

TRADING DEMOCRACY FOR JUSTICE:
THE SPILLOVER EFFECTS OF IMPRISONMENT ON VOTER REGISTRATION IN ATLANTA

Traci Burch, Ph.D.
Assistant Professor, Department of Political Science, Northwestern University
Research Fellow, American Bar Foundation

Paper prepared for the Center for the Study of Law and Society Lunch Series
October 27, 2008

*Draft. Please do not cite without author's permission.

I. Introduction

In an election year in which every vote counts and the United States' political system seems poised for dramatic change, understanding the factors that inhibit voter participation is important. Previous work has highlighted how growing imprisonment rates (and conviction rates more generally) affect democratic outcomes by increasing the number of people legally prevented from voting in recent elections (Manza & Uggen 2004; Manza & Uggen 2006). As of last year, more than 2.25 million people were in prison in the U. S., a figure which translates into nearly one in one hundred adults (Pew Center 2008). Clearly, imprisonment and felon disenfranchisement laws hinder ability of many Americans to participate in elections. However, what about the people they leave behind? Might the removal, imprisonment, and disenfranchisement of convicted offenders affect the voter registration of their families, friends, and neighbors?

This paper examines the relationship between imprisoning individuals and neighborhood voter registration in Atlanta, Georgia, arguing that imprisoning neighborhood residents decreases registration. The idea that sending a person to prison dampens the political activity of that person's entire neighborhood is surprising, even counterintuitive at first glance as one might think that capturing and punishing criminals should improve life in a neighborhood. However, I argue that participation is suppressed not just because incarceration removes convicted voters from the community, but also because incarceration produces spillover effects that threaten the livelihood, resources, mental state, and social networks of inmates' families and friends. Because they are subject to the disruption of having a family member or friend imprisoned, people who might have registered may take longer to register or even fail to do so, at least in the short term.

I employ geospatial technology rarely used by political scientists to pinpoint locations of voters and offenders, situating them in neighborhoods around Atlanta. The relationship between

neighborhood imprisonment rates and voter registration rates is complex and often coincides with neighborhood poverty, crime, and other social conditions. This research takes advantage of the timing of sentencing to set up a natural experiment in which neighborhoods that experience imprisonment are compared with a control group matched in terms of crime rates, dependence on public assistance, racial composition, and other factors. The key innovation of this research design is that all neighborhoods in the comparison group, like those in the treatment group, have residents sentenced to prison but at a slightly later date.

The findings indicate that sending individuals to prison decreases neighborhood voter registration. Neighborhoods (defined as block groups) that experience the imprisonment of residents in April and May of 2006 reported lower voter registration rates in June 2006 than those neighborhoods that had not had residents imprisoned, even after controlling for racial composition, median income, poverty rates, median age, the percentage of people living in group quarters, homeownership rates, and crime rates. The relationship persists when church density and the presence of colleges and universities are taken into account. Based on simulations, the average difference in registration between the neighborhoods in which the residents were imprisoned in the spring and their counterparts in the control group was 0.092 percentage points; the 95 percent confidence interval ranges from .006 to .176.

The results of this study demonstrate that incarceration produces at least a short-term effect on voter registration at the neighborhood level. Regardless of the duration of this effect, in an election year, depressing registration even for a short time might produce lasting effects through electoral outcomes. In raising this possibility, this project addresses gaps in several areas of research. No research attempts to theorize or estimate the manner in which governments unintentionally affect individual or community political behavior by punishing criminals. Within po-

litical science, the few studies that investigate this relationship look only at the effect of disenfranchisement laws on individual political behavior and the resulting national electoral outcomes (Brown-Dean 2003; Miles 2004; Yoshinaka and Grose 2005; Manza and Uggen 2006). Sociologists and criminologists have done a much better job of interrogating the relationship among imprisonment and some neighborhood outcomes, but not voting or other forms of political participation (Morenoff and Sampson 1997; Rose and Clear 1998; Morenoff, Sampson et al. 2001; Western, Kling et al. 2001; Braman 2002; Clear 2002; Lynch, Sabol et al. 2002; Richie 2002; Edin, Nelson et al. 2004; Fagan, et al. 2004; Western, Lopoo et al. 2004). Meanwhile, the policy feedback literature in political science focuses on policies that provide financial benefits to clients rather than on state actions that do not involve the provision of benefits or services (Soss 1999; Campbell 2003; Mettler 2005). Finally, and most importantly, the political participation literature looks at racial and socioeconomic differences in participation while ignoring the most important phenomenon to affect poor and minority communities in recent decades: the war on drugs and increasing incarceration rates (Cohen and Dawson 1994). Thus there is ample room for this project to contribute to several literatures.

The paper proceeds as follows. Because this project integrates research from several disciplines, the theoretical basis for this work is discussed in greater detail than most articles in the next section. The research design is then introduced; again, because of the complexity of the analysis, this section provides ample detail on the logic and logistics of testing the research question. Next, three competing hypotheses derived from the theoretical discussion are outlined in light of the research design. The fifth section introduces the data employed to test the hypotheses and the model used to estimate the treatment effects. Next, the results are presented and dis-

cussed. Finally, the implications of these findings for representation and the balance of power within and across communities is considered.

II. Literature Review and Theory.

As the number of neighborhood residents who experience conviction and punishment increases, the costs of our current systems of law enforcement and corrections may begin to outweigh the benefits because of “neighborhood effects” (Shaw and McKay 1942; Sampson 1988; Mayer and Jencks 1989; Sampson and Groves 1989; Bursik and Grasmick 1993). A neighborhood effect can be described as “a social interaction that influences the behavior or socioeconomic outcome of an individual” (Dietz 2002). These effects may also “include influences on individual behavior or outcomes due to the characteristics of an individual’s neighbors and neighborhood” (*ibid*). The impediments imposed by imprisonment on individuals matter for their families and neighborhoods because the deteriorating circumstances of one neighborhood resident tends to affect the mental health, attitudes, social connectedness, and financial well-being of the entire neighborhood, factors that Verba, Schlozman, and Brady argue are critical for understanding voter registration (Verba, et al. 1995). Imprisonment may affect the overall voter registration of a neighborhood through several mechanisms: cultural deviance, social disorganization, resource deprivation, and demobilization.

A. Cultural Deviance

The cultural deviance model suggests that individuals within communities engage in undesirable activities because they learn them from their closest associates (Verba and Nie 1972; Kornhauser 1978; Hannerz [1969] 2004). For instance, as previous research suggests, families are important sites for the transmission of political behavior (Campbell, Converse et al. 1960; Rosenstone and Hansen 1993; Stoker and Jennings 1995; Plutzer 2002). Cultural deviance theo-

ries suggest that incarcerated individuals, because they are not around to register and vote, influence the voting patterns of those around them by not providing examples of participation to their partners, children, and friends. Experiences with criminal justice also promote negative attitudes toward government and a belief that participation is futile among incarcerated individuals; offenders may communicate these attitudes to other neighbors and friends, thus influencing their levels of trust in government and efficacy as well. In communities in which neighbors increasingly experience hostile interactions with the government, anti-government attitudes, which have been found to suppress voting, may spread rapidly among those left behind (Campbell, Converse et al. 1960; Verba and Nie 1972; Wilson 1996; Foreman Jr. 2002).

B. Social Disorganization

The social disorganization, model, in contrast, posits that individuals within communities engage in undesirable activities because their neighbors have no power to stop them (Shaw and McKay 1942; Sampson 1988; Sampson and Groves 1989; Bursik and Grasmick 1993). In line with this theory, neighbors might support the idea of voting and participating in politics. However, due to weakened social networks, they have no mechanism by which to enforce the norms of political participation. Incarceration is perhaps the best example of how the conviction of individual offenders immediately can affect the social organization of entire neighborhoods. Incarcerated people are removed from their families and friendship networks, thereby destroying links among neighborhood residents through which social and civic norms are enforced. Incarceration prevents families and friends from seeing each other on a daily basis and often disrupts parental bonds with their children (Abu-Jamal 1995; Braman 2002; Edin, Nelson et al. 2004; Western, Lopoo et al. 2004). Those left behind may withdraw from remaining relationships due to the shame of having a family member in prison (Braman 2002; Austin 2004). Incarceration

also imposes stigmas on offenders and their families; the shame of the criminal record might lead families to withdraw from community life and positive influences even when a convicted family member is not sent to prison (Austin 2004). Convicted individuals and their families may be ostracized involuntarily by other neighborhood residents, removing them from the formal and informal networks that provide political information and encourage voting and participation.

C. Resource Deprivation

According to Verba, Schlozman, and Brady's Civic Voluntarism Model, resources such as time, money, and civic skills play an important role in political participation (Verba et al. 1995). Imprisonment may suppress participation by depriving families and friends of these resources. Families are poorer as a result of having those who contribute to their upkeep (through legal and illegal means) removed from the labor market (Rose and Clear 1998; Braman 2002). The poverty imposed by the loss of a wage-earning member of the household may also destabilize living situations and increase residential mobility (Braman 2002). Money is not the only resource affected by incarceration; time also becomes scarce for people who take on extra work or caring responsibilities when a person they know is sent to prison.

D. Demobilization

Finally, the demobilization explanation would argue that incarcerating residents hurts a given neighborhood in the short term to the extent that imprisoning residents makes it less likely that parties, campaigns, interest groups and local organizations will contact potential voters from that neighborhood. Mobilization--activities designed to get people to register, vote or otherwise participate in politics--is undertaken most visibly by campaigns, parties, interest groups, and non-profit organizations, but also occurs through person-to-person contacts. Large-scale mobilization efforts are costly, and organizations employ time and resources strategically in order to

reach those voters most likely to participate (and participate on behalf of the right side). Incarceration damages the formal and informal mechanisms of mobilization. Political parties, campaigns, interest groups and non-profits tend to concentrate their efforts in places where mobilization is more effective. For instance, parties often fail to mobilize communities with low socioeconomic status members (Huckfeldt and Sprague 1992). They tend to contact people who have voted before, especially those who have voted in primaries (Huckfeldt and Sprague 1992). Using voter registration lists to mobilize voters makes it unlikely that people who have never voted will be contacted or mobilized. Although this technique is most effective for mobilization, going door-to-door may yield fewer voters in high-conviction neighborhoods if residents are afraid to open their doors to strangers (Rosenstone and Hansen 1993; Gerber and Green 2000; Green, Gerber et al. 2003). Interpersonal networks of mobilization may also falter in communities where many citizens have been sent to jail or prison. There are fewer voters available to serve as discussion partners in high-imprisonment neighborhoods (Huckfeldt and Sprague 1987). Potential voters that live in these communities may be less exposed to placards, yard signs, and bumper stickers, all of which communicate important political information (Huckfeldt and Sprague 1992). The disruption of social networks that occurs because of imprisonment or social ostracism may also impede the dissemination of political information (Foldare 1968; Huckfeldt and Sprague 1987; Burbank 1997).

E. Summary and Implications

To reiterate, incarceration is so disruptive to communities that its effects could influence registration and turnout even in the span of a couple of months. The explanation outlined above fits well with the Civic Voluntarism Model outlined by Verba, Schlozman, and Brady, which argues that people fail to participate in politics because they cannot, were not asked, or do not

want to (Verba, et al. 1995). Much of the impact of sending people to prison stems from the fact that incarceration leaves the families and friends of inmates in emotional and financial chaos and thus unable to participate. For instance, a woman may not have time to register because she is working more hours to make up for the lost income of her convicted spouse. Foster parents struggling to care for a convicted mother's children might be too overwhelmed with their new responsibilities to make it out to register. The parent of a convicted offender might be too upset over the fate of his or her child to volunteer for a voter registration drive, thus leaving the event short-staffed and unable to reach many residents. Likewise, it is more difficult to mobilize people living with and around individuals who are convicted of crimes because they often move or are ostracized from their communities. Finally, imprisonment may decrease the desire to register within the neighborhood. The accomplices of an inmate might be reluctant to contact any public officials, including the board of elections. Even among neighbors without an explicit connection to an offender, a highly publicized, contentious, or controversial imprisonment might decrease efficacy or trust in government temporarily.

At the same time, mobilizing organizations, while pursuing their routine strategies of mobilization, unwittingly might exacerbate these effects on registration. For example, because of increased residential mobility or other factors, an organization that relies on lists of registered voters to get contact information will be less likely to contact successfully households in neighborhoods where residents have been incarcerated recently. The appearance or demographic profile of campaign workers may generate distrust among citizens suspicious of police or government officials. In cases where a convicted offender is registered to vote, the removal of that offender from the voter rolls might decrease the likelihood that an organization will contact his

or her household. In these ways, imprisoning individuals may lead to measurable differences in registration even in the short term.

Although it seems obvious that incarceration and the ensuing absence of residents in a community would decrease political participation, it is entirely plausible that the number of people from the community who are sent to prison would have no effect or even the opposite effect on participation in the aggregate once intervening factors are taken into account. The apparent relationship between incarceration and political activity could be spurious. Several factors could account for both a community's number of incarcerated members and its political activity, including racial composition, poverty, and crime rates. Once these factors are taken into account, participation may be unrelated to incarceration rates. Alternatively, the imprisonment of neighborhood residents may *increase* participation by making neighborhoods safer and by restoring social trust among law-abiding community members (Wilson and Kelling 1982; Putnam 2000).

III. Research Design

Studying the relationship between imprisonment and political participation at the neighborhood level is daunting; perhaps the theoretical and practical complexity of this task accounts for the dearth of work on this subject in political science and other disciplines. The ideal test of this relationship would randomly assign neighborhoods to experience the imprisonment of residents independently of poverty rates, racial heterogeneity, and other potentially confounding factors. Of course, such an experiment is impossible in the real world, but this research approximates such random variation by taking advantage of variation in the timing of criminal sentences (Thistlethwaite and Campbell 1960; Hahn, Todd et al. 2001).¹

Arguably, within a small time frame, the date on which a community member actually is sentenced is random, dependent on factors such as individual officers' schedules, the date the offender committed the crime, the court

The analysis compares the percentage of adults registered to vote on June 10, 2006, an arbitrary date nine days before the primary election registration deadline, in neighborhoods that had residents sentenced to prison in the two months prior to that date with a control group that did not have residents sentenced to prison during those two months. The control group does not include all neighborhoods that did not have a resident sent to prison before June 10th; rather it is limited in two respects. Most importantly, only neighborhoods that had an individual sent to prison up to two months after June 10, 2006 were included in the control group. Neighborhoods that had no one imprisoned between June 10, 2006 and August 10, 2006 are excluded from the control group. Second, balancing the treatment and control groups by matching on the demographic characteristics of the neighborhood ensures that each treated neighborhood has a corresponding neighborhood in the control group, making the neighborhoods as similar as possible across the treatment and control groups (Ho, Imai et al. 2004). Matching also provides a check against model dependence in that comparing the average voter registration in the treated group with that of the control group produces an estimate that does not make assumptions about functional form.

Limiting the control group in this way makes it more likely that the treatment and control communities have the same underlying distribution of factors that lead to the conviction and sentencing of their members; the actual timing of the treatment within the small window around June 10th is random and thus independent of those confounding factors. As a result, unless there is some systematic process that determines both the particular week or month that a neighborhood has a resident convicted *and* that neighborhood's voter registration, any differences in the

docket, the length of the trial, and the like. Within the set of communities that experience imprisonment between April and August of 2008, one could compare communities that receive the treatment before the arbitrary cut-off date (June 10th) with those that receive the treatment after the date as a way of testing the effects of incarceration on voter registration.

dependent variables across the treated and control groups should be due to imprisonment. To reiterate, with this research design, any differences across neighborhoods would have to be correlated with *both* the timing of the sentence and voter registration in order to bias the results.

A. Benefits of the Design

In sum, the research design relies on the vagaries of the criminal justice system in the short term to divorce the effects of having residents from the community imprisoned from the other confounding factors that may influence whether a neighborhood has a resident imprisoned. All neighborhoods included in the analysis are neighborhoods in which at least one resident is sentenced during the study period. Excluding neighborhoods from which no individuals are sent to prison decreases bias because individuals are sent to prison are so different from those in which no imprisonment takes place that unbiased comparisons between these neighborhoods are not possible given the available control variables. The random assignment of the remaining neighborhoods to the treatment and control conditions (residents sentenced to prison before or after June 10th, respectively) will ensure that the neighborhood racial heterogeneity, residential mobility, poverty, and other factors are uncorrelated with the treatment; thus, the average treatment effect (ATE) is the average difference in registration between the treated and control groups. The ATE is an unbiased estimator of the effects of imprisonment on neighborhood voter registration.²

This particular research design is beneficial because it avoids three potentially complicating factors: omitted variable bias, post-treatment bias, and data availability. Omitted variable bias occurs when a model fails to account for factors that are related to both imprisonment and

² One potential problem with this research design concerns discerning intent-to-treat effects from the average treatment effect. To take an extreme example, neighborhoods that had individuals sentenced to prison the day before the cut-off date have received the treatment in theory, but if the effects take longer than 24 hours to manifest themselves, then there will be no measurable treatment effect. In this analysis, thirty inmates (out of 184 convicted before the cut-off date) were convicted less than ten days before the cut-off date.

participation rates; failing to account for these important alternative causes can lead to spurious results (King & Zeng 2006). For instance, as argued above, poverty increases incarceration while decreasing voter turnout; failing to account for poverty in the analysis could lead the effects of poverty to be attributed mistakenly to imprisonment. Post-treatment bias, on the other hand, results from the attempt to control for variables that are consequences of the phenomenon of interest. Again, to use this research as an example, if it is correct that imprisoning neighborhood residents ultimately increases neighborhood poverty, then poverty rates are also a consequence of imprisonment and by that logic should be excluded from the model. Unfortunately, most of the factors relevant to the relationship between participation and imprisonment rates are themselves both causes and consequences of imprisonment. Therefore, over time, poverty, racial composition, residential mobility, crime, and imprisonment can be thought of as a perpetual chicken and egg spiral of causality that makes it almost impossible to measure the long-term effects of imprisonment on turnout (King and Zeng 2006).

Perhaps collecting longitudinal data could help solve this problem. However, the third difficulty with estimating the effects of imprisonment on participation arises due to data availability. Although some data are available by year for the past several decades at the neighborhood level (poverty, racial composition, etc.), the dependent variable, registration rate, is nearly impossible to get at the neighborhood level. Although states keep electronic voter registration and turnout records, it is often difficult to obtain accurate registration and turnout records for past years because many states maintain their voter registration files as “snapshots” of current registration.³ In Georgia, the Secretary of State keeps a complete list of every voter registration number that was used to vote in the 2004 general election. However, because some of the individuals

³ Moreover, many states began keeping statewide electronic records of voter registration only recently as a result of the National Voter Registration Act of 1993 and the Help America Vote Act of 2002.

who voted in 2004 subsequently have been purged from the voter registration list, information is available only for a subset of the voters who participated in that election. Moreover, this subset of voters who participated in 2004 would be a biased sample, as being purged from the voter rolls probably is correlated with factors such as being convicted of a crime.

B. Limitations of the Research Design

This analysis avoids the problems of inference and data availability described above by attempting to estimate only the marginal effect of incarcerating one more person in neighborhoods that had a resident sent to prison during a four month period (April 10 – August 10, 2006). In setting up the research in this way, the intervention is to measure the average effect of convicting one more person in the short term rather than the effects of a longer history of imprisonment. The design implies that this effect is almost instantaneous: if this theory is correct, convicting someone of a crime, no matter how many people were imprisoned previously, on average will result in a measurable decrease in neighborhood participation within two months of sentencing. Of course, it is unrealistic to imagine ten people ‘unregistering’ because someone in the neighborhood was convicted of a crime. However, it is more plausible that, in a season in which neighborhoods are being encouraged to register, some people who might have registered do not (or take longer to do so) because someone in their neighborhood is convicted of a crime. Do these short-term suppressions of political activity add up to a permanent decrease in voter registration rates? Perhaps, but it is likely that the hypothetical individuals described in the previous section would eventually register and vote. Longer-term effects, should they exist, might be driven by different processes.

The design is also limited in that it accounts for the effects of only one type of removal: imprisonment. Only instances of sending people to prison rather than jail are measured because

these data are available for the complete population of individuals sentenced to prison. While it is possible to tell whether these individuals awaited trial in jail before they were convicted and sentenced, no information is available for people who were incarcerated in jails who ultimately were not sentenced to prison (for instance, if they were released or found guilty of a misdemeanor). Because it is not possible to sample from the complete set of neighborhoods that had someone sent to prison or jail, any results that attempted to incorporate incarceration in jails would be biased.

Finally, the analysis relies on the sentencing date rather than the arrest date in constructing the treatment for several reasons. First, similar to the problem with counting incarceration in jails, relying on the arrest date also results in a truncated sample because only data on arrests that end in a felony conviction are available. Second, although the arrest date may be the actual date of permanent removal from the household in most cases, the actual pre-conviction and sentencing phase of supervision may vary considerably among those people eventually sent to prison. First, at least in Georgia, families still can visit loved ones in jail relatively easily. Some people awaiting trial are out on bail, while others are on work release or trustee status, while still others await trial on home confinement. Thus, it is not clear that arrest is the actual point at which an individual is fully removed from the community. Third, arrest dates are a bit more related to crime dates; because this research design relies on the notion that the date of ‘removal’ is random over a short time horizon, it is better to use sentencing dates because it introduces other factors that disconnect crime and removal (for instance, scheduling). Finally, sentencing dates are preferable to arrest dates because only convicted offenders experience disenfranchisement and removal from the voter rolls in Georgia.

IV. Hypotheses

Based on the research design and the theory, the evidence should support the following hypothesis:

1. *Neighborhoods that had members imprisoned in the two months prior to June 10, 2006 should have lower rates of voter registration than control neighborhoods in which no one was sentenced to prison prior to that date even after taking potentially confounding factors such as poverty, racial composition, and residential mobility into account.*

However, the following alternative hypotheses are also plausible based on arguments made by previous researchers:

2. *There is no difference in voter registration between neighborhoods in which residents were sentenced to prison and those in which no residents were sentenced to prison before June 10th after taking confounding factors such as poverty, racial composition, and residential mobility into account.*
3. *Neighborhoods that had members imprisoned in the two months prior to June 10, 2006 should have higher rates of voter registration than control neighborhoods in which no one was sentenced to prison prior to that date after taking potentially confounding factors such as poverty, racial composition, and residential mobility into account.*

The second hypothesis reflects the notion that the relationship between imprisonment and voter registration and turnout is spurious, which means that it should disappear once all of the characteristics of the neighborhood are taken into account. The third hypothesis is based on the social capital literature and is consistent with the notion that capturing and punishing criminals leads to an increase in social capital or social trust that in turn facilitates voting.

V. Data

The data for this study were obtained by combining updated demographic estimates for block groups with data on prison inmates and voters within the city limits of Atlanta, Georgia. The result of this massive effort is the combining of voter registration records, criminal records, and geographic data into two data sets on which spatial analyses can be performed. Atlanta is

ideal for this study because its high imprisonment rate and large population lend themselves to a relatively large number of observations.

A. Demographic Data

Block groups are the units of analysis. Estimates for the 2006 demographic characteristics on block groups were obtained from Scan/US and ESRI. Because Atlanta has experienced the fastest growth of all metropolitan areas in the U. S. since the decennial census, population data at the block group level from the 2000 census are inaccurate ("News Release" 2007). Scan/US produces updated estimates of block group populations each year using U. S. Postal Service delivery statistics, direct marketing databases, credit bureau reporting agencies, and other data sources (Scan/US 2008).

Block groups are the smallest level of aggregation for which data on population size were available for 2006 and thus represent "communities" in this analysis. According to the Census Bureau, block groups typically contain 300 to 3,000 people, with an optimum size of 1,500 ("Glossary of Geographic Terms" 2007). Block groups boundaries do not map precisely onto the city boundaries. There are 293 block groups in Atlanta; they were identified with ArcGIS as the block groups whose centroids fell within the city limits.⁴ Of these, 140 had no individuals sentenced to prison, 72 block groups had individuals sentenced before June 10th (but not after), 34 had individuals sentenced after June 10th (but not before), and 47 had individuals sentenced both

⁴ The choice of block groups as the unit of analysis matters in spatial analysis because of three well-known problems: boundary, scale, and modifiable area units (Barber 1988; Chou 1997; Anselin, Cohen et al. 2000). The boundary problem refers to how different choices with respect to boundaries (block groups instead of blocks) can lead to different statistical relationships depending on the data. For instance, a pattern of incarcerations may appear dispersed if one is looking at one block, but clustered if one enlarges the picture to include four other blocks in which no one is imprisoned. The scale problem refers to the fact that spatial descriptive statistics can vary as increasingly aggregated units are used. Thus, the relationship between incarceration and registration may be different when measured at the census tract level as opposed to the block group level. The modifiable units problem refers to the fact that units may be aggregated differently (for instance, the assignment of census blocks to block groups may be arbitrary) and that different patterns of aggregation may result in different statistical results.

before and after June 10th. The sample size before matching is therefore 153 block groups: 119 in the treatment and 34 in the control group.⁵

For each block group, the rates of homeownership, racial diversity (as the percent Hispanic and percent White), percent of households making less than ten thousand dollars per year, median income, median age, percent in group housing, adult population, were obtained using the data from Scan/US.

B. State Data

The inmate research file is the primary source of data on all offenders who were ever held in prisons by the Georgia Department of Corrections (GDC). The information in this file is obtained from the Georgia Offender Tracking and Information System (OTIS), initial diagnostic testing, medical testing, GDC records, FBI records, court records, and information provided by the Georgia Board of Pardons and Paroles. The unit of analysis in this file is the “prison episode,” meaning instance of incarceration. An individual offender will have as many entries in the research file as he or she has had prison visits or supervisions. The information contained in this file is updated regularly with valuable information about each offender and includes both offenders who are serving sentences and those who have completed their sentences. Most importantly for this research, the data contain the last name, first name, year of birth, race, and gender and for inmates, the last known address. The inmate research file used for this paper was generated in February 2007 and contains more than 500,000 prison episodes. Please see Table 1 for information about the background of inmates.

TABLE 1 ABOUT HERE

⁵ The models also are estimated without block groups that have inmates convicted in both time periods; the N for these analyses before matching is 106.

The statewide voter registration list was generated June 10, 2006 and contains more than four million voters. The list contains data on the first and last names, address, precinct, race, gender, date of birth, and voter history of all Georgians who were registered to vote on that date.

C. Geocoding

Addresses for prisoners and voters were converted to points with latitudes and longitudes and then to census blocks by geocoding with ArcGIS. Further matching of inmates and voters was conducted by hand. ESRI data, included with ArcGIS, were also used to geocode post-secondary educational institutions and churches to block groups.

For some inmates, not enough information was provided to for geocoding. In the entire state, 2748 inmates were convicted between April 10 and August 10 2006. Of these, 79 were not located. Of these 79 inmates, 24 addresses were blank, 34 were incomplete, 2 inmates were homeless, 10 listed a highway or rural route, 1 listed a post office box, and 8 addresses were not unique. It is impossible to tell if these inmates would have ended up in the final sample, as it is unclear whether these inmates resided within the city limits of Atlanta.

D. Methodology: Estimating the Effect of Incarceration on Registration

For the multivariate analysis of the effect of incarceration on registration, the data were analyzed at the block group level. As discussed earlier and below, many factors may affect the voter registration rate of block groups and thus must be controlled in this analysis. Income, crime, the presence of young residents, and racial diversity have been shown to influence both neighborhood outcomes and the voter registration rate; thus, they are included in these models as median income, percent of households making less than \$10,000, crime rate, median age, percent white and percent Hispanic (Foldare 1968; Mayer and Jencks 1989; Sampson and Groves 1989; Cohen and Dawson 1993; King, Keohane et al. 1994; Verba et al. 1995; Morenoff et al. 2001;

Plutzer 2002). Further, the presence of college students, nursing home residents, or others in group quarters might affect voter registration so a measure of the percent of the population in group quarters is included in the model. As a final check, models that include measures of church density and the presence of a post-secondary educational institution are included as well.

The incarceration variable is a measure of the number of people imprisoned from each block group between April 10, 2006 and August 10, 2006. In the “treated” blocks, all of these people were imprisoned before June 10, 2006. In the “control” blocks, all of those imprisoned were convicted after June 10, 2006. The dependent variable, voter registration, is the number of people from the block group registered as of June 10, 2006, divided by the 2005 block group population.

In order to make the treated block groups as similar to the control groups as possible, the data will be pre-processed using MatchIt (Ho, Imai et al. 2004). The nearest neighbor method with replacement will be used, which matches each treated neighborhood with the comparison unit with the closest propensity scores (Morgan and Harding 2006). Matching makes it possible to compare similarly situated neighborhoods to each other—apples to apples. The process discards incomparable data points that may bias the results. For instance, neighborhoods full of millionaires may be different from more heterogeneous communities; if there were no corresponding high income neighborhoods in the treatment group, this outlier neighborhood might be discarded. More formally, this process discards neighborhoods outside the range of common support because including these neighborhoods in the analysis could bias the results (Ho, Imai et al. 2004, King and Zeng 2006).

The following equation describes the exact model of the relationship between voter registration and incarceration at the neighborhood level tested using ordinary least-squares regression on the matched data:

$$\text{Voter Registration} = \text{Residents Imprisoned Before June 10th} + \text{\# Sent to Prison} + \text{Crime Rate} + \text{\% White} + \text{\% Hispanic} + \text{\% Owner Occupied} + \text{\% Group Quarters} + \text{Median Age} + \text{Median Income} + \text{\% Under \$10K}$$

Where:

- “Voter Registration” is the percentage of adults registered to vote in the community on June 10, 2006
- “Residents imprisoned before June 10th” is a dummy indicating whether a neighborhood had residents sentenced to prison before June 10th
- The “number imprisoned” is the number of people sentenced to prison in the neighborhood during the study period
- The “crime rate” is the number of crimes committed in that neighborhood per resident in 2005
- The “percent white” is the non-Hispanic White percentage of the total population in 2006
- The “percent Hispanic” is Hispanic percentage of the total population in 2006
- “Percent Owner Occupied” is a proxy of residential mobility, defined as the proportion of the population that owned their primary residence in 2006
- “Percent in group quarters” is the percentage of the block group population living in group quarters in 2006
- “Median Age” is the median age of the total population in 2006.
- “Median income” is the median income of the block group in 2006.
- “Percent under \$10K” is the percentage of households with income less than \$10,000 in 2006.
- “Residents convicted in both time periods” is included only in the model that includes block groups with people convicted before and after June 10, 2006.

The results of the nearest neighbor matching are shown below. The treated and control groups were matched based on the number of residents sent to prison, the percent white, and the median age of the residents. These three factors were selected because examination of the initial sample revealed the means in the treatment and control groups of all the other covariates were already within a quarter of a standard deviation of each other and thus balanced. Comparing the means of the matched treated and control groups in Tables 2 & 3 reveals that the samples are now balanced on all the covariates; there is less than a quarter of a standard deviation difference between the means of the treated and control groups for each variable (Ho, Imai et al. 2004).

TABLES 2 & 3 ABOUT HERE

VI. Results

The results support the hypothesis that imprisoning a neighborhood's residents decreases voter registration in that neighborhood. Also, neighborhoods in which a majority of residents are black were more likely to have a member sentenced to prison during the study period.

First, the visual evidence shows that prisoners tend to come from predominantly African American communities in Atlanta. Figure 1 presents a map of the city of Atlanta. In this map, block groups are shaded by their racial composition, with the darkest tracts having a higher percentage of black residents. Superimposed over this map are points representing the addresses of inmates who were sentenced to prison between April 10 and August 10 2006 from Atlanta. The relationship between imprisonment and the racial composition of neighborhoods is readily apparent; nearly all the prisoners in this study came from a community that was greater than 25 percent African American.

FIGURE 1 ABOUT HERE

With respect to the main hypothesis, the visual evidence also supports the claim that imprisonment suppresses voting. Figure 2 presents another map of the city of Atlanta. This time, prisoner addresses are superimposed over a map of voter registration rates by block group. It is clear from this map that registration rates are lower in communities in which an individual is sent to prison. High registration rates and high incarceration rates seem mutually exclusive, at least at the block group level. The average voter registration rate for neighborhoods in which at least one person was sent to prison is 67.6 percent; in the control groups it is 73.5 percent.

FIGURE 2 ABOUT HERE

However, this map alone does not provide conclusive evidence that incarceration suppresses turnout. The relationship could be spurious, for many of the factors that produce prisoners also reduce political participation. Thus, a multivariate analysis could help tease apart many of the confounding variables described previously in the model. As a reminder, in this research design, the ‘treatment’ consists of having at least one resident of the block group sent to prison between April 10 and June 10, 2006. If having a person from the neighborhood convicted suppresses participation, then block groups that had a person convicted before June 10 should have lower voter registration rates on June 10 than those block groups that did not have a person sentenced in the two months leading up to that date. As shown in Table 4, this is exactly the case. The coefficients on the indicators for having a resident sentenced before June 10 is in the expected direction and significant for all three models, even after controlling for income, poverty, the percent of residents living in group quarters, racial diversity, the median age of residents, the crime rate, home ownership rates, Church density (Model 2) does not seem to affect these results, nor does the presence of a college or university in the block group (Model 3). Interestingly enough, the number of residents sent to prison from the block group does not affect registration,

nor does having residents sentenced both before and after June 10th. Such a result appears to suggest that the exact number of residents sent to prison is less relevant than the fact that any are sent.⁶

TABLE 4 ABOUT HERE

Figure 3 presents the simulated difference in registration rates between the control and treated communities. Setting the continuous variables in model 1 at their means, the number of inmates sentenced at 1, and the indicator for having inmates convicted in both time periods at 0, the predicted registration rate in block groups where a resident is sent to prison is percent. In the control neighborhoods, the expected voter registration on June 10th was 74.4 percent. The simulations of first differences shows that having a person sentenced to prison before June 10th decreases voter registration an average of .090 percentage points (std. dev. 0.04513) from what it would have been if that person had been convicted after June 10th. The 95 percent confidence interval, computed from simulations, reveals that the true difference is likely to be anywhere between -0.6 percent and -17.6 percent. Such a result would be consistent with the effect of removing one voter if the population of block groups were much smaller; however, the average block group in this study has about 1400 residents, suggesting that incarcerating community members has important spillover effects that suppress registration not only of the incarcerated individual, but also those living around him or her.

FIGURE 3 ABOUT HERE

At first glance, a nine-percentage-point reduction in voter registration appears to be a very large result. However, simple estimates of effect size⁷ show that this is actually a “small”

⁶ In essence, the effect of the number of people sent to prison has to be non-linear; if 50 percent of a neighborhood is registered, then imprisoning the non-voting 40 percent of the neighborhood may not affect turnout. However, imprisoning 50-percent-plus-1 of the neighborhood necessarily decreases voting; registration will continue to decrease further as more residents are removed past this point.

effect ($d = .275$) (Cohen 1988). Analysis of Variance shows that including the treatment indicator in the model accounts for about three percent more of the variance. This effect persists consistently through several models even after matching, suggesting that more research should be done in the future to verify these findings.

VII. Discussion

The results of this study confirm that, on average, incarcerating residents of a community suppresses voter participation not only by excluding that one person from the electorate, but also by suppressing registration among residents of the whole community. Among neighborhoods from which an individual was sent to prison, voter registration is higher in neighborhoods prior to the sentencing and removal of the offender. While this analysis cannot shed much light on the particular mechanisms by which this suppression occurs, by controlling many of the neighborhood characteristics that would confound the analysis, the results at least provide compelling evidence that *something* is happening to the families and neighbors of imprisoned offenders. Moreover, this phenomenon has its greatest impact on African American neighborhoods, as these are the neighborhoods most likely to experience the imprisonment of its residents. While it is possible that participation increases in some neighborhoods that experience imprisonment, the findings presented here show that the net effect of imprisoning residents is to decrease participation.

These results do contradict conventional wisdom and thus raise important questions. The most frequently raised objection is that of omitted variable bias. Studying neighborhoods invites bias because it is impossible to know or measure all differences across neighborhoods that are correlated with both the treatment and the dependent variable. In response, it is important to note

⁷ Measured as difference in the mean voter registration between the matched treatment and control groups divided by the pooled standard deviation of voter registration (Cohen, 1988).

that many of the relevant control variables *are* included in the analysis, and the added step of pre-processing the data with matching further makes the treated and control neighborhoods as similar as possible. Moreover, the design of the research ensures that for bias to occur, the omitted variable has to be correlated not only with voter registration rates, but also with the specific timing of the sentence and removal. The treatment is the *timing* of imprisonment rather than the fact of having residents imprisoned itself; I argue that timing of the sentence in the short term is random. Granted, focusing on neighborhoods in which a resident is imprisoned and discarding those that do not experience imprisonment limits the claims one can make with these data. However, the tradeoff is that this narrower focus also helps limit potential sources of omitted variable bias.

Another likely critique concerns the size of the treatment effect. It is surprising that sending one person to prison could decrease voter registration nine percentage points in two months (or less). However, one should note that nine percentage points is only an estimate; at the low end of the 95 percent confidence interval, much smaller, but still negative effects are possible. Moreover, tests of statistical power reveal that this effect is actually small. Furthermore, one should also note that 2006 was a year with a highly salient midterm election; many individuals would have been registering to vote in preparation for July's election primary election during the time period of this study. As stated previously, one does not need to believe that people "unregister" because someone in their neighborhood is sent to prison in order to find these results compelling. Rather, it is more likely that some people who might register take longer to do so because of the financial and emotional upheaval imprisonment causes the families and friends of offenders. Alternatively, it might be harder for mobilizing influences such as friends, churches,

or parties to locate potential voters because they move, work more, or withdraw from their social networks.

VIII. Implications

The results of this study provide further evidence in support of a hypothesis gaining favor among sociologists and criminologists: imprisonment has the power to hurt, as well as help, neighborhoods. The analysis presented here applies the extant literature to voting behavior. In doing so, it further complicates our understanding of how context affects behavior.

Whether by increasing cultural deviance or social disorganization, or decreasing contact with mobilizing influences, having a large number of people in a community who cannot or do not vote due to their and their neighbors' experiences with the criminal justice system has important implications for politics, even in the short term. As described earlier, such alienation from politics shifts the power dynamics both within and outside of communities. Again, this problem is especially relevant for the study of black communities, which have been hardest hit by the growth of the criminal justice system (Fagan, West et al. 2004; Travis 2004). Scholars that study politics can no longer ignore the importance of the criminal justice system in shaping the power, coalitions, and resources available to neighborhoods, especially those of blacks.

For instance, within communities suffering from high levels of non-voting, voters benefit from incarceration because it augments the power of their votes. Residents who cannot or do not vote fail to communicate their needs to officials and are less likely to be encouraged to influence government through other channels. If decreased participation in community politics means that influence is shifted toward more advantaged members of the community, then the disadvantaged suffer (Verba, Schlozman et al. 1995). Increasing civic engagement in communities where the

most disadvantaged members are barred from participating only leads to greater “unrepresentativeness” (Fiorina 1999).

Incarceration may shift the balance of power in all neighborhoods. However, the evidence presented in this chapter suggests that these processes are more likely to affect power dynamics in disadvantaged black communities because they experience the incarceration of their residents to a worse degree than other communities. Cohen writes that the worst-off members of marginalized communities may be further marginalized by better-off members of their group, who exercise power by denying group rights and policing behavior (Cohen 1999). The marginalized members of marginalized groups are least able to communicate their needs to police and other government leaders; in many communities, this dynamic is especially important with respect to crime and other community problems. Skogan writes that the “homeowning, long-term residents of a community” are the ones “who learn about and participate in area-based programs” like community policing; the better-off residents are thus able to exercise power over their more disadvantaged neighbors (Skogan 1990). Randall Kennedy notes that many members of the Congressional Black Caucus supported the disparate crack-cocaine sentences that resulted in the mass incarceration of young black men since the 1980s, primarily in response to residents of black neighborhoods who were victims of crack-induced violence and crime (Kennedy 1998).

However, even though black and white voters in high-incarceration communities enjoy greater advantages at the local level, those benefits are offset by the disadvantages they face at higher levels of aggregation. Because political power is based partly on numeric strength when it comes to votes, low turnout among citizens with certain interests can hurt the ability of other voters who share those same interests to achieve their goals. This dynamic has been shown to operate at the state level; citizens of states with lower levels of mobilization among lower class

voters enjoy fewer social benefits (Hill and Leighley 1992). Disadvantaged communities also suffer from less effective social services, perhaps because they must rely on “altruism, guilt, or fear” rather than electoral threats to achieve their goals (Massey and Denton 1993: 160; Clark [1965] 1989). Low participation influences the distribution of resources across localities; Ansolabehere and Snyder also note that “governing parties skew the distribution of funds in favor of voters in areas that provide them with the strongest electoral support” (Ansolabehere and Snyder 2003). Thus, voters in areas where participation is low often are ignored in favor of areas where participation is higher.

IX. Conclusion

This paper has demonstrated a relationship between imprisonment rates and voter registration, finding that sending neighborhood residents to prison suppresses voter registration not only among inmates, but also among the family, friends, and neighbors they leave behind. This work implies that *convictions appear to affect participation independently of legal disenfranchisement*. Having one person in a community sent to prison seems to affect voter registration even among those residents who are not convicted or disenfranchised; thus, a community does not have to experience legal disenfranchisement in order for incarceration to affect politics. The increasing incidence of imprisonment in a community contributes to other social problems such as poverty, joblessness, crime, and the breakdown of family structures that further diminish the institutional resources that encourage registration. Imprisonment might also send signals to other community members about their relationship to government; incarcerating residents may influence community attitudes. This point suggests that removing disenfranchisement laws constitutes only the first step in breaking the link between criminal justice and political participation. People for and against removing disenfranchisement laws for convicts might well agree that

some steps need to be taken at least to prevent criminal justice from affecting citizens who do not commit crimes.

Moreover, these results point out a direction for future research. This paper focuses on the short term effects of imprisonment, but the potential for long term consequences should not be ignored. Not only might incarceration affect neighborhoods through social disorganization and cultural deviance, but also by decreasing the viability of institutions that support voting. Imprisonment deprives neighborhoods of economic resources. The decrease in family resources discussed earlier may translate into fewer resources to donate to churches and community organizations, both of which foster voting (Skocpol 1999, Putnam 2000). Furthermore, because the distribution of government resources is based on population, the removal and transfer of neighborhood residents to other locations means that schools and other social services in neighborhoods receive less funding as a result of imprisonment-induced depopulation (Huling 2002). Such neighborhoods are also allocated less representation in state and national legislatures; their political power is transferred to areas that house inmates (Huling 2002).

Works Cited

- Abu-Jamal, M. (1995). Live From Death Row. Reading, Addison-Wesley.
- Ansolabehere, S. and J. M. Snyder (2003). "Party Control of State Government and the Distribution of Public Expenditures." Cambridge, Massachusetts Institute of Technology.
- Austin, R. (2004). "The Shame of it All: Stigma and the Political Disenfranchisement of Formerly Convicted and Incarcerated Persons." Columbia Human Rights Law Review **36**: 173-192.
- Barber, G. M. (1988). Elementary Statistics for Geographers. New York, Guilford Press.
- Braman, D. (2002). Families and Incarceration. Invisible Punishment. M. Mauer and M. Chesney-Lind. New York, New Press.
- Brown-Dean, K. L. (2003). Culture, Context, and Competition: Explaining State-Level Variation in Felon Disenfranchisement Laws. Annual Meeting of the Midwest Political Science Association. Chicago, IL.
- Bursik, R. J. and H. G. Grasmick (1993). Neighborhoods and Crime. New York, Lexington
- Burbank, M. J. (1997). "Explaining Contextual Effects on Vote Choice." Political Behavior **19**(2): 113-132.
- Campbell, A. L. (2003). How Policies Make Citizens. Princeton: Princeton University Press.
- Campbell, A. and e. al. (1960). The American Voter. New York, Wiley & Sons.
- Chou, Y.-H. (1997). Exploring Spatial Analysis in Geographic Information Systems. Santa Fe, OnWord.
- Clark, K. B. ([1965] 1989). Dark Ghetto: Dilemmas of Social Power. Hanover, Wesleyan University Press.
- Clear, T. R. (2002). The Problem with "Addition by Subtraction": The Prison-Crime Relationship in Low-Income Communities. Invisible Punishment. M. Mauer and M. Chesney-Lind. New York, New Press.
- Cohen, C. J. (1999). The Boundaries of Blackness. Chicago, University of Chicago Press.
- Cohen, C. J. and M. C. Dawson (1993). "Neighborhood Poverty and African-American Politics." American Political Science Review **87**(2): 286-302.

Cohen, J. (1988). Statistical Power Analysis for the Behavioral Sciences, 2nd Edition . Hillsdale, NJ, Lawrence Erlbaum Associates.

Dietz, R. D. (2002). "The Estimation of Neighborhood Effects in the Social Sciences: An Interdisciplinary Approach." Social Science Research **31** : 539-575.

Edin, K., T. J. Nelson, et al. (2004). Fatherhood and Incarceration as Potential Turning Points in the Criminal Careers of Unskilled Men. Imprisoning America: The Social Effects of Mass Incarceration. M. Pattillo, D. Weiman and B. Western. New York, Sage: 46-75.

Fagan, J., V. West, et al. (2004). "Neighborhood, Crime, and Incarceration in New York City." Columbia Human Rights Law Review **36**: 71-107.

Fiorina, M. (1999). "Extreme Voices: A Dark Side of Civic Engagement". Civic Engagement in American Democracy. T. Skocpol and M. P. Fiorina. Washington DC, Brookings Institution: 395-426.

Foldare, I. S. (1968). "The Effect of Neighborhood on Voting Behavior." Political Science Quarterly **83**(4): 516-529.

Foreman Jr., J. (2002). Children, Cops, and Citizenship: Why Conservatives Should Oppose Racial Profiling. Invisible Punishment. M. Mauer and M. Chesney-Lind. New York, New Press: 150-162.

Gerber, A. S. and D. P. Green (2000). "The Effects of Canvassing, Telephone Calls, and Direct Mail on Voter Turnout." American Political Science Review **94**(3): 653-663.

Gerber, A. S., D. P. Green, et al. (2003). "Voting May Be Habit-Forming: Evidence from a Randomized Field Experiment." American Journal of Political Science **47**(3): 540-550.

"Glossary of Geographic Terms." U. S. Bureau of the Census. (2007). Retrieved 6 Apr 2007, from <http://www.census.gov/geo/www/tiger/glossary.html#glossary>.

Hahn, J., P. Todd, et al. (2001). "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." Econometrica **69**(1): 201-209.

Hannerz, U. ([1969] 2004). SoulSide: Inquiries into Ghetto Culture and Community. Chicago, University of Chicago Press.

Hill, K. Q. and J. E. Leighley (1992). "The Policy Consequences of Class Bias in State Electorates." American Journal of Political Science **36**(2): 351-365.

Ho, D. E., K. Imai, et al. (2004) Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference. Retrieved 15 October 2005, from <http://GKing.harvard.edu/files/matchp/pdf>.

- Holzer, H., S. Rafael, et al. (2004). Will Employers Hire Ex Offenders? Employer Preferences, Background checks, and Their Determinants. Imprisoning America: The Social Effects of Mass Incarceration. M. Pattillo, D. Weiman and B. Western. New York, Sage: 205-246.
- Huckfeldt, R. and J. Sprague (1987). "Networks in Context: The Social Flow of Political Information." American Political Science Review **81**(4): 1197-1216.
- Huckfeldt, R. and J. Sprague (1992). "Political Parties and Electoral Mobilization: Political Structure, Social Structure, and the Party Canvass." American Political Science Review **86**(1): 70-86.
- Huling, T. (2002). "Building a Prison Economy in Rural America." Invisible Punishment. M. Chesney-Lind and M. Mauer. New York, New Press: 197-213.
- Kennedy, R. (1998). Race, Crime and the Law. New York, Vintage Books.
- King, G., R. O. Keohane, et al. (1994). Designing Social Inquiry. Princeton Princeton University.
- King, G. and L. Zeng (2006). "The Dangers of Extreme Counterfactuals." Political Analysis **14**: 131-159.
- Kornhauser, R. R. (1978). Social Sources of Delinquency. Chicago, University of Chicago Press.
- Lynch, J. P. and W. J. Sabol (2004). "Assessing the Effects of Mass Incarceration on Informal Social Control in Communities." Criminology and Public Policy **3**(2): 267-294.
- Lynch, J. P., W. J. Sabol, et al. (2002). Crime, Coercion and Community: The Effects of Arrest and Incarceration Policies on Informal Social Control in Neighborhoods. National Criminal Justice Reference Service. 26 Feb 2007. from <http://www.ncjrs.gov/pdffiles1/nij/grants/195172.pdf>.
- Manza, J. and C. Uggen (2004). "Punishment and Democracy: Disenfranchisement of Nonincarcerated Felons in the United States." Perspectives on Politics **2**(3): 491-505.
- Manza, J. and C. Uggen (2006). Locked Out: Felon Disenfranchisement and American Democracy. Cambridge, Oxford.
- Massey, D. S. and N. A. Denton (1993). American Apartheid. Cambridge, Harvard.
- Mayer, S. E. and C. Jencks (1989). "Growing Up in Poor Neighborhoods: How Much Does it Matter?" Science **243**: 1441-1445.
- Mettler, S. (2005). Soldiers to Citizens. New York, Oxford.
- Miles, T. J. (2004). "Felon Disenfranchisement and Voter Turnout." Journal of Legal Studies **33**(1): 85-129.

- Morenoff, J. D. and R. J. Sampson (1997). "Violent Crime and the Spatial Dynamics of Neighborhood Transition: Chicago 1970-1990." Social Forces **76**(1): 31-64.
- Morenoff, J. D., R. J. Sampson, et al. (2001). "Neighborhood Inequality, Collective Efficacy, and the Spatial Dynamics of Urban Violence." Criminology **39**(3): 517-560.
- Morgan, S. L. and D. J. Harding. (2006). "Matching Estimators of Causal Effects: Prospects and Pitfalls in Theory and Practice." Sociological Methods and Research **35**(3): 3-59.
- "News Release." US Bureau of the Census. (2007). Retrieved 10 Apr 2007, from <http://www.census.gov/Press-Release/www/releases/archives/population/009865.html>.
- Pew Center on the States (2008). "One in 100: Behind Bars in America 2008." http://www.pewcenteronthestates.org/uploadedFiles/8015PCTS_Prison08_FINAL_2-1-1_FORWEB.pdf ; retrieved 18 Mar 2008.
- Plutzer, E. (2002). "Becoming a Habitual Voter: Inertia, Resources, and Growth in Young Adulthood." American Political Science Review **96**(1): 41-56.
- Putnam, R. D. (2000). Bowling Alone. New York, Simon & Schuster.
- Richie, B. (2002). The Social Impact of Mass Incarceration on Women. Invisible Punishment. M. Mauer and M. Chesney-Lind. New York, New Press.
- Rose, D. R. and T. R. Clear (1998). "Incarceration, Social Capital, and Crime: Implications for Social Disorganization Theory." Criminology **36**(3): 441-479.
- Rosenbaum, P. R. (1987). "The Role of a Second Control Group in an Observational Study." Statistical Science **2**(292-306).
- Rosenstone, S. J. and J. M. Hansen (1993). Mobilization, Participation, and Democracy in America. New York, MacMillan.
- Sampson, R. J. (1988). "Local Friendship Ties and Community Attachment in Mass Society: A Multilevel Systemic Model." American Sociological Review **53**(5): 766-779.
- Sampson, R. J. and W. B. Groves (1989). "Community Structure and Crime: Testing Social-Disorganization Theory." American Journal of Sociology **94**(4): 774-802.
- Scan/US. (2008). "Scan/US Methodology White Paper." from <http://www.scanus.com/pdfs/mwp2004.pdf>.
- Shaw, C. R. and H. D. McKay (1942). Juvenile Delinquency and Urban Areas. Chicago, University of Chicago.

- Skocpol, T. (1999). "How America Became Civic." Civic Engagement in American Democracy. T. Skocpol and M. Fiorina. Washington D. C., Brookings Institution: 27-80.
- Skogan, W. G. (1990). Disorder and Decline. New York, Free Press.
- Soss, J. (1999). "Lessons of Welfare: Policy Design, Political Learning, and Political Action." American Political Science Review **93**: 363-380.
- Stoker, L. and M. K. Jennings (1995). "Life Cycle Transitions and Political Participation: The Case of Marriage." American Political Science Review **89**(2): 421-433.
- Thistlethwaite, D. L. and D. T. Campbell (1960). "Regression-Discontinuity Analysis: An Alternative to the Ex Post Facto Experiment." Journal of Educational Psychology **51**: 309-317.
- Travis, J. (2004). Reentry and Reintegration: New Perspectives on the Challenges of Mass Incarceration. Imprisoning America: The Social Effects of Mass Incarceration. M. Pattillo, D. Weiman and B. Western. New York, Sage: 247-268.
- Verba, S. and N. Nie (1972). Participation in America. New York, Harper and Row.
- Verba, S., K. L. Schlozman, et al. (1995). Voice and Equality. Cambridge, Harvard University.
- Western, B., J. R. Kling, et al. (2001). "The Labor Market Consequences of Incarceration." Crime & Delinquency **47**: 410-427.
- Western, B., L. Lopoo, et al. (2004). Incarceration and the Bonds between Parents in Fragile Families. Imprisoning America: The Social Effects of Mass Incarceration. M. Pattillo, D. Weiman and B. Western. New York, Sage: 21-45.
- Wilson, J. Q. and G. L. Kelling (1982). Broken Windows. Atlantic Monthly. **243**: 29-38.
- Wilson, W. J. (1987). The Truly Disadvantaged. Chicago, University of Chicago Press.
- Wilson, W. J. (1996). When Work Disappears. New York, Knopf.
- Yoshinaka, A. and C. R. Grose (2005). "Partisan Politics and Electoral Design: The Enfranchisement of Felons and Ex-Felons in the United States, 1960-1999." State and Local Government Review **37**(1): 49-60.

<i>Inmate Data</i>	<i>Full Sample</i>	<i>Reduced Sample</i>
Women	26	16
Men	265	142
White	18	13
Black	273	145
Average Age	35.59	36.16
Convicted before 6/10/06	184	118
Convicted after 6/10/06	107	40
Convicted between 6/1/06 and 6/10/06	30	17
N	291	158

Table 1: Characteristics of Inmates.

Matched Data	Means (Treated)	Means (Control)	SD	Difference in Means
Voter Registration	0.676	0.733	0.218	-0.057
Median Income	31124	36023	23480	-4899
Percent White	0.15	0.184	0.185	-0.034
Percent Hispanic	0.033	0.0747	0.1876	-0.0417
Crime Rate	0.039	0.0576	0.0962	-0.0186
Number Convicted	2.059	1.891	0.823	0.168
Percent Group Quarters	0.039	0.02	0.046	0.019
Percent Owner Occupied	0.361	0.33	0.2548	0.031
Percent Under 10k	0.239	0.222	0.1694	0.017
Median Age	33.927	35.213	6.679	-1.286
Church Density	4.69	4.287	5.064	0.403
College	0.454	0.5	0.5099	-0.046
N	119	26		

All Data	Means (Treated)	Means (Control)	SD	Difference in Means
Voter Registration	.676	.703	0.2167	-0.027
Median Income	31123	36090	21066	-4967
Percent White	0.15	0.252	0.252	-0.102
Percent Hispanic	0.033	0.07	0.167	-0.037
Crime Rate	0.039	0.055	0.08	-0.016
Number Convicted	2.059	1.265	0.511	0.794
Percent Group Quarters	0.039	0.026	0.052	0.014
Percent Owner Occupied	0.361	0.347	0.249	0.014
Percent Under 10k	0.239	0.202	0.158	0.037
Median Age	33.927	36.191	7.101	-2.264
Church Density	4.69	5.3	7.52	-0.61
College	0.454	0.441	0.504	0.013
N	119	34		

Table 2: Descriptive Statistics for All Data and Matched Data. Includes block groups with residents convicted both before and after June 10. Matched data were pre-processed using MatchIt (Ho, Imai et al. 2004).

Matched Data	Means (Treated)	Means (Control)	SD	Difference in Means
Voter Registration	.685			
Median Income	35756	35459	23830	-296
Percent White	0.173	0.207	0.242	-0.034
Percent Hispanic	0.026	0.077	0.191	-0.051
Crime Rate	0.038	0.056	0.092	-0.018
Number Convicted	1.597	1.320	0.557	0.277
Percent Group Quarters	.030	0.013	0.036	0.017
Percent Owner Occupied	.402	0.33	0.261	0.071
Percent Under 10k	.207	0.227	0.173	0.020
Median Age	35.14	35.28	7.444	-0.143
Church Density	4.41	4.67	5.064	-0.26
College	0.5	0.440	0.5099	0.06
N	72	25		

All Data	Means (Treated)	Means (Control)	SD	Difference in Means
Voter Registration	.685	.703	0.2167	-0.018
Median Income	35756	36090	21066	-334
Percent White	0.173	0.252	0.252	-0.079
Percent Hispanic	0.026	0.07	0.167	-0.044
Crime Rate	0.0389	0.055	0.08	-0.017
Number Convicted	1.597	1.265	0.511	.333
Percent Group Quarters	.030	0.026	0.052	.004
Percent Owner Occupied	.402	0.347	0.249	.056
Percent Under 10k	.207	0.202	0.158	.004
Median Age	35.140	36.191	7.101	-1.505
Church Density	4.41	5.3	5.33	-0.89
College	0.5	0.441	0.507	0.059
N	72	34		

Table 3: Descriptive Statistics for All Data and Matched Data. Excludes block groups with residents convicted both before and after June 10. Matched data were pre-processed using MatchIt (Ho, Imai et al. 2004).

	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4</i>
Convicted before 6/10/06	-0.091 (.044)	-0.090 (0.044)	-0.091 (0.044)	-0.080 (.047)
Residents Convicted before & after 6/10/06	0.012 (.041)	0.012 (0.041)	0.016 (0.041)	N/A
Median Income	7.14E-07 (0.000)	7.03E-07 (0.000)	6.51E-07 (0.000)	2.06E-07 (0.000)
Percent White	-0.111 (.121)	-0.109 (0.121)	-0.100 (0.122)	-0.032 (.146)
Percent Hispanic	-0.486 (.177)	-0.493 (0.178)	-0.484 (0.177)	-0.321 (.215)
Crime Rate	0.691 (.357)	0.722 (0.362)	0.656 (0.362)	0.573 (.407)
Number Convicted	0.014 (.017)	0.014 (0.017)	0.013 (0.017)	0.022 (.022)
Percent Group Quarters	0.118 (.13)	0.118 (0.130)	0.102 (0.132)	0.239 (.197)
Percent Owner Occupied	0.280 (.11)	0.278 (0.111)	0.289 (0.111)	0.387 (.139)
Percent Under 10k	-0.173 (.157)	-0.161 (0.159)	-0.162 (0.158)	-0.206 (.219)
Median Age	1.23E-03 (.003)	1.28E-03 (0.003)	1.21E-03 (0.003)	-5.12E-05 (.004)
Church Density		-0.001 (0.002)		
College or University			0.021 (0.032)	
Intercept	0.610 (.126)	0.611 (0.126)	0.598 (0.127)	0.597 (.164)
N	145	145	145	91
Adjusted R-squared	.21	.205	.206	.19

Table 4: Estimated effects of incarceration on voter registration in neighborhoods. Standard errors in parentheses; * =P < .05; ** P < .01; *** P < .001.

Incarceration and Race by Block Group
Atlanta, Georgia
April to August 2006

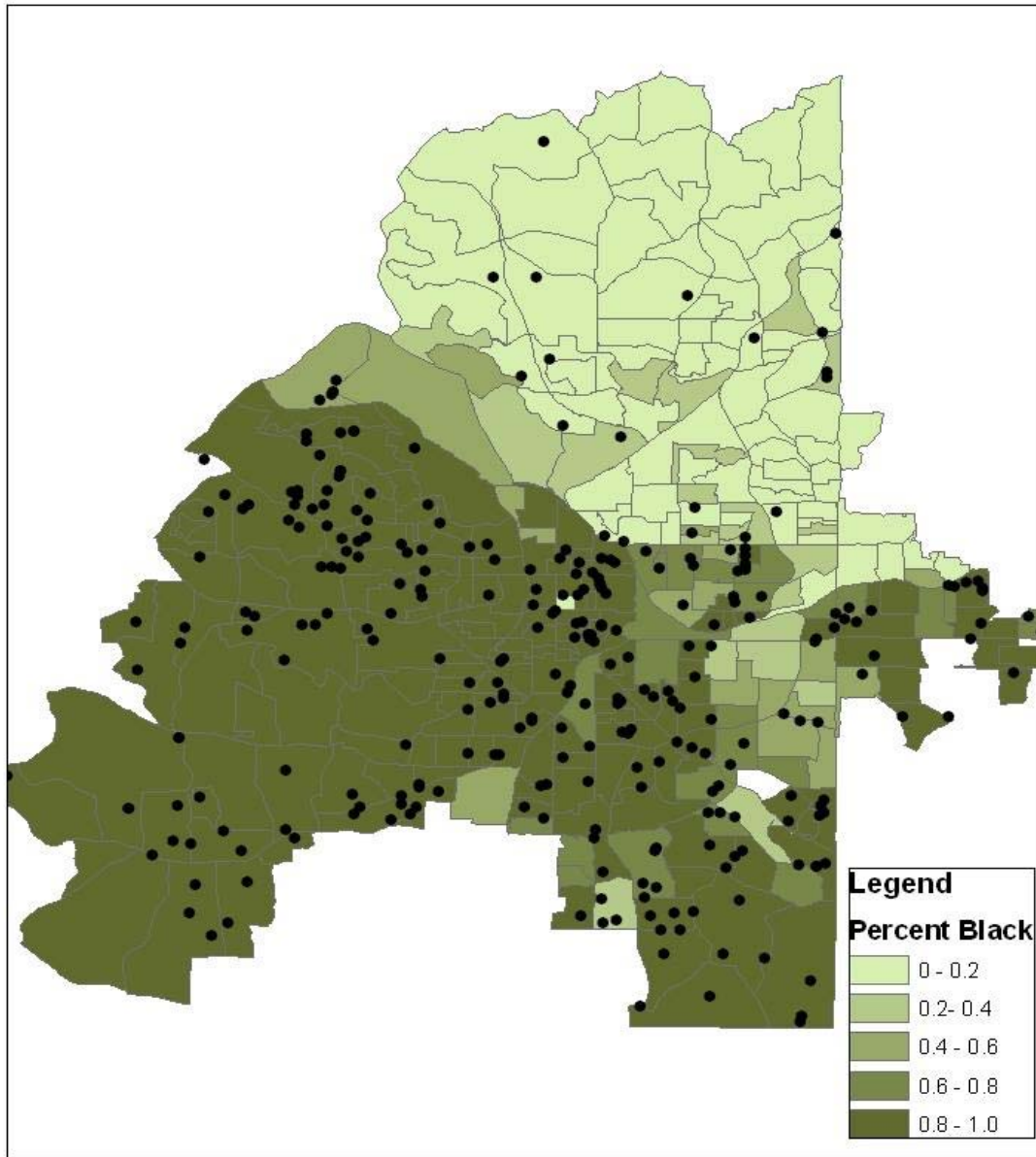


Figure 1: Imprisonment and Racial Composition by Block Group, Atlanta, GA. The map includes data on the percent of non-Hispanic black residents in each tract. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the tracts and include inmates from neighborhoods in which offenders were sentenced both before and after June 10, 2006.

Incarceration and Voter Registration by Block Group Atlanta, Georgia April - August 2006

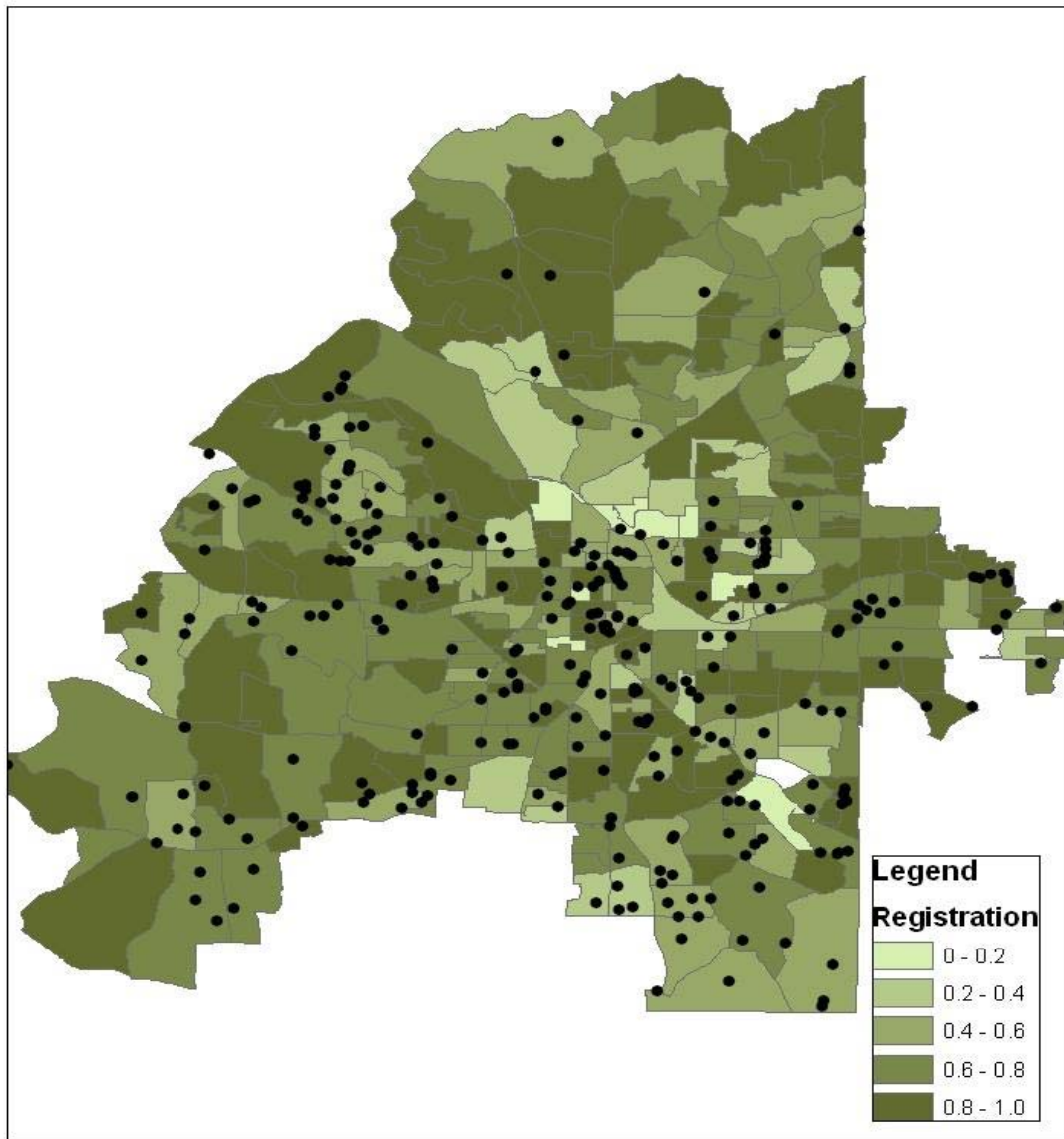


Figure 2: Imprisonment and Voter Registration by Block Group, Atlanta, GA. Voter Registration is measured as a count of voters registered in each tract, divided by the 2006 estimated adult population in each tract. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the tracts and include inmates from neighborhoods in which offenders were sentenced both before and after June 10, 2006.

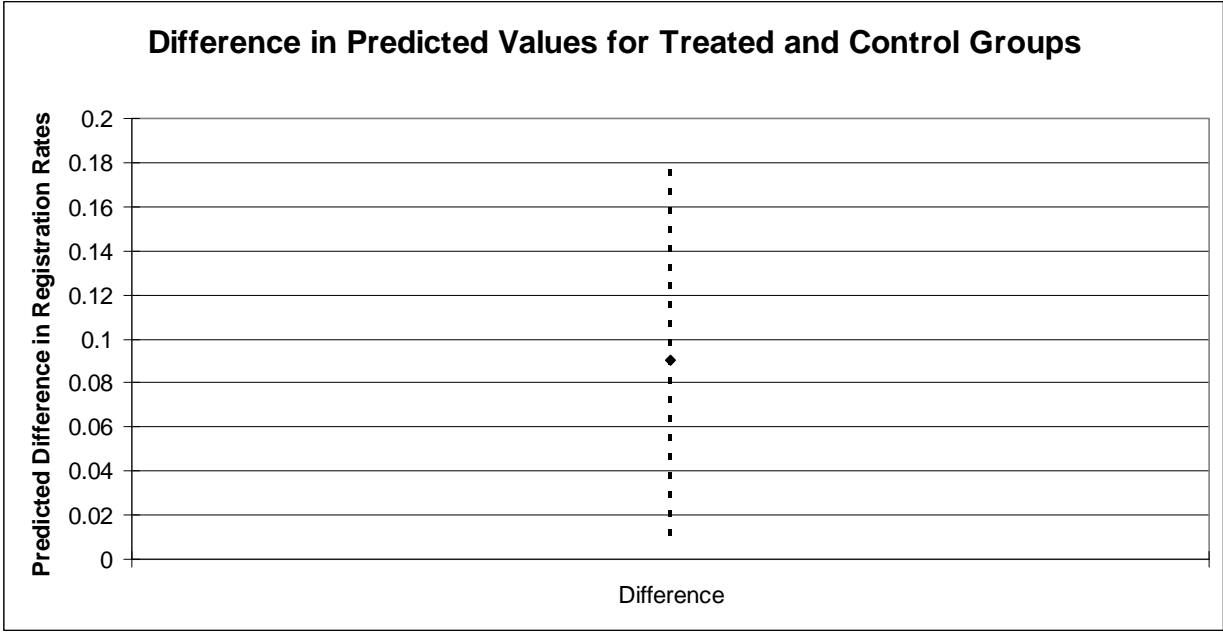


Figure 3: Predicted First Differences in Registration Rates. The dashed lines surrounding the point represent the 95% confidence intervals. Estimates based on Model 1, with the number of residents sent to prison set to 1 and the indicators for ‘residents convicted in both time periods’ set to zero. All other variables are set at their means.