect. Recall that a change in offender score has only a small effect on the probability of being incarcerated. The stability of my incapacitation estimates over a large range of possible sentence enhancements, as show in the second and third columns of Table 4, lead me to believe that this econometric limitation does not have a substantial effect on my final estimates of crimes averted by incapacitation.

the sentence took place under the old guidelines regime; the total delinquent points, D_i, that the offender would qualify for if the guidelines had not been revised; and the interaction of total delinquent points and being sentenced under the old guidelines. I identify the sentence enhancement unlucky former delinquents received as the difference in expected sentences of former delinquents before and after July 1st 2001 that is different from the change in expected sentences of non-delinquents. I control for other observed factors affecting sentence length by including additional information from the sentencing worksheet. These facts of the case, $Facts_i$, include the offender score minus any delinquent points, the offense scores of the three most serious offenses,[92] and the total number of criminal acts for which the offender is convicted. I also control for whether or not the offender is black, whether or not the offender is later convicted of an additional offense (that could have occurred at any time) while serving the current sentence, and the offender's age at the time of the most serious offense. Since additional unobserved factors may be related to both my observed characteristics and sentence length, I include dummy variables for the jurisdiction of sentence, which control for time-invariant differences in judicial and policing practices across counties, and either a set of bi-annual dummy variables[93] or jurisdiction-specific time trends, which control for statewide changes over time that may affect sentencing such as political pressure (circuit court judges are elected every two years), prison overcrowding, or statewide changes in police

[92] While I include the total number of convicted criminal offenses, I control for the offense types and offense scores for only the three most serious convicted offenses. The Sentencing Commission's Guidelines worksheet only contains spaces for three offense scores, and additional worksheets must be filled out for any other criminal acts to be included. Some individuals in my sample are convicted of over 30 criminal acts in one sentence, but these represent less than 5% of observations in my sample.
[93] In a linear model, the difference-in-difference, which is marginal effect of interest, is completely captured in the coefficient on the interaction term. In a non-linear model such as this one, the first order effects do not completely drop out of the marginal effect, and therefore must be identified. In order to be able to identify any first order effect of being sentenced under the old guidelines, in the fixed effects model, I drop both bi-annual dummy variables in 2001. See Ai and Norton (2003)

tactics. Because the offense scores of drug, property, and violent offenses are calculated differently, I allow the effect of the offense score to vary with offense type,[94] and I adjust the standard errors in my model to allow for unobserved correlation over time in incarceration length for offenses of the same type within a jurisdiction.[95]

In 766 cases I observe that an individual enters prison or jail but is not released. I do not know S_i* in these right censored cases.[96] Assuming that the censoring is unrelated to having a delinquent, there is still some information that can be obtained from these censored observations that places an upper bound on the relationship between the inclusion of delinquency points and sentence length. Imagine two sentences of two otherwise identical former delinquents, one lucky and one unlucky. The unlucky delinquent is incarcerated for one year and then released. I observe that the lucky delinquent is incarcerated for at least six months, but is not released. Based on those two observations, I can conclude that the unlucky delinquent spent at most six additional months incarcerated due to his delinquent points. In order to estimate the true relationship between the guidelines change and total sentence length the estimation procedure is split into two further steps; the effect of the independent variables on sentence length and the effect of the independent variables on whether or not the sentence is completed. Let S_i^c be the length of the spell completed at the time when the sample was drawn. The relationship between the actual length and measured length is then

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94 Ideally, I would estimate single event drug, property and violent convictions separately, but because of my limited sample size I am unable to do so. The Maryland Sentencing Guidelines provide some rational for pooling all offense types; in the case of multiple event convictions, the appropriate recommended sentence is the sum of all the minimum and maximum sentences regardless of offense type.

95 In particular, I am controlling for local police agency "crack downs" on particular types of crime, used by Levitt (1998b) as a source of variation.

96 To the best of my knowledge, this is due to an actual incomplete sentence, not missing data on release.

$$S_i = \begin{cases} S_i^* & \text{if } S_i^* < S_i^c \\ S_i^c & \text{if } S_i^C \leq S_i^* \end{cases}$$

where S_i=S_i* if S_i* is greater than zero and the offender is released by the end of my sample. Let d_i equal one if sentence i is complete, and zero otherwise. The likelihood function is therefore

$$L(\Lambda_i) = \sum \left[d_i \, ln(h(S_i, \Lambda_i)) + (1 - d_i) ln\left(1 - H\left(S_i^c - 1, \Lambda_i\right)\right) \right]$$

where Λ_i represents the values of the independent variables associated with sentence i , $h(S_i, \Lambda_i)$ is the negative binomial probability density function evaluated at S_i, and $H\left(S_i^c - 1, \Lambda_i\right)$ is the negative binomial cumulative density function evaluated at S_i^c -1. Bear in mind that the inclusion of censored sentences forces me to rely heavily on the assumption that true sentence length, and thus the error term ε_i, follow a negative binomial distribution since I am essentially imputing the values of S_i* for the censored observations based on the values of Λ_i and the distribution of ε_i,. The marginal effect of interest is the relationship between the guidelines change, delinquent points and S_i*, so following Wooldridge (2001) I do not include an adjustment for incomplete spells in my calculation of the sentence enhancement.

The goal of this first stage is to estimate the hypothetical sentence "disenhancement" given to lucky former delinquents relative to unlucky former delinquents. Due to the nonlinear specification of the relationship between sentence length and the independent variables, the estimated marginal effect of having delinquent points included in the offender score on sentence length is not the coefficient on the interaction term, θ_{OGD}, alone. Since I exclude sentences that result in no incarceration time, it follows that expected sentence length for any given conviction is equivalent to the expected number of days incarcerated multiplied by the probability that the sentence results in any incarceration.[97] Mathematically, this can be expressed as

$$E\left(S_i^* \mid X_i\right) = \frac{\exp\left(\omega + \beta^N Facts_i + \theta_{OG} OldGuide_i + \theta_D D_i + \theta_{OGD}\left(OldGuide_i * D_i\right)\right)}{\delta} * P\left(S_i^* > 0 \mid X_i\right)$$

where

(2) $$P\left(S_i^* > 0 \mid X_i\right) = \Phi\left(\varpi + \beta^P facts_i + \alpha_{OG} OldGuide_i + \alpha_D D_i + \alpha_{OGD}\left(OldGuide_i * D_i\right)\right)$$

where Φ(x) is the standard normal cumulative distribution function evaluated at x. Using the chain rule, the additional number of days per delinquent point that an offender would have been incarcerated after sentence i is estimated to be

$$Enhancement_i^k = \Phi\left(\hat{\beta}^P facts_i + \hat{\alpha}_{OG} OldGuide_i + \hat{\alpha}_D D_i + \hat{\alpha}_{OGD}\left(OldGuide_i * D_i\right)\right)\left(\frac{\Delta_i^k}{k}\right) + \frac{\exp\left(\hat{\beta}^N Facts_i + \hat{\theta}_{OG} OldGuide_i + \hat{\theta}_D D_i + \hat{\theta}_{OGD}\left(OldGuide_i * D_i\right)\right)}{\hat{\delta}}\left(\frac{\Omega_i^k}{k}\right)$$

where

$$\Delta_i^k = \left(\frac{exp\left(\hat{\beta}^N Facts_i + \hat{\theta}_{OG} + k\hat{\theta}_D + k\hat{\theta}_{OGD}\right) - exp\left(\hat{\beta}^N Facts_i + k\hat{\theta}_D\right)}{\hat{\delta}}\right) - \left(\frac{exp\left(\hat{\beta}^N Facts_i + \hat{\theta}_{OG}\right) - exp\left(\hat{\beta}^N Facts_i\right)}{\hat{\delta}}\right)$$

and

[97] Recall that some of the independent variables that effect sentence length, particularly being convicted of an additional offense while being incarcerated for conviction i and jurisdiction of sentence are not appropriate for inclusion as independent variables affecting the probability of being incarcerated. It is obvious why only those incarcerated for sentence i could be reconvicted of an additional offense while incarcerated. Jurisdiction of sentence is excluded because all individuals convicted in jurisdictions that are not Anne Arundel County, Prince George's County, Washington County, or Baltimore City are sentenced to state facilities, meaning that being sentenced outside of one of these four jurisdictions is a perfect predictor of incarceration.

$$
\begin{aligned}
\Omega_i^k = &\left(\Phi\left(\hat{\beta}^P facts_i + \hat{\alpha}_{OG} + k\hat{\alpha}_D + k\hat{\alpha}_{OGD}\right) - \Phi\left(\hat{\beta}^P facts_i + k\hat{\alpha}_D\right)\right) \\
&- \left(\Phi\left(\hat{\beta}^P facts_i + \hat{\alpha}_{OG}\right) - \Phi\left(\hat{\beta}^P facts_i\right)\right)
\end{aligned}
$$

For each sentence I first use the estimated values of α_{OG}, α_D, α_{OGD}, θ_{OG}, θ_D, and θ_{OGD} to find the expected number of days incarcerated for all possible combinations of D_i and $OldGuide_i$. Next, for each possible number of delinquent points, I isolate the number of extra days that sentence would be if the only difference was the inclusion of any delinquent points in the offender score by subtracting the expected sentence under the new guidelines from the expected sentence under the old guidelines.

I am left with two marginal effects evaluated at each observation, one based on a change from zero to one delinquent point, and one based on a change from zero to two delinquent points. I then calculate the expected sentence enhancement per point by weighting those two marginal effects by the fraction of the delinquent population that qualifies for each point. The mean value of this enhancement per point over all positive sentences is taken to be the mean sentence disenhancement per point that lucky 23-25 year-olds with delinquent histories received because their offender scores include their delinquent histories.

4.52: Results

Table 4-3 reports my estimates of the pre-2001 delinquent "enhancement." In columns one and two I include six month dummy variables to control for changes in incarceration length over time. In column one I present estimated values of α_{OG}, α_D, and α_{OGD}. I estimate the coefficient on the interaction of the dummy $OldGuide_i$ and potential delinquent points D_i,, α_{OGD}, to be 0.279, with a clustered standard error of 0.104. Taking the first order effects of actually delinquent history and guidelines regime into account, my results suggest that each delinquent point included in the offender score increased the probability being incarcerated by 6 percentage points, slightly less than 10% of the sample mean. When I focus on the intensive margin in the negative binomial model, I estimate that θ_{OGD} is 0.211. The first-order effects of delinquency and the old guidelines regime are of roughly equal magnitude and opposite sign, implying that lucky juvenile delinquents also received shorter sentences on average than non-delinquents sentenced under the new guidelines regime. Incorporating the change in probability of being incarcerated, I estimate that unlucky delinquents were incarcerated for an additional 262 days per delinquent point relative to lucky former delinquents.

My identification of the primary effect of being sentenced under the old guidelines regime is largely based on the difference between convictions in the in first and second halves of 2001 when six month fixed effects are included. An alternative specification is to use jurisdiction-specific time trends so the primary effect to be averaged over all years. When I include jurisdiction specific time trends instead of half year fixed effects, the first order effect of being sentenced under the old guidelines is no longer precisely estimated, but the marginal effect of official delinquent points on the probability of being incarcerated and expected days incarcerated are roughly equivalent. Using the estimated values of α_{OG}, α_D, α_{OGD}, θ_{OG}, θ_D, and θ_{OGD} I calculate the mean sentence disenhancement per point to be 222 days, meaning that a 23-25 year-old sentenced after July 1st 2001 who qualified for one delinquent point would have been on average released approximately 222 days later under the old system than he actually was. Using the delta method, I estimate a 95% confidence interval for this mean effect to be 140 to 304 days.

The lucky former delinquents who completed their sentences were incarcerated for an average of 402 days. My estimation results suggest that, ceteris paribus, if sentenced under the old guidelines these offenders would have been incarcerated for an additional 75 to 601 days. In both the fixed effect and time trend models the magnitudes of the additional coefficients, reported in Table 4-3, generally confirm typical *a priori* assumptions about incarceration and crime. The number of criminal events has a statistically significant positive relationship to incarceration length, and individuals with a larger offender or offense score act have significantly longer incarceration spells than those with lower offender or offense scores.

Regression estimates that use censored data may be greatly affected by the assumed distribution of the unobserved part of the dependant variables; i.e., the incomplete incarceration spells. I test the sensitivity of my estimate of the delinquent enhancement to my econometric specification in the next two columns. First, I model the relationship between sentence length, $Facts_i$, D_i and $OldGuide_i$ as a censored duration process using a double limit Tobit model and six month fixed effects. Taking the corner solution on the left hand side into account, these coefficient estimates suggest a mean sentence enhancement of 186 days, well within the 95% confidence interval produced by the negative binomial model.

This is encouraging since the censored Tobit model assumes that sentence length follows a normal distribution instead of a negative binomial distribution. The similar magnitude of the estimated change in sentence length due to the guidelines change suggests that my results are not driven by my distributional assumptions. An OLS estimate of the effect of the guidelines change on completed sentences is yields an estimated mean sentence enhancement of 123 days per point (se=55.8).[98] Alternate functional forms do not produce mean estimates that are statistically different from each other, but the magnitudes of the differences are non-trivial. With this in mind, I will test the robustness my estimates of crimes averted through incapacitation to changes in the size of the estimated sentence enhancement.

Another possible concern with this analysis is that my identification is contaminated by other unobserved factors that cause the lucky delinquents to serve shorter sentences but is unrelated to this particular guidelines change. For example, there could have been some movement towards consistent sentencing that resulted in age at offense becoming the dominant factor in sentencing decisions, or a reduced emphasis on the offender score as a whole. To address this possibility, I test my identification strategy by repeating the difference-in-difference analysis with delinquent and non-delinquent 20-22 year-olds.[99] In the final two columns of Table 4-3 I present results from estimating the difference-in-difference censored negative binomial and probit models for males convicted of crimes they committed between the ages of 20 and 22 years old using jurisdiction specific time trends. In both the probit and negative binomial model the estimated coefficient on $OldGuide_i *D_i$ is negative and the standard error is almost as large as the estimate. The estimated coefficient on the primary effect of having delinquent points D_i, in the probit model is 0.47 (se=0.08), and 0.07 (se=0.03) in the negative binomial, implying that unlike the older age group, 20-22 year-old former delinquents were both more likely to be incarcerated and were incarcerated for longer

[98] Restricting a censored OLS estimate to only convictions resulting in incarceration (n=2537) produces an estimated sentence enhancement of 251.6 days per point (se=76.9). Limiting my sample to only completed spells of incarceration (n=1,771) suggests an enhancement of 113.02 days per point (se=61.4).
[99] Descriptive characteristics of this sample are available in Appendix B.

periods relative to non-delinquents. Using the same procedure outlined above to calculate marginal effects, the estimated values of α_{OG}, α_D, α_{OGD}, θ_{OG}, θ_D, and θ_{OGD} for this sample suggest that qualifying for delinquent points and being sentenced under the old guidelines actually reduced the sentences of 20-22 year-old former delinquents by 30 days, with a standard error of 31 days. Repeating the analysis with 20-22 year-olds suggests that the reduction in incarceration length for delinquent 23-25 year-olds is in fact due to the exclusion of their juvenile records from the sentencing decision, and not some other arbitrary correlation between date of sentencing and delinquent history.

4.6: The Incapacitative Effect of Sentence Enhancements

4.61: The Incapacitation Sample

Each delinquent point assigned to a 23-25 year old increased the probability of incarceration by 7 percentage points. The goal of this paper is to estimate the additional incapacitative effect of sentence enhancements, and I therefore have a two-part decision rule for selecting the relevant sample of 23-25 year-olds. First, the offender must serve at least one day in jail or prison. Second, I must observe the offender when he is released. This decision rule limits my sample to 133 out of 230 delinquent 23-25 year-olds, 73 of whom were sentenced after 2001. Focusing on lucky former delinquents, 28 out of 133 sentences of lucky former delinquents do not result in incarceration, and an additional 32 sentences are not complete. Of the 97 sentences of unlucky former delinquents, only 6 do not result in incarceration, and 34 are not complete. Columns 2 and 3 of Table 4-4 compare characteristics of the lucky and unlucky former delinquents who were incarcerated and released. In terms of offender and worst offense scores there does not appear to be a substantial difference between the lucky and unlucky delinquents who are released, in fact I cannot reject the null hypothesis that the prior records of these two groups of offenders are equal (t=0.78). This lends support to my assumption that even within this particular subset of delinquent offenders, the post release criminal behavior of lucky offenders can be used as a reasonable estimate of the crimes unlucky offenders would have committed had their juvenile history not been a factor at sentencing.

Using the estimated sentence enhancement that lucky delinquents would have received under the old guidelines, I calculate a hypothetical late release date for each of the 73 former delinquents. Up to four possible events can happen between these two dates. The former delinquent can be arrested, he can be re-incarcerated, he can be re-released, and he can also be right censored if his hypothetical release date is after January 5th, 2006. I define the free time of these 73 former delinquents as the difference between their hypothetical release date (or January 5th, 2006, whichever comes first) and actual release date, subtracting out any time they spent re-incarcerated in that period. Taking re-incarceration into account, these offenders spent an average of 164 free days on the street when they would have been incarcerated under the old guidelines, with the shortest potential free period being one day, and the longest slightly over one year. I observe the number of arrests each of these individuals accumulates during their free time and divide the number of arrests by the number of days they were at risk of committing a crime. Multiplying this daily arrest rate number by 365 gives me an annual arrest rate.

For many reasons, arrest rates do not necessarily correspond with actual offense rates. Not all crimes reported to the police are solved (“cleared by arrest”), and some crimes go unreported to law enforcement agencies. The Uniform Crime Reports record both the number of offenses reported to an agency and the number of offenses that are cleared- the ratio of offenses known to offenses cleared is called the clearance rate for that crime. One possible way to correct for the inexact mapping between arrests and offenses is to multiply the number of times an individual is arrested by the clearance rate for the arresting agency.[100] This calculation is straightforward for index crimes (murder, manslaughter, assault, robbery, rape, car theft, burglary, and larceny). The clearance rate is the number of confirmed offenses known to agency j in year t divided by the number of offenses cleared due to an arrest by agency j in year t, as reported in the Uniform Crime Reports.[101] Because no measurement of drug offenses analogous to the Uniform Crime Reports exists I report only the number of expected arrests on drug charges per person per year. [102]

4.62: Results

Table 4-5 presents my estimate of the average number of crimes prevented through incapacitation per person per year, based on the number of times the 73 lucky former delinquents were arrested between their actual released date and hypothetical release date. Just over 30% of the lucky former delinquents were arrested for a total 102 criminal acts while they otherwise would have remained incarcerated, and their offense rates suggest that incarcerating a former delinquent for one additional year prevents 2.8

[100] Spelman (1994) says there is "no dispute" over the use of this procedure (p 168).

[101] Appendix C displays these average clearance rates by index crime for law enforcement agencies nationwide and in Maryland. Nationwide, an average of 7.61 crimes are reported to police for every "cleared" offense, which is approximately equivalent to clearance rates in Maryland. As noted earlier, criminologists have traditionally argued that most crimes are committed by a small fraction of the population, implying that one criminal committing seven crimes is a more likely scenario than seven offenders each committing one crime, which is the assumption this exercise requires. In addition Spelman (1994) notes that self reports of the number of times an individual is arrested divided by the number of times that individual reports offending is reasonably close to the inverse of the clearance rate for the respective area law enforcement agencies. The inverse of the clearance rate can be interpreted as the probability of arrest after committing a given crime in a particular agency's jurisdiction.

[102] Estimates by Abt Associates, the Uniform Crime Reports, and the Office of National Drug Control Policy imply that anywhere from 40 to 2,030 drug transactions occur per arrest for drug offenses. Thanks are due to Jonathan Caulkins for directing me to these national estimates and pointing out their limitations.

arrests in that year. Of those arrests, the state files an average of 2 charges per year against former delinquents in that marginal year[103] A large fraction of these offenses are drug offenses – 1.6 drug arrests are made per person per year, roughly 57% of all charges. Applying crime and agency specific clearance rates to the non-drug arrests, the arrests accumulated by individuals suggest that 23 to 25 year-old unlucky former delinquents would have been involved in 1.4 index crimes per person per year had they not been incarcerated. Of those individuals who recidivate, the mean number of arrests per person per year is 9.7, with 6.9 charges filed per person per year. Clearance rates suggest that individuals who are rearrested are probably involved in 5.2 index crimes per person per year. These estimates are notably similar to the "high-rate offender" rate of 7 offenses per person per year assumed by Greenwood et al. (1994). Lucky delinquents are arrested for a large number of violent crimes during their free time, 20% of the arrests accumulated are for assault or attempted murder[104] 4% for arson, and 3% for weapons violations. Approximately 17% of arrests are for property and general crimes such as car theft, conspiracy, trespassing, and uttering false statements to police. The pattern of recidivism suggests some degree of specialization in crime; individuals convicted of drug offenses are arrested an average of 3.1 times per person per year, roughly equal to the mean arrest rate for the entire group, but appear to be less likely than average to be involved in index crimes, implying that they are re-arrested for drug offenses.

In order to evaluate the benefit crime reductions, it is useful to assign a monetary cost to criminal acts. Using the cost of crime estimates derived in Miller, Cohen, and

[103] I define "arrests" as the number of charges the arresting agency files against the individual, and the number of "charges" as the number of charges which do not result in a decision nolle prosequi. No arrests accumulated by individuals during their free window are dismissed by the court.
[104] Because these charges are for attempted murder, for cost-benefit purposes I classify these charges as assaults.

Wiersema (1996), the social cost of the crimes potentially prevented through by incapacitation due to a one year sentence enhancement varies from a low estimate of $16,000, if each arrest is assumed to represent one crime, to just under $27,000, if each arrest is inflated by the appropriate clearance rates. More recent studies (Cohen, Rust, Steen and Tidd (2004), Rockoff and Linden (2006)) estimating the social cost of crime have suggested that the social cost of crime may be significantly higher, but to my knowledge Miller Cohen and Wiersema (1996) provide the most disaggregated social cost estimates for the largest number of crimes. In addition, when evaluating the social benefit of a program like sentence enhancements, a conservative estimate of social benefits is useful as a strong test of cost effectiveness.

In order to provide a possible range of likely values of incapacitation and test the sensitivity of my estimates to changes in the first stage, I alter the expected sentence disenhancement received by each lucky offender and re-estimate these measures of criminal activity. These estimates are presented in columns 3 and 4 of Table 4-5. Clearly, more offenders recidivate when the window of "free time" is lengthened -over 38% are rearrested after 208 days, and only 20% of offenders are re-arrested within an average of 113 days. The magnitude of the expected crime rate however, is relatively stable regardless of the length of "free time" for which offenders are observed, implying that offenders who recidivate immediately are relative high rate offenders relative to those who recidivate later. The stability of my estimates over the different lengths of post release window lends weight to the conclusion that the expected number of arrests avoided through incapacitation due to a sentence enhancement is approximately 2.8 per person per additional year of incarceration. The expected number of charges avoided by incapacitation is probably about two per person per year. Applying the previous mapping of arrests into crime suggests that offenders would have been involved in between 1.4 and 1.6 index crimes per person per year when incarcerated. The magnitudes of the social cost of crimes averted per person per year incapacitated are robust to my choice of the length of the sentence enhancement, although the standard errors of the mean estimates increase when the fraction of offenders who recidivate falls.

These estimates are substantially lower than estimates produced by previous studies that used individual level data, which ranged from 187 crimes averted per person per year to fewer than 12. This difference highlights the contribution this paper makes to the incapacitation literature. First, these previous individual level studies estimated the number of crimes averted by incapacitation by sampling current prison or jail inmates. I estimate the number of crimes averted by additional incapacitation due to sentence enhancements for offenders who are about to be released, who will on average be less criminal than the average inmate. Second, there are good reasons to believe that a given offender's criminal activity prior to incarceration is significantly higher than the criminal activity he would partake in if released early.[105] Studies that survey the stock of convicted offenders and use past criminal behavior to create mean estimates of incapacitation create samples in which no individual has non-zero hypothetical offense rate by construction. In contrast, my estimates suggest approximately two thirds of individuals would not recidivate during the enhancement portion of their sentence. In addition, I present incapacitation estimates for a particular subset of the offending population. While past estimates often break down the benefit of incapacitation by

[105] This is one of Zimring and Hawkins (1995) critiques of "window" based incapacitation estimates. Some evidence of the "spike" in criminal activity prior to incarceration is seen in Figure 3, which plots the average offense rate by age for individuals convicted between 23 and 25.

offense type, my estimates are averaged over all offense types, but my population is comparatively homogenous with respect to age. While not necessarily at the end of their criminal career, 23-25 year-olds, who are between the ages of 25 and 27 when released, are past their peak criminal years, which are generally agreed to be between 18 and 21.[106] Since criminality decreases with age,[107] one would expect that the number of crimes averted through incapacitation to decline as the population being targeted ages. Finally, it should be noted that past studies used data from the 1980s and 1990s, while my estimates are based on data from 2001 to 2005. If the decline in crime over the end of the 20th century is due to individuals committing less crime, as opposed to fewer individuals committing crime, the number of crimes averted through incapacitation in the 21st century should be reasonable assumed to be lower than in the 20th century.

I do not adjust my incapacitation estimates to include unreported crime, which means that my estimates may underestimate the true number of crimes in which released offenders were involved.[108] Inflating the individual arrests that occur during the "free time" of a lucky offender by a national average of unreported to reported offenses using the National Crime Victimization survey could either overstate or understate the number of crimes potentially avoided through incapacitation. The percentage of crimes committed by recently released offenders that are known to police may be higher or lower than for the average offender. Recently released offenders may still be under justice supervision in the form of parole, and may be the "first suspects investigated" for

[106] Hirschi and Gottfredson (1983), Farrington (1986), Blumstein and Cohen (1987)
[107] ibid
[108] DiIulio and Piehl (1991, 1995), for example, use self reported estimates on criminal activity, and assuming that inmates report accurately, avoid this problem. The Bureau of Justice Statistics estimates that almost half of all violent crimes and 60% of all property crimes are unreported to the police, although the fraction of crimes that go unreported has been declining over time.

any given crime known to police.[109] At the same time, the older offenders in my sample may be "better" criminals than the average one time offender, due experience gained during their long criminal career.

4.63: Incarceration and Crime Rates

I estimate that 23-25 year-olds incarcerated for one additional year early would likely have been arrested at a rate of approximately 2.8 times per year per person if released early, and prosecuted for two offenses. There are two important and potentially confounding issues which must be addressed in order to say that these crimes are averted through incapacitation: temporal displacement of crime and criminal replacement.

Incarcerated offenders who receive sentence enhancements may commit the crimes they were prevented from committing during incarceration after they are released. If this were the case, sentence enhancements would only cause temporal shifts in crime. This phenomenon has been observed over short periods,[110] and it is possible that the unlucky former delinquents sentenced under the old guidelines committed more crimes after they were released to make up for the "free time" that they did not receive. Temporal displacement can be thought of in terms of "calendar age" and "street age". Calendar age is the number of years an individual has been alive, whereas street age is the number of years that an individual has been free to commit crime. If incarceration results in temporal displacement of crime, then criminality would be strongly decreasing in street age, but less so in calendar age. Figure 4 presents a graphical depiction of perfect temporal displacement, with actual age on the horizontal axis and number of crimes committed per year on the vertical axis. The number of crimes committed per

[109] Raphael and Stool (2004)
[110] Jacob and Lefgren (2004) , Hessling (1994)

year decreases with age while both offenders X and Y are free, but while offender Y's street age and potential crime rate remain constant while Y is incarcerated. As a result, upon release offender Y commits more crimes per day than offender X. If this hypothetical comparison reflects the actual relationships between the crime rates of lucky and unlucky former delinquents, my estimates of crimes averted by incapacitation have no real meaning in terms of policy; sentence enhancements have no effect on the total number of crimes unlucky offenders actually commit over their lifetimes.

I am able to test for temporal displacement of criminal activity by comparing the offending rates of the lucky 23-25 year-olds with the post release offending rates of the unlucky former delinquents sentenced prior to July 1st 2001 by using the inclusion of delinquent points in the offender score as an instrument for the difference between an offender's calendar age and street age. The unlucky delinquents will necessarily be older than similar lucky delinquents when they are released, which should decrease their relative offending rate if there is no temporal displacement in crime. This lack of common support will force me to rely heavily on the assumption of a constant relationship between age and crime. In order to correct for these problems, I shift the "window" in which I observe the criminal behavior of lucky delinquents forward. For example, I compare the crime rate of an unlucky former delinquent in the 222 days after their release with the offending rate of an otherwise identical lucky former delinquent between 222 and 444 days after their release. Adjusting the window in which I observe the lucky 23-25 year-olds will control for differences in calendar age. The creation of distinct windows of observation provides an intuitive frame through which to interpret the coefficient estimates. Consider a three period model with two identical agents and two possible states, incarceration and freedom. Both agents are incarcerated in period one. In period two the lucky agent is free and the unlucky agent is incarcerated. In period three both agents are free. The difference between the criminal behavior of the lucky and unlucky agents in period three reflects the effect of their different treatments in period two on their future behavior, which is captured by the number of delinquent points included in the individuals offender score.

My estimation includes all 57 former delinquents who are sentenced under the old guidelines, serve at least one day and are released prior to the end of my sample. However, 23 of the 73 former delinquents sentenced under the new regime are re-incarcerated during their initial post-release window and not released, and seven released only a short time prior to January 5th 2006, the end of my arrest data. Individuals who are excluded because of censoring should not necessarily bias my estimates, since I am controlling for the number of days of free time that I observe. However, because the lucky former delinquents with the highest criminal propensities are the most likely to be incarcerated for the entire second post-release window, I expect my estimate of temporal displacement to be biased upward. In order to increase the precision of my estimates of the relationship between sentence length and criminal behavior, I also include 1,628 incarcerated and released non-delinquent 23-25 year-olds during the 222 days after they are released in this regression. Of these non-delinquents, 717 of them were sentenced under the old guidelines. Descriptive characteristics of the former delinquents and non-delinquents used in my test for temporal displacement are presented in Table 4-4. In terms of adult criminal history, which I expect to be directly related to underlying criminality, the 57 unlucky 23-25 year-olds are similar to the group of 73 lucky offenders. By the same measure, the 50 lucky offenders who I observe in the second free window do appear to be potentially less likely to re-offend, although I cannot reject the null hypothesis that the adult offender scores are the same (t=1.04).

Offenders are released into different social, economic, and criminal environments. The criminal environment, in particular, can vary dramatically based on the number of police per capita, the policing strategy of the local force, and the relative vibrancy of the legal and illegal markets in the area. For this reason, I include the jurisdiction of sentence and year of release as proxies for environment.

I assume that the number of crimes an offender i commits in their free window, C_i, follows a negative binomial process, with

$$P(C_i = z) = \frac{\Gamma(\delta + z)}{\Gamma(z+1)\Gamma(\delta)} \left(\frac{\delta}{\delta + \kappa_i} \right)^{\delta} \left(\frac{\kappa_i}{\delta + \kappa_i} \right)^{z}$$

and

(3) $$\kappa_i = FreeTime_i^{\mathrm{E}} \exp\left(\alpha + X_i\phi + \beta_{Cal}Calendar_i + \beta_{Juv}Juv + \beta_{Del}Del_i + \varepsilon_i\right)$$

I model the number of times an individual is arrested in a given window as a function of their age in days at the beginning of the window, $Calendar_i$, a dummy variable indicating that the individual is a former delinquent, Juv_i , and a second dummy if their delinquent history was included in their offender score, Del_i. I include in *Xi* offender i's offender score minus any delinquent points, recall that this includes information on i's adult criminal record and history of committing crimes while under state supervision, whether or not his most serious convicted offense was a drug offense, and whether or not the individual is black. I also include jurisdiction specific dummy variables for the first year of the window in which I measure criminal activity of each offender. I use a negative binomial regression because of the count nature of the data and the overdispersion in the dependent variable; the average number of arrests and charges is approximately half the standard deviation, and even among incarcerated offenders most crimes are committed by a small fraction of the population.[111] Since the number of times an offender is arrested is mechanically related to the number of days he is at risk of offending (i.e., not incarcerated or censored) I allow the total number of days the offender is "on the street," $FreeTime_i$, to enter into the estimated equation logarithmically.[112] If the longer incarceration sentences caused the former delinquents sentenced under the old regime to increase their offending, the coefficient on the number of delinquent points included in the individuals offender score, β_{Del}, should be positive, indicating that relative to similar lucky delinquents of the same age, this group has a higher post-release offense rate.

Results of my tests for temporal displacement using both all arrests and only charges are displayed in Table 4-6. I fail to find a statistically significant relationship between street age and crime rates in all specifications, and β_{Del} takes both positive and negative signs. The signs of other variables generally follow a-priori assumptions. Age is negatively related to arrests and charges, while severity of adult criminal history and being black are positively related to arrests and charges. Given that I expect the true value of β_{Del} to be lower than my estimate of β_{Del}, these results could be interpreted as potential evidence of sentence enhancements causing specific deterrence, meaning offenders are rehabilitated during their longer prison sentences. However, I can only

[111] For a review of the statistical issues regarding estimating crime and arrest frequency with OLS and negative binomial models, see Pridemore (2005)
[112] Cameron and Trivedi (1998)

conclude that I do not find any evidence that the crime reduction due to incapacitation is undone by temporal displacement of crime.

A second threat to my incapacitation estimates is the issue of criminal replacement, which Ehrlich (1981) identifies in a seminal article on incarceration and crime.[113] The idea of criminal replacement is that some fraction of the crimes that would have been committed by incarcerated individuals is committed by non-incarcerated offenders. In regard to drug offenses and organized crime, criminal replacement is clearly observed. In fact, Caulkins et al. (1998) suggest that, in terms of reducing the total supply of illegal drugs, a drug "crackdown" in which more arrests are made is a more effective policy than increasing the sentence of one drug offender.[114] I am unaware of empirical evidence of significant criminal replacement for other types of offenses.

4.7: A Cost-Benefit Analysis

4.71: The Elasticity of Crime With Respect to Sentence Enhancements

I find evidence that sentence enhancements do lower crime through incapacitation, although the marginal number of crimes averted by incapacitation due to sentence enhancements is less than the average number of crimes prevented by incarceration in general. How do the social benefits of these crime reductions compare to the additional cost to society of imposing longer sentences? There may be non-negligible external social costs that are positively related to sentence length, although Kling (2006)

[113] An issue tangential to criminal replacement is co-offending. I have no way on knowing whether or not individuals in my data are arrested for offenses they commit with another person, but criminologists generally assume that co offending rates are highest relative to total offending among juveniles. With an average post release age of 25, I assume co-offending is not a large issue for my analysis.

[114] The intuition behind this finding is that arrests directly decrease the drug supply- drugs are seized- whereas an incarcerating a drug dealer only creates a possible opening for a new potential dealer.

fails to find that longer sentences reduce wages. Spelman (2000) argues that if the elasticity of crime with respect to prison capacity is more than 0.3, then the benefits of incapacitation should justify the total cost to society. A 2003 study by the Urban Institute reports that 9,448 people were been released from Maryland state prisons in 2001.[115] If each of these prisoners had been given a one year sentence "disenhancement," this would have reduced the average population of Maryland state prisons by 35.6% in 2001. According to the Uniform Crime Reports, 269,033 arrests were made by Maryland police in 2000. If all state prisoners released in 2001 were released in 2000 and all state prisoners behaved like the males sentenced between the ages of 23 and 25, the 9,448 people released in 2001 would on average have been arrested 2.8 times each during that year. This implies that 26,454 arrests were avoided through incapacitation, an 8.9% reduction in arrests. Assuming that the ratio of the arrests to crimes for recently released offenders is equal to the ratio of the aggregate levels reported to the Uniform Crime Reports, my estimates suggest that the elasticity of crime with respect to prison capacity is approximately 25%. If the 30% hurdle accurately reflects total social costs of incarceration, my results suggest that applying enhancements to the sentences of offenders older than 23 is not necessarily socially optimal, but should not be dismissed as bad policy without further investigation.

<u>4.72: The Marginal Cost of Sentence Enhancements</u>

If total government spending on incarceration in Maryland is typical of spending nationwide, my estimate of a 25% elasticity of crime with respect to prison capacity suggests that incapacitative effects alone do not necessarily justify applying sentence

[115] La Vigne and Kachnowski (2003).

enhancements to older offenders. Temporarily setting aside the issue of the social cost of sentence enhancements, the benefit of incapacitation in Maryland can be assessed by directly estimating the fiscal cost of housing an additional state prisoner for an additional year in Maryland.

In 2004, the United States Department of Justice estimated that the average annual operating cost of a state prison nationwide was $24,032 per inmate in 2005 dollars. Between 1989 and 2005, the state of Maryland spent approximately $23,748 per inmate, per year. Past studies of incapacitation, (DiIulio and Piehl (1991, 1995), Donohue and Siegelman (1998), Spelman (1994)) have evaluated the benefit of crimes averted against this average cost per prisoner. Average cost is only an appropriate benchmark against which to evaluate the benefits of incapacitation if the increase in state spending when a facility incarcerates its first prisoner is exactly the same as the increase in state spending when that facility incarcerates its 500th prisoner. The marginal cost of holding an additional inmate for an extra year does not equal the average cost per inmate, primarily due to the high fixed costs of maintaining and operating a prison. If the prison population increases by one person the state will have to provide an additional meal, but not an additional cook.

In principle, the marginal cost of an additional prisoner approaches infinity after the institution reaches full capacity, but anecdotal evidence and the existence of prison overcrowding legislation suggests that operationally, corrections departments will "make room" for new prisoners. Prior to 2000 the Maryland state budget books reported official prison capacity as well as average daily population. According to these sources, Maryland state facilities were operating at or above capacity over half of the time between fiscal years 1989 and 2000. Kuziemko and Levitt (2004) find that the increased number of prisoners convicted of drug offenses did result in the "crowding out" of inmates convicted of minor offenses, but the fraction of sentences served by offenders convicted of violent crimes or major property offenses was unchanged by increases in the inflow of inmates.

In order to estimate the marginal cost of housing a former delinquent for an additional year, I estimate the following equation:

$$\begin{aligned} Cost_{it} &= \alpha_i + \delta_t + EXP_{it}\theta + DOWN_{it}\phi + POP_{it}\beta \\ &+ (POP_{it} * EXP_{it})\beta_{\exp} + (POP_{it} * DOWN_{it})\beta_{down} + (POP_{it} * MAX_i)\beta_{\max} \\ &+ (POP_{it} * MIN_i)\beta_{\min} + \varepsilon_{it} \end{aligned} \tag{4}$$

Where $Cost_{it}$ is the total amount spent in 2005 dollars by facility i in fiscal year t, and α_i and δ_t are facility and year specific intercepts, respectively; EXP_{it} is a dummy variable that equals one if that facility expanded its capacity that year while $DOWN_{it}$ equals one if the capacity of facility i was downgraded in year t; MAX_i and MIN_i are dummy variables representing the security level of the facility; and finally, POP_{it} is the average daily population of facility i in year t, implying that β is the marginal effect of housing an additional inmate for one year on total state prison expenditures. I allow the marginal cost of an additional prisoner to vary according to whether there is a change in capacity, and whether or not the prisoner is housed in a maximum, medium, or minimum security facility. Data on average daily population, total facility expenditure, and capacity for a total of 24 state run institutions are taken from the Maryland State Budget Books from 1990-2007, which report actual values for fiscal years 1989-2005. The fiscal year 1993 is excluded due to missing data.

Estimates of the marginal cost of housing an additional prisoner are presented in Table 4-7. Controlling for upgrades and downgrades in official capacity, increasing the average daily population by one person for one year requires an increase in spending of \$11,350 (t=5.54), less than half the average cost of incarceration per inmate. Not surprisingly, in years in which the capacity of the facility increases, the marginal cost of an extra prisoner is about \$1,000 less. Housing an additional prisoner in a moderate security level facility costs the state \$13,030 (t = 5.29), approximately \$8,000 more than housing the additional prisoner in a minimum security facility. In order to address concerns that the marginal cost of a prisoner was increasing in prison population, I included average daily population squared as a dependant variable. Surprisingly, I estimate the coefficient on this term to be negative, although small in magnitude and statistically insignificant.

I estimate the cost of incarcerating an additional offender for an additional year to be at most \$13,800. This marginal cost is slightly more than half of annual expenditure per inmate. It is also roughly equivalent to the lower estimated social cost of crimes avoided per person per year through incapacitation in Table 4-5. Recall that there are potential unobserved factors that may cause me to underestimate the total cost of an additional year of incarceration, such as increased likelihood of HIV transmission during longer incarceration spells. In addition, because my estimates of crimes averted through incapacitation are produced using individual arrest histories, I do not take into account co-offending or criminal replacement. However, I am able to rule out temporal displacement in criminal activity. The social cost of crimes that could have been averted through incapacitating a 25-25 year old former delinquent are likely to fall between

$12,500 and $26,000 a year. Depending on the security level of the facility and changes in that facility's official capacity, the fiscal cost of an additional year of incarceration in Maryland ranges from $4,700 to $13,800 per person. This suggests that there may be some net benefit to increasing the sentence length of offenders between 23 and 25 years old.

4.8: Conclusion

Sentence enhancements reduce crime by deterring potential offender from committing crimes and by preventing convicted offenders from re-offending for a longer period of time. In this chapter I focus on the latter effect. Judges assign sentence enhancements to the worst offenders and parole boards release rehabilitated offenders early. This non-random assignment has made the effect of sentence enhancements on crime particularly difficult to assess. I use a change in Maryland sentencing law that eliminated juvenile history as a factor in the sentencing of 23-25 year olds as a source of quasi-experimental variation in sentence length, allowing me to avoid bias due to non-random assignment. I argue that variation in sentence length dues to this policy change is unrelated to underlying criminality, resulting in a predictable and replicable sentence enhancement being applied to a specific group of individuals. I identify this sentence enhancement as the difference in the sentence lengths of similar 23-25 year-old former delinquents who were sentenced under the old and new sentencing regimes that is different from variation in the sentences of similar non-delinquents over sentencing regimes. Using this difference-in-difference strategy, I find that, on average, unlucky 23-25 year-olds who were sentenced under the old regime received 222 day sentence enhancements per delinquent point.

In the second section of the chapter, I estimate the number of crimes that sentence enhancements prevent through incapacitation alone. I am able to do so using individual level data that identify when offenders are at risk of offending and when they are physically prevented from doing so. Using my results from the first stage, I estimate the expected sentence enhancement that lucky former delinquents would have received had they been sentenced under the older guidelines regime. I then observe the number of times a lucky former delinquent is arrested between the date that he was actually released and his hypothetical release date. I observe that some lucky former delinquents are re-incarcerated before their hypothetical release date, so I calculate crime rates for each individual as the number of charges brought against them divided by the number of days they were "on the street," and actually at risk of committing an offense. Under my identifying assumption, the average criminal behavior of the lucky former delinquent during this period is equivalent to the expected criminal behavior of an unlucky former delinquent if he had not been given a sentence enhancement. This is by definition the number of crimes this particular sentence enhancement prevented by incapacitation. Upon release, the lucky delinquents may have expected to receive shorter sentences than the unlucky delinquents, but incapacitation refers to the number of prevented (or allowed to occur) if a sentence enhancement had (or had not) been applied. If unlucky delinquents had received the same sentences as the lucky delinquents, there is no apparent reason that their expectations of future punishment would be different than that of the lucky former delinquents.

I estimate that an incarcerated 23-25 year old former delinquents are arrested at a rate of 2.8 times per person per year, and are involved in 1.4 to 1.6 index crimes per person per year while they would have otherwise been incarcerated. This estimate of crimes averted is much smaller but more precise than previous estimates of crimes avoided through incapacitation, because I identify the additional benefit of incarcerating an offender for one additional year, whereas previous studies have tended to look at the average benefit of the entire incarcerated population. Bearing in mind the small sample of offenders off which these estimates are based and the assumptions necessary to turn arrests into social costs, my results suggest that the social cost of the crimes that offender would otherwise commit in that one year period is higher than the fiscal cost of extending the sentence of an offender with an established criminal history. The difference between my findings and past research highlights the importance of identifying the specific population affected by a policy change. History-based sentence enhancements do prevent a non trivial fraction of offenders from recidivating through incapacitation, but during that additional period of incarceration most offenders would have avoided being re-arrested anyway. The difference between the marginal and average incapacitative effects of incarceration suggests that there is a large return to further study of the marginal social costs of incarceration.

Tables

Table 2-1: Characteristics of in-sample and out of sample agencies

	Number of Cities		Mean Population		Mean Officers per 10,000 people	
City Size	In Sample	Out of Sample	In Sample	Out of Sample	In Sample	Out of Sample
>250,000	61	2	738,886	662,173	25.55	20.04
100,001-250,000	127	16	150,915	128,943	19.19	18.10
50,001-100,000	283	74	68,352	68,879	18.18	16.38
25,001-50,000	517	193	34,627	34,619	18.21	17.21
10,001-25,000	1086	690	16,319	14,854	19.05	18.83

Table 2-2: Characteristics of COPS Programs by City Size

	City Size				
	>250,000	100,001-250,000	50,001-100,000	25,001-50,000	10,000-25,000
Cities	61	127	283	517	1086
Total officers, 1993	124,631	34,207	31,836	29,415	29,429
Officers/10,000 people, 1993	28.7	18.8	17.5	17.4	18.3
			UHP/DNP Hiring Grants		
% cities that received a hiring grant	98.4%	90.5%	92.6%	91.7%	89.2%
Paid officers granted/year	5567.7	1216.2	984.8	1039.3	1165.8
Paid officers granted/10,000 people*	1.40	0.92	0.86	0.98	1.22
Paid officers granted/Officers, 1993	0.357	0.284	0.247	0.283	0.317
Million of dollars granted/year	$165.0	$31.8	$28.2	$29.4	$32.62
Dollars granted per person*	$4.15	$2.40	$2.46	$2.78	$3.43
			MORE Grants		
% cities that received a MORE grant	90.2%	45.7%	27.6%	19.3%	13.7%
Millions dollars granted/year	$29.3	$4.4	$1.5	$1.4	$1.2
Dollars granted per person*	$3.57	$2.68	$2.03	$2.78	$3.43

*Calculated for years/cities with a grant. Dollar values are in real 2000 dollars.

Table 2-3: OLS Estimates, Size of UHP and MORE Grant Equations, 1994-2002

Independent Variables	Paid officers granted per 10,000 residents	MORE $/capita
Log(population, 1993)	-1.514 (0.474)	0.141 (0.230)
Population growth rate, 1991-1993	26.672 (3.249)	2.304 (1.577)
Log(real salary, covered workers, 1993)	2.295 (1.249)	0.904 (0.606)
Log(real per capita income, 1993)	-1.283 (1.078)	-0.183 (0.523)
Employment/population, 1993	-0.160 (0.139)	-0.089 (0.067)
% Black	2.042 (1.337)	-1.864 (0.649)
% 18-24	4.020 (4.803)	2.140 (2.331)
Crime rate, 1993	0.003 (0.001)	0.0008 (0.0003)
% Change in Crime, 1991-1993	-0.154 (0.735)	0.443 (0.356)
Per capita police, 1993	0.115 (0.023)	0.058 (0.011)
% Change in Police, 1991-1993	0.193 (0.858)	0.130 (0.416)
Observations	1808	1808
Mean of dependent variable	5.189	0.859
R^2	0.13	0.15

Standard errors in parenthesis.

Table 2-4: OLS Estimates of Police Employees per 10,000 Equations, 1990-2001

	Officers per 10,000 residents			Civilian Employees per 10,000 residents	
Independent variable	(1)	(2)	(3)	(4)	(5)
Lag(Paid officers granted/10,000 people)	0.689	0.692	0.713	0.043	0.030
	[0.077]	[0.075]	[0.075]	[0.089]	[0.037]
1-year Lead(Paid officers granted/10,000 people)			-0.022		
			[0.039]		
2-year Lead(Paid officers granted/10,000 people)			0.082		
			[0.083]		
3-year Lead(Paid officers granted/10,000 people)			0.123		
			[0.065]		
Lag(MORE $/capita)		-0.031	-0.030		0.074
		[0.047]	[0.045]		[0.022]
2-year Lag(MORE $/capita)		-0.038	-0.038		0.052
		[0.021]	[0.020]		[0.023]
3-year Lag(MORE $/capita)		-0.052	-0.052		0.112
		[0.043]	[0.040]		[0.061]
Lag(COPS in Schools $/capita)		0.325	0.325		0.084
		[0.074]	[0.070]		[0.046]
Lag(SCGP $/capita)		0.144	0.149		0.033
		[0.167]	[0.159]		[0.075]
R^2	0.93	0.93	0.93	0.72	0.72

The mean officers per 10,000 residents are 18.9 and the mean civilian employees per 10,000 residents are 5.0. There are 23,335 observations in each regression. Standard errors in brackets allow for arbitrary covariance across observations within the same city. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed-effects and year-specific effects for the pre-COPS cell group.

Table 2-5: OLS Estimates of First Responder Employment per 10,000 people, 1990-2001

	Firefighters per 10,000 residents			Police Officers per 10,000 residents	Police and Firefighters per 10,000 residents	Civilian Firefighters per 10,000 residents
	City Size				City Size	
Independent variable	>100,000	100,000 – 250,000	>250,000	>100,000	>100,000	>100,000
Lag(Paid officers granted/10,000 people)	0.302 [0.118]	0.285 [0.145]	0.132 [0.155]	0.750 [0.232]	1.049 [0.247]	-0.042 [0.039]
Lag(MORE $/capita)	-0.049 [0.030]	-0.066 [0.053]	-0.021 [0.032]	0.004 [0.036]	-0.046 [0.052]	0.017 [0.019]
2-year Lag(MORE $/capita)	-0.042 [0.042]	-0.069 [0.087]	-0.010 [0.037]	-0.029 [0.032]	-0.072 [0.055]	0.003 [0.016]
3-year Lag(MORE $/capita)	-0.068 [0.041]	-0.127 [0.072]	-0.021 [0.052]	-0.034 [0.032]	-0.104 [0.053]	0.030 [0.022]
Lag(COPS in Schools $/capita)	-0.021 [0.283]	-0.180 [0.287]	-0.367 [0.458]	0.169 [0.253]	-0.017 [0.428]	-0.069 [0.081]
N	1,436	479	957	1,473	1,436	1,419
R^2	0.97	0.93	0.97	0.98	0.98	0.83
Mean of Dependant Variable, 1993	15.2	15.2	15.2	20.7	35.9	1.03

Standard errors in brackets allow for arbitrary covariance across observations within the same city. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed-effects and year-specific effects for the pre-COPS cell group.

Table 2-6: Reduced-Form and 2SLS Estimates of Crime Rate Regressions, 1990-2001

	Property Crimes				Violent Crimes				
	Burglary	Auto Theft	Larceny	All	Robbery	Murder	Rape	Assault	All
	Reduced-Form Estimates of Crime Rate Regressions								
Lag(Paid officers granted/10,000 people)	-2.368 [0.661]	-1.556 [0.614]	-1.054 [2.270]	-4.978 [2.868]	-0.855 [0.297]	-0.019 [0.011]	-0.056 [0.049]	-1.407 [0.481]	-2.246 [0.692]
R^2	0.90	0.90	0.92	0.93	0.93	0.73	0.72	0.84	0.90
	2SLS Estimates of Crime Rate Regressions Using lag(Paid Officers granted/10,000 people) as an Instrument for Sworn Officers/10,000 People								
Sworn Officers/10,000 People	-3.437 [1.024]	-2.259 [0.940]	-1.530 [3.326]	-7.226 [4.363]	-1.241 [0.480]	-0.027 [0.015]	-0.081 [0.072]	-2.042 [0.728]	-3.258 [1.084]
Mean crime rate, 1993	106.9	48.5	352.2	507.6	16.8	0.6	3.5	38.8	59.8
Implied Elasticity, impact of officers on crime	-0.59	-0.85	-0.08	-0.26	-1.34	-0.84	-0.42	-0.96	-0.99

Mean officers per 10,000 people in 1993 was 18.2. Standard errors in brackets allow for arbitrary covariance across observations within the same city. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed-effects and year-specific effects for the pre-COPS cell group.

Table 2-7: Reduced-Form Estimates of Crime Rate Regressions, 1990-2001

	Property Crimes				Violent Crimes				
	Burglary	Auto Theft	Larceny	All	Robbery	Murder	Rape	Assault	All
Lag(Paid officers granted/10,000 people)	-2.205 [0.642]	-1.472 [0.597]	-0.591 [2.062]	-4.269 [2.600]	-0.779 [0.253]	-0.017 [0.010]	-0.053 [0.049]	-1.342 [0.466]	-2.093 [0.631]
Lag(MORE $/capita)	-0.944 [0.302]	-0.685 [0.221]	-2.515 [1.219]	-4.145 [1.476]	-0.485 [0.179]	-0.009 [0.007]	-0.042 [0.021]	-0.446 [0.253]	-1.014 [0.338]
2-year Lag(MORE $/capita)	-1.046 [0.367]	-0.368 [0.273]	-2.182 [0.939]	-3.596 [1.318]	-0.319 [0.195]	-0.008 [0.006]	0.004 [0.022]	-0.512 [0.492]	-0.885 [0.652]
3-year Lag(MORE $/capita)	-1.188 [0.419]	-0.544 [0.292]	-5.005 [2.844]	-6.668 [3.262]	-0.626 [0.266]	-0.013 [0.008]	-0.010 [0.025]	-0.196 [0.261]	-0.884 [0.407]
Lag(COPS in Schools $/capita)	-0.458 [0.777]	0.182 [0.975]	2.437 [1.752]	2.160 [2.583]	-0.222 [0.313]	0.007 [0.011]	-0.079 [0.069]	-0.376 [0.502]	-0.652 [0.768]
Lag(SCGP $/capita)	-0.066 [1.458]	0.829 [2.085]	-5.848 [4.384]	-5.085 [5.232]	-1.092 [0.446]	-0.014 [0.042]	-0.074 [0.113]	-2.794 [1.638]	-3.966 [1.828]
R^2	0.90	0.90	0.92	0.93	0.93	0.73	0.73	0.84	0.90
Mean crime rate, 1993	106.9	48.5	352.2	507.6	16.8	0.6	3.5	38.8	38.8
Implied Elasticity, impact of officers on crime	-0.542	-0.798	-0.044	-0.221	-1.220	-0.745	-0.399	-0.910	-0.920

Mean officers per 10,000 people in 1993 was 18.2. Standard errors in brackets allow for arbitrary covariance across observations within the same city. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed-effects and year-specific effects for the pre-COPS cell group.

Table 2-8: Reduced-Form Estimates of Crime Rate Regressions 1990-2001 Coefficient on Lag(Paid Officers Granted / 10,000 people)

	Property Crimes				Violent Crimes				
Change in Model	Burglary	Auto Theft	Larceny	All	Robbery	Murder	Assault	Rape	All
(1) Basic model, Table 6	-2.368	-1.556	-1.054	-4.978	-0.855	-0.019	-1.407	-0.056	-2.246
	[0.661]	[0.614]	[2.270]	[2.868]	[0.297]	[0.011]	[0.481]	[0.049]	[0.692]
(2) All COPS grants, with group*year effects	-2.836	-1.649	-1.043	-5.528	-0.877	-0.026	-1.664	-0.057	-2.575
	[0.642]	[0.609]	[2.051]	[2.583]	[0.265]	[0.011]	[0.451]	[0.048]	[0.626]
(3) Model (1) but use crime-specific group*cell*year effects	-1.684	-1.325	-0.522	-3.449	-0.605	-0.014	-1.160	-0.052	-1.882
	[0.663]	[0.559]	[2.029]	[2.633]	[0.265]	[0.011]	[0.444]	[0.046]	[0.599]
(4) Model (1) but use census division*city size*year effects	-2.249	-1.81	-2.018	-6.078	-0.780	-0.022	-1.207	-0.052	-2.029
	[0.650]	[0.510]	[2.075]	[2.610]	[0.240]	[0.011]	[0.425]	[0.051]	[0.641]
(5) Model (1) but use 1985-1990 crime trends to create cells	-1.616	-1.614	0.559	-2.671	-0.641	-0.015	-0.832	-0.033	-1.451
	[0.666]	[0.643]	[2.098]	[2.682]	[0.282]	[0.013]	[0.503]	[0.049]	[0.686]
(6) Model (1) but use 1985-1994 crime trends to create cells	-1.479	-1.623	0.276	-2.826	-0.614	-0.019	-0.490	-0.025	-1.393
	[0.664]	[0.620]	[2.101]	[2.655]	[0.267]	[0.012]	[0.202]	[0.052]	[0.658]
(7) Model (1) but including all other hiring grants in county	-2.254	-1.554	-0.647	-4.466	-0.810	-0.017	-1.169	-0.053	-1.996
	[0.642]	[0.595]	[2.061]	[2.594]	[0.252]	[0.010]	[0.440]	[0.049]	[0.605]
(8) Model (1) but only for the 61 largest cities	-6.091	-13.99	-7.602	-27.68	-7.869	-0.073	-3.247	-0.073	-11.24
	[2.009]	[5.58]	[7.686]	[9.15]	[1.623]	[0.039]	[1.530]	[0.123]	[3.00]
(9) Model (1) except include agency specific time trends	0.047	-0.398	0.689	0.339	-0.217	-0.015	-0.343	0.033	-0.527
	[0.687]	[0.212]	[1.643]	[2.096]	[0.172]	[0.016]	[0.412]	[0.058]	[0.492]
(10) Model (7) except include agency-specific time trends	-3.107	-8.237	-3.166	-14.508	-1.092	-0.082	-1.780	0.066	-2.864
	[2.083]	[2.531]	[2.531]	[6.801]	[1.478]	[0.047]	[0.939]	[0.055]	[1.986]

Standard errors in brackets allow for arbitrary covariance across observations within the same city. Mean officers per 10k residents in 1993 was 18.3 Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed effects, and year-specific effects for the pre-COPS cell group

Table 3-1: Characteristics of UHP grants, 1994-2000

	City Size				
	>250,000	100,001-250,000	50,001-100,000	25,001-50,000	10,000-25,000
Cities	61	127	283	517	1086
Officers/10,000 people, 1993	28.7	18.8	17.5	17.4	18.3
	Number of UHP grants awarded				
1994	45	56	78	84	82
1995	41	77	165	345	623
1996	33	53	90	178	333
1997	18	35	99	143	270
1998	20	39	73	143	248
1999	17	26	56	99	172
2000	11	9	9	24	40
2001	9	6	4	11	23
	Mean UHP officers granted / 10,000 people *				
1994	0.48	0.85	1.09	1.45	2.07
1995	0.77	0.75	0.71	0.72	0.88
1996	1.58	1.14	0.97	1.12	1.38
1997	1.61	1.23	1.17	1.34	1.73
1998	1.76	1.33	1.37	1.62	2.01
1999	1.70	1.50	1.55	1.62	2.08
2000	1.62	1.45	1.26	1.62	1.90
2001	1.45	1.05	0.95	1.07	1.37
	Mean Expired UHP officers / 10,000 people *				
1997	0.48	0.87	1.08	1.48	1.93
1998	0.59	0.69	0.71	0.82	0.94
1999	0.88	0.77	0.69	0.76	0.95
2000	1.15	0.93	0.94	0.98	1.25
2001	1.55	1.31	1.17	1.39	1.67

* in cities that received a UHP grant

Table 3-2: OLS Estimates of Sworn Officers per 10,000 residents by City Size, 1990-2001

	All Cities	City Size				
		>250,000	100,001-250,000	50,001-100,000	25,001-50,000	10,000-25,000
Lag(Active UHP Officers / per 10,000 residents	0.556 [0.058]	0.699 [0.134]	0.531 [0.201]	0.704 [0.118]	0.696 [0.161]	0.47 [0.065]
Lag(Expired UHP Officers/ per 10,000 residents)	0.587 [0.109]	0.759 [0.317]	0.575 [0.197]	1.032 [0.179]	0.706 [0.258]	0.452 [0.133]
R^2	0.93	0.98	0.98	0.97	0.97	0.89
N	23,335	713	1,482	3,170	5,828	12,142

Standard errors in brackets allow for arbitrary covariance across observations within the same city. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed effects, and year-specific effects for the pre-COPS cell group.

Table 3-3: OLS Estimates of COPS grants on Police Employment by City Size, All Grants Included, 1990-2001

	Dependant Variable: Officers / 10,000 residents						Dependant Variable: Civilian Employees / 10,000 residents
		City Size					
	All Cities	>250,000	100,001-250,000	50,001-100,000	25,001-50,000	10,000-25,000	All Cities
Lag(Active UHP Officers / 10,000 residents	0.558 [0.057]	0.73 [0.135]	0.541 [0.201]	0.696 [0.113]	0.736 [0.142]	0.47 [0.065]	0.032 [0.028]
Lag(Expired UHP Officers/ 10,000 residents)	0.584 [0.108]	0.71 [0.298]	0.557 [0.207]	0.973 [0.175]	0.742 [0.258]	0.452 [0.134]	0.104 [0.059]
Lag(DNP Officers / 10,000 residents)	0.943 [0.364]	0.78 [0.493]	0.811 [0.505]	1.262 [0.585]	1.238 [0.488]		-0.272 [0.320]
Lag(MORE $/capita)	-0.047 [0.021]	-0.038 [0.035]	0.032 [0.030]	-0.047 [0.041]	-0.23 [0.114]	0.021 [0.077]	0.076 [0.019]
2-year Lag(MORE $/capita)	-0.049 [0.021]	-0.064 [0.039]	0.011 [0.026]	-0.037 [0.058]	0.009 [0.063]	-0.065 [0.039]	0.055 [0.020]
3-year Lag(MORE $/capita)	-0.073 [0.043]	-0.088 [0.044]	-0.007 [0.041]	-0.056 [0.085]	-0.199 [0.150]	-0.009 [0.051]	0.113 [0.056]
Lag(COPS in Schools $/capita)	0.308 [0.068]	0.573 [0.611]	0.397 [0.261]	0.182 [0.148]	0.31 [0.129]	0.322 [0.089]	0.081 [0.043]
Lag(SCGP $/capita)	-0.088 [0.165]			-1.184 [0.490]	-0.026 [0.196]	-0.012 [0.159]	-0.01 [0.076]
R^2	0.93	0.98	0.98	0.97	0.97	0.89	0.72
N	23,335	713	1,482	3,170	5,828	12,142	23,335

Standard errors in brackets allow for arbitrary covariance across observations within the same city. The mean number of officers per 10,000 residents in 1993 was 18.3, and the mean number of civilian employees was 5.0. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed effects, and year-specific effects for the pre-COPS cell group.

Table 3-4: Reduced-Form Estimates of Crime Rate Regressions 1990-2001

	Property Crimes				Violent Crimes				
	Burglary	Auto Theft	Larceny	All	Robbery	Murder	Assault	Rape	All
Lag(Active UHP Officers / 10,000 residents	-2.004	-1.099	-0.691	-3.794	-0.704	-0.015	-1.115	-0.052	-1.852
	[0.488]	[0.467]	[1.699]	[2.127]	[0.197]	[0.008]	[0.345]	[0.038]	[0.484]
Lag(Expired UHP Officers/ 10,000 residents)	-5.98	-1.233	-2.296	-9.509	-2.524	-0.05	-4.092	-0.223	-6.869
	[1.167]	[0.886]	[2.815]	[3.895]	[0.429]	[0.020]	[0.980]	[0.081]	[1.341]
R^2	0.90	0.90	0.92	0.93	0.93	0.73	0.85	0.73	0.90
Mean Crime Rate, 1993	106.9	48.5	352.2	507.6	16.8	0.6	3.5	38.8	59.8
% reduction in crime with average active UHP grant	-2.7%	-3.3%	-0.3%	-1.1%	-6.0%	-3.6%	-4.1%	-2.1%	-4.5%
% reduction in crime with average expired UHP grant	-5.4%	-2.4%	-0.6%	-1.8%	-14.4%	-8.0%	-10.1%	-6.1%	-11.0%
∂Crime Rate/ ∂Police	-1.897	-1.040	-0.654	-3.591	-0.666	-0.014	-1.055	-0.049	-1.753
Deterrence	[0.604]	[0.523]	[1.798]	[2.324]	[0.236]	[0.009]	[0.404]	[0.041]	[0.587]
∂Crime Rate/ ∂ Police	-4.083	-0.194	-1.642	-5.918]	-1.858	-0.036	-3.037	-0.174	-5.116
Incapacitation	[1.151]	[0.692]	[2.458]	[3.481]	[0.362]	[0.017]	[0.918]	[0.071]	[1.225]

Standard errors in brackets allow for arbitrary covariance across observations within the same city. Mean active UHP grant is for 1.44 officers, and the mean expired UHP grant is for 0.96 officers. Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed effects, and year-specific effects for the pre-COPS cell group. All regressions include 23,335 observations, with the exception of rape and aggregate violent crime, which contains 23,295 observations.

Table 3-5: Reduced-Form Estimates of Crime Rate Regressions, All Grants Included, 1990-2001

	Property Crimes				Violent Crimes				
	Burglary	Auto Theft	Larceny	All	Robbery	Murder	Assault	Rape	All
Lag(Active UHP Officers / 10,000 residents	-1.931	-1.063	-0.406	-3.400	-0.668	-0.014	-1.075	-0.049	-1.773
	[0.480]	[0.461]	[1.564]	[1.969]	[0.179]	[0.008]	[0.341]	[0.038]	[0.464]
Lag(Expired UHP Officers/ 10,000 residents)	-5.814	-1.09	-1.381	-8.285	-2.36	-0.047	-3.928	-0.218	-6.538
	[1.180]	[0.889]	[2.731]	[3.827]	[0.432]	[0.020]	[0.996]	[0.082]	[1.363]
Lag(DNP Officers / 10,000 residents)	-11.491	-12.708	-15.907	-40.107	-8.793	-0.173	-5.674	-0.058	-14.078
	[4.414]	[4.751]	[9.410]	[12.223]	[2.882]	[0.057]	[2.959]	[0.230]	[4.767]
Lag(MORE $/capita)	-0.714	-0.533	-2.308	-3.555	-0.342	-0.006	-0.046	-0.038	-0.860
	[0.293]	[0.212]	[1.212]	[1.452]	[0.134]	[0.007]	[0.213]	[0.021]	[0.288]
2-year Lag(MORE $/capita)	-0.837	-0.185	-1.932	-2.954	-0.169	-0.005	-0.246	0.006	-0.429
	[0.370]	[0.256]	[0.947]	[1.308]	[0.112]	[0.006]	[0.255]	[0.022]	[0.298]
3-year Lag(MORE $/capita)	-0.826	-0.372	-4.772	-5.970	-0.454	-0.01	-0.249	-0.003	-0.732
	[0.394]	[0.266]	[2.776]	[3.174]	[0.217]	[0.008]	[0.247]	[0.024]	[0.341]
Lag(COPS in Schools $/capita)	-0.313	0.201	2.46	2.349	-0.167	0.008	-0.324	-0.073	-0.567
	[0.729]	[0.921]	[1.671]	[2.469]	[0.275]	[0.011]	[0.456]	[0.063]	[0.682]
Lag(SCGP $/capita)	2.199	1.197	-5.387	-1.991	-0.197	0.004	-0.927	0.013	-1.110
	[1.513]	[2.021]	[4.137]	[5.091]	[0.471]	[0.041]	[1.327]	[0.113]	[1.591]
R^2	0.90	0.90	0.92	0.93	0.93	0.73	0.85	0.73	0.91

Standard errors in brackets allow for arbitrary covariance across observations within the same city. Mean officers per 10k residents in 1993 was 18.3 Other covariates include log per capita income, fraction black, fraction aged 18-24, employment/adults ratio, log salary, city fixed effects, and year-specific effects for the pre-COPS cell group. All regressions include 23,335 observations, with the exception of Rape, which contains 23,295 observations.

Table 4-1: Characteristics of In-Sample and Out-of-Sample Sentences for 23-25 Year-Olds, 1999-2004

	Final Sample	Not Matched to Arrest Records	Not Matched to Incarceration Period
Sentence Length (in months)	61.2 (200.60)	20.22 (64.73)	4.89 (14.76)
% Incarcerated (post sentence)	66.0%	48.6%	43.6%
Total Charges	1.52 (1.47)	1.30 (0.78)	1.33 (0.83)
Worst Offense Type			
Drug	61.9%	59.4%	44.3%
Violent	28.5%	27.7%	33.1%
Property	9.1%	12.2%	20.9%
VOP	0.48%	0.68%	1.63%
Worst Offense Score	5.06 (2.19)	4.24 (1.84)	3.53 (2.00)
Delinquent Points *Offender Score Element*	0.09 (0.34)	0.03 (0.21)	0.05 (0.27)
Prior Points *Offender Score Element*	2.64 (1.76)	1.84 (1.82)	1.46 (1.50)
Violation Points *Offender Score Element*	0.32 (0.47)	0.21 (0.41)	0.19 (0.39)
Relationship Points *Offender Score Element*	0.40 (0.49)	0.26 (0.44)	0.24 (0.42)
Percent Sentenced under Old Guidelines	38.1%	35.9%	37.5%
Age at Offense	23.9 (0.818)	23.9 (0.813)	23.9 (0.804)
Percent Black	82.1%	72.2%	49.6%
Sentences	3,345	1,336	1,166

Standard deviation in parenthesis

Table 4-2: Characteristics of In-Sample 23-25 Year-Olds by Delinquent Status

	Old Guidelines		New Guidelines	
	Non Delinquent	Delinquent	Non Delinquent	Delinquent
Total Time Served (in days)	686.59 (818.44)	1,274.45 (919.61)	509.94 (548.72)	471.30 (488.89)
Sentence Length (in months, serious offenses excluded)	37.20 (56.23)	96.18 (100.14)	45.09 (84.26)	50.40 (69.77)
% Incarcerated	76.9%	93.8%	74.5%	78.9%
% Not Released (right censored)	15.1%	35.0%	26.9%	24.1%
Total Charges	1.40 (0.94)	1.98 (1.79)	1.54 (1.41)	1.96 (3.83)
2nd Sentence	2.0%	4.1%	1.8%	2.2%
Worst Offense Type				
Drug	64.0%	42.3%	61.2%	68.4%
Violent	27.6%	38.1%	28.8%	23.3%
Property	8.2%	19.6%	9.2%	8.3%
VOP	0.2%	0.0%	0.8%	0.0%
Worst Offense Score	5.23 (2.19)	5.85 (2.64)	4.92 (2.14)	5.00 (2.29)
Delinquent Points	n/a	1.28 (0.45)	n/a	1.27 (0.44)
Non-Delinquent Offender Score	3.16 (2.30)	4.82 (1.79)	3.36 (2.14)	4.48 (2.20)
Age at Offense	24.0 (0.81)	23.7 (0.75)	23.9 (0.82)	23.6 (0.77)
% Black	83.9%	76.3%	81.4%	80.4%
Sentences	1,179	97	1,940	133

Standard deviations in parenthesis

Table 4-3: Maximum Likelihood Estimates of Delinquent Enhancement, 1999-2004

	Six month fixed effects		Jurisdiction specific time trends				20-22 year olds (time trends)	
	Probit	Censored neg. bin.	Probit	Censored neg. bin.	Censored duration model	OLS – completed spells only	Probit	Censored neg. bin
Old guidelines * delinquent points	0.279 [0.104]	0.211 [0.065]	0.300 [0.113]	0.167 [0.048]	291.84 [89.10]	123.11 [55.83]	*-0.071* *[0.174]*	*-0.021* *[0.033]*
Sentenced under old guidelines	-0.139 [0.104]	0.139 [0.035]	-0.274 [0.093]	-0.054 [0.034]	-120.87 [41.52]	-9.14 [20.98]	*-0.056* *[0.059]*	*-0.044* *[0.028]*
Delinquent points	0.032 [0.068]	-0.164 [0.046]	0.012 [0.069]	-0.167 [0.023]	-78.44 [26.62]	-9.06 [16.47]	*0.470* *[0.076]*	*0.068* *[0.028]*
Offender Score	0.253 [0.015]	0.046 [0.010]	0.252 [0.013]	0.044 [0.004]	105.93 [11.52]	62.38 [4.67]	*0.278* *[0.024]*	*0.026* *[0.010]*
Worst Offense Score	0.144 [0.036]	0.023 [0.009]	0.140 [0.030]	0.026 [0.015]	96.61 [16.45]	51.11 [8.74]	*0.141* *[0.037]*	*0.010* *[0.010]*
Age	-0.025 [0.018]	-0.007 [0.017]	-0.029 [0.021]	-0.033 [0.009]	14.30 [10.74]	4.42 [5.16]	*-0.003* *[0.029]*	*-0.007* *[0.015]*
Log likelihood† / R^2	0.23	-13,587.3	0.23	-13,596.9	0.18	0.38	*0.26*	*-20,795.9*
N	3,345	2,537	3,345	2,537	3,345	2,579	*5,270*	*3,894*
Jurisdiction and post conviction activity controls?	no	yes	No	Yes	Yes	yes	*no*	*yes*
Mean enhancement per delinquent point	**0.06** **[0.02]**	**262.3** **[61.4]**	**0.07** **[0.03]**	**222.5** **[41.6]**	**185.8** **[71.6]**	**123.11** **[55.83]**	***-0.01*** ***[0.02]***	***-29.6*** ***[31.6]***

Controls: Type and score of three worst offenses, jurisdiction of sentence, race, total counts, subsequent sentence while incarcerated.
Standard errors allow for arbitrary correlation within a jurisdiction-year.

Table 4-4: Characteristics of Incarcerated and Released Former Delinquents, 1999-2004

	Non-delinquents	Unlucky Delinquents	Lucky Delinquents	Lucky Delinquents In regression
Time Incarcerated (in days)	492.0 (438.8)	847.9 (621.3)	401.6 (318.8)	369.3 (258.1)
Worst Offense Type				
Drug	63.3%	45.6%	75.3%	74.0%
Violent	25.9%	33.3%	16.4%	18.0%
Property	10.2%	21.0%	8.2%	8.0%
Worst Offense Score	4.71 (1.58)	5.08 (1.78)	4.59 (1.80)	4.54 (1.93)
Delinquent Points	n/a	1.28 (0.45)	1.30 (0.46)	1.22 (0.42)
Non Delinquent Offender Score	3.55 (2.08)	4.70 (1.73)	4.60 (2.18)	4.30 (2.25)
Age at Observation	26.7 (1.76)	27.4 (2.02)	25.8 (1.32)	26.5 (1.34)
% Black	80.5%	68.4%	80.8%	82.0%
Total Arrests in Window (level)	1.17 (2.98)	1.30 (2.44)	1.38 (3.35)	0.94 (2.21)
Total Charges in Window (level)	0.74 (2.24)	0.79 (1.41)	0.90 (2.28)	0.48 (1.31)
N	1,628	57	73	50

Standard deviation in parenthesis

Table 4-5: Crimes Averted through Incapacitation: Indicators of Mean Annual Crime Rate of Early Released 23-25 Year-Olds

	All Offenders	Drug Offenders (n=55)	All Offenders, Long Enhancement	All Offenders, Short Enhancement
Free Days	164 (9.46)	151 (9.86)	208 (13.5)	113.3 (5.7)
Recidivism Rate	30.1% (0.05)	34.5% (0.06)	35.6% (0.06)	20.5% (0.05)
Arrest Rate	2.79 (0.72)	3.09 (0.83)	2.67 (0.65)	2.66 (0.73)
Prosecuted Charge Rate	1.96 (0.59)	2.00 (0.62)	1.95 (0.57)	2.03 (0.72)
Drug Arrest Rate	1.65 (0.52)	1.98 (0.67)	1.52 (0.44)	1.63 (0.68)
Implied Index Crime Rate	1.44 (0.66)	1.07 (0.66)	1.65 (0.61)	1.50 (0.83)
Estimated Social Cost – Arrests only	$16,111 (8,555)	$15,182 (9,786)	$13,363 (6,920)	$12,549 (10,931)
Estimated Social Cost – Implied index crimes	$26,855 (13,897)	$22,378 (14,773)	$22,992 (11,576)	$21,062 (16,976)

Mean values for 73 offenders presented unless otherwise noted. Standard errors of mean estimates in parenthesis. Index crimes include murder, assault, rape, robbery, burglary, car theft, and larceny.

Table 4-6: Negative Binomial Estimates of Offending During "Free" Window for Incarcerated 23-25 Year-Old Males, 1999-2004

	All Arrests			All Charges Filed		
Calendar age	-0.136 (0.038)	-0.118 (0.039)	-0.099 (0.034)	-0.131 (0.047)	-0.140 (0.049)	-0.111 (0.036)
Unlucky delinquent (Yes=1, No=0)	-0.123 (0.461)	0.121 (0.475)	-0.096 (0.434)	-0.104 (0.580)	0.605 (0.575)	0.386 (0.490)
Former delinquent (Yes=1, No=0)	0.133 (0.379)	0.150 (0.350)	0.081 (0.327)	0.125 (0.507)	0.113 (0.426)	-0.194 (0.389)
Non-delinquent offender score	0.149 (0.035)	0.127 (0.036)	0.091 (0.027)	0.161 (0.042)	0.144 (0.041)	0.103 (0.031)
Convicted of drug offense	0.336 (0.150)	0.236 (0.151)	0.219 (0.137)	0.418 (0.174)	0.170 (0.173)	0.184 (0.154)
Black	0.511 (0.170)	0.249 (0.184)	0.003 (0.169)	0.515 (0.203)	0.191 (0.215)	0.035 (0.200)
Jurisdiction * year controls?	No	Yes	Yes	No	Yes	Yes
Dispersion	Mean	Mean	Constant	Mean	Mean	Constant
Pseudo log likelihood	-1,997.1	-1,922.3	-1,904.8	-1,534.7	-1,464.5	-1,454.7

Robust standard errors in parenthesis. All regressions contain 1,735 observations

Table 4-7: Spending Per Inmate in Maryland, Fiscal Years 1989-2005

	Effect on Actual Expenditure (in thousands of 2005 dollars)					
Average daily population (ADP)	11.04 [2.02]	12.16 [2.54]	11.35 [2.05]	11.55 [2.05]	13.03 [2.46]	13.04 [4.04]
ADP^2						-0.0005 [0.001]
Capacity increase			-128.5 [356]	1,199.32 [565.92]	1,075.08 [640.08]	-128.3 [378]
Increase * ADP				-1.13 [0.32]	-0.968 [0.280]	
Capacity reduction			1839.02 [1196.94]	1505.26 [1042.38]	1229.63 [937.72]	1778.5 [1160]
Reduction * ADP				0.299 [0.629]	0.786 [0.690]	
Maximum security * ADP		-0.426 [3.40]			-1.22 [3.16]	
Minimum security * ADP		-7.76 [2.80]			-8.15 [2.80]	
N	328	328	328	328	328	328
R^2	0.95	0.95	0.95	0.95	0.95	0.95

Fiscal year and facility fixed effects included. Standard errors allow for arbitrary correlation of error terms within each facility

Figures

Figure 4-1: Mean Days Served by Convicted 23-25 year-olds by Date of Sentence, 1999-2004

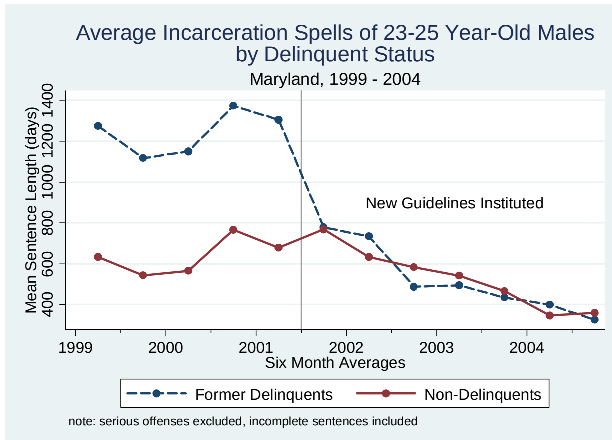


Figure 4-2: Mean Days Served by Convicted 23-25 year-olds by Date of Sentence, Completed Spells Only

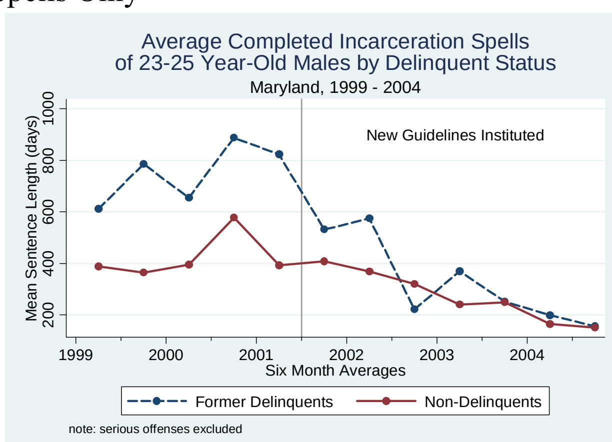


Figure 4-3: Probability of Incarceration for Convicted 23-25 year-olds by date of sentence, 1999-2004

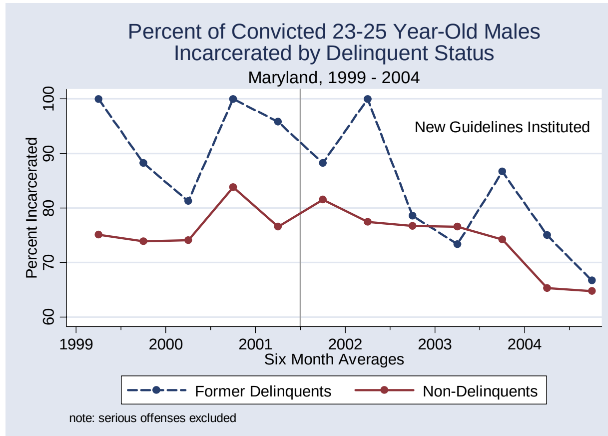


Figure 4-4: Mean Days Served by Convicted 23-25 year-olds by date of sentence, 1999-2004, Completed Spells Only

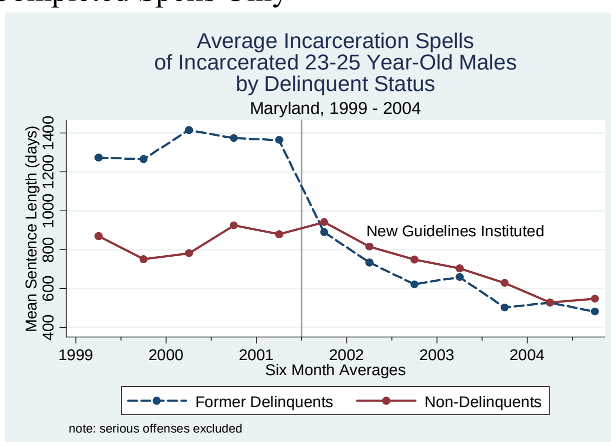


Figure 4-5: Mean Number of Charges per Person by Age

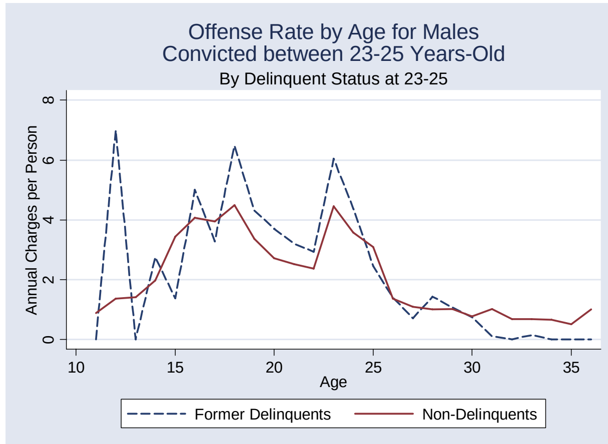
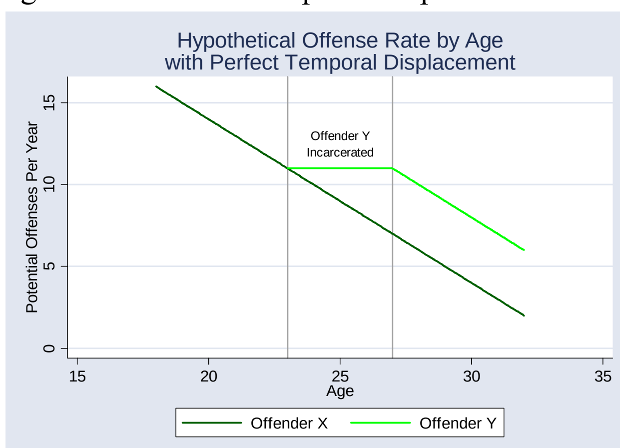


Figure 4-6: Perfect Temporal Displacement



Appendices

Appendix A: Test for Heterogeneous Enhancements by Worst Offense Type:
Maximum Likelihood Estimates of Total Days Incarcerated for 23-25 year-olds, 1999-2004

	Censored OLS		Censored Negative Binomial	
Old guidelines * delinquent points	356.03 [114.95]		0.227 [0.116]	
Old guidelines * delinquent points * violent offense	-190.72 [261.63]		0.101 [0.085]	
Old guidelines * delinquent points * property offense	-145.93 [232.92]		0.027 [0.212]	
Total offender score		106.51 [14.78]		0.060 [0.029]
Total offender score * violent offense		-11.38 [17.48]		0.037 [0.024]
Total offender score * property offense		43.44 [23.24]		0.054 [0.070]
Log likelihood /R^2	-20,717.5	-20,721.1	-13,582.8	-13,593.7
N	3,345	3,345	2,537	2,537

Controls: Type and score of three worst offenses, jurisdiction of sentence, race, total counts, subsequent sentence while incarcerated, six month fixed effects. Standard errors allow for arbitrary correlation within a jurisdiction for each type of offense.

Appendix B: Descriptive Characteristics of 20-22 year-old males by Delinquent Status, 1999-2004

	Old Guidelines		New Guidelines	
	Non Delinquent	Delinquent	Non Delinquent	Delinquent
Total Time Served (in days)	671.5 (853.1)	1,014.8 (911.2)	435.5 (539.7)	735.2 (595.5)
Sentence Length (in months)	41.12 (106.5)	82.2 (161.4)	42.68 (112.4)	100.4 (339.0)
% Incarcerated	75.0%	93.0%	67.3%	88.8%
Total Charges	1.39 (0.88)	1.74 (1.30)	1.52 (1.11)	1.72 (1.37)
2nd Sentence while incarcerated	1.7%	3.7%	1.4%	2.7%
Worst Offense Type	60.5%	49.5%	58.0%	49.9%
Drug	31.0%	38.7%	31.7%	38.7%
Violent	8.5%	11.8%	9.2%	11.2%
Property	0.1%	0.0%	1.1%	0.3%
Worst Offense Score	5.19 (2.30)	5.65 (2.72)	4.85 (2.18)	5.30 (2.46)
Delinquent Points	n/a	1.30 (0.46)	n/a	1.24 (0.43)
Non-Delinquent Offender Score	2.26 (2.06)	3.23 (2.09)	2.15 (2.02)	3.05 (1.95)
Age at Offense	20.9 (0.81)	20.8 (0.78)	21.0 (0.82)	20.8 (0.81)
% Black	83.9%	82.8%	80.8%	74.2%
Sentences	1,634	372	2,649	625

Appendix C: Mean Offenses per Arrest per Agency, 1999-2003

Crime	National Average 20,388 agencies	Maryland 185 agencies	Baltimore City Police Dept.
Assault	1.78 (1.84)	1.63 (1.04)	2.06 (2.06)
Murder	1.28 (0.96)	1.26 (0.85)	1.56 (0.39)
Rape	2.39 (2.57)	1.86 (1.29)	2.76 (0.90)
Robbery	3.05 (2.87)	3.64 (4.41)	4.36 (0.62)
All Violent Crime	2.06 (2.96)	2.05 (1.63)	2.57 (0.13)
Burglary	9.44 (12.10)	7.16 (6.63)	6.43 (1.34)
Larceny	10.07 (17.26)	9.08 (14.72)	7.42 (1.92)
Car Theft	6.70 (15.68)	6.17 (11.47)	6.23 (1.42)
All Property Crime	9.79 (18.53)	9.42 (17.70)	6.88 (1.45)
All Crime	7.61 (19.01)	6.96 (15.98)	4.89 (0.72)

Standard deviations in parenthesis. Source: author's calculations from Uniform Crime Reports

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