TRADING DEMOCRACY FOR JUSTICE?

THE SPILLOVER EFFECTS OF IMPRISONMENT ON VOTING IN ATLANTA & CHARLOTTE

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

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I. Introduction

Although the 2008 Presidential Election was marked by high political interest and excitement, understanding the factors that inhibit voter participation remains important even during this time of increased civic engagement. Previous work has highlighted how one such factor, growing imprisonment rates, may have changed recent electoral outcomes by increasing the number of people who could not vote due to felon disfranchisement laws (Manza & Uggen 2004; Manza & Uggen 2006). Imprisonment and felon disfranchisement laws hinder ability of many Americans to participate in elections. However, what about the people they leave behind? Might the removal, imprisonment, and disfranchisement of convicted offenders affect the voter turnout of their families, friends, and neighbors?

This paper examines the relationship between individual imprisonment and neighborhood voter turnout rates in Atlanta, Georgia and Charlotte, North Carolina, arguing that imprisoning neighborhood residents decreases voting. The idea that sending a person to prison dampens the political activity of that person's entire neighborhood seems surprising or even counterintuitive at first glance as one might think that capturing and punishing criminals should improve life, and thus civic engagement, in a neighborhood. However, I argue that incarceration suppresses turnout; this outcome occurs not only 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 close to Election Day, people who might have in ordinary times may fail to do so.

To test this claim, I employ geospatial technology rarely used by political scientists to pinpoint locations of voters and offenders, situating them in neighborhoods around Atlanta and

Charlotte. The relationship between neighborhood imprisonment rates and voter turnout rates is complex and often coincides with neighborhood poverty, crime, and other social conditions. This research takes advantage of the timing of prison admissions to set up a natural experiment in which turnout rates of voters in neighborhoods that experience imprisonment are compared with those in control neighborhoods matched in terms of crime rates, poverty rates, racial composition, and other factors. For each city, I compare neighborhoods from which prisoners were incarcerated within 90 days of Election Day with neighborhoods from which no prisoners were incarcerated during that time period. The key innovation of this research design involves the analysis of a reduced sample in which the comparison group, like the treatment group, includes only neighborhoods that have residents sent to prison. However, in this reduced sample, all inmates from the control neighborhoods were admitted up to 90 days *after* the election. Comparing neighborhoods that experience imprisonment within this narrow window around the election further lessens the impact of unmeasured differences across neighborhoods that do and do not experience imprisonment.

The findings indicate that sending individuals to prison decreases neighborhood voter turnout. In both cities, neighborhoods (defined as block groups) that experienced the imprisonment of residents 90 days before the November General Election voted at lower rates 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, home vacancy rates, crime rates, church density, and the presence of colleges and universities in the neighborhood. Moreover, this relationship between incarceration and voter turnout is present in Charlotte even after excluding neighborhoods that had people sentenced to prison in neither or both time periods. Based on simulations, imprisoning neighborhood residents in Char-

lotte decreased turnout about five percentage points on average in both the full and reduced samples. In the typical block group with a population of 1100 adults, this figure translates into a difference of about 55 votes; however, this figure could be as low as eleven votes. In Atlanta, neighborhoods in the full sample that experienced imprisonment before the election had turnout rates almost nineteen percentage points lower than those that did not, a difference of almost 200 votes. However, the confidence intervals for this estimate, though statistically significant, are fairly wide; the actual difference may be as low as .98 percentage points, or eleven votes.

The results of this study provide evidence that incarceration produces at least a short-term effect on voter turnout at the neighborhood level. Although a decrease in turnout as low as ten people seems small, its effects are magnified considering just how prevalent imprisonment has become among certain segments of the population. Western, Pattillo, and Weiman, summarizing previous research, argue that although people who have been convicted of crimes currently constitute a small proportion of the overall population, "Nine out of ten prison inmates are male, most are under the age of 40, African-Americans are seven times more likely than whites to be in prison, and nearly all prisoners lack any education beyond high school" (Western, Pattillo et al. 2004). For high school dropouts, incarceration is fast becoming "a pervasive event" in the life cycle; 32.4 percent of young black male high school dropouts aged 22-30 were in prison or jail; for comparable whites, the figure is 6.7% (Western, Pattillo et al. 2004: 7). Thus, imprisonment has the potential to devastate the ability of poor, young men and their families to participate in politics.

Depressing turnout might change electoral outcomes, particularly at the local level. 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 political science, the few studies that investigate this relationship look only at the effect of disfranchisement 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 are 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.

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). Cultural deviance theories 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 (Campbell, Converse et al. 1960; Rosenstone and Hansen 1993; Stoker and Jennings

1995; Plutzer 2002). Likewise, 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).

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. 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 (Braman 2002, Austin 2004).

Imprisonment may suppress participation by depriving families and friends of the time, money, and civic skills that facilitate voting (Verba et al. 1995). 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.

Finally, 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. Typical get-out-the-vote methods may yield fewer voters in high-conviction neighborhoods if residents are suspicious of 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. 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).

To reiterate, incarceration is so disruptive to communities that its effects could influence turnout even in the span of a few 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 individuals who are convicted of crimes because they often move or are ostracized from their communities. Finally, imprisonment may decrease the desire to vote 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. At the same time, mobilizing organizations, while pursuing their routine strategies of mobilization, unwittingly might exacerbate these effects on registration. 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 voting, 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 turnout in the aggregate once intervening factors are taken into account. The apparent relationship between incarceration and turnout 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 or by mobilizing the community against further imprisonment (Wilson and Kelling 1982; Putnam 2000; Cohen 2009).

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 ac-

counts 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]

I conduct two analyses for each city. Both analyses compare the percentage of adults who voted in the November 4, 2008 General Election in Atlanta and Charlotte neighborhoods that had residents admitted to prison within 90 days of the election with voter turnout in a control group of neighborhoods that did not have residents admitted to prison before the election. The treated group for the initial analysis includes neighborhoods that had residents admitted to prison before the election, including neighborhoods that also had residents admitted after the election. This control group includes neighborhoods from which residents were admitted only after the election and neighborhoods from which no residents were admitted in either time period.

For the analysis on the reduced sample, the treated group includes only neighborhoods that had residents imprisoned before the election and not after; neighborhoods that sent residents in both time periods are discarded from this reduced treatment group. Likewise, the control group for the reduced analysis does not include all neighborhoods that did not have a resident sent to prison before the election. Only neighborhoods that had an individual sent to prison up to 90 days after the election were included in this control group. Neighborhoods that had no one imprisoned before or after the election are excluded from the reduced analysis.

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 docket, the length of the trial, and the like. Within the set of communities that experience imprisonment around Election Day, one could compare communities that receive the treatment before the election with those that receive the treatment afterward as a way of testing the effects of incarceration on turnout.

In the full sample, the initial comparison between the treatment and control groups provides an upper bound on the estimate of the effects of imprisonment on neighborhood turnout. However some might argue that unmeasured factors cause some neighborhoods to produce prisoners while others do not. The tighter comparison among neighborhoods in the reduced sample avoids this problem and provides a more conservative estimate of the effects of imprisonment on turnout. Limiting the control group in the reduced analysis 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 the election 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 an individual from a given neighborhood is incarcerated *and* that neighborhood's voter turnout rate, any differences in the dependent variables across the treated and control groups should be due to imprisonment. To reiterate, within the reduced sample, any differences across neighborhoods would have to be correlated with *both* the timing of the sentence and voter turnout in order to bias the results.

For both analyses, 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 turnout in the treated group with that of the control group produces an estimate that does not make assumptions about functional form.

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 both imprisonment and voter turnout. All neighborhoods included in the reduced 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. Likewise, excluding neighborhoods from which individuals are sent to prison in both time periods reduces bias as well. The random assignment of the remaining neighborhoods to the treatment and control conditions (residents sentenced to prison before or after Election Day, respectively) should 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 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

[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 Election Day 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, eight Charlotte inmates and thirty-three Atlanta Inmates (out of 191 and 307 sentenced before the election, respectively) were convicted less than a week before the election.

while decreasing voter registration; 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, turnout, 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 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

[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.

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. Since 2000, North Carolina has kept a list of all removed and inactive voters.

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 new incarcerations that take place right before the election. In setting up the research in this way, the intervention is to measure the average effect of imprisoning additional people 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, sending another person to prison, no matter how many people were imprisoned previously, on average will result in a measurable decrease in voter turnout within three months of sentencing. It is plausible that some people who might have voted do not because someone in their neighborhood or household is sent to prison. Do these short-term suppressions of political activity add up to a permanent decrease in voter turnout in subsequent elections? Perhaps, but it is likely that the hypothetical individuals described in the previous section would eventually vote in the future. 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 admit 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, 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 admission dates because it introduces other factors that disconnect crime and removal (for instance, scheduling). Finally, admit dates are preferable to arrest dates because only convicted offenders experience disfranchisement and removal from the voter rolls in Georgia and North Carolina.

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 90 days prior to Election day should have lower voter turnout than control neighborhoods in which no one was sentenced to prison 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 turnout between neighborhoods in which residents were sentenced to prison and those in which no residents were sentenced to prison before Election Day after taking confounding factors such as poverty, racial composition, and residential mobility into account.*

3. *Neighborhoods that had members imprisoned in the 90 days before the election have higher rates of voter turnout than control neighborhoods in which no one was sentenced to prison 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 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 residents leads to in increase in social capital or social trust that in turn facilitates voting. Alternatively, the third hypothesis also is consistent with increased political mobilization as a result of anger over imprisonment.

V. Data

The data for this study were obtained by combining updated demographic estimates for block groups with data on prison inmates, crimes, and voters within the city limits of Atlanta, Georgia and Charlotte, North Carolina. The result of this massive effort is the combining of voter registration and history records, criminal records, and geographic data into two data sets on which spatial analyses can be performed. Both Charlotte and Atlanta are ideal for this study because high imprisonment rates and large city populations lend themselves to a relatively large number of observations. Table 1 provides basic demographic information on each city.

TABLE 1 ABOUT HERE

A. Demographic Data

Block groups are the units of analysis. Estimates for the 2008 demographic characteristics on block groups were obtained from Scan/US and ESRI. Because Charlotte and Atlanta have experienced rapid growth 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 2008 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 group boundaries do not map precisely onto the city boundaries. Block groups within each city were identified by ArcGIS as the block groups whose centroids fell within the city limits.[4] There are 293 block groups in the city of Atlanta; only the 267 block groups in Fulton County are included in the analysis after discarding one extreme outlier. Of these block groups, 80 had no individuals admitted to prison, 52 block groups had individuals admitted before the election (but not after), 36 had individuals admitted after the election (but not before), and 99 had individuals admitted both before and after the election. The size of the full Atlanta sample before matching is 267 block groups: 151 in the treatment and 116

[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.

in the control group. The reduced sample contains 88 block groups: 52 with individuals sentenced before the election and 36 with individuals sentenced after the election.

The city of Charlotte contains 290 block groups. Of these, 57 neighborhoods had people admitted before the election (but not after), 36 neighborhoods had individuals admitted after the election (but not before), 55 neighborhoods had individuals admitted in both time periods, and 142 neighborhoods had no one sent to prison. Thus, the size of the full sample in Charlotte before matching is 300 block groups: 112 in the treatment group and 178 in the control group. The size of the reduced Charlotte sample before matching is 93 block groups: 57 with residents convicted before the election and 36 with residents convicted after the election. These sample sizes are summarized in Table 2.

TABLE 2 ABOUT HERE

For each block group, vacancy rate, percent black, percent Hispanic, poverty rate, median income, median age, percent in group housing, adult population, and population density were obtained using the data from Scan/US. Crime rates were obtained from local police departments.

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 Paroles and Pardons. 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 March 2010 and contains more than 500,000 prison episodes.

The North Carolina Department of Correction (NDC) provided deindividuated data on the race, gender, offense, age, sentence length, punishment type, and address of all individuals who had been admitted to state supervision for felonies or misdemeanors between January 1 2007 and June 1, 2009. Please see Table 3 for information about the background of inmates included in the analysis. As shown in the table, the overwhelming majority of people sent to prison from both cities were black and male.

TABLE 3 ABOUT HERE

The statewide voter registration lists for both North Carolina and Georgia were generated in November 2008 and contain more than seven million and six million voters, respectively. The lists contain data on the first and last names, address, precinct, race, gender, date of birth, and voter history of all Georgians or North Carolinians who were registered to vote in the November 2008 election.

C. Geocoding

Addresses for prisoners and voters were converted to points with latitudes and longitudes and then to census blocks by geocoding with ArcGIS. Inmates and voters who could not be matched through ArcGIS were matched using BatchGeo.com (which relies on the Yahoo!Maps geocoder). ESRI data, included with ArcGIS, were also used to geocode post-secondary educational institutions and churches to block groups.

D. Methodology

For the multivariate analysis of the effect of incarceration on turnout, the data were analyzed at the block group level. As discussed earlier and below, many factors may affect voter turnout and thus must be controlled in this analysis. Income, crime, the presence of young residents, and racial and ethnic diversity have been shown to influence both neighborhood outcomes including voter turnout; thus, they are included in these models as median income, poverty rate, crime rate, median age, citizenship rate, percent black, 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 turnout so a measure of the percent of the population in group quarters is included in the models. As a final check, the models include measures of church density and the number of post-secondary educational institutions.

The inmates variable is a measure of the number of people imprisoned from each block group between August 4, 2008 and January 4, 2009. The dependent variable, voter turnout, is the number of people from the block group who voted November General Election divided by the 2008 adult population of the block group.

In order to make the treated block groups as similar to the control groups as possible, the data are pre-processed using MatchIt (Ho, Imai et al. 2004). The nearest neighbor method without replacement is 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 turnout and incarceration at the neighborhood level tested using ordinary least-squares regression on the matched data:

Voter Turnout	=	Convicted before Election	+	# Inmates	+	Crime Rate	+	% Black & Hispanic	+	% Vacant	+	% Group Quarters	+	Non Citizen	+	Church Density	+	Colleges	+	Median Age	+	Median Income	+	% Poverty

Where:

- "Voter Turnout" is the percentage of adults who voted in November 2008
- "Convicted before election" is a dummy indicating whether a neighborhood had its residents sentenced to prison before the November election
- "Inmates" 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 2008
- The "percent black" is the black percentage of the total block group population in 2008
- The "percent Hispanic" is Hispanic percentage of the total block group population in 2008
- "Percent vacant" is a proxy of residential mobility, defined as the proportion of housing units that were vacant in 2008
- "Percent in group quarters" is the percentage of the block group population living in group quarters in 2008
- "Noncitizen" is the percentage of the block group who were not citizens in 2000
- "Church density" is the number of churches per square mile for each block group

- Colleges is the number of postsecondary institutions in the block group.
- "Median age" is the median age of the total population in 2008.
- "Median income" is the median household income of the block group in 2008.
- "Poverty Rate" is the percentage of households with income less than $25,000 in 2006.

The results of the nearest neighbor matching are shown below. The treated and control groups were matched based on all the covariates. Comparing the means of the matched treated and control groups in Tables 4 through 7 reveals that the matching improved the balance across all the covariates in each set of data (Ho, Imai et al. 2004).

TABLE 4 ABOUT HERE

TABLE 5 ABOUT HERE

TABLE 6 ABOUT HERE

TABLE 7 ABOUT HERE

VI. Results

The results support the hypothesis that imprisoning a neighborhood's residents decreases voter turnout in that neighborhood. This phenomenon has a racially disparate impact, as residents of black neighborhoods disproportionately experienced imprisonment during the study period.

First, the visual evidence shows that prisoners tend to come from predominantly African American communities in Charlotte and Atlanta. Figure 1 presents a map of the city of Charlotte. In this map, block groups are shaded by their racial composition, with the darkest block groups having a higher percentage of black residents. Superimposed over this map are points representing the addresses of inmates who were sentenced to prison between August 4, 2008 and January 4, 2009. Figure 2 presents the same information for Atlanta. In both cities, the relation-

ship 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

FIGURE 2 ABOUT HERE

With respect to the main hypothesis, the visual evidence also supports the claim that imprisonment suppresses voting. Figures 3 and 4 present new maps of the Charlotte and Atlanta. This time, prisoner addresses are superimposed over a map of turnout rates in the November 2008 Election by block group. It is clear from this map that turnout rates are lower in communities in which an individual is sent to prison. High turnout and high incarceration rates seem mutually exclusive, at least at the block group level.

FIGURE 3 ABOUT HERE

FIGURE 4 ABOUT HERE

However, these maps alone do not provide conclusive evidence that incarceration suppresses voter 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 in the 90 days before the November Election. If having a person from the neighborhood convicted suppresses participation, then block groups that had a person convicted before Election Day should have lower turnout than those block groups that did not have a person sentenced in the three months before the election. As shown in Table 8, this is exactly the case for three of the four analyses. Table 8 shows the regression coefficients for the Charlotte and Atlanta full

and reduced samples. The coefficients on the indicators for having a resident sentenced before Election Day is negative and significant for three out of four 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 vacancy rates, the presence of a college or university in the block group, citizenship, and church density. The reduced model for Atlanta is the exception; the coefficient on the treatment variable is very close to zero. Interestingly enough, many of the factors that should be significant in the model such as racial and ethnic diversity, citizenship, or the number of residents sent to prison from the block group do not affect registration. This finding is likely due to the process of matching, which reduced much of the variation between the treatment and control groups along these dimensions.

TABLE 8 ABOUT HERE

Figure 5 presents the simulated turnout rates for in control and treated communities for the three models with significant effects. Setting the continuous variables at their means, the predicted voter turnout in block groups where a resident is sent to prison before the election in Charlotte is 44.7 percent in the full sample and 43.9 percent in the reduced sample. For the control neighborhoods, the predicted turnout is 49.5 for the full sample and 49.6 for the reduced sample. The simulations of first differences show that having a person sentenced to prison before the election decreases voter turnout period an average of five percentage points from what it would have been if that person had been convicted after the election. The 95 percent confidence interval, calculated from simulations, reveals that the true difference is likely to be between -0.105 and -0.008 percentage points in the full sample and between -0.083 and -0.015 percentage points in the reduced sample. Such a result would be consistent with the effect of removing only one inmate if the population of block groups were much smaller; however, the average block

group in this study has about 1100 residents, which translates into a minimum decrease of 11 voters. Similarly, in the Atlanta full sample, the treatment reduced voter turnout by eighteen percentage points. However, the confidence intervals on this estimate are much wider, ranging from -0.357 to -0.009. This difference translates into a minimum decrease of about 11 voters as well. These findings suggest that incarcerating community members has important spillover effects that suppress voting not only of the incarcerated individual, but also those living around him or her.

FIGURE 5 ABOUT HERE

VII. Discussion

The results of this study confirm that, on average, incarcerating residents of a community suppresses voter turnout not only by excluding that one person from the electorate, but also by suppressing voting among residents of the whole community. Among neighborhoods from which an individual was sent to prison, the rate of voter turnout is higher prior to the 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 in some instances that turnout increases in 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 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 turnout, but also with the specific timing of the residents' admission to prison. In particular, in the reduced samples the treatment is the *timing* of imprisonment rather than the fact of having residents imprisoned itself; the design assumes 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 produces arguably conservative estimates of the effect of imprisonment. However, this narrower focus also helps limit potential sources of omitted variable bias.

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 register to 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 paper 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 registration and 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.

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 or-

ganizations, 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).

IX. Conclusion

This paper has demonstrated a relationship between imprisonment rates and voter turnout, finding that sending neighborhood residents to prison suppresses registration not only by removing inmates from the voter rolls, but also by decreasing turnout among the family, friends, and neighbors they leave behind. This work implies that *imprisonment appears to affect participation independently of legal disfranchisement.* Having one person in a community sent to prison seems to depress voter turnout even among those residents who are not convicted or disfranchised; thus, a community does not have to experience legal disfranchisement 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 disfranchisement laws constitutes only the first step in breaking the link between criminal justice and political participation. People for and against removing disfranchisement 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.

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	Atlanta	*Charlotte*
Total Population	445,709	668, 796
Adult Population	352,929	493,296
Foreign Born	6.8%	13.7%
Median Income	$47,464	$53,125
Family Poverty	17.2%	8.8%
Vacancy Rate	20.9%	9.7%
White Non-Hispanic	36.5%	49.9%
Black	55.4%	32.9%
Hispanic or Latino	4.9%	11.2%
Violent Crime Rate	.14%	.09%
Party of Mayor	Democratic	Democratic

Table 1: Selected Characteristics of Sample Cities. Source: U. S. Census Bureau American Community Survey 2006-2008.

		Convicted Before Election	
		Yes	No
Convicted After Election	Yes	Charlotte: 55 Atlanta: 99	Charlotte: 36 Atlanta: 36
	No	Charlotte: 57 Atlanta: 52	Charlotte: 142 Atlanta: 80
	Total Full Sample:	Treated Charlotte: 112 Atlanta: 151	Control Charlotte: 178 Atlanta: 116

Table 2: Distribution of Block Groups before matching.

Inmate Descriptives	*Charlotte*	*Atlanta*
Race		
White	32	23
Black	264	549
Other	26	8
Gender		
Male	310	537
Female	12	44
Admission		
Admitted Before Election	191	307
Admitted After Election	131	274
Admitted Less Than 7 days before Election	8	33
Crime Type		
Violent Crime	106	152
Property Crime	68	245
White Collar Crime	16	N/A
Drug Crime	82	132
Other	50	52
N	322	581

Table 3: Inmate Descriptives. Includes inmates from all block groups.

All Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.757	0.153	0.218	0.604	0.649	0.606
Inmates	2.402	0.281	0.646	2.121	2	2.125
Crime Rate	0.246	0.151	0.148	0.095	0.082	0.094
Poverty Rate	0.064	0.041	0.038	0.023	0.027	0.025
Vacancy Rate	0.112	0.101	0.037	0.012	0.009	0.012
Percent Black	0.567	0.285	0.229	0.282	0.327	0.284
Percent Hispanic	0.138	0.104	0.148	0.035	0.023	0.037
Group Quarters Percent	0.025	0.021	0.072	0.005	0	0.007
Population Density	3421.611	3097.389	1651.539	324.222	382.375	438.887
Median Age	35.163	38.426	6.381	-3.263	3.285	3.232
Median Income	45.596	74.833	53.886	-29.238	20.804	28.977
Citizenship	0.101	0.096	0.092	0.005	0.011	0.013
Turnout (not included)	0.471	0.489	0.108	-0.018	0.02	0.021
N	112	178				
Matched Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.968	0.538	0.251	0.43	0.576	0.43
Inmates	2.853	1.412	0.743	1.441	1	1.441
Crime Rate	0.268	0.223	0.229	0.045	0.053	0.064
Poverty Rate	0.075	0.046	0.034	0.029	0.029	0.029
Vacancy Rate	0.12	0.109	0.031	0.011	0.008	0.013
Percent Black	0.644	0.455	0.274	0.19	0.238	0.19
Percent Hispanic	0.158	0.124	0.142	0.034	0.013	0.037
Group Quarters Percent	0.014	0.019	0.071	-0.005	0	0.01
Population Density	3756.064	3328.707	1514.684	427.357	527.588	527.809
Median Age	34.362	35.857	5.295	-1.494	1.46	1.745
Median Income	39.887	50.614	19.372	-10.727	10.452	10.727
Citizenship	0.105	0.105	0.092	0	0.009	0.012
Turnout (not included)	0.46	0.481	0.085	-0.021	0.016	0.023
N	34	34				

Table 4: Descriptives for Charlotte Matched Data. Includes all block groups. Matched using nearest neighbor method without replacement.

All Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.651	0.553	0.148	0.098	0.102	0.095
Inmates	1.526	1.389	0.728	0.137	0	0.139
Crime Rate	0.258	0.226	0.229	0.033	0.015	0.038
Poverty Rate	0.06	0.046	0.034	0.015	0.015	0.015
Percent Black	0.516	0.447	0.27	0.069	0.036	0.065
Percent Hispanic	0.125	0.123	0.138	0.002	0.014	0.019
Vacancy Rate	0.104	0.107	0.031	-0.003	0.005	0.007
Population Density	3277.597	3346.821	1495.744	-69.223	215.546	310.664
Median Income	50.231	51.061	18.948	-0.83	7.759	8.098
Group Quarters Percent	0.025	0.018	0.069	0.007	0	0.009
Median Age	35.223	36.057	5.21	-0.834	1.945	2.289
Citizenship	0.1	0.113	0.095	-0.013	0.021	0.021
Turnout (not included in matching model)	0.462	0.477	0.084	-0.015	0.017	0.027
N	57	36				

Matched Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.691	0.553	0.148	0.138	0.142	0.141
Inmates	1.528	1.389	0.728	0.139	0	0.139
Crime Rate	0.211	0.226	0.229	-0.015	0.012	0.028
Poverty Rate	0.059	0.046	0.034	0.014	0.018	0.019
Percent Black	0.494	0.447	0.27	0.047	0.028	0.059
Percent Hispanic	0.143	0.123	0.138	0.02	0.018	0.037
Vacancy Rate	0.097	0.107	0.031	-0.01	0.011	0.01
Population Density	3644.43	3346.821	1495.744	297.61	245.546	326.335
Median Income	54.649	51.061	18.948	3.588	4.74	10.106
Group Quarters Percent	0.024	0.018	0.069	0.006	0	0.008
Median Age	35.01	36.057	5.21	-1.047	1.94	2.399
Citizenship	0.106	0.113	0.095	-0.007	0.021	0.026
Turnout (not included in matching model)	0.449	0.477	0.084	-0.028	0.024	0.037
N	36	36				

Table 5: Descriptives for Charlotte Matched Data. Excludes block groups sending inmates to prison in neither time period and in both time periods. Matched using nearest neighbor method without replacement.

All Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.828	0.224	0.249	0.603	0.682	0.6
Inmates	3.49	0.466	0.849	3.025	3	2.983
Crime Rate	0.123	0.081	0.069	0.042	0.034	0.041
Poverty Rate	0.073	0.051	0.043	0.022	0.027	0.024
Vacancy Rate	0.218	0.212	0.073	0.006	0.01	0.016
Percent Black	0.699	0.392	0.3	0.306	0.325	0.304
Percent Hispanic	0.062	0.06	0.073	0.002	0.01	0.018
Group Quarters Percent	0.031	0.082	0.228	-0.051	0	0.053
Population Density	5094.818	5971.646	4826.104	-876.828	452.381	1090.77
Median Age	36.54	36.992	7.923	-0.452	0.55	0.782
Median Income	35.523	76.42	73.097	-40.897	28.497	41.025
Citizenship	0.043	0.042	0.071	0.001	0.008	0.012
Turnout (not included)	0.508	0.535	0.232	-0.027	0.042	0.046
N	151	116				

	Means Treated	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.984	0.502	0.244	0.482	0.535	0.482
Inmates	3.6	1.3	0.992	2.3	2	2.3
Crime Rate	0.146	0.105	0.089	0.04	0.025	0.04
Poverty Rate	0.077	0.069	0.041	0.008	0.016	0.014
Vacancy Rate	0.226	0.214	0.063	0.012	0.009	0.012
Percent Black	0.745	0.555	0.31	0.19	0.025	0.19
Percent Hispanic	0.062	0.07	0.107	-0.008	0.01	0.023
Group Quarters Percent	0.016	0.028	0.083	-0.012	0	0.019
Population Density	4220.157	5407.528	3268.406	-1187.37	749.684	1252.497
Median Age	37.422	35.959	8.797	1.463	2.085	2.143
Median Income	31.172	46.771	27.363	-15.599	9.977	15.684
Citizenship	0.049	0.059	0.111	-0.01	0.005	0.017
Turnout (not included)	0.472	0.534	0.237	-0.063	0.053	0.066
N	40	40				

Table 6: Descriptives for Atlanta Matched Data. Includes all block groups. Matched using nearest neighbor method without replacement.

All Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Med*	*eQQ Mean*
Distance	0.633	0.531	0.154	0.102	0.073	0.096
Inmates	1.481	1.5	0.878	-0.019	0	0.083
Crime Rate	0.117	0.089	0.053	0.028	0.008	0.034
Poverty Rate	0.062	0.07	0.042	-0.007	0.008	0.009
Percent Black	0.595	0.605	0.285	-0.009	0.019	0.034
Percent Hispanic	0.066	0.051	0.038	0.016	0.006	0.034
Vacancy Rate	0.213	0.222	0.065	-0.009	0.016	0.016
Population Density	5672.428	5830.379	3360.123	-157.952	587.141	967.532
Median Income	47.677	44.997	25.04	2.68	4.599	10.035
Group Quarters Percent	0.028	0.082	0.233	-0.054	0	0.048
Median Age	38.388	35.942	8.777	2.446	2.65	2.691
Citizenship	0.05	0.029	0.045	0.021	0.008	0.024
Turnout (not included in matching model)	0.585	0.516	0.246	0.069	0.088	0.08
N	52	36				

Matched Data	*Means Treated*	*Means Control*	*SD Control*	*Mean Diff*	*eQQ Median*	*eQQ Mean*
Distance	0.633	0.553	0.125	0.08	0.065	0.081
Inmates	1.471	1.529	0.896	-0.059	0	0.118
Crime Rate	0.09	0.094	0.051	-0.003	0.006	0.009
Poverty Rate	0.062	0.072	0.041	-0.01	0.01	0.011
Percent Black	0.611	0.602	0.293	0.009	0.011	0.025
Percent Hispanic	0.034	0.048	0.034	-0.013	0.011	0.013
Vacancy Rate	0.208	0.214	0.054	-0.006	0.014	0.015
Population Density	5125.029	5590.598	3294.086	-465.569	902.096	1138.298
Median Income	50.796	44.083	25.277	6.713	3.151	10.237
Group Quarters Percent	0.007	0.03	0.089	-0.023	0	0.023
Median Age	40.294	35.881	9.032	4.413	4.1	4.85
Citizenship	0.028	0.03	0.046	-0.002	0.003	0.01
Turnout (not included in matching model)	0.597	0.543	0.224	0.054	0.091	0.09
N	34	34				

Table 7: Descriptives for Atlanta Matched Data. Excludes block groups sending inmates to prison in neither time period and in both time periods. Matched using nearest neighbor method without replacement.

	All Charlotte		*Reduced Charlotte*		*All Atlanta*		*Reduced Atlanta*
Constant	3.02E-01	**	4.03E-01	***	1.89E-01		4.82E-01
	(0.10)		(0.10)		(0.22)		(0.31)
Convicted Before Election	-5.57E-02	*	-4.80E-02	**	-1.88E-01	*	6.28E-04
	(0.02)		(0.02)		(0.09)		(0.06)
Inmates	2.61E-02	*	1.62E-02		4.07E-02		-2.37E-03
	(0.01)		(0.01)		(0.03)		(0.04)
Crime Rate	1.63E-01	***	2.08E-01	**	5.56E-01	**	5.10E-01
	(0.05)		(0.08)		(0.20)		(0.74)
Poverty Rate	1.09E-01		3.55E-01		-3.08E-01		1.81E-01
	(0.37)		(0.39)		(0.71)		(0.93)
Vacancy Rate	4.06E-03		-3.19E-01	.	1.32E-01		2.55E-01
	(0.22)		(0.29)		(0.37)		(0.53)
Percent Black	1.22E-01	*	6.98E-02		4.91E-02		-2.42E-02
	(0.06)		(0.05)		(0.14)		(0.20)
Percent Hispanic	-1.94E-01	.	-9.47E-02	***	1.22E-01		-8.83E-01
	(0.11)		(0.11)		(0.46)		(1.73)
Group Quarters Percent	-1.86E-01		-3.10E-01		-3.02E-01		-8.88E-01
	(0.16)		(0.17)		(0.31)		(0.51)
Population Density	-2.89E-06		-4.22E-06		-1.24E-05		-1.75E-05
	(0.00)		(0.00)		(0.00)		(0.00)
Median Age	-2.02E-03		-1.94E-03	*	5.82E-03	.	1.71E-03
	(0.00)		(0.00)		(0.00)		(0.00)
Median Income	3.38E-03	***	2.81E-03		1.85E-03		9.33E-04
	(0.00)		(0.00)		(0.00)		(0.00)
Citizenship	-2.07E-01		-4.15E-01		-3.53E-01		1.33E+00
	(0.19)		(0.17)		(0.47)		(1.55)
Church Density	8.96E-03		-8.46E-03		-1.02E-03		-1.81E-03
	(0.01)		(0.01)		(0.00)		(0.00)
College	4.06E-02		-2.44E-02		-7.03E-02		-8.03E-02
	(0.04)		(0.04)		(0.08)		(0.16)
N	68		72		80		68
Adjusted R^2	.559		.638		.281		-.006

Table 8: Estimated Effect of Incarceration on Neighborhood Voter Turnout. Standard errors in parentheses; * =P < .05; ** P < .01; *** P < .001.

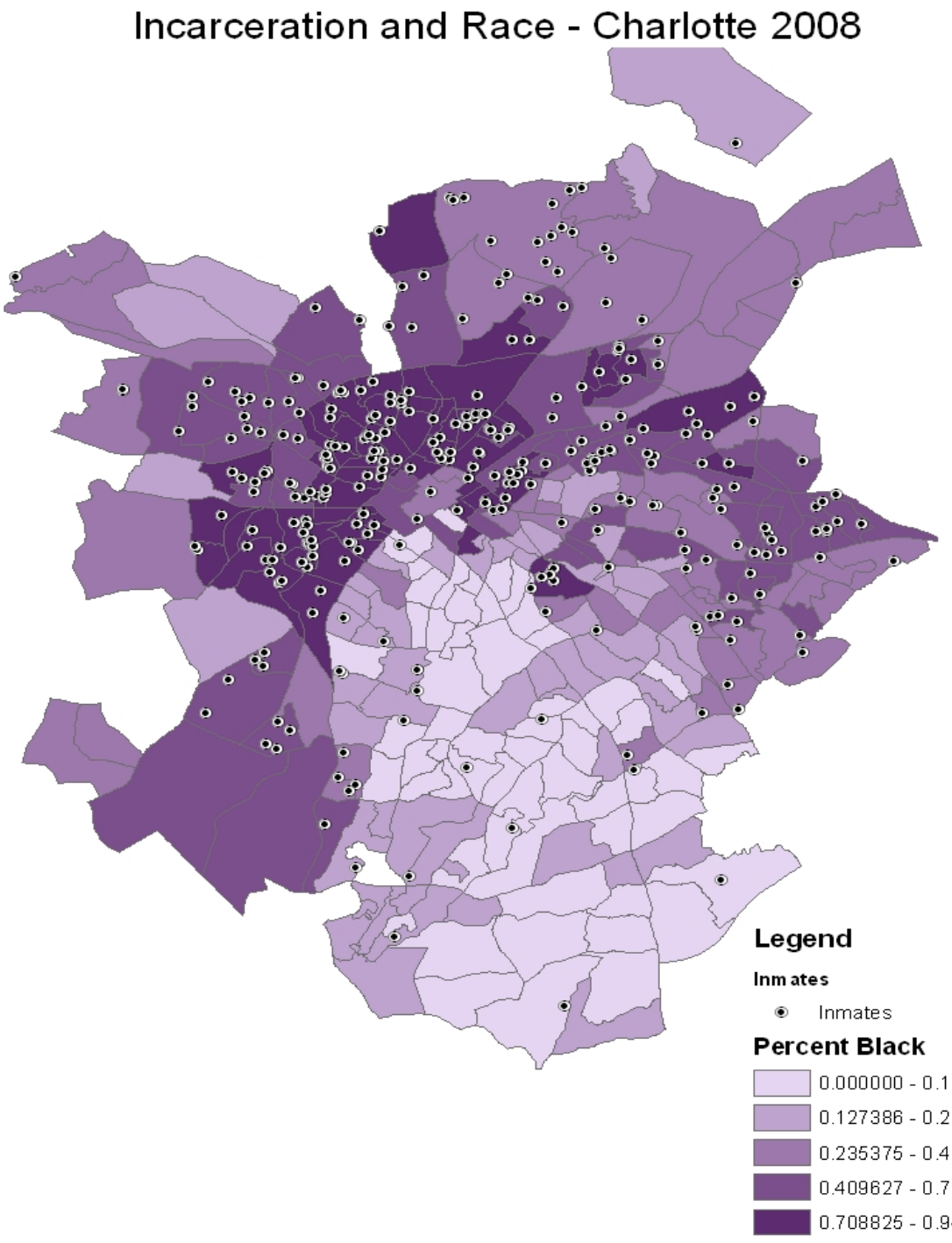

Figure 1: Imprisonment and Race by Block Group, Charlotte, NC. Race is measured as the percent of black residents living in each block group in 2008. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the block groups and include inmates from neighborhoods in which offenders were sentenced both before and after Election Day.

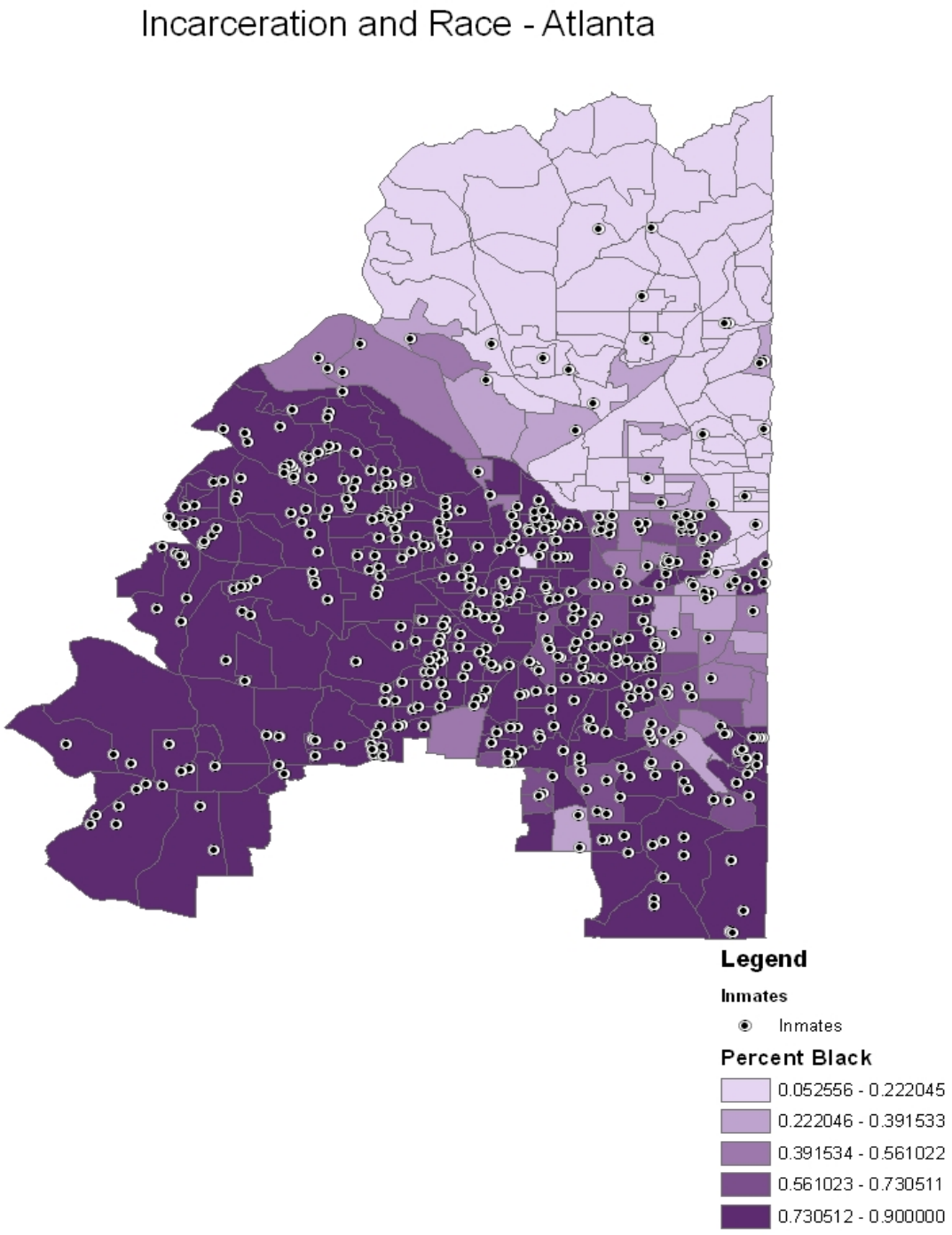

Figure 2: Imprisonment and Race by Block Group, Atlanta, GA. Race is measured as the percent of black residents living in each block group in 2008. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the block groups and include inmates from neighborhoods in which offenders were sentenced both before and after Election Day.

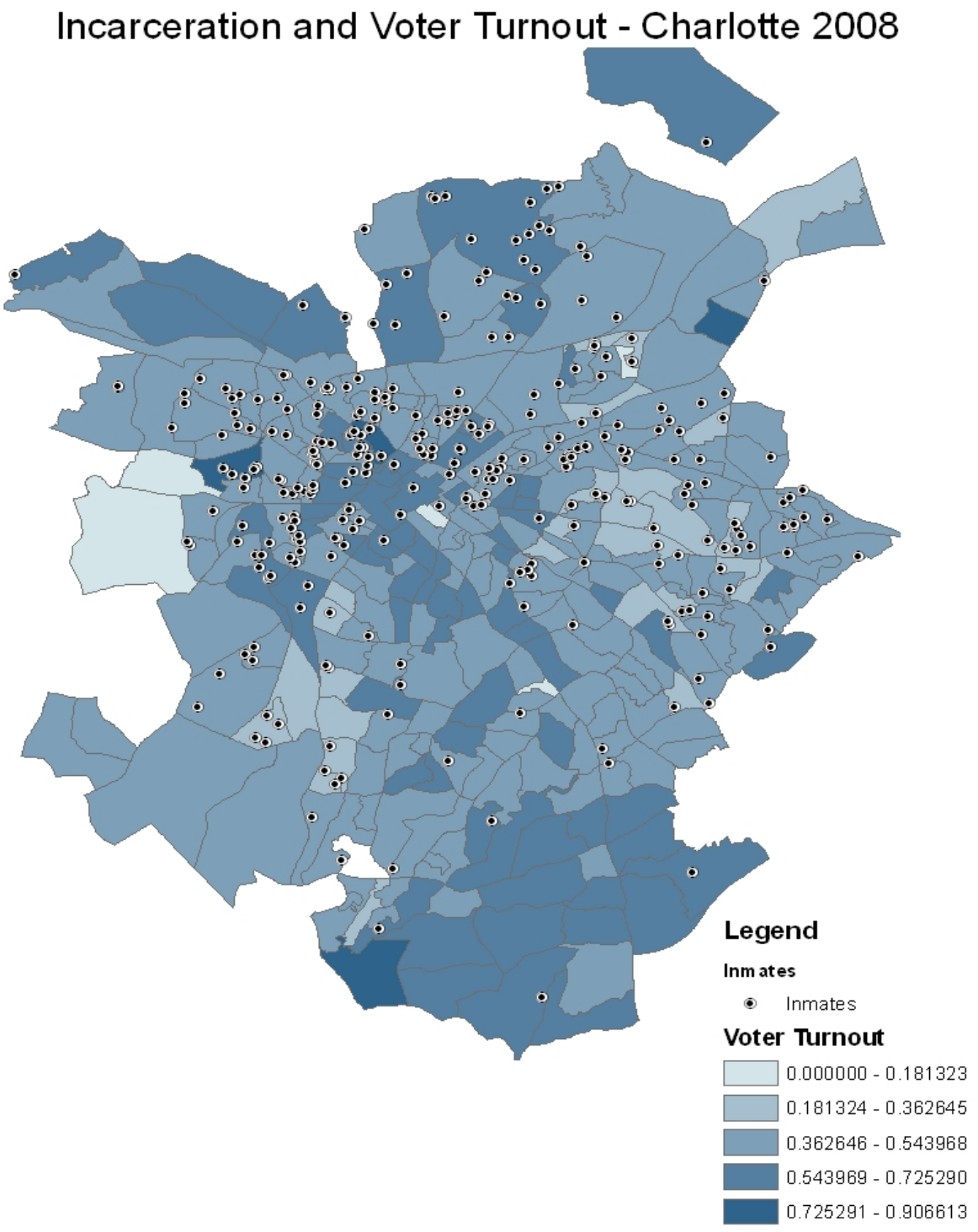

Figure 3: Imprisonment and Turnout by Block Group, Charlotte, NC. Voter Turnout is measured as a count of voters who voted in each block group in the 2008 general election, divided by the 2008 estimated adult population in each block group. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the block groups and include inmates from neighborhoods in which offenders were sentenced both before and after Election Day.

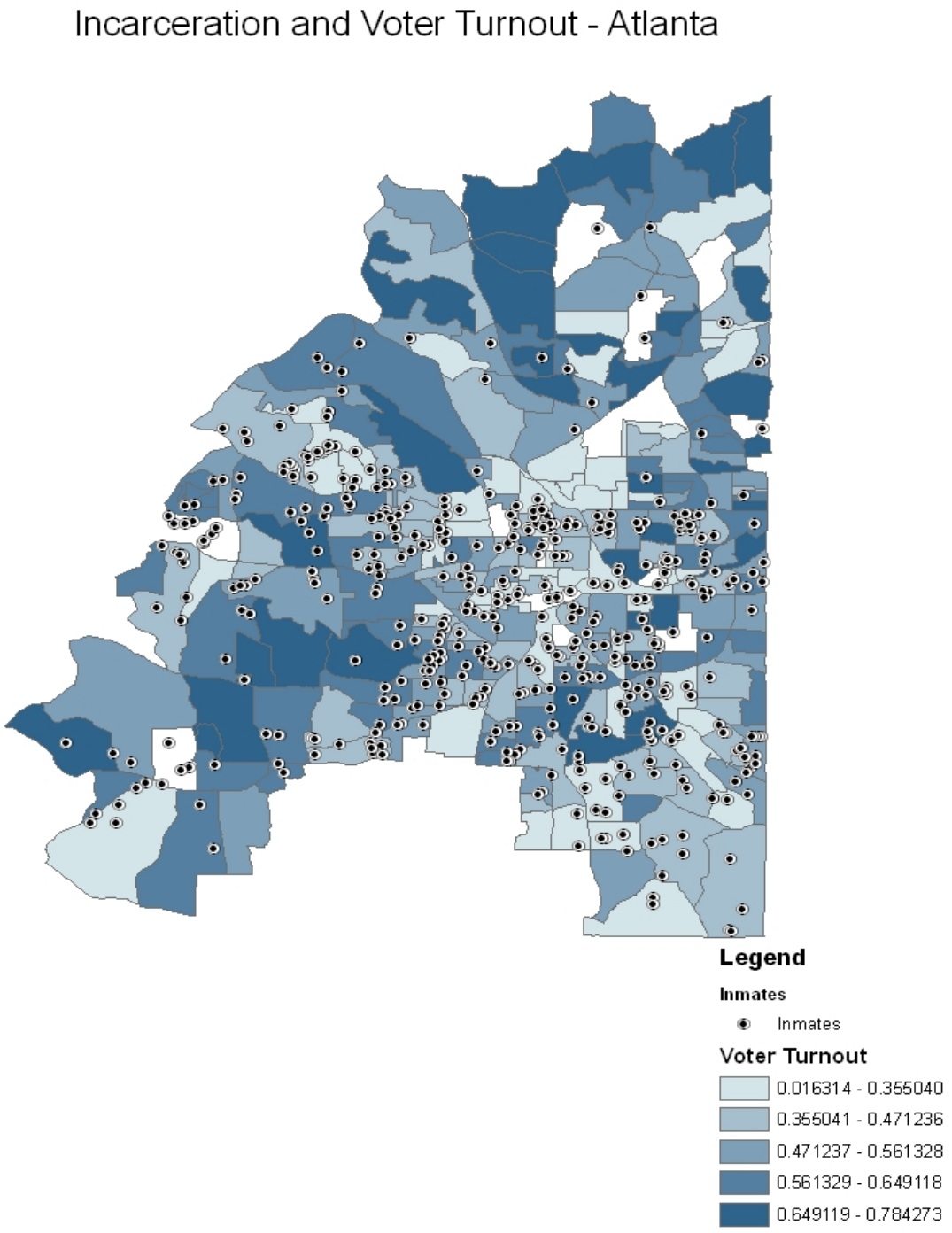

Figure 4: Imprisonment and Turnout by Block Group, Atlanta, GA. Voter Turnout is measured as a count of voters who voted in each block group in the 2008 general election, divided by the 2008 estimated adult population in each block group. Points representing the last known residence of inmates imprisoned during the study period are superimposed over the block groups and include inmates from neighborhoods in which offenders were sentenced both before and after Election Day.

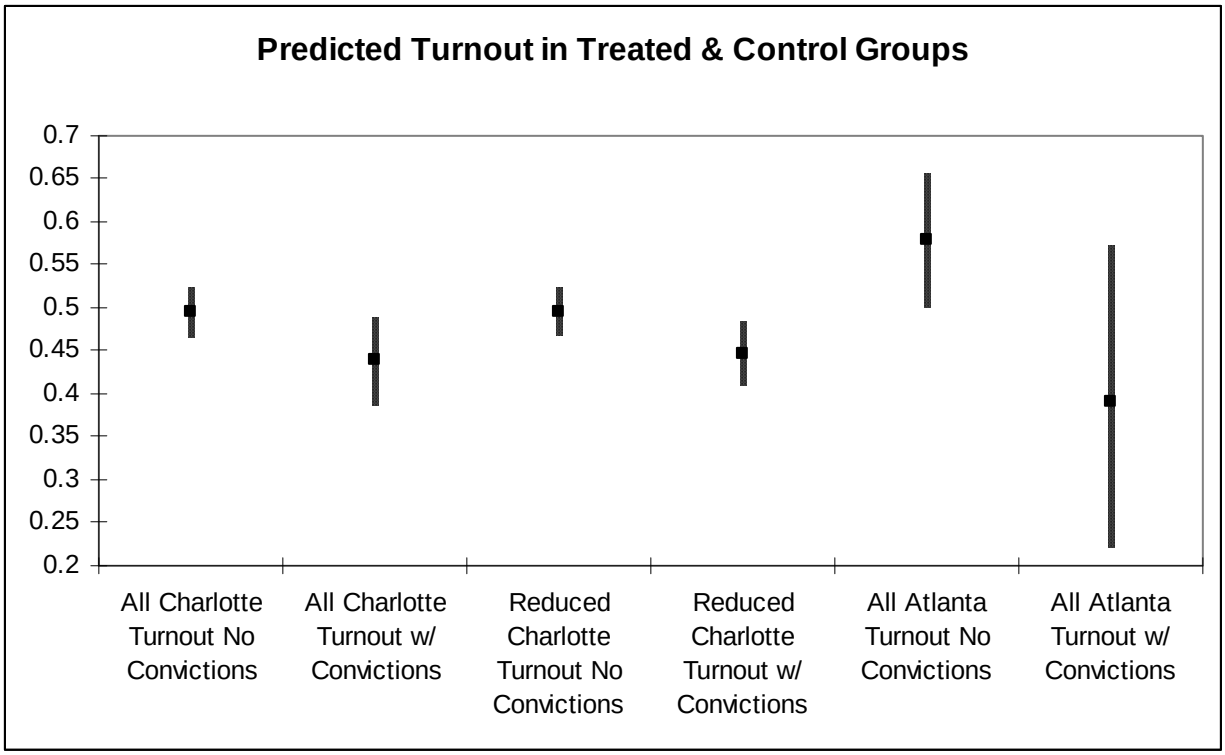

Figure 5: Predicted Turnout across Neighborhoods. The dashed lines surrounding the point represent the 95% confidence intervals. The number of residents sent to prison was set to 1. All other variables are set at their means.